

# Experimentation with Human Beings



# Experimentation with Human Beings

*The Authority of the Investigator,  
Subject, Professions, and State  
in the Human Experimentation Process*

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*with the assistance of*  
**Alexander Morgan Capron**  
*and Eleanor Swift Glass*

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**To My Father**



# Preface

Shortly after joining the Yale Law School faculty thirteen years ago, I came upon the Nuremberg proceedings against the Nazi physicians in a casebook on criminal law then being written by my colleagues Richard C. Donnelly, Joseph Goldstein, and Richard D. Schwartz.<sup>1</sup> The excerpts they had selected, and the voluminous trial transcripts which I read later on, recounted in sickening detail the “medical experiments” conducted in the concentration camps “with unnecessary suffering and injury and . . . very little, if any, [protection of subjects from] injury, disability, or death.”<sup>2</sup> In reflecting on these documents I increasingly thought that the victims of those investigations deserved a thoroughgoing exploration of the entire human research process in order to impede a repetition of such atrocities. Yet had I begun to work on this book then, my preoccupation with and intense feelings about the concentration camp experiments would have dominated it and limited its value. Investigators would have felt unfairly compared to the lowest common denominator in their ranks and, even more important, students of human experimentation, reacting to the unparalleled cruelty of those studies, would have been tempted either to condemn the entire research process or to deny that the exploitation of research subjects, as revealed at Nuremberg, is an ever-relevant problem for human experimentation. Thus, as I read more about research with human beings and began to teach a seminar on the topic, I realized that if a book were to result, it should be designed to provide a climate for the scholarly analysis of the human experimentation process. Only a thoughtful and persistent educational effort, for which this volume seeks to furnish a set of materials, can bring about real change in long-standing practices and thereby give some meaning to the suffering of those who were harmed by human experimentation against their will.

As I became increasingly involved in the world of law, I learned much that was new to me from my colleagues and students about such complex issues as the right to self-

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<sup>1</sup> *Criminal Law*, New York: The Free Press (1962).

<sup>2</sup> Judgment of Beals, Sebring and Crawford, JJ. at pp. 305–306 *infra*.

determination and privacy and the extent of the authority of governmental, professional, and other institutions to intrude into private life. Although these issues affect the interactions of physician-investigators with patient-subjects and of the professions as a whole with the research process, they had rarely been discussed in my medical education. Instead it had been all too uncritically assumed that they could be resolved by fidelity to such undefined principles as *primum non nocere* or to visionary codes of ethics.

I also came to face the same issues personally in reviewing my own research on hypnotic dreams. I had abandoned these investigations a few years before coming to the law school because of an uneasy concern, not fully conscious at the time, about their possible psychological reverberations, however helpful or harmful, in my volunteer subjects. What troubled me was not so much the nature of the project but rather that, before proceeding, I had not posed for myself many more questions about the extent of my personal and professional duty to discuss with my subjects the impact such investigations might have on them.

Thus, in reflecting about the revelations at Nuremberg, my own education and my research experience, I realized that medicine has neglected to address itself to an important educational task. My subsequent contact with professionals from other disciplines made me aware that they too have much to learn from the exploration of the questions posed by human experimentation, for that exploration would lead them to confront similar long neglected issues in their professional interactions with human beings, be they called clients or patients rather than subjects. Lawyers have a special obligation to study these problems because they may come before courts in an increasing number of cases, and their judicial resolution would benefit from prior legal scholarship. Moreover, there is a pressing need for lay readers and public officials to become aware of these issues, and the references throughout this volume to "students of human experimentation" include such readers as well as those involved in classroom study.

Because the study of human experimentation, by the very nature of its intricate and delicate subject matter, evokes strong emotions, I found it necessary, as will the reader, to remain alert to the impact of deeply held convictions and value preferences. They need not be brushed aside, but must be identified so that they can be examined and assigned their proper place in the analysis of these issues. Otherwise the questions posed will remain distorted and obscure the problems which require decision. Furthermore, the reader who immerses himself in these materials will soon learn, as I did, that there are no easy answers to the questions raised by human experimentation in which, as in all human endeavors, a variety of values, interests, and costs press for recognition. Thus, the reader will be disappointed if he expects "correct" or "ethical" solutions to leap off the pages of this volume. Yet since choices have to be made and prices have to be paid, it is at least possible to try to do so more thoughtfully and with greater conscious recognition of the values to be preferred or to be neglected. It is to these goals that this volume speaks.

In light of these personal and scholarly considerations, I began work on the book seven years ago with my colleague and friend Richard C. Donnelly. His tragically premature death, shortly after we had embarked on our initial explorations, made me proceed alone, though in recent years with the able assistance of Alex Capron and Eleanor Glass. And,

from the beginning, an ever-renewing collaboration was established with my students at the Yale Law School who contributed much to the development and revision of the book's many drafts.

The book presents materials from many sources, organized around an analytical framework which is explained in the introduction to the volume. That introduction and those provided at the beginning of every Part and chapter, as well as for certain sections within chapters, are intended to raise some of the questions which students should entertain in examining the materials that follow. Beyond such aids the book is deliberately designed to permit students and teachers to come to their own conclusions about the ordering of the human experimentation process.

Though the volume is conceived along the general lines of a law school casebook, it is not intended solely, or even primarily, for students of law. It is addressed to students, both graduate and undergraduate, of many disciplines. The legal "case method" has been transplanted successfully to other settings before. The use of clinical cases to instruct medical students originated in a suggestion made at the turn of the century by Dr. Walter B. Cannon, then a student at the Harvard Medical School. Impressed by his law school roommate's enthusiasm for the case method of instruction at the Harvard Law School, Cannon urged his professors to adopt the system for medical teaching.<sup>3</sup> Under the leadership of Dr. Richard C. Cabot, the "Clinicopathological Conferences" became an integral part of medical education, both for Harvard students and, through their weekly publication in *The New England Journal of Medicine*, for students and practitioners around the world. If students and teachers of human experimentation outside the law find the present adaptation of the case approach a useful one, they may wish to employ it more widely in place of the didactic method, still all too prevalent within many disciplines. Even more important, I hope that this volume will stimulate the kind of interdisciplinary teaching I have found so gratifying.

The work on this book has been assisted by an appropriation from Russell Sage Foundation, and I am grateful to its president, Orville G. Brim, Jr., whose encouragement and interest went far beyond financial support. Another study undertaken pursuant to a grant from the National Center for Health Services Research and Development<sup>4</sup> has also contributed substantially to the analysis developed in this volume. Over the years Dean Louis H. Pollak and his successor Abraham S. Goldstein have not only provided additional funds but also the academic atmosphere in which I could feel happily at home. I have profited from the critical stimulation of friends and colleagues, most particularly Robert C. Arnstein, Alexander M. Bickel, Renée C. Fox, Joseph Goldstein, Quintin Johnstone,

<sup>3</sup> "Undoubtedly the most brilliant example thus far of the use of cases in education is seen in the study of law. The change from the textbook to the case system wrought out in the Harvard Law School has been called America's greatest contribution to educational reform. The newer method has roused an ardor and a keenness of interest among the students such as was never known before. They learn their law not by dreary grubbing at text-books or lecture notes, but by vigorously 'threshing out a case' with one another." W. B. Cannon: "The Case Method of Teaching Systematic Medicine," 142 *Boston Medical and Surgical Journal* 31, 36 (1900). See also W. B. Cannon: "The Case System in Medicine," *ibid.* at 563.

<sup>4</sup> "Social Factors Affecting the Modern Treatment of Catastrophic Diseases," Contract No. H.S.M. 110-69-213, Health Services and Mental Health Administration, Department of Health, Education and Welfare.

Joseph Katz, Arthur A. Leff, Charles E. Lindblom, Leon Lipson, Ernst Prelinger, Roy Schafer, Vaughn Stapleton, and Stanton Wheeler. I am especially indebted to Douglas Rosenthal who read the entire manuscript and made valuable suggestions which are reflected throughout the book. Among the many students who have played a special role in the preparation of this volume I thank Robert Carter, Julian Fisher, Ronnie F. Heyman, Alan Meisel, John C. Ladd and Leonard S. Spector.

I am particularly grateful to Brad Gray, Ernst Prelinger, Derek DeS. Price, and Charles L. Remington for contributing articles specifically written for this book. Though I do not acknowledge them by name, I also wish to express my thanks to the authors who gave permission to print unpublished manuscripts or to reproduce excerpts from their writings. Among the latter I am specially appreciative of those who, in the spirit of free inquiry, granted reprint permission even though they had reservations about the purpose of this volume.

I owe a great deal to the assistance of the staff of the Yale Law School library, particularly Robert E. Brooks, Arthur Charpentier, Gene Coakley, James M. Golden, Isaiah Shein, Solomon C. Smith, and Iris Wildman. They met my unending requests for books and magazines, sometimes most difficult to locate, with gracious and dedicated efforts which speeded this venture to completion.

I thank my secretary Kathy Lewis for her expert and cheerful typing of numerous drafts of the manuscript; Elizabeth Albert, Elsa Dixler, Dorothy Egan, Isabel Malone, Doris Moriarty, Walter Moriarty, and Kathy Murray for their assistance in preparing the manuscript for publication; Janet Turk for her skillful copyediting and preparation of the index; and Jean Yoder and William Bennett of Russell Sage Foundation for the thoughtful and understanding direction they gave to the production of this volume.

I have saved for the end two most important acknowledgements: First, my gratitude to my wife Esta Mae and my children Sally, Daniel, and Amy who over the years, with minimal complaints, allowed me the time to work on this book. Their feelings for me and my work made the completion of this book possible, and I am grateful for their understanding collaboration. Finally, I thank Eleanor Glass and Alex Capron who have worked closely with me over the years. Eleanor assisted me most with earlier drafts when we were still searching for materials and trying out a variety of organizing schemes. Alex, for the last year and a half, has involved himself in this book with an intensity and intellectual support which significantly affected its final shape. It was during this period of time that in daily meetings we edited and re-edited the selections, refined and substantially reworked the book's conceptual framework, and wrote the textual introductions. My debt to him is great. My appreciation to both is reflected on the title page.

J. K.

New Haven, Connecticut  
January 1972

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# Introduction

When science takes man as its subject, tensions arise between two values basic to Western society: freedom of scientific inquiry and protection of individual inviolability. Both are facets of man's quest to order his world. Scientific research has given man some, albeit incomplete, knowledge and tools to tame his environment, while commitment to individual worth and autonomy, however wavering, has limited man's intrusions on man. Yet when human beings become the subject of experimentation, allegiance to one value invites neglect of the other. At the heart of this conflict lies an age-old question: When may a society, actively or by acquiescence, expose some of its members to harm in order to seek benefits for them, for others, or for society as a whole?

Recent experience with human experimentation in a variety of disciplines has prompted renewed concern among the professions and the public that the present regulation of the research process is unsatisfactory. Some critics call for increased governmental controls, more detailed codes of ethics, more powerful professional review committees, or more active participation of nonscientists in research decisions. Others fear that involvement of outsiders or more stringent controls will "put a ceiling price on truth" and dry up all reservoirs of creativity and scientific progress. Yet perhaps the most pervasive viewpoint is that experimentation cannot be rationally controlled. Before accepting any of these judgments it is the task of the student of human experimentation to seek answers to three questions: (1) What limits, if any, should be placed on scientific inquiry, and what implications do these limits have for

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society's democratic and egalitarian aspirations? (2) Who should have the authority to formulate these limits? (3) By what means should they be imposed?

In searching for answers to these questions, this book examines and evaluates the authority which should be vested in each of the chief participants in the human experimentation process—the *investigator* who initiates and conducts the experiment, the human being who is its *subject*, and the *professions* and the *state* which appraise, support, or restrict research. These participants provide the book with its structural framework. Following Part One, which presents an introductory view of the problems raised by experimentation with man, the three succeeding Parts scrutinize, respectively, the roles of investigator, subject, and professional and public institutions. The participants are introduced in this sequence in order first to evaluate the problems which arise if total authority is assigned to investigators and to identify their special qualifications and values, then to examine the competence of subjects (and their parents, spouses, or guardians) to collaborate in decisionmaking, and finally to explore the capacity of the professions and the public to play a role in the experimentation process. All the participants have unique and conflicting constellations of motivations, capacities, and value preferences by which they chart their courses. Therefore, to analyze the tensions which arise between the participants over the objectives and conduct of research, one must not only identify each actor's values but also assess his capacity and willingness to act upon them.

To sharpen the analysis, the book also adopts a functional framework based on what are seen as three basic stages in the process of making decisions about human experimentation—the *formulation* of research policy, the conduct and supervision of research which comprise its *administration*, and the *review* of research and its consequences. This complementary approach is grounded in the assumption that the authority to be assigned to each participant is not identical throughout the human experimentation process. We suggest, instead, that the nature and extent of this authority ought to be shaped and defined by the different issues and particular problems which require resolution at the three stages of the process. For example, investigators have asserted that the conduct of research should be left largely to their discretion once they have fulfilled certain obligations toward their subjects. Whatever the merits of this claim, it need not suggest that they should be given similar authority over the formulation of research policy or the review of the research enterprise.

Human experimentation cannot be analyzed in isolation, however, for inherent in its dynamics are several ubiquitous forces which shape all social interaction—man's quest for knowledge and mastery, his willingness to risk human life, and his readiness to delegate authority to professionals and to rely on their judgment. These general themes are first examined in Part One, and their importance to a definition of the participants' roles continually reemerges in Parts Two and Three. In those Parts, we have employed the structural and functional frameworks to highlight a number of issues which must be scrutinized if the student is to move toward increasingly precise conclusions about the authority that should be assigned to investigators and subjects. Thus Parts Two and Three are organized around such tasks as identifying categories of harm and other significant elements in experimental interventions (e.g., the investigator's attitude toward and method of selecting subjects and his ability to predict the consequences of his work), as well as examining the extent and limits of man's

capacity for insightful self-determination. In Part Four the structural-functional matrix becomes the explicit frame of reference, for the student must not only evaluate the role of the professions and the state but also integrate this evaluation with his prior analysis of the role of investigators and subjects. While this framework and the issues it highlights (*e.g.*, the definition of "harm") have proved to be useful analytic tools, they are nothing more than that. None of the problems for decision can be neatly confined either to a single participant or to a single stage in the process. Problems about the role of each participant must be re-examined in light of the capacities and incapacities of the other participants to make decisions. Similarly, the student's conclusions about problems of administration will need to be rethought in light of his examinations of, and conclusions about, formulation and review mechanisms. Thus the evaluation of the authority of each decisionmaker at each point in the process ought ideally to rest on an analysis of the roles played by all decisionmakers at all points in the process. In the end, the student of human experimentation may also wish to reappraise the conceptual categories proposed in this book.

In selecting the materials for this volume we have not been constrained by a single definition of what constitutes human experimentation. Consequently, a threshold question must be posed: What is human experimentation, and for what purposes should it be defined? The ambit of experimentation is blurred on at least two borders: First, it may or may not include "poorly designed" or "fruitless" research. Second, an intent to give or receive "benefits" may or may not remove an intervention from the category of "experiment." The materials do not draw hard and fast lines between interventions for the "acquisition of knowledge" or for the "subject's benefit" so as to leave open the question whether the authority assigned to the participants, and any restrictions imposed upon them, in one setting should apply equally to the other.

This volume includes experimental studies from several disciplines—medicine, psychology, sociology, biology, and law—and materials from many sources—trial transcripts, congressional hearings, panel discussions, appellate decisions, administrative regulations, editorial comments, legislation, private agreements, scholarly publications, and newspaper stories. These have been interwoven with commentary from philosophy, political science, economics, genetics, medicine, anthropology, psychoanalysis, biology, jurisprudence, psychology, theology, and literature. Both the relevance and the reliability of these materials, as data and as evaluations, must be subjected to critical appraisal. Scrutinizing case studies about events in the past can sometimes seem petty, sterile, or disheartening. Yet any hope for a better and more thoughtful resolution of the issues in the future depends on our willingness to engage in unstinting examination of the past, as imperfectly as it may be recalled.

This book is addressed to students of human experimentation, be they actual or potential decisionmakers. Although the questions posed throughout this volume may seldom be raised explicitly in the course of experimentation, answers to them are implicit in all decisions concerning human research. Only by bringing these questions to a conscious level can the process be subjected to much needed scrutiny. To begin this task, the student of human experimentation may find it useful to consider the following questions:

1. What goals should man seek to achieve by scientific inquiries? Are these desirable in themselves or as means to desirable but more remote ends?

#### 4 INTRODUCTION

2. What value conflicts arise in the human experimentation process?
  - a. Under what circumstances should significance to science and society outweigh what rights of patients and subjects?
  - b. Who, under what circumstances and by what means, should have the authority to make decisions about experimentation in order to mediate these value conflicts?
  - c. What procedures will permit decisionmakers to examine advances in scientific knowledge and technique so as to minimize undesired results?
3. What interventions should be labeled “experimental”?
  - a. What consequences should follow the designation “experimental” and how do they differ from the consequences of other designations?
  - b. How do the consequences differ once investigations move from theory to studies on inanimate objects and then to experimentation with animals, investigators themselves, other individuals, groups, or society?
4. What constitutes a harmful intervention?
  - a. To what extent should the degree or type of harm to individuals or society affect the authority of decisionmakers?
  - b. To what extent is the harm of an intervention mitigated by what immediate or long-range, certain or uncertain, benefits, and to whom should benefits accrue?
  - c. To what extent is the harm of an intervention aggravated or mitigated by an explanation of the risks and benefits involved?
  - d. To what extent should knowledge or lack of knowledge about harm affect the authority of decisionmakers?
5. Under what circumstances and to what extent should the consent of the subject or patient affect the decision to intervene?
  - a. To what extent should the capacity of subjects or patients to comprehend, to communicate, or to make rational choices affect the validity of consent?
  - b. How and to what extent should this capacity be evaluated?
  - c. Under what circumstances should the balancing of risks and benefits be left to the persons affected and when, if ever, should other decisionmakers impose limits on risktaking?
6. To what extent and for what purposes should a coexisting intention to give or receive “benefits” affect the authority of decisionmakers?
  - a. Who, and by what standards, has the authority to decide whether an intervention is “beneficial”?
  - b. Should any constraints imposed on the participants in experimental settings apply equally to therapeutic ones?
7. What persons and institutions should have the authority to formulate, administer, and review the human experimentation process?
  - a. What qualifications should these participants possess at the various stages in this process?
  - b. What information should be supplied to these participants and by what procedures?
  - c. To what extent should the authority of what persons and institutions be modified



once a subject is labeled "normal volunteer," "patient volunteer," or "patient-subject"; "competent," "uncomprehending," "captive," or "dying"?

d. Who should determine the limits of experimentation and how adequate are the procedures for making such decisions?

8. What procedures should be established for obtaining data and evaluating their relevance to decisions being made throughout the process?

If this book in some measure documents man's inhumanity to man, it only serves to remind us how pervasive that phenomenon is. Human experimentation has been severely criticized on this ground. Yet in raising questions about experimentation we do not intend to indict science or stifle research, for the failure to experiment is equally an experiment which may also have unsatisfactory consequences. The real need to which this volume speaks is for greater conscious awareness and relentless scholarly analysis of the conflicting purposes of human experimentation—protecting man, advancing science, and improving the well-being of society and future generations. Only if students and decisionmakers are prepared to sort out these conflicts and to acknowledge the reality of harm to individuals and society can they begin to formulate rules and procedures which will minimize harm without erecting insuperable impediments to the acquisition of knowledge. In addressing this task for human experimentation, significant contributions may also be made to decisionmaking in other areas of law, science, and politics, for the conflicts presented in this volume are inherent in all affairs conducted by and with man.



## **PART ONE**

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# **An Introduction to the Human Experimentation Process**

This Part introduces the major issues raised by experimentation with human beings. Two cases, one from medicine and one from law—social science, present the chief participants in the human experimentation process—the investigator, the subject, the professions, and the state—and bring into view their expectations, capacities, and value preferences. These first two chapters should begin to raise questions about the rights and duties of each participant and about the rules and procedures for resolving conflicts among them.

Research conducted by professional investigators with human subjects is only one piece in the mosaic of ubiquitous experimentation in society, for both individuals and society continuously try out new ways of comprehending and improving the human condition. To understand the problems posed by the human experimentation process it is vital to appreciate the larger societal context in which the interactions between investigators and their subjects are embedded. Chapter Three therefore scrutinizes three sets of conflicting forces which influence human experimentation: man's quest for the extension of knowledge and technology versus his fear of the unknown; man's willingness to risk human life versus his urge to protect it; and man's readiness to delegate authority to experts versus his desire to govern his own fate.

Finally, we pause briefly in Chapter Four to examine a variety of decisionmaking theories which may serve as useful models for making decisions about human experimentation. These materials provide the background for analyzing the roles of the participants at various stages in the process as it unfolds in succeeding chapters.

## 8 INTRODUCTION TO THE HUMAN EXPERIMENTATION PROCESS

Throughout we ask:

1. What are the criteria for designating an intervention an experiment? How do experiments differ from other interventions into the lives of human beings?
2. What value conflicts arise among the participants in the human experimentation process?
3. Who, under what circumstances and by what means, should have authority to make decisions about the formulation, administration, and review of the human experimentation process?
4. For what purposes is it useful to distinguish between the various stages of this process?

## CHAPTER ONE

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# The Jewish Chronic Disease Hospital Case

In July 1963, three doctors, with approval from the director of medicine of the Jewish Chronic Disease Hospital in Brooklyn, New York, injected "live cancer cells" subcutaneously into twenty-two chronically ill and debilitated patients. The doctors did not inform the patients that live cancer cells were being used or that the experiment was designed to measure the patients' ability to reject foreign cells—a test unrelated to their normal therapeutic program.

The cancer experiment engendered a heated controversy among the hospital's doctors and led to an investigation by the hospital's grievance committee and board of directors. William A. Hyman, a member of the board who disapproved of the experiment, took the hospital to court to force disclosure of the hospital's records, claiming that the directors' approval of the experiment had not been properly obtained. As *Hyman v. Jewish Chronic Disease Hospital* wound its way up from the trial court through two appellate tribunals, it became clear that the legal issue involved in the suit, whether a hospital director is entitled to look at patients' medical records, only provided the backdrop for the questions really at issue which concerned the duties and obligations that the various participants in the human experimentation process should have toward one another.

Subsequently, these issues were confronted more directly when the Board of Regents of the University of the State of New York heard charges brought by the attorney general against two of the doctors involved. The board imposed sanctions, under the authority given it by New York Education Law § 6514(2) to revoke, suspend, or annul the license of a prac-

tioner of medicine upon determining "after due hearing . . . that a physician . . . is guilty of fraud or deceit in the practice of medicine [or] that a physician is or has been guilty of unprofessional conduct."

In examining these materials, consider the following questions:

1. What values does human experimentation seek to implement, and are they in conflict with other values?
2. How do the participants weigh these conflicting values, and what weight should be given to these values?
3. What values are preserved or undermined by delegating decisionmaking power to each participant respectively?
4. Under what circumstances should the extent of actual or potential harm and benefit to subjects or society affect the authority of each participant in the human experimentation process?

## A.

### How and by Whom Should Research Policy Be Formulated?\*

**Letter from Chester M. Southam, M.D.  
to Emanuel Mandel, M.D.—July 5, 1963**

I want to thank you for the courtesy shown to me and Dr. Levin on our recent visit and the interest that you showed in our proposed research collaboration. This letter is to record and perhaps clarify the principal points of that conversation.

The study we discussed would permit evaluation of the immunologic status of patients with chronic non-neoplastic diseases, as revealed by promptness of rejection of subcutaneous cancer cell homografts. My own interest in these studies stems from their importance to the understanding and possible treatment and diagnosis of cancer, but I am sure that you would have an equally great interest in their potential importance for the understanding of autoimmune and degenerative diseases and in the budding field of organ homotransplantation.

Clinical research on this phenomenon is quite new—my own work started only ten years ago—but is accelerating rapidly as would be expected from its importance and as attested by the recent entry of several hospitals and research

institutes into the fields of cancer, skin, and organ transplantation in man. To date the studies carried out by me, with numerous collaborators here and at the Ohio State University Medical School, have revealed that healthy persons reject the cancer cell homografts completely and promptly (in 4 to 6 weeks) as one would obviously predict, but many patients with widespread cancer have a delayed rejection (over 6 weeks and sometimes 3 months or more). In either group of recipients the usual reaction is development of a painless subcutaneous nodule up to 2 or 3 cm in diameter at the time of maximum development. The immunologic derangement responsible for the comparative slowness of rejection in patients with cancer is still unidentified, but the search is narrowing down and an impairment of cell-associated immune mechanisms now seems probable.

There is a gap in our data in that we have not yet studied this reaction in people who do not have cancer but who do have chronic and debilitating diseases of other kinds. I would expect that the homograft rejection reaction would be normal or near normal in such patients. This estimate is based on results of scattered studies of skin homografts by others and on our recent demonstration of intact macrophage mobilization (a non-specific cellular immunologic mechanism) in such patients, whereas in cancer patients macrophage mobilization is depressed and correlates with homograft rejection. But suppo-

\* This is an actual record. Without indicating deletions we have edited the record primarily to reduce repetition. Some repetition was necessary, however, to allow each participant to present his own understanding of the events.

sitions are not knowledge and it is the need for direct evidence on this point that brought me to you.

We do not have patients with debilitating diseases other than cancer at Memorial or James Ewing hospitals, and therefore we are seeking collaboration in some hospital with a large population of such patients. The Jewish Chronic Disease Hospital was suggested to me as a hospital which had not only the patients but also an interest in medical teaching and research, as evidenced by the Isaac Albert Research Institute and by its teaching arrangements with Kings County Hospital.

The procedure, as I explained, requires simply the hypodermic injection of a suspension of tissue-cultured cells at two sites on the anterior thigh or arm and observation of the sites at about weekly intervals for six weeks or until regression is complete. These cells are of two or more cancer cell lines. These cancer cell lines were chosen because they have the necessary growth capacity to produce a measurable reaction. It is, of course, inconsequential whether these are cancer cells or not, since they are foreign to the recipient and hence are rejected. The only drawback to the use of cancer cells is the phobia and ignorance that surrounds the word *cancer*. It would be possible to study the same process by experimental skin grafts, but this is less satisfactory for quantitation, is much more difficult technically, and is unacceptably annoying to your patients. Other than the two hypodermic injections and observation of the reaction, the only other procedure would be drawing serum for study of antibody reactions to the trans-

planted cells at approximately two-week intervals during the observation period.

I have no hesitation in suggesting these studies since our experience to date includes over 300 healthy recipients and over 300 cancer patients, and for two years we have been doing the tests routinely on all postoperative patients on our gynecology service as a measure of immunologic status, with the collaboration of Dr. Alexander Brunschwig, chief of the gynecology service. You asked me if I obtained (written) permissions from our patients before doing these studies. We do not do so at Memorial or James Ewing hospital since we now regard it as a routine study, much less dramatic and hazardous than other routine procedures such as bone marrow aspiration and lumbar puncture. We do get signed permits from our volunteers at the Ohio State Penitentiary but this is because of the law-oriented personalities of these men, rather than for any medical reason.

Collaboration in this research effort would involve no expense to the Jewish Chronic Disease Hospital or its patients since these studies are supported by a grant from the United States Public Health Service and the American Cancer Society, and I would supply all cultures and equipment from my laboratory. On the other hand, the Jewish Chronic Disease Hospital and collaborators there would be appropriately acknowledged in such scientific papers and lay publications as may ensue, subject of course to your prior approval.

I hope that this opportunity for research continues to interest you and that you will find it possible to participate in this program.

## B.

### How and by Whom Should the Research Process Be Administered?

#### 1.

#### **Petition of William A. Hyman— December 12, 1963**

To the Supreme Court of the State of New York, Kings County:

The petitioner, William A. Hyman, respectfully states as follows:

That the said Jewish Chronic Disease Hospital is governed by a board of directors.

That your petitioner is still a member of said board of directors and continues to act as such director.

That in the month of September, 1963, your petitioner was informed that injections of live cancer cells had been made and were being made into non-cancerous patients at the hospital without their consent, either written or oral, and without their knowledge of the nature of the injections and that these injections were not for purposes of therapy or treatment of patients at the hospital but were done for the purpose of determining whether cancer can be induced by injection of live cancer cells and that, furthermore, some certain medical employees of the hospital were undertaking these experiments in

## 12 JEWISH CHRONIC DISEASE HOSPITAL CASE

cooperation and in concert with certain parties not affiliated with the said hospital and that all of this was being done without the approval, sanction, authorization and consent of the proper authorities of the said hospital.

That on September 30, 1963, a meeting of the board of directors of the hospital was held at which time and place Solomon Siegel, executive director of the hospital, read a report which purported to show that these injections of live cancer cells into non-cancerous patients were done with the oral consent of these patients. Furthermore, at this said meeting of the board of directors on September 30, 1963, Benjamin Saltzman, chairman of the executive committee of the said hospital, orally reported on a hearing held by him and certain associates and stated that the injection of live cancer cells into non-cancerous patients were harmless "tests" although he admitted before the board of directors that the patients who received such injections were never informed that live cancer cells were being injected into them but rather they were told that these were skin tests.

That your petitioner strenuously opposed the acceptance of the written report of the said Solomon Siegel, executive director of the hospital, and the oral report of said Benjamin Saltzman as a whitewash which imposed upon the board of directors serious civil and perhaps criminal responsibility if the facts as reported to your petitioner were correctly stated and, accordingly, petitioner made a motion that the said report of the said Solomon Siegel be rejected and that an independent committee be appointed to make a further investigation into the circumstances attending the injection of live cancer cells into non-cancerous patients.

Although to the best of my recollection there was a second to this motion by one of the directors present, there resulted considerable disorderly conduct and confusion, and a superseding motion was made to accept the report of said Solomon Siegel but, however, in the midst of the discussions and arguments back and forth at said meeting Mr. Herman W. Shane, chairman of the board of directors, suddenly declared the meeting adjourned.

At this meeting of the board of directors, he called attention to certain of the Nuremberg trials in which Nazi doctors were found guilty and some hanged and some otherwise punished for using human beings for experimental purposes without their informed consent to and knowledge of the experiments being conducted

on them and that such practices could not and should not be tolerated by any organization.

Likewise, when the attention of the directors present at this meeting was called to the fact of the responsibility devolved upon the board of directors, under the circumstances, the suggestions were disregarded and even ridiculed.

The question that he raises is whether those directors who voted to adopt that whitewash report of Mr. Siegel's would permit themselves to be used for experimental purposes. Will these directors who voted for this whitewash report consent to having injections of live cancer cells made into their bodies to see if cancer can be induced in their bodies?

That petitioner, in order to protect the integrity of the hospital and to terminate any possible abuses that may have arisen and to avoid injury to any patients and possible liability therefor on the part of the hospital and of the directors, requested the secretary of the hospital to furnish petitioner, at his expense, with a copy of the minutes of the meeting of the board of directors.

That petitioner's aforesaid request was ignored.

That petitioner wishes to be fully informed of all actions taken by the board of directors of the Jewish Chronic Disease Hospital and by all committees therein relative to the investigations of the complaints made and relative to the findings upon such investigations and to be fully informed of all the facts pertaining to the injection of live cancer cells into patients at the hospital, and petitioner, who has been associated with the said hospital for many years in various capacities, believes that it is his obligation as a director of said hospital to inquire into such happenings, and to ascertain all the facts, and to take adequate steps to protect the patients of the hospital and the good name and reputation of the hospital and of the directors and of the physicians connected with the hospital, and to avoid any possible liability on the part of the hospital and of the directors as a result of any injury that may be suffered by any patient as a result of said injections.

That the Jewish Chronic Disease Hospital, through its executive director and medical director, have contended that although the patients gave no written consent to the injections they gave their oral consent; but said contention is false and entirely without any basis in fact because some of the patients were in such mental and physical condition that they could neither



know and understand the nature of the injections and the danger involved, nor consent to such injections, and other patients could speak only Yiddish, whereas Dr. Custodio could not speak one word of Yiddish, and, therefore, could neither ask for nor obtain oral consents.

That your petitioner has exhausted the remedy of requesting access to and examination and copies of the minutes of the meetings of the board of directors and the reports by Mr. Siegel and Dr. Abramson and the patients' charts and records and the other papers and documents pertaining to the experimental injection of cancer cells into non-cancerous patients, and by refusing petitioner's requests therefor the board of directors of said hospital have failed, neglected and refused to perform a duty enjoined upon them.

Wherefore, your petitioner respectfully prays that pursuant to Article 78, C.P.L.R., an order be made granting the inspection of the books, records, papers and documents sought by the petitioner herein, and granting such other and further relief as to the court may seem just and proper.

## 2.

### Affidavits for Petitioner

a.

#### David Leichter, M.D.\*— September 12, 1963

My name is David Leichter. I am a duly licensed physician in the State of New York having received my license in 1958 in New York State.

I have been associated with the Jewish Chronic Disease Hospital since July 1, 1959, first as chief resident in medicine and since 1960 as co-ordinator of medicine and in charge of cancer therapy and research. In this capacity all projects relating to the field of cancer were within my domain.

On or about July 3, 1963, I was approached by Dr. Emanuel Mandel, who was the director of medicine and medical education of the Jewish Chronic Disease Hospital, about a project which would involve the injection of live cancer cells

into non-cancer patients of our hospital. He stated that two doctors from Memorial Hospital who had done some prior experimental work in this field would supply the hospital with this cancer cell suspension and he asked me to see them and discuss taking over this project.

After this brief discussion I told Dr. Mandel that at first blush, such a project would certainly require the informed consent of the patients on whom it was to be done, and until such prior informed consent was obtained there was absolutely no reason for me to meet with these doctors from Memorial Hospital and, further, that I did not believe such consent could be obtained. By informed consent, I mean discussing the project with the patient, advising him of the dangers, if any, informing him of the agent to be used—in this case live cancer cells. It also means to me that the patient on whom the experiment is to be made must be mentally competent and aware of the full extent and dangers of such a project, and that such consent to be legal and proper would have to be obtained in writing.

On about July 31, 1963, Dr. Avir Kagan approached me and informed me that he had been requested by Dr. Mandel to conduct experiments on chronic patients of the Blumberg Building by injecting in them a suspension of live cancer cells. He told me that he had refused to become a part of this project because he could not see how the informed consent of any of these patients could be obtained once they were aware of the nature of the agent and the purpose of the project.

On or about August 15, 1963, I received a telephone call at my home from Mr. Sol Siegel, who is the executive director of our hospital and he told me that he wanted to see me at 9 o'clock the following morning on a matter of importance.

On August 16, 1963, in his office, Mr. Siegel asked me about this cancer project and what I knew of it. I told him in detail about my first encounter with Dr. Mandel and the fact that I had refused to become a party to it because I felt that it was immoral and illegal without the prior written informed consent of each and every informed patient.

I further told him that I had been informed by Dr. Samuel Rosenfeld, the co-ordinator of medicine of the Blumberg Building, that some 18 patients in his ward had received injections of this live cancer suspension, without his knowledge, without his consent and without the patients' knowledge and informed consent, and

\* The affidavits of Drs. Avir Kagan and Perry M. Fersko, coordinators in the department of medicine of the Jewish Chronic Disease Hospital, are essentially similar to the above affidavit.

under the auspices of Dr. Mandel and Dr. Custodio to whom he had assigned the project. He asked me some questions about the legality of Dr. Mandel's project and I told him that I had attended lectures given by Bryant L. Jones of the CCNSC (Cancer Chemo-Therapy National Service Center) whereby we were informed that it was illegal to administer experimental drugs to patients without their prior informed consent and knowledge and approval.

I also told him of the danger of rumors spreading in the hospital about giving these cancer cells to patients without their consent and the tremendous damage it could do to the reputation of the hospital and its standing in society. I also reminded him of the potential malpractice suits that might result from reactions in these patients who had received these injections. He asked me if Dr. Mandel could lose his license as a result of his action and I told him that I did not know but that it was a serious matter.

I repeated many times that I was against the project; that I had nothing to do with it; that I had not condoned it and that it was done without my knowledge and consent and against my express desires.

On or about August 26, 1963 I was informed that a meeting had been scheduled for the co-ordinators of the different divisions of the hospital to discuss this matter with Dr. Mandel and Mr. Siegel in an effort to hush it up. However, this meeting was cancelled.

On August 27, 1963 Dr. Avir Kagan, co-ordinator of the department of medicine, Dr. Perry M. Fersko, also a co-ordinator in the department of medicine, and I, in reviewing the project and our individual refusal to become a part of it and the fact that the parties responsible for it appeared to be passing the buck, thought it advisable to make our positions clear by resigning together and giving our reasons for such resignation. We realized that our position was untenable despite the fact that we had never become a party to this project and that the entire matter was unethical and immoral. Further, that if we remained, our silence or continued association with the hospital might be construed as condoning the actions of Dr. Mandel and Dr. Custodio and might be tantamount to our being co-conspirators. Therefore, on August 27, the three of us composed one letter of resignation, signed by all of us, wherein we stated the following:

We the undersigned co-ordinators in the department of medicine do hereby submit our resignations

as co-ordinators, effective immediately. The reasons for our decision are based upon disagreement and opposition to certain research practices in which the department of medicine has engaged.

Our position has been stated to you. Inaction on our part might be interpreted as condoning these acts which we feel, under the circumstances, would be morally wrong.

This letter was addressed to the executive director of the Jewish Chronic Disease Hospital and copies were sent to the president of the executive board, chairman of the medical board, administrator of welfare, and the chairman of the department of medicine.

On August 28, 1963, Mr. Siegel called me in his office, advised me that he had read the resignation and attempted to intimidate me by stating that I had improperly obtained the consent of the relatives of certain cancer patients in another project involving the administration of an anti-cancer drug in cancer patients, which drug was known as Thermonycin 401.

I reminded Mr. Siegel that these were cancer patients who were actually receiving anti-cancer treatment but that these patients did not know they had cancer and that under these circumstances the law permitted us to obtain the consent of the nearest relative in an effort to save the lives of these patients, and that this situation was not in any way similar to the improper project previously described.

Mr. Siegel then attempted to claim that our resignation amounted to an abandoning of the patients. Thereupon we informed Mr. Siegel that we would be at the disposal of the hospital and the patients for any necessary treatment, free of charge and whenever required.

To date we have never been asked by anybody connected with the hospital to service such patients.

On August 29, 1963 Dr. Mandel met me at the hospital and we discussed this problem at length wherein I reiterated the fact that in my opinion the entire project was unethical and immoral and against public good and violated the rights of the patients who had not been informed of the nature of the project (i.e., the inherent dangers associated with an unknown experimental agent involving live cancer cells) and who had actually not given their informed consent. Dr. Mandel then informed me that he could not get their consent because these patients were incompetent.

I have read my statement and it is true to the best of my knowledge.

b.

**Samuel Rosenfeld, M.D.—  
September 12, 1963**

My name is Samuel Rosenfeld. I have been a duly licensed physician in the State of New York since June, 1923.

I have been associated with the Jewish Chronic Disease Hospital for the past thirty years and have been the co-ordinator of medicine of the Blumberg Pavilion since 1956 and a visiting physician since 1945.

On Thursday, August 8, 1963, at about 10:30 A.M. while making my usual rounds in the Blumberg Pavilion, accompanied by Dr. Custodio, senior resident physician, a ward patient, Mr. Celi Stephano, stopped me, complaining bitterly of pain, and told me that he was injected under the right thigh and that that area was now swollen. He said that he had not been sick at the time he received the injection and he stated further that he knew that I had not ordered anything for him.

I inquired of Dr. Custodio, a resident in my service, and he motioned me away from the patient and then told me that he was doing experimental injections on orders from Dr. Emanuel Mandel. I was unable to pursue this subject further at the time but on the following day I again inquired of Dr. Custodio what had transpired. He told me that he was injecting material which was delivered to him by the Cancer Memorial Hospital.

On Monday, August 12, 1963, I again spoke to Dr. Custodio and he confirmed the fact that the material consisted of "cancer cells" and the project was to test the immunologic response of these patients to this agent.

On Tuesday, August 13, 1963, I was accosted by Dr. Avir Kagan, the then co-ordinator of the department of medicine, and he asked me if I was aware that Dr. Mandel had injected cancer cells in patients in my care and in my pavilion. He also spoke of other matters which made him unhappy and he told me he wished to resign. He specifically stated that Dr. Mandel had requested that he, Dr. Kagan, inject these "cancer cells suspensions" but that he flatly refused to do same.

Dr. Kagan then asked that I speak to Dr. David Leichter, who is in charge of the cancer research at our hospital, which I did, and he informed me that there was no project going on under his direction but that he had been approached by Dr. Mandel to authorize the injection

of live cancer cells in chronically ill patients and that he did not consider this project feasible because of the potential danger attached to it and because the prior written consent of each patient would be required.

Dr. Kagan also told me that Dr. Perry Fersko had been requested by Dr. Mandel to give these injections but that he too had refused.

With this information on hand I felt it my duty to inform the administration of my findings as all new projects involving experimental drugs or agents, prior to their being used on patients, had to be approved by the research committee. This had not been done nor had the project received the approval of Dr. David Leichter even though this was a cancer project. In addition, it was being performed on patients for whom I am responsible in the Blumberg Pavilion, which patients had not been advised of the nature of the project nor told of its potential dangers nor had they given their prior written or oral consent. There were 18 patients in my ward who received these injections and many of them were mentally incapable of giving their consent. In my opinion this project was, therefore, both illegal and immoral and it has been conducted surreptitiously without my knowledge or consent.

In view of the fact that Mr. Sol Siegel, the executive director of the hospital, was on vacation, and Mr. Isaac Albert, the president, was seriously ill at the hospital, I contacted Mrs. Minnie Tulipan, the director of welfare and gave her all the facts and together we discussed matters further with Dr. Abraham Rabiner, patriarch of our hospital and former chief of neurology.

In the afternoon of August 14, 1963, Mrs. Tulipan told me that she had phoned Mr. Sol Siegel in Florida, had given him the facts and that he was scheduled to return on August 15, 1963.

On August 15, 1963, a patient in Ward P 6, Mr. Grossman, who had received the injection of these cancer cells was visited by three doctors from Memorial Hospital. In response to my questioning, Dr. Custodio told me that these were the doctors who were investigating the effects of the cancer cell suspension injections.

Shortly thereafter Mr. Siegel came to my home office and I informed him of the entire matter. He stated he would look into it.

On August 26, 1963, Mr. Siegel told me he was scheduling a meeting for the following day and upon inquiry as to the purpose of the meeting, I learned that in substance it was a meeting

of the coordinators and his intention was to suppress this information and to take no action. I told him I would not attend this meeting as in my opinion the entire experimental project was dangerous and illegal and I would not condone what amounted to a crime. I told him that this situation was explosive, that it could result in malpractice actions and in the destruction of the reputation of the hospital. I told him in my opinion it should be handled by the board of directors and responsible physicians and that the president of the medical board and other true and loyal supporters of the hospital should be consulted.

The August 27th meeting was thereafter cancelled by Mr. Siegel.

On August 28, 1963, Mr. Siegel called me at 8:00 A.M. and told me he was going to see an important lawyer on this matter. I urged him again to make every effort to inform selective members of the board of directors of the situation since Dr. Joseph Abramson, the president of the medical board of the Jewish Chronic Disease Hospital, was not in the city.

Later the same morning, I learned that Drs. Leichter, Kagan and Fersko had resigned.

I was also told by Dr. Kagan that Dr. Mandel had called him into his office, had offered him an increase in salary and some private cases. The purpose of this effort was obvious. It was Dr. Mandel's attempt to keep Dr. Kagan and the others from talking about the project.

On August 29, 1963, I had a further discussion with Mr. Siegel who requested that I talk to Dr. Abramson who had just returned from abroad.

On August 30, 1963, at about 11:00 A.M. I discussed the matter with Dr. Joseph Abramson. After getting the entire report from me, he informed me that he would take this matter up before the grievance committee of the hospital for further evaluation.

To my knowledge no corrective action has been taken by any responsible committee or individual nor have I been asked to appear before the grievance committee or the medical board or any other responsible board of inquiry.

c. **Hyman Strauss, M.D.—November 23, 1963**

I am a physician and surgeon duly admitted to practice in the State of New York specializing in gynecology and I am an attending physi-

cian in gynecology in the Jewish Chronic Disease Hospital.

On October 28, 1963, I addressed a letter to the medical board of the hospital, attention of the secretary of the executive committee which was given to that board. This letter was prompted by the horrible news that had reached me to the effect that patients in our hospital where I had been an attending physician for approximately twenty-five years were now being used for experimental purposes not associated with their therapy or their ailments, and that these experiments comprised the injection of live cancer cells into these chronic invalids at the hospital who were not informed of the fact that they were being injected with live cancer cells. As I am informed and verily believe, they were told that these injections of live cancer cells were mere "skin tests."

I am informed that no action was taken on this letter of complaint of mine. Instead, a conspiracy of silence developed in which an effort to suppress the disclosure of these practices to the membership and to the proper authorities was quite apparent.

Since no report of any proper action to correct this deplorable and outrageous situation was given to me and various other members of the medical staff, and apparently no proper and thorough investigation was made of the situation to prevent this recurrence and to prevent the practice of and to prevent the commission of these acts which belong more properly in Dachau, where for similar acts there had been prosecutions against the Nazis, I sent a copy of my letter of complaint to the Division of Professional Conduct of the New York State Education Department enclosed in my letter of October 30, 1963.

## NOTES

### NOTE 1.

LETTER OF HYMAN STRAUSS, M.D. TO THE  
MEDICAL BOARD OF THE JEWISH CHRONIC  
DISEASE HOSPITAL—OCTOBER 28, 1963

Inasmuch as I am unable to attend meetings at the present time for reasons beyond my control, I am obliged to write this letter as an expression of my position.

While the executive director was vacationing, an abundance of rumors found their way into medical circles. My first knowledge of this affair about cancer experimentation upon pa-

tients known to be free of malignant disease, without their fully informed written consent, came from conversations at two other hospitals in this borough. I next heard from a professor at the State University of New York, and finally a top-ranking investigator from Sloan-Kettering, a man with an international reputation, discussed this with me on the phone. I have also been questioned by laymen and clergymen not connected with our hospital. The coordinators refused to talk, indicating that they had been instructed not to say a word. Obviously, they had been successfully frightened.

Upon Mr. Siegel's return, I went to his office and asked for the truth about the matter that had already been rumored around quite a bit. He refused to make any statement but asked instead that I tell him what I had heard. Since I informed him that I was willing to discuss it only following his affirmation or denial, and since he refused to make such a statement, I left his office with the feeling that nothing could be gained and that the conspiracy of silence was continuing.

An effort to obtain an explanation from Dr. Joseph Abramson was equally unsuccessful, although instead of silence, I encountered conflicting comments. Within a period of 15 minutes, I was told first, that proper consent had been obtained in advance and duly witnessed. Next, I was told that the consent had been oral and that only some of the patients understood English. Finally, I was informed that consent was not necessary, according to Mr. Harry Albert, one of the attorneys for the hospital, and that it was unlikely that Jewish patients would agree to live cancer cell injections, especially since they were free of cancer themselves.

Finally, at the last meeting, twice when Dr. Mandel endeavored to inform the committee during his report of the resignation of the coordinators and when the question of consent was raised following a report by Dr. Slepian, the chairman arbitrarily utilized the privilege of his office to defer the discussion until the end of the meeting. The unfortunate sequence, however, was that the meeting was prolonged unduly and then abruptly and, I believe, deliberately, adjourned by the chair. This technique, I understand, was used by him at another hospital whenever he wished to stifle discussion. It might be suggested that the chairman acquaint himself with the democratic foundation of parliamentary procedure which entitles him to guide but not

to dictate or interfere with full and free deliberation.

For three young coordinators to have resigned must have required some soul-searching on their parts. If undue pressures had been exerted to get them to act in ways inconsistent with their consciences, their sense of medical ethics, and in violation of the law, then their resignations, under such duress, are unacceptable. Further, men of such high ethical standards as to jeopardize their security when morality is at stake should not be lost to any hospital. Since medical liability insurance does not cover experimentation, no one has the right to demand that a physician perform any act for which he has no insurance protection without providing additional insurance which will cover him in the event of suit.

The matters at issue here are of extreme importance. A full and frank and complete hearing is absolutely necessary in order that no injustice be done to anyone. Denial of the opportunity to speak is undemocratic and should not be tolerated. All persons involved in this matter are entitled to ample opportunity to state their cases.

I therefore propose that the executive committee constitute a committee of the whole to investigate all sides in this issue. A small committee may be susceptible to instruction to act as a whitewash body. Unless we condemn what is improper, we share the guilt.

This is a medical matter and must be dealt with by medical persons. It is not within the province of the lay board of any hospital to pass upon medical ethics or to dictate what is or is not ethical in medical practice.

I feel that this is a matter involving my conscience and that no other solution can be considered satisfactorily.

#### NOTE 2.

LETTER OF HYMAN STRAUSS, M.D. TO DIVISION OF PROFESSIONAL CONDUCT, NEW YORK STATE EDUCATION DEPARTMENT—  
OCTOBER 30, 1963

The original of the previous letter was personally delivered to the secretary with the request that it be read and considered at the meeting held on October 28. I have been informed that the letter was not read and that no definitive action of any type was taken with reference to the matter described.

d.

**Mendel Jacobi, M.D.—  
December 11, 1963**

I am a physician and surgeon duly licensed to practice medicine in the State of New York and have been practicing since 1925.

I am the consultant pathologist at the Jewish Chronic Disease Hospital.

On October 4, 1963, I examined the charts of five of the 26 patients who were subjected to the injection of live cancer cells.

In this affidavit I shall not reveal the names of the patients, but shall refer to each patient by his chart number, so as not to disclose any confidential information.

Chart No. K-14397 shows that the patient was admitted on June 8, 1962 at the age of 67. The patient had been in chronic congestive heart failure for a year and a half prior to admission. One year prior to admission he had had a cerebral vascular accident with a right hemiparesis and acute myocardial infarction. A note by the social service dated in April, 1962, describes his attitude as one of isolation, perversity and negativism with resistance to all forms of treatment. His blood urea nitrogen levels had been consistently high since admission indicating a state of chronic uremia, in which cerebration is generally poor. A psychiatric note in May, 1963, indicated that he had been in a depressive state for a year. A nurse's note dated July 16, 1963, states "Cell suspension injected into right thigh by Dr. Custodio and resident." A progress note of the same day signed by Dr. Custodio reads: "Cell suspension injected left thigh."

Chart No. 2290 shows that the patient was admitted May 21, 1941, at the age of 41, so that he is now 63 years old. The patient is a postencephalitic Parkinsonian of advanced grade who on February 19, 1963, fell while in the ward and fractured his left wrist. An undated note in the record made some ten years after his admission, when he was 52 years old, describes him as showing a marked speech defect, markedly irritable personality and one who stands in a corner and cries steadily and in a shrieking manner when his requests are not instantly carried out and that persuasion or argument was of no avail "due to the patient's low mentality and lack of insight and judgment." A note by Dr. Custodio, dated July 16, 1963, reads: "Cell suspension injected right thigh." A progress note dated September 5, 1963, notes that for two or three days he has developed hematuria. After an operation of Sep-

tember 27, 1963, it was found that he had a transitional cell carcinoma of the bladder which was resected. A consent to be photographed for moving pictures had been given by him on July 31, 1957, at which time his signature was still decipherable. Consents for cystoscopy on September 5, 1963, and for the bladder operation on September 27, 1963, were also signed by him, but with a barely legible and very scrawl-like signature. All three consents were witnessed by a person designated as his sister. However, there is no indication in the chart of any consent for the injection of the cell suspension.

Chart No. 8183 shows that the patient was admitted January 2, 1958, at the age of 38 with a diagnosis of multiple sclerosis. A history showed that in 1954 and 1955 a craniotomy and Gasserian ganglion decompression was performed twice on the right side and once on the left side. In 1957 he had been a patient in the Brooklyn State Hospital with a depressive psychosis, at which time he was unaware of his surroundings and unmanageable and his condition was diagnosed as dementia praecox. The chart shows that when admitted to the Jewish Chronic Disease Hospital in 1958 he was deemed mentally unsound and in the subsequent years his neurological status had not improved. From the beginning of 1963 onward he had repeated bouts of bilateral pneumonia and these events are specifically indicated to have occurred about May 31, 1963, and again about September 17, 1963. The chart makes no mention of any cell suspension or cell injections.

Chart No. 3762 shows the patient was admitted on June 10, 1952, at the age of 44. The diagnosis was postencephalitic Parkinsons with speech so dysarthric as to be noted "hard to understand." The chart shows he had had several falls during 1961 and 1962 on which occasions he hit his head against the ground. On May 8, 1963, there is another notation of a fall from a chair and striking his forehead on the floor. A note by Dr. Rosenfeld, the attending physician in charge, dated August 13, 1963, indicated an attempted suicide and requested psychiatric consultation. There is also a nurse's note dated August 16, 1963, that he had threatened to kill himself. The patient has been on considerable sedation throughout the years. There is no mention of cell suspension injections into this patient.

Chart No. B-15918 shows that the patient was admitted May 20, 1963, at the age of 72. The diagnosis was arteriosclerotic heart disease, coronary insufficiency, emphysema, hiatus hernia

and spinal compression due to osteoporosis, with X ray showing compression fractures of the 6th, 7th and 12th dorsal vertebrae. On September 24, 1963, blood tests indicated a marked anemia and the biochemical changes of malnutrition. There are no notes of any injections of cell suspension in the chart of this patient.

I was informed by Dr. Rosenfeld that another patient who had received the cell suspension injections and who had had tabes dorsalis, central nervous system syphilis and diabetes had died. I was not able to locate the chart and have not been able to determine whether an autopsy was performed on this patient.

From my examinations of the five charts above described it is clear that these patients who were subjected to cancer cell injections were in no condition, mentally and physically, to understand the nature of the injections being given to them and to consent to such experimental injections. There was, of course, no written consent—and there is no entry in the charts of any oral consent. In fact three of the five charts do not even note the injections that were made.

### 3.

#### **Answer of the Jewish Chronic Disease Hospital—January 6, 1964**

The Jewish Chronic Disease Hospital herein referred to as "the hospital," by its attorney, Morris Ploscowe, for its answer to the petition herein avers as follows:

Admits that the petitioner is a director of the hospital, which is a membership corporation, and that the hospital is governed by a board of directors which presently numbers sixty.

The hospital is an institution for the care and treatment of the chronically ill, without regard to race, color or creed. The Isaac Albert Research Institute is affiliated with the hospital and has done some of the outstanding research in this country in the area of chronic diseases.

Denies that petitioner was the sole attorney for the hospital and is still one of the attorneys of record. The petitioner has never been the sole attorney for the hospital, and is not now an attorney of record.

Denies the statement that patients of the hospital were injected with live cancer cells "for the purpose of determining whether cancer can be induced." This statement is unqualifiedly false. There was absolutely no danger that any patient

would contract cancer from the subcutaneous injection given to him. Nor has any patient in fact contracted cancer or in any way been harmed by the tests administered to him. The injection was not designed to induce cancer, but to test the patient's immunologic reaction to cancer cells. The said experiment was conducted in collaboration with doctors from the Sloan-Kettering Institute. It was part of a bona fide attempt to advance human knowledge in dealing with cancer and has been financed by grants from the United States Public Health Service and the American Cancer Institute.

Admits that a board of directors' meeting was held on September 30, 1963. There were more than enough directors present for a quorum for the transaction of business.

At the said meeting, the petitioner did characterize as "whitewash" the report of Solomon L. Siegel and of the grievance committee of the medical staffs of the hospital. The said report was read at the board of directors' meeting.

The petitioner, at this meeting, did make the scurrilous analogy between Nazi experimentation at places like Dachau and the work done at the Jewish Chronic Disease Hospital with the collaboration of doctors from the Sloan-Kettering Institute and financed by United States Public Health Service funds.

The petitioner, by his emotionally rhetorical questioning at this meeting, did falsely spread the notion that the purpose of the experimentation was to see whether cancer could be induced.

The 22 members of the board of directors unanimously voted to approve the report of the grievance committee of the medical staffs of the hospital, despite the scurrilous, false and emotional statements of the petitioner. Only the petitioner dissented from this vote.

There is no truth in the petitioner's allegation that no vote was taken on the medical grievance committee report.

The documents and records which the petitioner is legally entitled to see as a director of the hospital have been mailed to him. These include the minutes of meetings of the board of directors, the report of Solomon Siegel, executive director of the hospital, the report of Dr. Joseph Abramson, the affidavit of Dr. Custodio, which were requested in the notice of motion submitted by the petitioner.

The Jewish Chronic Disease Hospital cannot legally turn over charts and records of patients, since the petitioner has not obtained the



consent of such patients. The turnover of such material to the petitioner without the patients' consent is prohibited by CPLR, Sec. 4504(a), and the cases decided thereunder.

There was no attempt on the part of Isaac Albert to maintain a veil of secrecy and prevent a full disclosure of the situation, as alleged in the petition.

The petitioner is fully informed concerning the experimental work done at the hospital by the report made to the board of directors' meeting, a copy of which has been sent to him.

The right to determine whether any physician in the hospital has committed an unprofessional, illegal, or immoral act is not entrusted to any single director of the hospital, but is entrusted to various committees provided by the constitution and by-laws of the hospital, as follows:

The executive committee of the board of directors, which has full power to act on any matter requiring immediate action.

The medical conference committee, consisting of physicians, members of the staff, and directors of the hospital, which makes recommendations to the executive committee concerning medical and surgical activities of the hospital.

The medical board, which enforces rules and regulations for the proper supervision and care of patients.

The grievance committee, which has the duty to investigate "all grievances arising between members of the staff as well as the actual or alleged transgression by members of the staff."

The research committee, which has the power to review all protocols of projects submitted to it by members of the staff.

The petitioner has sought unilaterally, by a process of innuendo and slander, to arrogate to himself powers which are in the scope of the aforementioned committees of the hospital. Although not a physician, the petitioner seeks to determine what is and what is not the proper practice of medicine.

There is no individual liability of directors of a hospital for medical acts done without their knowledge.

The petition herein is not brought in good faith, but is the product of a long standing feud and vendetta which the petitioner has carried on for years against the president of the hospital, Isaac Albert.

The claim is made in these paragraphs, quoting the affidavits of various doctors, that the injections were made on patients without their

consent. None of the doctors quoted was present when the injections were made, and any statement made in their affidavits concerning lack of consent is pure hearsay. Moreover, the affidavits of Doctors Custodio, Mandel and Southam, attached hereto, show that each patient was asked whether he would consent to the injection which was to be made. The injections were, in fact, made with the oral consent of the patients involved. There is no requirement in the law for written consent.

It is charged in the petition that the patients were mentally incapable of giving their consent to any injections. The refutation of this allegation is found in the affidavits of Doctors Mandel, Custodio and Abramson attached hereto. Their affidavits clearly demonstrate that these patients were not mentally incompetent to give a consent to the injections. Dr. Abramson, a neurologist and psychiatrist, who examined the affidavit of Dr. Jacobi, came to the following conclusion: "In the light of the above examination, I do not believe that any conclusion can be drawn that the five patients whose charts were examined by Dr. Jacobi were mentally incompetent."

There are charges in the above paragraphs concerning a conspiracy of silence to suppress disclosures, and to suppress facts. There has never been any such conspiracy. Nor has there been any attempt to suppress facts (see affidavits of Solomon L. Siegel and Dr. Mandel attached hereto). The complaints of the doctors whose affidavits support the petition were heard in due course by the grievance committee of the medical staff, provided for by the constitution and by-laws of the hospital. The report of the latter committee was fully discussed at the September 30, 1963 board of directors' meeting, at which the petitioner was present. All the 22 directors present, with the exception of the petitioner, voted to accept the report absolving the medical staff of any blame for alleged professional misconduct.

The charge is made that three of the doctors at the hospital (Kagan, Fersko and Rosenfeld) were asked to participate in the project by Dr. Mandel, the director of medicine, and that they refused because of either ethical grounds or because proper consents could not be obtained from the patients. The affidavit attached hereto of Dr. Mandel flatly denies that any such requests were made to these doctors.

Complaint is made that the consent of Dr. Rosenfeld was not obtained to the injections made on patients (see Fersko and Rosenfeld affidavits). Dr. Mandel, as director of medicine



at the hospital, was the superior of both of the aforementioned doctors. Under the constitution and by-laws of the hospital, he did not need the consent of his subordinates for a pilot study such as the one herein under consideration.

None of the affidavits of the doctors produced by the petitioner supports the charge in the petition, that the purpose of the tests conducted at the Jewish Chronic Disease Hospital was to see whether "cancer could be induced" in any of the patients. The latter notion is a figment of the petitioner's imagination. The affidavits of Doctors Mandel, Custodio, Korman and Southam, attached hereto, make it crystal clear that no harm resulted to the patients at the hospital from the injections made on them, and no harm was expected at the time the injections were made.

For a complete and affirmative defense to the petition herein, the Jewish Chronic Disease Hospital alleges:

Upon information and belief, the petitioner has made complaints similar to those contained in his petition, to the Kings County district attorney's office, which investigates and prosecutes the commission of crime in Kings County, and to the New York State Department of Education, which has the duty of investigating and prosecuting complaints concerning breaches of medical discipline and ethics by physicians. All the doctors involved in the research and experimentation complained of have been questioned by the said public agencies. These include, Dr. Chester Southam and Arthur J. Levin of the Sloan-Kettering Institute, and Dr. Emanuel E. Mandel and Dr. D. B. Custodio of the Jewish Chronic Disease Hospital. The hospital has co-operated fully with the aforementioned public agencies in making all data available to them. Upon information and belief, the petitioner's complaints are still under advisement in the said agencies.

Wherefore, the Jewish Chronic Disease Hospital prays that the petition herein be dismissed.

#### 4.

##### **Affidavits for Respondent**

a.

##### **Solomon L. Siegel—January 3, 1964**

I am the executive director of Jewish Chronic Disease Hospital.

The petitioner states that the injections were made in the patients at the Jewish Chronic Disease Hospital for the purpose of determining

whether cancer can be induced by the injection of live cancer cells. This is an outrageous falsehood. The attached affidavits of Drs. Southam, Mandel, Korman and Custodio, and medical literature throughout the world, will support the fact that cancer cannot be induced by injection of cancer cells. It is a recognized natural phenomenon that any foreign cells injected into the human body from an outside source will be rejected. Cancer cells are no exception. The purpose of the test was to determine whether the chronically ill, debilitated patients would reject the foreign cells at the same rate as cancer patients, or at the rate associated with normal humans.

Cancer cells were used in the experiment at the hospital, because unlike other foreign cells, the resultant nodule could be measured and the rejection time could thus be determined.

It is of significance, scientifically, that the patients injected rejected the cancer cells the same as normal humans, despite the fact that these were chronically ill, debilitated patients. The problem remaining now is to isolate the immunity factor which distinguishes the normal human from the cancer victim. Such isolation would provide an important clue in the conquest of the dread disease.

It should be noted that none of the doctors who submitted supporting affidavits made the claim contained in the petition that the purpose of the tests at the hospital was to determine whether cancer could be induced. As a matter of fact, Dr. Leichter, who was familiar with problems of cancer, has admitted that the injection of the cell suspensions which were used in the project constituted no possibility whatsoever of producing cancer in the patients involved.

It is not true that the injections were made without the knowledge and consent of patients. As appears from the affidavit of Dr. Custodio, who actually administered the injections, the consent of each patient was asked for orally and obtained. Neither medical ethics nor the law require that a consent to an injection be in writing. Dr. Custodio's affidavit is better testimony as to what happened than the hearsay statements of the petition and the doctors who were not present when the injections were made. Dr. Custodio was familiar with the patients. He has been a resident physician for two years. He found no difficulty in communicating with the patients in English. Dr. Arthur Levin, from Sloan-Kettering Institute, who was present when the injections were made, is familiar with the Yiddish language and was available to talk to the patients,

had this been necessary. Nor is the charge true that the patients were mentally incompetent to give their consent. Dr. Jacobi's statement concerning the mental condition of five patients, contained in his affidavit, and which is based only on an examination of their charts, is refuted by the affidavit of Dr. Joseph L. Abramson, who was president of the medical staff, and who is a consultant at the hospital in neurology and psychiatry. Dr. Abramson concluded that the charges of mental incompetence with respect to the five patients whose charts were examined by Dr. Jacobi were not justified. Dr. Jacobi did not know the number of patients involved. He stated that 26 patients were injected, whereas only 22 patients were involved in the tests at the hospital.

The fact is that neither the consent of Dr. Leichter nor Dr. Rosenfeld was required for the tests. Dr. Emanuel Mandel is the director of medicine of the hospital. He is the superior of both Dr. Leichter and Dr. Rosenfeld. Dr. Leichter was not in charge of cancer research, as claimed in Dr. Kagan's affidavit. Any project to do research required the approval of Dr. Mandel. Dr. Mandel approved the study and under the constitution and by-laws of the medical staff, he had the authority to give such approval. The said constitution and by-laws permit directors of services, without prior approval, to make pilot studies. The injection of 22 patients was such a pilot study. Dr. Mandel's only obligation was to submit a protocol to the research committee of the medical staff within 60 days.

The petitioner states that it is his obligation as a director to inquire into happenings and to ascertain all the facts and to take adequate steps to protect the patients and the good name of the hospital. Mr. Hyman is so obsessed with the notion that there has been wrongdoing at the hospital that he brushes aside the unanimous opinion of the directors who disagreed with him and of the medical grievance committee which found no wrongdoing. Moreover, he overlooks the fact that those who disagree with him are just as much interested in the welfare of the patients and the welfare of the hospital as he. One begins to wonder just how much Mr. Hyman has the interests of the hospital and patients at heart.

Mr. Hyman also overlooks an important fact: There is a basic distinction between the medical affairs of the hospital and the care of patients, on the one hand, and the administrative affairs of the hospital. It is accepted procedure in hospitals for the board of directors to detail its medical responsibilities to the medical staffs of the hospital. This is the case at the Jew-

ish Chronic Disease Hospital. The medical and dental staffs have their own constitution and by-laws. The by-laws provide for a grievance committee to handle matters of any questionable nature. A duly appointed grievance committee considered the problems raised by the tests on the 22 patients. The grievance committee report was approved by the board of directors. Mr. Hyman wishes to set himself up as a one man court of appeal from the judgment of the grievance committee and the board of directors.

There are statements in the affidavits that Doctors Kagan and Leichter had been requested by Dr. Mandel to make injections or to authorize injections and that these doctors had refused to do so. This is contrary to the information that I received from Dr. Mandel when I made an inquiry into the matter. He stated that no such requests were made. Moreover, as the superior of the doctors, he did not need their approval to make any appropriate studies or tests. Dr. Leichter has already repudiated the statement attributed to him (see affidavit of Dr. Samuel Korman attached hereto).

Dr. Rosenfeld alleges that the project was illegal and immoral and conducted surreptitiously without his knowledge and consent. The short answer is that his knowledge and consent were not required, and it is somewhat difficult to conceive that dedicated research workers of the Sloan-Kettering Institute would be engaged in illegal and immoral practices on our patients with the consent of our director of medicine. It should be noted that Dr. Rosenfeld has a personal animus against Dr. Mandel and on more than one occasion, when I discussed the problem with him, he insisted that Dr. Mandel be discharged immediately.

Petitioner as a lawyer should know that the hospital is prohibited by law from divulging the contents of patients records which are confidential, and which records he is demanding by court order.

The petitioner has embarked upon a reckless campaign to discredit the Jewish Chronic Disease Hospital unless he has his way. The court should not assist him in his campaign. May I therefore respectfully urge the court to deny the petition herein.

b.

**Emanuel E. Mandel, M.D.—  
December 31, 1963**

I am a physician duly licensed to practice medicine in the State of New York since 1939.

I am the director of the department of medicine and director of medical education at the Jewish Chronic Disease Hospital, having held these positions since November 1961. In addition, I am a clinical associate professor of medicine at the Downstate Medical Center, State University of New York. Previously, from 1957 to 1962, I was associate professor of medicine at the Chicago Medical School and associate director of medical education at Mount Sinai Hospital of Chicago, Illinois.

The petition of William A. Hyman and the affidavits annexed thereto are replete with falsehoods, distortions, and misrepresentations. I shall try to bring these to the attention of the court.

The most shocking misstatement in Mr. Hyman's papers is the allegation that the experiment and tests conducted at the hospital were "for the purpose of determining whether cancer can be induced by the injection of live cancer cells." Similarly misleading and fallacious are references in the petition to Nazi doctors, Nuremberg trials, and Dachau methods, as well as the inane argument as to whether the directors, who voted against Mr. Hyman when the matter of the tests was being considered, would "consent to having injections of live cancer cells made into their own bodies to see if cancer can be induced in their bodies."

It should be clearly understood that there was absolutely no danger arising to patients of the hospital who received hypodermic injections of suspensions of cells obtained from cultures of human cancer tissue. The purpose of the injections (which were given to 22 patients on July 16, 1963) was to determine the mechanism and rate of rejection of the injected material by the recipients. This material represented homologous transplants, i.e., tissue of one human being transplanted into another person. While other tissue, such as normal skin muscle, could be used for the same purpose, cancer cell lines were chosen because they have the necessary growth capacity to produce a measurable reaction. "It is inconsequential whether these are cancer cells or not, since they are foreign to the recipient and hence are rejected. The only drawback to the use of cancer cells is the phobia and ignorance that surrounds the word cancer (quoted from a letter written by Dr. C. M. Southam)." Indeed, the injections could not possibly and did not "induce cancer" in any of the patients. The innuendos concerning danger to patients from "injections of live cancer cells" contained in some of the physicians' affidavits attached to the petition can be

explained only by their ignorance in the subject, unless ulterior motives were at play.

The project was undertaken because of its vital importance not only to the understanding and possible treatment and diagnosis of cancer, but also to the understanding of other diseases, particularly those of autoimmune and degenerative nature, and because of its possible contribution to our knowledge in the general field of organ homotransplantation. Tests of the nature described above had been carried on for several years on patients at Memorial Hospital in New York and on healthy prisoners at the Ohio State Penitentiary. For the past 2 years, these tests have been routinely applied to all postoperative patients on the gynecology service of Memorial Hospital as a measure of their immunologic status. These studies had revealed that healthy persons rejected cancer cell homografts completely and promptly (in 4 to 6 weeks), while patients with advanced cancer usually showed a delayed rejection (6 weeks to 3 months). The typical reaction consists in a painless subcutaneous nodule (lump) which attains a maximal size of 2 to 3 cm. in diameter and disappears within the periods noted above. The project at the hospital was designed by Dr. C. M. Southam of the Sloan-Kettering Institute to determine whether the immunologic response (rate of rejection) in chronically ill and debilitated non-cancer patients was similar to that of healthy persons or conformed to that of cancer patients. Results of the project conducted at the hospital by Drs. Southam and Arthur G. Levin of The Sloan-Kettering Institute and by Dr. D. B. Custodio of the hospital clearly indicated that chronically ill, debilitated non-cancer patients reacted like normal individuals in rejecting homologous tissue cells. Hence, the delayed immunologic response of cancer patients must be attributed to the disease itself (cancer), rather than to the attendant metabolic changes (weakness, debilitation). This finding furthermore suggests that the spread of cancer in the human body is associated with an impairment of the normal immunologic defense mechanisms. If this impairment could be prevented or remedied, the spread of cancer might be halted. This discovery may prove to be of great importance in the future control of cancer.

Mr. Hyman was fully aware that the above-named investigators were not trying to "induce cancer" but that they were testing the immunologic response of our patients. This information was available from Dr. Rosenfeld's affidavit attached to the petition stating that "the project

was to test the immunologic response of these patients." This is a far cry from inducing cancer in the patients. Indeed, as I stated above, there was absolutely no danger of inducing cancer by these tests.

It is charged in the petition and affidavits that the patients' consent to the skin tests had not been obtained; that many of the patients were incompetent and incapable of giving a real consent; that some of the patients did not even speak English. Please note that Dr. D. B. Custodio, senior medical resident at the hospital, voluntarily, at my suggestion, undertook this research in collaboration with the two physicians from the Sloan-Kettering Institute, Dr. Chester M. Southam and Dr. Arthur G. Levin. Each of the patients was asked by Dr. Custodio for his consent to the injections, in the presence of those other 2 physicians. Dr. Custodio had known these patients for many months and was able to communicate with them in English, despite allegations in the petition to the contrary. Obviously, Dr. Custodio was in a better position to gauge the ability of patients to comprehend the requests which were being made of them than was Dr. Jacobi who drew his conclusions concerning the patients' mental status from charts. Moreover, when personally visiting on December 20, 1963, 4 of the 5 patients listed in Dr. Jacobi's affidavit, I found each to be in satisfactory condition for comprehending or giving consent to a diagnostic test or a surgical procedure. The fifth patient had died in October following a bladder operation.

The reference in the petition and affidavits to the requirement of written consent of patients for the tests is not consistent with my understanding of the law of New York. I believe that oral consent was adequate. Such consent was obtained in every instance, in accordance with the procedure which had been in vogue at Memorial Hospital for several years. There, these tests are used as routine studies and are being considered to be even less hazardous than such other routine diagnostic procedures as bone marrow aspiration and lumbar puncture.

The petition charges that the experiments on the patients were made without the "approval, sanction, authorization and consent" of the proper authorities at the hospital. This is untrue; Drs. Southam and Levin of the Sloan-Kettering Institute were initially referred to me by Mr. Sol Siegel, the executive director of the hospital, whom they had called over the telephone about this research project. As director of medicine

and of medical education of the hospital, I authorized the project after I had discussed it at great length with Drs. Southam and Levin; after I had ascertained Dr. Southam's reputation in the field of cancer research and reviewed his publication on the subject; and after I had become convinced that the proposed clinical study was likely to make a significant contribution to science without exposing our patients to any risk. Under the constitution, by-laws and rules and regulations governing the medical staff of the hospital, I had the right to engage in pilot studies and was under the obligation to inform the research committee of the hospital of such pilot studies within 60 days from the time when such project was undertaken. When complaints about the project came to the attention of the grievance committee of the medical staff at its meeting on September 7, 1963, my actions were upheld and, indeed, further studies of the same type were being encouraged by the committee. This is apparent from the annexed minutes of this meeting. Furthermore, the board of directors, at its meeting of September 30, 1963, expressed its approval, with only Mr. Hyman dissenting.

It is asserted that there may be liability on the part of the directors and the hospital because of "injuries that may be received by any patient" as a result of the injections that were made. It should be noted that no injury resulted to any patient from the injections. A small nodule (lump) formed within 2 weeks after the injection and disappeared completely not later than 2 months thereafter. This was the typical reaction which was being expected. As mentioned previously, the vital part of the project was measurement of the size of the nodule and of the time of its disappearance. With respect to the statement in Dr. Rosenfeld's affidavit that one patient complained bitterly of pain in connection with such an injection, I was advised by Dr. Custodio that this was incorrect; the patient did not complain of pain but merely inquired with respect to the lump which had formed on his thigh following the injection. This lump as well as the skin reactions in all the patients who received these tests have completely disappeared.

It is claimed that I asked three physicians at the hospital, namely, Drs. Fersko, Kagan and Leichter, to "undertake a project of injecting cancer cells" and that they all turned me down. The falsehood of this claim is, in part, proven by the affidavit concerning Dr. Leichter signed by Dr. S. Korman. The only one of those three

physicians ("coordinators") to whom I suggested participation in the research project was Dr. Kagan. However, several days later, I informed Dr. Kagan that Dr. Custodio had consented to such participation so that Dr. Kagan's assistance was no longer needed. I also discussed the proposed research project with each of the other two coordinators on separate occasions in order to acquaint them with it and to obtain their respective opinions about the scientific value of the project. The opinion of each of the three physicians was indeed favorable.

The charges in Dr. Rosenfeld's affidavit that the experiment was conducted "surreptitiously" and in Dr. Jacobi's affidavit that proper notations on the patients' charts were not made are totally unwarranted and without foundation. There was no reason at all for secrecy concerning a bona fide research project which was being carried out in collaboration with one of the outstanding research institutions in the country. Dr. Custodio had been instructed by me to make appropriate entries in each of the patients' charts who received the test and, as far as I know, he complied with this instruction.

The claim contained on the last page of Dr. Rosenfeld's affidavit is completely unwarranted that I attempted to keep "Dr. Kagan and the others from talking about the project" by offering him an increase in salary and some private patients. The truth is that I reassured Dr. Kagan on August 27th that the request for an increase in salary which he had made repeatedly over the preceding 4 months would be granted. I also indicated that I would turn some of my private cases over to him and to other members of the staff in order to find more time for research. This discussion took place the day after my return from a two-week vacation, at which time I was unaware of Dr. Kagan's antagonistic attitude and of his intention to resign abruptly.

A report on this study is scheduled to be presented to the sixth Biennial International Transplantation Conference at the New York Academy of Sciences in February 1964.

c.

**Deogracias B. Custodio, M.D.—  
January 3, 1964**

I am a resident physician at the Jewish Chronic Disease Hospital. I have held this position since July 1, 1961 to June 30, 1962 and from July 1, 1963 up to date.

Some time in July of 1963 I was asked by

Dr. Mandel whether I would be interested in participating in a research project which would be done at Jewish Chronic Disease Hospital with doctors from Sloan-Kettering Institute. I was told that the project was financed by a grant from the U.S. Public Health Service and that the work was being carried on under the direction of Dr. Chester Southam of Sloan-Kettering Institute. The project involved injecting patients with cancer cell suspensions in order to determine what their rate of rejection of the injected material would be. I was told that similar tests had been made on cancer patients and on healthy prisoners at the Ohio State Penitentiary. The purpose of the project was to determine whether weak, debilitated, chronically sick patients would reject this material like normal individuals or whether the rate of rejection would resemble cancer patients. I knew from my medical experience and studies that there was no possibility of any patient developing cancer as a result of these injections. The material injected was a foreign body and must by the laws of biology and medicine be rejected by the human organism.

The charge in the petition that the purpose of the injections made at the Jewish Chronic Disease Hospital was to determine whether cancer could be induced by such injections just simply is not true. Cancer cannot be induced by such injections. The purpose of the test was to determine the patients' immunological reaction to the injection of cancer cells and not "to induce cancer."

On July 16, 1963, I met with Dr. Mandel, the director of medicine of the hospital, and Dr. Chester Southam and Dr. Arthur Levin of the Sloan-Kettering Institute. Dr. Southam demonstrated the techniques of injection on three patients. I injected the other 19 patients under his general supervision. The appropriate entry showing this injection was made on the chart of each patient in accordance with proper medical practice. Before any patient was injected, Dr. Mandel obtained the oral consent from the first 2 patients and I did from the next 20 patients. The patient was told that an injection of a cell suspension was planned as a skin test for immunity or resistance. The patient was also told that a lump would form within a few days which would last several weeks and gradually disappear. The patient was not told that the injection would contain cancer cells. The reason for this is that we did not wish to stir up any unnecessary anxieties, disturbances or phobias in our patients. There was no need to tell the

patients that the injected material contained cancer cells because it was of no consequence to the patients.

Drs. Southam and Levin were present when I asked for the oral consent of 20 patients. Dr. Mandel obtained the consent from the first 2 patients. Dr. Mandel was present when the first 2 patients were injected as well as Dr. Southam and Dr. Levin. I had no difficulty in communicating with the patients in English. The charge that some of the patients spoke only Yiddish is not correct. Dr. Levin, I am advised, speaks Yiddish, and he could have spoken to the patients in this language had it been necessary.

Nor is there any truth in the assertion made in the petition and in Dr. Jacobi's affidavit that some of the patients were mentally incompetent to give this consent. I have known some of these patients for at least 6 months and I have no difficulty in communicating with them. In my opinion, none of the patients was mentally incompetent so that they could not give their consents to the injections.

After the injections were made, I observed the patients with Dr. Southam and/or Dr. Levin twice a week during the first 3 or 4 weeks and weekly thereafter. As expected, the lump or nodule developed and disappeared within an average period from six to eight weeks. As expected, no harm or injury occurred, as a result of these injections, to any of the patients, with the exception of the transient lump mentioned.

For my services in connection with this project I was paid the sum of \$100.00 out of the research funds available to the Sloan-Kettering Institute from the U.S. Public Health Service grant.

d.

**Chester M. Southam, M.D.—  
January 5, 1964**

I am a licensed physician in the State of New York. I am employed as a full-time staff member of the Sloan-Kettering Institute for Cancer Research where I am chief of the section of clinical virology of the division of clinical chemotherapy, and chief of the section of oncogenic virology of the division of virology and immunology. I am an associate attending physician of Memorial Hospital for Cancer and Allied Diseases and an associate visiting physician of the James Ewing Hospital of the City of New York, on the chemotherapy service of the department of medicine. I am an associate profes-

sor of medicine of the School of Medicine of Cornell University. I have been with these institutions for approximately 15 years engaged in the practice of clinical medicine (medical management of cancer), medical teaching, and research in various phases of clinical and laboratory oncology.

Since 1954 one of the major types of research in which I have been engaged is the study of the relationships between immunological responses and cancer. The studies in 1954 revealed for the first time evidence of a major immunological defect in patients with advanced cancer. This was evidenced by the delayed rejection of homotransplants of neoplastic tissue-cultured human cells by such patients, in contrast to the prompt rejection which would, of course, be expected to occur since these are homotransplants (that is, these cells are foreign to the individual into whom they are injected).

A major deficiency of these investigations until the present year was the lack of direct evidence that the immunological deficiency observed in cancer patients was specifically related to cancer. Recently it was possible to establish this important point by the demonstration that patients who are chronically ill and debilitated due to various diseases other than cancer have a normal or near normal capacity to reject this same type of tissue-cultured cell transplant. This work was made possible through the collaboration of Dr. Emanuel Mandel, chief of medicine of the Jewish Chronic Disease Hospital in Brooklyn, and the cooperation of patients in that hospital.

It was possible to arrange for this collaboration because my present clinical research fellow, Dr. Arthur Levin, through personal acquaintances was able to discuss this work and the need for similar studies in non-cancer patients with persons affiliated with the Jewish Chronic Disease Hospital. Through such persons we were referred to Mr. Siegel, executive director of that hospital, who referred us to Dr. Mandel, chief of medicine, to discuss the problem and the possibility of a collaborative research project. After thorough discussion of the purpose, importance, procedures, reactions, and previous scientific publications, Dr. Mandel indicated his interest in such studies and some time later arrangements for a preliminary study were made. He was, of course, aware that homologous cells (cells from a human other than the person in whom they are injected) could not long continue to persist unless immunological reactivity was



severely impaired, and that there was no possibility of inducing cancer or causing any severe reaction in his patients.

This study was initiated on July 16, 1963, at which time each of the 22 patients was given two injections under the skin of a suspension of tissue-cultured cells of human neoplastic origin. These represented three long-established cell lines known as HEp 3, HEp 2, and RP 41. The injections were made on the anterior surface of one thigh at two sites just beneath the skin. The injections were made in the first three patients by me, as a demonstration of the technique for the instruction of Dr. Mandel and the medical resident (Dr. Custodio) who had expressed an interest in assisting in this project and who did all of the injections after my demonstration. After observing the technique, Dr. Mandel left and all of the tests on the remaining patients were done by Dr. Custodio in the presence of Dr. Arthur Levin and myself. Dr. Levin and I prepared the cell suspensions in the syringes ready for the injections and recorded details of the procedure and the patient's name in our date book. Explanations to the patient, obtaining the consents, and the writing of notes in the chart, as well as the injection itself, were the activities of Dr. Custodio. To the best of my knowledge each patient on whom these tests were done was informed that it was a test that would measure his or her reactions or defense reactions and further information was given whenever the patient wished it. To the best of my knowledge each patient did indicate his consent to this test.

The test sites were checked by Dr. Custodio and Dr. Levin at intervals of not longer than four days for the next four weeks by which time almost all reaction had disappeared. They were further checked for an additional three or four weeks until no person showed any further evidence of reaction at the test site. I, too, accompanied Drs. Custodio and Levin on three of these follow-up visits (July 19th, August 13th and August 20th).

I have seen the petition herein, in which it is charged that the purpose of the tests at the Jewish Chronic Disease Hospital was to see whether "cancer could be induced" in the patients who were injected with the cell-suspension material which was used. I wish again to assert categorically that this was not the intention of the study and that it is biologically and medically impossible to induce cancer by this means. The purpose was to test the immunological re-

sistance of these patients to cancer. No patient who was injected was harmed in the slightest by the test which was made on him, except for the lump or nodule which formed at the test site, and which disappeared within four to eight weeks. During the past 10 years, as we have seen, cancer cells have been implanted in almost 600 well persons and cancer patients. They have caused no untoward effects and have not resulted in the development of any cancers in either the well persons or those already suffering from their own cancers. The technique for measuring immune reactions is now standardized and the results are predictable. All the patients in the study undertaken at the Jewish Chronic Disease Hospital rejected the transplants as promptly as did the healthy persons. Thus it has been demonstrated that cancer patients lack an immune mechanism present in other individuals, including chronically diseased patients.

Thus the research done at the Jewish Chronic Disease Hospital has enabled us to take one more step forward in the continuing battle against cancer.

e.

**Joseph L. Abramson, M.D.—  
January 3, 1964**

I am a physician, duly licensed to practice medicine in the State of New York since 1924. I hold the rank of consultant at Jewish Chronic Disease Hospital. I am a psychiatrist and neurologist. I was president of the medical staff of Jewish Chronic Disease Hospital from January 1, 1962 to December 31, 1963. I am consulting neuro-psychiatrist at Brooklyn Jewish and Swedish hospitals; I am a Diplomate in Neurology and in Psychiatry. I am assistant clinical professor in neurology at Downstate Medical Center; and a qualified psychiatrist in the State of New York.

Dr. Mendel Jacobi has submitted an affidavit in which he comments on the charts of five patients who allegedly were injected with cancer cells. The conclusion has been drawn from his analysis that these patients were mentally incompetent and could not give a rational consent to any injections made on them. This conclusion is not justified from the data used by Dr. Jacobi. I have examined Dr. Jacobi's affidavit and wish to make the following comments on the cases examined by him:

K14397: Does not say that he did or did not have aphasia and no indication that he did

not talk or fails to understand what was said to him. Even though "perverse, negative, resistant to therapy," does not indicate that he did not comprehend. No justification for statement that "a state of chronic uremia in which cerebration is generally poor" applied in this particular case.

#2990: At age 52 a note indicates that patient had "marked speech defect, marked irritable personality—who cries steadily and in a shrieking manner." The conclusion of the examiner at the time was that "this is due to the patient's low mentality, and lack of insight and judgment." There is nothing in the above statement to justify that the patient had a low mentality. Two months after the alleged injection, the patient signed consent which was acceptable to the surgeon, for an operation on the bladder. One can obviously say, that two months before, in July, he was just as well aware of his environment, and could give consent at that time.

#8183: Admitted to Jewish Chronic Disease Hospital 1958. He had been a patient at Brooklyn State Hospital in 1957 with diagnosis of "dementia praecox." There is nothing in the chart to indicate that he was incompetent, and a diagnosis of this condition, per se, does not indicate incompetence. There is a note to the effect that the "neurologic status had not improved." There is no note in the allegation as to his mental status when he was admitted.

#3762: There is absolutely nothing in the allegation to indicate that the patient was not competent mentally even though he made a suicidal attempt one month after the alleged injection.

B15918: There is absolutely nothing in the allegation of the patient's incompetency.

In the light of the above examination, I do not believe that any conclusion can be drawn that the five patients whose charts were examined by Dr. Jacobi were mentally incompetent.

f.

**Samuel Korman, M.D.—  
December 19, 1963**

I am a physician duly licensed to practice medicine in the State of New York.

I am the associate director of the Department of Medicine of the Jewish Chronic Disease Hospital and chief of the division of neoplastic diseases in that department.

On Tuesday, December 17, 1963, between 9:30 and 10:30 A.M., I was present in the office

of Dr. E. E. Mandel, director of medicine of the Jewish Chronic Disease Hospital, when he talked with Dr. David Leichter who had come to see Dr. Mandel about his reappointment to the attending staff of the hospital.

In the course of this conversation, Dr. Mandel read aloud the summary of an affidavit which Dr. Leichter had signed on September 12, 1963 pertaining to a research project that had been undertaken by Dr. Mandel in collaboration with Drs. Southam and Levin of the Sloan-Kettering Institute.

Upon listening to the reading of this summary, Dr. Leichter admitted that certain statements mentioned in the summary were untrue; to wit, that he (Dr. Leichter) had never been asked by Dr. Mandel "to undertake the project of injecting live cancer cells into non-cancer patients." Dr. Leichter further stated that he had not used the expression attributed to him in the summary that "... efforts were made to hush-up the complaints about the project." In addition, Dr. Leichter indicated that he (and Drs. P. M. Fersko and A. Kagan) had been quite upset at the time of their abrupt resignations from their salaried positions in the hospital (about August 27, 1963), and he conceded that they may not have used their best judgment in taking that action. Dr. Leichter also informed Dr. Mandel that he and the two doctors mentioned had made their affidavits on September 12th largely because of rumors that the hospital was going to make formal charges against them for abandonment of patients. Finally, Dr. Leichter agreed with Dr. Mandel and the undersigned that injection of the cell suspensions which were used in the project constituted no hazard whatsoever to the patients involved with respect to production of cancer.

## 5.

### **Minutes of Grievance Committee of the Medical Staff, Jewish Chronic Disease Hospital—September 7, 1963**

Attendance: Drs. David Kershner, Mayer  
E. Ross, Harry Weiner  
Nathan A. Lewis (D.D.S.),  
Joseph L. Abramson  
By Invitation: Solomon L. Siegel, execu-  
tive director  
Benjamin Saltzman, chair-  
man of executive com-  
mittee of board of direc-  
tors



Harry B. Albert, director  
and hospital counsel  
Absent: Dr. Samuel Millman, chair-  
man of grievance commit-  
tee  
Herman W. Shane, chair-  
man of board of directors

In the absence of Dr. Millman, Dr. Abramson presided. He opened the meeting indicating that a complaint had been brought to his attention in regard to certain research activities which had been done under instructions of Dr. Mandel, director of medicine. He read a letter of resignation by three coordinators in medicine, signed jointly by Drs. Kagan, Fersko and Leichter. (A copy of the letter is made part of these minutes). He then called upon Mr. Siegel, executive director, to relate the sequence of events leading up to his meeting.

*Testimony by Solomon L. Siegel, Executive Director*

He arrived at Miami Beach on vacation on Wednesday, August 14, 1963. Within an hour after checking in at a hotel, he received an emotionally frantic call from Mrs. M. Tulipan urging him to return to the hospital immediately because "something terrible has happened, which cannot be discussed on the telephone." Alarmed, Mr. Siegel arranged to return by plane and called Mrs. Tulipan at her home that evening to make arrangements to be picked up at the airport. It was during this call that Mrs. Tulipan told Mr. Siegel that "some of our patients were injected with live cancer cells" and indicated that terrible consequences would result.

Mr. Siegel arrived at the hospital Thursday afternoon and immediately saw Mrs. Tulipan. She had learned through Dr. Rosenfeld that a number of patients had been injected with live cancer cells without knowledge of Dr. Rosenfeld; that Dr. Rosenfeld learned of these injections when one of the patients called him to ask why he had been given the injection; that he then investigated and traced the injections to Dr. Custodio (resident) who gave these injections for a certain Dr. Southam and another doctor, both from Memorial Hospital who are friends of Dr. Mandel.

Under obvious emotional strain, Mrs. Tulipan practically demanded that Mr. Siegel fire Dr. Mandel at once or get his resignation. She further advised Mr. Siegel that the patients were not advised on nature of or reason for

injection. She wanted to know if the medical board, research committee, or executive director knew of this project. Mr. Siegel told her he had no knowledge of this project and was quite sure neither of the other bodies had prior knowledge.

Mr. Siegel visited Dr. Rosenfeld at his private office that afternoon. He related the story substantially the same as had been presented by Mrs. Tulipan; while he had no personal reason for finding fault with Dr. Mandel, he was terribly hurt because Dr. Mandel had not discussed the project with him. He kept advising Mr. Siegel that Dr. Mandel should be fired, or that he should be forced to resign and he placed great emphasis on the fact that three coordinators had been approached by Dr. Mandel to participate in this project and each had turned it down because they told him written informed consent was required. He named Drs. Kagan, Fersko and Leichter as the individuals who had refused to participate. He also named Dr. Custodio, a resident, as the physician who had done the injections. Mr. Siegel told Dr. Rosenfeld he intended interviewing all the persons named and advised him that Dr. Mandel was on vacation and he did not feel any conclusions could be drawn without discussing the problem with Dr. Mandel. Dr. Rosenfeld kept warning Mr. Siegel that "the thing would blow up," that he'd "better get good legal advice," "that this was a terrible thing that had been done," "that he'd better fire Dr. Mandel at once," etc., etc.

The following morning, Friday, August 16, 1963, Mr. Siegel interviewed Dr. Leichter, Dr. Kagan and Dr. Custodio. Dr. Fersko was on vacation and was not available.

Dr. Leichter told him that early in July, Dr. Mandel had discussed the project with him and had advised Dr. Mandel that written informed consent was required, and that Dr. Mandel never again approached him on this matter. He was very resentful of Dr. Mandel, he did not know how many patients were injected, or who the patients were. Mr. Siegel felt the resentment and bitterness directed toward Dr. Mandel were really based on the fact that he was being superseded as chief of the cancer service by a new full-time physician (Dr. Korman) who was to join the staff as associate director of medicine. Dr. Leichter placed emphasis on his distrust of Dr. Mandel and also implied that the thing would blow up.

Mr. Siegel told Dr. Leichter that the matter of injecting the patients appeared to be in the province of the grievance committee of the

medical staff and that it would be given to Dr. Abramson upon his return from vacation. He requested that Dr. Leichter not be hasty in his actions.

Mr. Siegel then met with Dr. Kagan who likewise was approached by Dr. Mandel on the proposed project. Dr. Kagan stated that he had advised him that written informed consent was required and that he did not think the patients would give such consent. Dr. Mandel never again discussed the matter with him. He had heard that injections had been given but did not know how many or to which patients. He felt this was done illegally. He appeared confused as to what course to take, feeling that knowledge that an illegal act was done implicated him and that he was morally bound to make it known. Further conversation revealed that he had other complaints against Dr. Mandel as follows:

1. He had been inveigled into using other experimental drugs without getting written consent and that he was fearful of possible damaging consequences;
2. That he wanted an appointment at the State University and did not feel Dr. Mandel was really trying to get him one.
3. That he thought he was entitled to an increase in salary and did not believe Dr. Mandel was trying to get him one.

Mr. Siegel stated he had learned that Dr. Mandel had spoken and written to Dr. Eichna at the university regarding Dr. Kagan's appointment, but that the latter had hurt his own cause when he wrote directly to Dr. Eichna without Dr. Mandel's knowledge. Dr. Eichna looked unfavorably upon such behavior.

In regard to an increase, Dr. Mandel had spoken and written to Mr. Siegel, requesting an increase for Dr. Kagan and was advised this was to be referred to the finance committee. Not believing that Dr. Mandel was trying, Dr. Kagan wrote directly to Mr. Siegel without Dr. Mandel's knowledge. Mr. Siegel looked upon this procedure with disfavor and so advised Dr. Kagan.

Mr. Siegel advised Dr. Kagan that the findings in regard to the injections would be turned over to Dr. Abramson upon his return from vacation and requested that Dr. Kagan take no hasty action. Dr. Kagan was extremely bitter toward Dr. Mandel.

Mr. Siegel then interviewed Dr. Custodio, a resident in the Blumberg Building. Dr. Custodio stated that he was engaged in a project to study the immunological response of chroni-

cally ill, debilitated patients to a cell suspension of tissue cultures taken from cancer patients. He stated he was interested in research and was glad to cooperate, since this appeared to be a worthwhile study, that he was assured by Dr. Mandel (Southam and Levin of Sloan-Kettering Institute) with absolute certainty that there could be no ill effects on the patients; that written consents were not really required because of the negative emotional impact of reference to "cancer"; that he had advised each patient and gotten their verbal consent, witnessed by Drs. Southam and Levin, that they would be given a skin test to determine their immunological reaction to foreign injections; that small nodules would develop and would then disappear after a few weeks; that on the advice of Dr. Mandel, he wrote into each patient's chart that a "cell injection had been given in either right or left thigh." He submitted the names of the patients who had been injected.

Mr. Siegel, after these interviews, called Mr. Samuel Bisgyer, hospital attorney, informing him of this problem and to seek his advice. Mr. Bisgyer pleaded not to get him involved, since he was not well and not really well-informed on such hospital problems.

He then called Mr. Harry Albert, an attorney on the board of directors and met with him that night. Mr. Albert thought there was no great urgency and that Mr. Siegel could return to Florida.

Mr. Siegel returned to Florida, but feeling ill-at-ease, returned the following Thursday, called Dr. Abramson's office with a message for Dr. Abramson to call Mr. Siegel immediately upon his arrival.

In frequent conversations with Mrs. Tulipan and Dr. Rosenfeld, Mr. Siegel stated he was constantly being reminded that he'd better get Dr. Mandel out of the hospital. He decided to visit Mr. Benjamin Saltzman, chairman of the executive committee, who was the next ranking officer in the absence of Mr. Isaac Albert, to acquaint him with the facts.

In the interim, Dr. Mandel returned from vacation. Mr. Siegel advised him of the nature of the problem and that he was planning to present the case to the medical staff. Dr. Mandel confidently stated that he had done nothing wrong and would gladly submit to evaluation by this body.

Dr. Abramson returned from vacation and Mr. Siegel met with him on Wednesday, August 28. Dr. Abramson already had on his desk a

letter of resignation signed by Drs. Kagan, Fersko and Leichter. He readily accepted the matter as one within the province of the medical staff, felt that Mr. Siegel had handled the matter properly up to this point.

*Testimony by Dr. Abramson, President of the Medical Staff*

Dr. Abramson stated that he personally interviewed the doctors who had resigned. In regard to their attitude that written consent was required, he tried to allay their fears, since they had not participated in this project in any way and were in no way involved. He felt that none of the men, Drs. Kagan, Fersko and Leichter, had any substantial basis for their position, and were using this incident to support other personal complaints against Dr. Mandel. Dr. Abramson felt none of their reasons justified their actions and so advised these doctors.

Mr. Saltzman suggested that the committee consider the medical and legal aspects of the complaint, rather than the extraneous attitude of the coordinators.

*Testimony by Dr. Southam*

At this point, Dr. Southam of the Sloan-Kettering Institute arrived and was invited for questioning. The following facts were brought out in the questioning:

1. The work has been in progress for about 10 years, and various papers on the subject have been published in medical journals.
2. Cancer cells are used rather than other tissue cells because cancer cells are reproduced easier in measurable amounts and the rejection period is measurable.
3. In normal patients, a measurable nodule develops in about two weeks and disappears in four to six weeks. In cancer patients, the nodule might not disappear for a few months since the immunological rejection is impaired.
4. Purpose of using our patients was to determine whether the delayed rejection was unique for cancer patients only, or whether a similar reaction would be present in chronically ill patients suffering from debilitating diseases.
5. All patients injected showed the normal rejection associated with healthy individuals except one patient, and this one

had a prior history of rectal surgery, based on information subsequently taken from his chart.

6. Each patient was told in advance of the test and each one consented. The word "cancer" was not used because of emotional reaction to use of word in addition to the fact that the use of cancer cells was immaterial. There was absolute certainty that there would be no permanent side effects.
7. This hospital was approached because of its reputation as a progressive medical institution interested in research and teaching, coupled with the fact that we had large numbers of debilitated patients with diseases other than cancer.
8. The tests were extremely useful. Dr. Horsfall, director of Sloan-Kettering Institute, who was never overly enthused about this project, upon hearing of the test results at our hospital, called Dr. Southam to congratulate him on his successful findings.
9. Dr. Southam's concern was for scientific progress and he would be extremely pleased if the tests could continue at our hospital.

Dr. Abramson read a notarized affidavit signed by Dr. Custodio stating that each patient had given verbal consent for the injection and that the consent was witnessed by Drs. Southam and Levin. Dr. Southam indicated that he would willingly testify that he witnessed such consent.

*Testimony by Dr. Abramson (continued)*

Dr. Abramson learned from Mr. Siegel that a reporter from the *World-Telegram* had called for information regarding resignation of three doctors because of certain research work. Dr. Abramson stated he saw Dr. Kagan who also stated he was approached by a man from the *World-Telegram*. He swore he gave no information and referred the reporter to officials at the hospital.

*Discussion*

Dr. David Kershner was highly impressed by the facts as presented. He felt the reaction of the coordinators to the project was not at all their affair, since they did not participate in the project.

He felt that it is an obligation of an institu-

tion such as ours to encourage research. We have a wealth of patient material that had never been properly utilized.

While it is advisable, wherever possible, to get written consent, it is not required by law. He suggested that use of the word "cancer" should be avoided for purpose of advancing ease of doing research projects.

Dr. Kershner stated that he was fully aware, based on his readings and clinical work, that there could not possibly be any danger to the patient in the project in question. He offered a vote of thanks to Mr. Siegel for the calm and professional manner in which the problem was handled, under very trying, emotional circumstances. He suggested that Dr. Mandel explain why the project was not handled through the research committee.

Dr. Kershner recommended the prompt acceptance of the resignations of the three coordinators and continuation of the research project, and that any calls for information from any source be referred to proper channels—Mr. Siegel, Dr. Abramson and Dr. Mandel.

Further discussion revealed that under our medical staff constitution, pilot studies may be initiated by directors of services without prior review by the research committee. However, should the director desire to continue a project, he must submit a protocol to the research committee within 60 days after initiation of the pilot study.

Dr. Mandel was called in. He indicated that in addition to the fact that this was a pilot study with a very limited number of patients, he had every intention of referring a protocol to the research committee. He admitted an oversight in not advising Dr. Rosenfeld of the study, but it was the general consensus of opinion that the director of service is not really obligated to advise all his subordinates on such matters. This question could offer no basis for charges against Dr. Mandel.

Dr. Mandel stated that the coordinators were never asked to give the injections. The projects were merely discussed with them to get their opinions. Each one thought the project had merit. It was totally untrue that they advised him that written informed consent was necessary.

He stated that only Dr. Kagan was asked if he was interested in participating, but not having gotten an answer in three days, Dr. Mandel assumed that he was too busy studying for

his board examinations and didn't want to become involved.

Mr. Harry Albert indicated that Dr. Mandel's intent should be judged in association with the very reputable Memorial Hospital and with the outstanding work done by Dr. Southam who is also associate professor of medicine at Cornell Medical School. He also questioned the involvement of Mrs. Tulipan in a complicated medical matter and strongly urged that she be forbidden to be further involved in this matter.

Dr. Abramson stated that none of the coordinators interviewed could adequately explain why Mrs. Tulipan was involved in a medical matter and why they had sent a copy of the letter of resignation to her.

Dr. Harry Weiner drew the following conclusions:

1. Dr. Mandel did not violate the constitution.
2. Resignations should be accepted.
3. A special committee should seek out facts as to who was involved in disseminating misinformation.
4. Report should be presented to medical board.
5. Responsibility for report should be the executive director's and chairman of the executive committee.

Dr. Lewis felt that the coordinators deserved to be heard. However, it was stated that they resigned without notice after being advised as to proper procedure for disposing of said matter; and that they resigned without arranging for adequate coverage of patients.

### *Conclusions*

1. Resignations by Drs. Kagan, Fersko and Leichter were irresponsible and should be accepted.
2. There were no reasonable complaints against Dr. Mandel under the medical staff constitution.
3. A report should be submitted to medical staff.
4. All public relations matters related to this incident be referred to executive director.
5. Any medical reports resulting from the tests should be referred to research committee.
6. The scientific information resulting from this study was of outstanding significance and we should lend our support in continuing this project.

## 6.

**Reply Affidavits for Petitioner**

a.

**Bernard J. Pisani, M.D.—  
January 17, 1964**

I was admitted to the practice of medicine and surgery in the State of New York since 1933. I have been a past president of the Medical Society of the County of New York. From 1954 to date I have served as director of obstetrics and gynecology at St. Vincent's Hospital.

The question has been put to me as to the propriety of a nontherapeutic experiment consisting of injecting live cancer cells into non-cancerous patients who have not been told that this injection consists of live cancer cells and who have not given their informed consent to this experiment. (By *informed* consent is meant the voluntary agreement of a patient capable of normal comprehension, *after* this patient has been told in lay language, the nature of the experiment, its hazards, present and potential, its complications and sequelae).

In answer to this question I state unequivocally that such an experiment, without the informed consent of the patient, is improper, unethical and immoral. Under no circumstances as a physician would I participate in or condone this type of experiment on any human being. The *known* hazards of such experiments include growth of nodules and tumors and may result in a *metastases of cancer* if the patient does not reject these cells.

In my practice of medicine and surgery and under the rules and regulations of St. Vincent's Hospital the informed written consent of the patient is required for all unusual or major procedures, therapeutic and experimental.

b.

**Mendel Jacobi, M.D.—January 16, 1964**

I hereby reaffirm the correctness of the statements made by me in my affidavit dated December 11, 1963.

It is stated in various affidavits filed with the answer of the respondents that the injections of cancer cells into patients at the Jewish Chronic Disease Hospital were performed merely to test the patients' immunologic reaction to these cells, not in order to determine whether cancer could be produced by such injections.

It is further stated that Dr. Mandel "was, of course, aware that homologous cells (cells from a human other than the person in whom they are injected) could not long continue to persist unless immunological reactivity was severely impaired." Finally it is indicated that previously performed experiments had demonstrated that delayed reaction of neoplastic tissue-cultured human cell homotransplants had occurred in cancer patients "due to an impaired immunological capacity" and that "At present the only method of evaluating this type of immunologic capacity is to observe the efficiency with which homotransplants are rejected."

From these statements alone it follows that the very measure of the immunologic response the experiments performed at the Jewish Chronic Disease Hospital were to test was to be the rate of rejection of the cancerous nodule expected to develop at the injection site. If the patients' immunologic responsiveness were severely impaired—and this, from the above-quoted statement, could only have been determined after such rejection of the locally produced cancer—cancer development could have occurred. Obviously, then, saying that the injections were for the purpose of testing immunological responsiveness is merely a reverse manner of saying that it was for the purpose of establishing whether a cancer would be rejected by these debilitated patients and if so, at what rate or with what degree of completeness.

As a matter of fact, if these homotransplants were to behave as had those in cancerous patients, there was the real possibility that the locally produced cancerous nodule would grow progressively and even metastasize. In a paper published in *Science*, Dr. Southam and associates described the homotransplants of cancer cells into 14 cancer-bearing patients, noted the development of a cancer nodule at the implantation site in from 5 to 10 days after the injection, and that it attained a maximum diameter of  $\frac{1}{2}$  to 2 centimeters, in 1 to 2 weeks, at which time the nodules were excised completely for histologic study.

It is urged in several of the affidavits that cancer was not produced by the injections and/or that no patient was harmed in the slightest.

From one with the specific experience in the cancer field such as Dr. Southam has, or even from one who has not so limited his experience but has been in practice as long as Dr. Mandel, such statements are quite surprising in view of

the fact that cancer, even when completely clinically eradicated by adequate and even intensive treatment, is known to recur after long periods of latency free of all evidence of cancer. It is precisely for this reason that, in the field of cancer, one speaks not of cure but of 5-year, 10-year, 15-year, 20-year, etc. cure, meaning only that the cancer has not reappeared during such intervals. In view of the recurrence of the homotransplanted cancer in the debilitated cancerous patients, and in view of the fact that this was deemed due to debility rather than to their intrinsic cancer per se, and that, even now after the instant experiments, the basis for injected tumor rejection remains unknown, all that is presently warranted is the statement that the homotransplants of July 16, 1963 have disappeared as determined by local inspection and/or palpation and that some 2 months after the injections (or possibly some 5 months thereafter if the patients were re-examined at or about the date of the affidavits) no cancer is apparently present, a statement by no means equivalent to the non-development of, or freedom from, cancer in the patient.

Actually the fact that one patient, clinically free from evidences of cancer for the many preceding years of his hospital stay, developed overt clinical cancer of the bladder some 2 months after the homotransplants is rather disquieting. There is animal experimental evidence to indicate that delicate tumor-host relationship balances exist and that the growth rate of implanted tumors may upset these balances in manner adverse to the animal host. In the present vague state of knowledge as to the nature of these relationships one wonders whether the very development of a tumor nodule at the site of the cancer cells injected into this patient and its subsequent rejection by the patient—presumably this rejection involved the patient's defense mechanisms against cancer—had, in fact, exhausted these defenses, and had so upset the patient's tumor-host balance to the end that the bladder cancer, previously latent (i.e. kept from growth activity by the patient's body defenses) had now attained active growth capability and overt clinical activity. If this is the patient who, according to Dr. Mandel's affidavit "died in October following a bladder operation," this sequence of events is even more possibly significant and disturbing.

From the above facts one must conclude that the statements anent the non-development of cancer in the patients injected on July 16,

1963 or that they were harmed in no way are presently premature and unwarranted. One patient in this series is certainly dead under circumstances possibly indicating at least an indirect influence of the injections; as to the others, the post-injection period will have to extend for many years before such statement can become unequivocally demonstrable. Parenthetically it should be noted that, to the best of my knowledge, no one other than the people involved in these injection experiments at the hospital has made an independent examination of these injected patients with respect to the presence of cancer or of other complications possibly sequential to the injection, and that, if the 5 charts examined by me on October 4, 1963, are indicative, the records in the charts of the patients injected are in such a state that no conclusions as to any such developments were, or will be, possible.

#### NOTE

CHESTER M. SOUTHAM, ALICE E. MOORE, AND  
CORNELIUS P. RHOADS  
HOMOTRANSPLANTATION OF HUMAN  
CELL LINES\*

The development of human neoplastic cell lines that can be grown serially in tissue cultures and in heterologous hosts has made necessary the investigation of the capacity of such cells to grow in a homologous (human) recipient. Such studies are of fundamental importance to our knowledge of tissue transplantation and host defense mechanisms. In addition, there is the possible danger of initiating neoplastic disease by accidental inoculation during laboratory investigation or by injection with such cells or cell products if they should be used for production of virus vaccine. This article is a preliminary report of a continuing study of (i) the persistence and growth of neoplastic human cell lines after homologous transplantation and (ii) host reactions to such implants.

All recipients were volunteers who were aware of the general purposes of the study and the nature of the implanted materials and who were agreeable to subsequent biopsies.

\* \* \*

. . . Usually a single preparation was inoculated at one or two sites, but a few recipients

\* 125 *Science* 158-160 (1957). Reprinted by permission.

received two to four cell types simultaneously, and one received a total of seven preparations on two occasions. Complete excisional biopsies were usually performed as soon as a definitely palpable nodule appeared. In some recent studies, excision was delayed to study duration of growth and the process of regression.

Initial studies were restricted to volunteer patients with advanced incurable cancer and a very short life expectancy. Many had infectious and metabolic complications and cachexia. None had received treatment with steroid hormones, ACTH, marrow-depressing agents such as nitrogen mustard, or x-rays during the three months preceding the studies, and none received any of these treatments during the course of the studies.

Slight local induration and erythema frequently followed inoculations but subsided completely by the third day. Human embryonic fibroblasts with normal cytology were inoculated in three patients. No growth was detected, but neoplastic cells inoculated simultaneously into the same patients did grow. No other normal cells were available for study. Four epithelial cell lines of normal origin were inoculated in seven patients, usually produced nodules, and one of these recurred in one patient. These cells cannot be considered normal because they had developed neoplastic characteristics during tissue culture passage, and the nodules were histologically diagnosed as cancer.

Twenty-four homologous implantations of seven cancer cell lines (originally isolated from cancer tissues) were made in 14 cancer patients between February 1954 and July 1956. All seven lines multiplied in most of the recipients, as indicated by formation of a palpable nodule at the implantation site and by the finding of healthy cancer cells with active mitoses in the biopsies. . . . Usually the nodule appeared 5 to 10 days after implantation, reached a maximum diameter of  $\frac{1}{2}$  to 2 cm in 1 to 2 weeks, and was then excised completely for histologic study.

If they were not immediately excised, the implants usually regressed spontaneously and completely by 4 to 6 weeks. However, in four patients there was recurrence of cancer growth after biopsy at several implant sites. Three of these recurrences were completely removed by a repeat excision on the 19th, 42nd, and 77th days, respectively. In two patients, some of the recurrent nodules grew progressively until the patients died, 42 days and 57 days after the implantations. . . . In one of these patients, the

HEp#3 metastasized to the axillary nodes. This patient's own cancer was uterine adenocarcinoma, readily distinguished from the implanted cells, and at autopsy was found to be confined to the abdomen and perineum.

Studies to determine whether these homologous cancer cells had a similar capacity for propagation in normal healthy human beings were undertaken at the Ohio State Penitentiary in collaboration with Charles Doan and Richard Brooks and with the cooperation of John Porterfield and R. W. Alvis, warden. From a large group of volunteers, 14 were chosen for the initial study. Methods were identical with the aforementioned ones, except that only tissue cultures were used, and all studies were done simultaneously in June 1956.

There were well-defined differences in the behavior of the implants as compared with those seen in cancer patients. The initial inflammatory reaction was more marked in degree and duration, usually persisting for 1 week or more. The greatest reactions were at HEp#3 implantation sites where (in two patients) sterile abscesses formed. A firm nodule appeared at each site and reached 1 to 3 cm diameter by 14 days. By this time, erythema and edema were subsiding, and one or two nodules were excised from each volunteer. The remaining implants started to regress spontaneously by 3 weeks after implantation and were nonpalpable by 4 weeks. There has been no recurrence in the subsequent 5 months. Histologic sections of all day-14 biopsies showed a marked inflammatory reaction with mononuclear cells predominating. Aggregates of cancer cells with mitotic activity were present in only four of the 15 biopsies, and from two of these the cancer cells were reisolated in tissue culture. HcLa cells and Chang's conjunctive cells were not found in these biopsies.

Although it is tempting to postulate that the observed difference in receptivity of cancer cell homografts between normal and cancer patients is related to cancer per se, there is no present evidence against the more plausible explanation that the difference is due merely to the general debility of the cancer patients. However, no consistent differences such as uremia, hematologic abnormalities, or medications can be adduced to explain the apparent weakness of defenses in the cancer patients. Neither did the cancer patients have an "immunologic paralysis" since they did produce antibodies against viruses that were inoculated at about the same



time in experimental therapeutic studies. Further studies designed to detect possible differences in cellular and humoral defense mechanisms are in progress.

\* \* \*

We, as well as our collaborators, wish to express our appreciation of and admiration for these volunteers, both cancer patients and normal individuals, who, without expectation or possibility of personal gain, have made these studies possible.

\* \* \*

c. **David Leichter, M.D.—January 16, 1964**

In reply to the false, unwarranted accusations made against me in the answering affidavits submitted by the respondent, I wish to state the following:

As to the affidavit verified by me on September 12, 1963, I reaffirm the correctness of all my statements contained therein. The respondent seeks to impeach my credibility and the correctness of my statements by alleging that I have repudiated certain statements therein contained. This is utterly false.

7.

**Rebuttal Affidavits for Respondent**

a. **Chester M. Southam, M.D.—  
February 4, 1964**

I address myself first to the question of the measure of risk of bodily harm to the patients who were the subject of the procedures in question at the Jewish Chronic Disease Hospital. At the outset I should say that in clinical procedures neither I nor any scientist or doctor can deal in absolutes. We are always limited, at least when dealing with the human body, to speaking in terms of measurable risks. Thus while no doctor or scientist can say as to any clinical procedure, even the simplest, that there is *no possibility* of untoward results, we are constantly required, both in therapeutic and in investigative procedures, to make judgments as to whether there is any unusual risk of untoward results, and if so, the degree of that risk. In terms of this standard I unhesitatingly assert that on the basis of

present biological knowledge supplemented by clinical experience to date there was no practical possibility of untoward results to the patients who received injections of homotransplants in the form of tissue-cultured cells derived from other patients. The probability of any unforeseen deleterious consequences of this test is so extremely small as to be comparable to numerous other procedures used routinely in clinical medicine for therapeutic, diagnostic, or investigative purposes, e.g., blood transfusions, intravenous pyelograms (kidney x-rays), or tuberculin tests. The fact that these cells were tissue-cultured cancer cells did not measurably increase any risk inherent in the procedure because, being foreign to the recipient (the person injected), they bring about an immunologic reaction (defense reaction, rejection reaction) that ultimately causes their destruction and elimination.

It has been known for many years that a human being will reject cells transplanted from another human being unless both are of precisely the same genetic makeup (i.e., identical twins). In fact, intensive clinical studies are now being carried on at many research centers attempting to find methods (such as treatment with certain drugs or x-ray) to overcome this rejection reaction in the hope that diseased organs, such as kidneys, might be successfully replaced. While the precise mechanisms of cell rejection are not yet known, the fact that such mechanisms exist is beyond question. The efficiency of this type of immunological reaction can be measured in terms of the time required for complete rejection of homotransplanted cells. As yet no other method of measuring this reaction has been found, and tissue-cultured cancer cells are the only kind of cells which provide sufficient reproducibility for comparison of results in different individuals at different times.

The three lines of cells derived from human cancer which were used in the studies at the Jewish Chronic Disease Hospital were derived from tumor tissues of three patients, from 4 to 12 years ago. Since that time these cells have been cultivated in sterile bottles in the laboratory in a solution of nutrients which include salts, vitamins and blood serum. This is the process called tissue culture. After such years of growth under these artificial laboratory conditions each line of cultured cells has a high degree of uniformity and, consequently, the reaction which it will produce is highly predictable. I have had an extensive experience with each of these three cell lines in homotransplantation studies in cancer



patients and in healthy volunteers during the past several years.

In the early 1950's it began to appear that the defense mechanisms (i.e., the mechanism of rejection of homotransplants) of those persons who develop cancer might be in some way impaired. The most striking indication of this was the result of clinical tests on a limited number of patients with terminal cancer as reported over the signatures of myself and Drs. Rhoads and Moore in *Science*. These were all patients suffering from advanced stages of widely disseminated cancer for whom there was no known method of treatment to either inhibit their disease or prolong their lives, each of whom died as the result of his own cancer within a relatively short time. In view of the then state of knowledge the precise details of the procedure were explained and the patients freely and readily consented.

The significant result of the test was that the rate of rejection of the foreign transplants was in all cases slower than would have been expected, indicating that there was some impairment of their immunological reaction. Because these patients had far advanced cancer before the homotransplants were injected, they did not survive for long after the tests were performed. Obviously this was not the result of the test, but rather was the reason that these particular patients were selected for these earliest tests. In no case was the patient deleteriously affected by the implants. Several patients in this initial group and in subsequent groups had not rejected their transplants in the brief interval between the start of the test and their death. In fact, at autopsy a lymph node from the armpit of one of these patients contained unrejected cancer cells of the type used for the test. (These lymph nodes are in the natural route of drainage from the forearm where the test was made in this patient.)

Prior to the publication of the article in question tests were made on a number of volunteer healthy human beings in the Ohio Penitentiary. In all such cases the foreign transplants were quickly and completely rejected, as would have been expected.

After the initial tests reported in *Science*, intensive studies were undertaken, designed to increase our body of knowledge as to the immunological reaction both of normal healthy persons and those with cancer, to homotransplants of tissue-cultured lines of human cells derived from normal and tumor tissues. Between the time of the initial tests and July 16, 1963 (the

date on which the injections were made in Jewish Chronic Disease Hospital) approximately 600 persons had been studied by means of the techniques employed at Jewish Chronic Disease Hospital, approximately 300 of whom were patients with cancer and 300 healthy, normal persons. In every healthy recipient of tissue-cultured cells, these foreign transplants were rejected with uniform promptness. Some patients with cancer rejected the cells less rapidly and after significantly varying intervals of time. Patients in the earlier stages of neoplastic disease showed normal or only slightly impaired rejection reaction. Patients in the terminal stages of cancer showed the greatest deficiency in these immunological defense mechanisms (as measured by the length of time to effect rejection) and in several such persons rejection had not been accomplished in the few weeks or months that elapsed between injection of the test cells and the patient's death from his own cancer. These patients died from the effects of their own cancer before the expected ultimate rejection of the implants. The studies also demonstrated a correlation between the rate of rejection of homotransplanted cancer cells and the patient's apparent ability to restrain his own disease, thus providing additional direct evidence that patients may have immunological (defense) mechanisms to restrain their own cancer. These results, of course, give hope that, through further clinical research, methods of stimulating such mechanisms to greater efficacy can be developed.

The studies of healthy, normal persons at the Ohio Penitentiary, aside from demonstrating that the normal body will reject cancer cell homotransplants with the same efficiency as other types of homotransplants, further indicated the potentially highly significant fact that the body's rate of rejection increased with successive implantations of foreign cancer cells, suggesting long-run possibilities of building up the immunological mechanisms where deficiencies now occur. At present, studies are being continued to verify these scientific observations and to investigate their possible applicability to the treatment and prevention of human cancer. Such studies of human cancer can be accomplished only through the cooperation of patients and healthy volunteers.

Until the investigation conducted at the Jewish Chronic Disease Hospital, there was no direct clinical evidence that the impairment of the immunologic responses in patients with advanced cancer (as measured by the slow rate at

which they rejected homotransplants) was associated with the fact that they had cancer rather than with the fact that they were in a debilitated state. This study provided direct clinical evidence that indeed the impairment was associated with the fact of cancer rather than general debilitation. The patients at Jewish Chronic Disease Hospital reacted in essentially the same manner as normal, healthy human beings. I want to make perfectly clear that the question in this investigation was not whether the patients would reject the tissue-cultured cancer cell homotransplants. The only question was how fast would the body mobilize its resources of rejection. Three patients known to have cancer were also included in these tests. It was expected that rejection in the three cancer patients might be delayed, consistent with our previous experience in cancer patients, but that rejection would occur after the predicted delay unless these patients succumbed very rapidly to their own cancer.

I next turn to the question of procedures. In the early stages of this clinical research and, indeed, until the last few years a full explanation was given to the patient or healthy volunteer, including the fact that the techniques employed were not designed for his own therapy, the nature of the cultured cells involved, the general purposes of the test and the expected reactions. More recently, as our body of knowledge has increased and the course of reaction to the injections became predictable, we have simply explained that the procedure was a test which had nothing to do with treatment, that it involved the injection of foreign material, described the expected course of reaction, and that its purpose was to determine the rate at which the expected nodules would develop and then regress. In all instances in which the test was done the patients have readily given their consent, and the tests were not performed if such consent was not readily given. Unless the patient inquired, we refrained from describing the precise nature of the human cells (i.e., that they had originally been derived from tumors and then grown in tissue culture) for the reason that in my own professional judgment as well as that of my professional colleagues who had followed the course of these experiments, the precise nature of the foreign cells was irrelevant to the bodily reactions which could be expected to occur.

This course was followed, I submit, not out of any disregard for the rights or best interests of the patient nor of my responsibilities as a practitioner of medicine. It was a sincere profes-

sional judgment, based upon extensive scientific and clinical experience, that the procedures involved only the same low degree of risk inherent in many routine clinical test procedures, the patient in all such cases being informed only of the facts which are important from his standpoint. I submit that but for the highly emotion-charged term "cancer cells," this conclusion would be unquestioned by those in the medical profession who are fully cognizant of the present stage of knowledge with respect to immunological reactions.

Furthermore, in my own clinical judgment—based on fifteen years of clinical management of advanced cancer patients—to use the dreaded word "cancer" in connection with any clinical procedure on an ill person is potentially deleterious to that patient's well-being because it may suggest to him (rightly or wrongly) that his diagnosis is cancer or that his prognosis is poor. Some cancer patients do not know that their diagnosis is cancer, and even those who have been informed rarely discuss it and may even deny it. It is seldom possible for the physician to be full cognizant of the cancer patient's extent of knowledge of and his attitude toward his disease. The doctor's choice of words in discussions with the patient has a great influence upon the patient's mental attitude. Since the initial neoplastic source of the test material employed was not germane to the reaction being studied and not, in my opinion, a cause of increased risk to the patient, I believe that such revelation is generally contraindicated in the best consideration of the patient's welfare and therefore to withhold such emotionally disturbing but medically non-pertinent details (unless requested by the patient) is in the best tradition of responsible clinical practice.

On these questions concerning procedure, I will readily submit to the judgment of my colleagues after they are fully informed.

b.

**Frank L. Horsfall, Jr., M.D.—  
February 4, 1964**

I am now and have been since 1937 licensed to practice medicine in the State of New York. I am now and have been since April 1, 1960 president and director and chief executive officer of the Sloan-Kettering Institute for Cancer Research, New York, New York, as well as director, Sloan-Kettering Division, Graduate School of Medical Sciences, Cornell University Medical College.

I hold now and have held since April 1, 1960 the rank of professor of medicine, Cornell University Medical College.

I have read the affidavit of Chester M. Southam sworn to February 4, 1964. I have been generally familiar with the clinical tests described therein and their results to date. I am in complete accord with the professional opinions expressed by Dr. Southam in his affidavit.

c.

**Henry Thomas Randall, M.D.—  
February 4, 1964**

I am now and have been since 1941 licensed to practice medicine in the State of New York. I am vice president for medical affairs of Memorial Hospital for Cancer and Allied Diseases and medical director of this hospital; also vice-president for clinical affairs of the Sloan-Kettering Institute for Cancer Research, and professor of surgery in Cornell University Medical College.

I have read the affidavit of Chester M. Southam sworn to on February 4, 1964. I am generally familiar with the clinical tests described therein and their results to date. I am in complete accord with the professional opinions expressed by Dr. Southam in his affidavit.

d.

**Emanuel E. Mandel, M.D.—  
February 4, 1964**

The method of obtaining the consents to the Sloan-Kettering tests, outlined in the answering papers, must be evaluated in relation to the basic medical principle that the extent of information to be imparted to the patient must be left to the judgment of the responsible physician. There are many standard techniques used by physicians for the purpose of diagnosis and treatment which may result in injury, or even death, to patients. Yet, in the interest of the patient, they are not normally preceded by any thoroughgoing explanations, or even by any written or oral consents (e.g., penicillin injections, the obtaining of intravenous pyelograms, "BSP" tests, X-ray treatment for non-cancerous patients, the administration of radioactive substances (iodine and phosphorus), etc.).

It must be patent that the investigative team of Sloan-Kettering and JCDH acted in full compliance with conventional procedure accepted by the medical profession at large. The injections of cell suspensions in question here

were no more hazardous than any of the above named routine tests, and, indeed, far safer than most of them or perhaps all of them. In fact, consideration was being given at the outset of this study to the possibility of adopting those injections as routine tests to uncover hidden (sub-clinical) cancer, since it was regarded as a routine test at Memorial Hospital (see minutes of the hearing in the offices of the New York State Education Department on December 19, 1963). For even advanced (metastatic) cancer can escape the physician's attention in a patient suffering from other chronic and debilitating disease, and even advanced cancer can, at times, be treated with success. There is no basis for the argument of Dr. Strauss and other medical witnesses that the tests were "dissociated" from the "patient's ailment and condition."

**NOTE**

**EARL UBELL  
INJECTING CANCER CELLS—THE CASE FOR  
THE DEFENSE\***

Would you take an injection of a million cancer cells in your arm? The thought of it will send shudders through any normal person unfamiliar with modern biology. He thinks: what if those cells took hold and grew into a full, deadly cancer?

Yet almost every cancer biologist knows that one of the hardest biological tricks to pull is to transplant a cancer from one animal to another. And nobody has ever transplanted a cancer from one human being to another.

At the same time such experiments on human beings—injecting cancer cells—have the possibility of an enormous pay-off: a vaccine against cancer or a technique for helping the body get rid of cancer.

Given this information, one wonders why cancer injection tests on patients at the Jewish Chronic Disease Hospital in Brooklyn raised such a brouhaha. It is entirely possible that the doctors involved made a tactical error in failing to describe fully to the patients or to their families every step of the experiment. But even if they omitted the deadly word: cancer, have they hurt their volunteering patients? The answer is no.

The experiments in this field began with a question: is there something wrong with the can-

\* *New York Herald Tribune* 29, col. 5 (January 26, 1964). Reprinted by permission.

cer patient's defenses against cancer? The possibility had been raised by a whole series of tests on animals.

\* \* \*

Dr. Southam induced 96 healthy men incarcerated at Ohio State Penitentiary at Columbus to volunteer with the full knowledge of what he was going to slip under their skin.

Since that time more than 300 prisoners have volunteered for the tests. In not one of them did the cancer cells become a full-blown cancer. Most of the cells died within days; in some volunteers it took a couple of weeks. When the same volunteers received additional injections, their bodies killed off the cells even more quickly.

\* \* \*

But what about cancer patients? At about the same time Dr. Southam secured volunteers among the dying cancer victims at Memorial Hospital which is associated with Sloan-Kettering. Most of them were not only willing, but eager to help saying: "I know it is too late for me. Maybe this will help somebody else."

The cancer cells lived longer in the cancer victims than in the healthy volunteers. In one instance, a patient with an advanced cancer of his own died of his disease six months after receiving the injection of cancer cells. The injected cells were still alive and localized.

These tests indicated that in the cancer patient the defenses were down or at least weak. Similar results followed in almost 300 cancer patients. But none of the injected cells turned into full-blown cancers on their own.

Because of the results—namely that the injected cells never took over—Dr. Southam and his associates told their volunteers less about the nature of the injections to save them any possible anguish. Each patient was told that he was volunteering for a test, not a treatment.

Still, a basic biological issue remained open. Was the weakened defense a result of cancer or did it come simply from a person's being very sick and debilitated? Wasn't it possible that if somebody suffered a severe heart attack, say, and lost 60 pounds and was very weak, that the defenses against cancer might also be low?

It was this question which Dr. Southam tried to answer with the experiments carried out by the doctors at the Jewish Chronic Disease Hospital in Brooklyn. The results are in: these patients had the same response to the cancer cells as the healthy volunteers: the cells died in a few

days to two weeks, at most. The anti-cancer defenses were strong.

Here, then, we have a wide possibility: if there is such a biological mechanism as a defense against cancer, then it may be possible to stimulate it either before cancer strikes or perhaps even later when the cancer has taken hold.

This is the question which Dr. Southam is trying to pursue. It would be a shame if a squabble over who-told-what-to-whom should destroy a thrilling lead in cancer research.

\* \* \*

## 8.

### Sur-Reply Affidavit for Petitioner

**Statements by Nathan Fink—  
January 25 and February 1, 1964**

[i] I, Mr. Nathan Fink, aged 73, make this statement, while a patient at the Brooklyn Chronic Disease Hospital.

Sometime in July or August of 1963, while a patient at the above hospital, two doctors visited me at my bedside and told me that I was to get an injection. This was supposedly a skin test, I was informed. They did not ask my approval nor consent.

A few days later, I detected a hardening under the top layer of my skin, in the area where I had previously been injected, my right thigh. This hardening enlarged about 2½" in length.

During the next six or seven weeks, I was visited by these two doctors, every second or third day, at which time, one would measure the area with a ruler, and the other one would make notations in a small book. I do not know the name of these two doctors but one of the doctors, the one who made the notations in the book, was a Filipino.

Within a period of seven weeks, the hardened area seemed to have subsided, and I was informed, by the two doctors that I had a good resistance, and that the skin injection, performed upon me, had been successful.

After reading the most recent newspaper articles, about the cancer injections, performed on patients at this hospital, I now have reason to believe that I was one of the patients used as a guinea pig, in conjunction with this cancer experiment.

I again state that I was never given an op-

portunity by the doctors at this hospital, to accept or refuse this injection, nor was I ever told what was the actual purpose of this experiment.

[ii] I recently submitted a statement regarding an injection which I was given while a patient at B.C.D. Hospital. In that statement, I advised that I believed that this injection was a cancer experiment and that I had never given any one my oral or written consent for this experiment.

I now wish to amend this statement previously submitted. About 1 month after the injection given me in July 1963, I was approached by the same 2 doctors, at which time they suggested that I sign a blank questionnaire.

I asked what this was all about and was informed that they intended to give me a new experimental pill to pep up my appetite. They further stated that my signature was necessary for them to administer this pill. Naturally I signed this questionnaire because at this time I was actually suffering from lack of appetite.

I never was given this pill after they obtained my signature, although I asked about the pill on many occasions.

Now that I realize about the unauthorized injection given me in July 1963 and the subsequent signature taken from me, I have more reason to believe I was tricked into taking a cancer experiment with subsequent authorization.

## 9.

### Opinions of the Court

a.

**Hyman v. Jewish Chronic Disease Hospital**  
42 Misc.2d 427, 248 N.Y.S.2d 245  
(Sup.Ct. 1964)

CONE, J.

In this article 78 proceeding the petitioner, a member of the board of directors of the respondent, seeks an order directing the respondent to permit the inspection and the making of copies by the petitioner of the minutes of the board of directors and the report of its executive director made at such meeting held on September 30, 1963, the report of Dr. Abramson made by him at the medical board meeting of October 28, 1963, the affidavit of Dr. Custodio, as well as the charts and records of the patients who submitted to the subject tests. In addition, by affidavit dated February 6, 1964, the petitioner enumerates other records that he demands access to.

It appears undisputed that the respondent, as a result of this application, has furnished the petitioner with the minutes of the board of directors, the report of the executive director and the report of Dr. Abramson, as well as the affidavit of Dr. Custodio, but refuses to turn over to the petitioner the charts and records of the patients without the consent of such patients as being precluded under section 4504(a), CPLR.

The court in making its present determination, is not passing upon the merits of the alleged improper acts or upon the technical aspects of the tests given to the patients of the respondent. It is merely called upon to determine the narrow issue as to the right of a member of the board of directors to obtain an order permitting an inspection and the making of the copies requested.

It is the well-established law of this state that a director has an absolute and unqualified right to the inspection of the corporate records regardless of his motives (*Matter of Cohen v. Cocoline Products*, 309 N.Y. 119; 127 N.E. 2d 906).

The basis for this premise taken by our courts is aptly stated by the court in *Cohen v. Cocoline* (*supra*): "In order properly to perform his directing duties, a corporate director must, of course, keep himself informed as to the policies, business and affairs of the corporation and as to the acts of its officers. He owes a stewardship obligation to the corporation and its stockholders, and he may be subjected to liability for improper management during his term of office. Because of those positive duties and potential liability the courts of this state have accorded to corporate directors an absolute, unqualified right, having its roots in the common law, to inspect their corporate books." (Emphasis supplied.)

\* \* \*

Accordingly, the petition is granted . . .

b.

**Hyman v. Jewish Chronic Disease Hospital**  
21 App.Div.2d 495, 251 N.Y.S.2d 818 (1964)

PER CURIAM.

The question presented on this appeal is whether a member of the board of directors or of the board of trustees of a hospital membership corporation is entitled as a matter of right to an inspection of medical charts of patients at the hospital. Special Term held that he is so entitled. We are of the contrary opinion.

Special Term directed that the hospital also permit petitioner to inspect records of a financial and administrative nature (e.g., books of account, fiscal records, minutes of the meetings of the board of directors, of its medical boards and committees, and rules and regulations governing the handling of patients). The hospital has acceded to such direction and has allowed petitioner to inspect such records; and such records are not involved on this appeal.

The genesis of this controversy and the facts giving rise to it may be briefly stated:

As the result of approximately ten years of research, Dr. Chester M. Southam of the Sloan-Kettering Institute for Cancer Research found that cancer patients did not have as marked a defense against cancer as did non-cancer patients. It is a biological law that human beings will reject cells which are transplanted from another human being unless both persons are of precisely the same genetic constitution (e.g., identical twins). It was found that, when a healthy individual was injected with the cancer cells of another individual, the healthy person promptly rejected the transplant, whereas when a cancer patient was injected with such foreign cancer cells, rejection of the transplant was delayed. What was not known was whether the foreign cancer cells lived longer in cancer patients (as contrasted with non-cancer patients) as the result of the pre-existing cancer or as the result of the patient's general weakness and debilitation. It was this question which Dr. Southam attempted to answer by the experiments conducted at the Jewish Chronic Disease Hospital; and it is these experiments which are involved in the present appeal.

The experiments showed that the sick and debilitated non-cancer patients had the same response to foreign cancer cells as healthy volunteers, that is, there was a prompt rejection of the transplant. This in turn opened a wide possibility that, if there be such a biological mechanism as a defense against cancer, it may be possible to stimulate it either before cancer strikes or perhaps even later when the cancer has taken hold.

The project was financed by the United States Public Health Service and by the American Cancer Society. It was undertaken by Drs. Southam and Levin of the Sloan-Kettering Institute at the Jewish Chronic Disease Hospital, with the permission of Dr. Mandel, director of the department of medicine and director of medical education of the hospital.

On July 16, 1963, under the supervision of

Drs. Southam and Levin, 22 patients at the hospital were injected with foreign cancer cells on the anterior surface of one thigh at two sites just beneath the skin. The patients were not told that the injection was of cancer cells because the doctors did not wish to stir up any unnecessary anxieties in the patients. The doctors felt there was no need to tell the patients that the injected material contained cancer cells because: (a) it was of no consequence to the patients; (b) the precise nature of the foreign cells was irrelevant to the bodily reactions which could be expected to occur; (c) it was not germane to the reaction being studied; and (d) it was not a cause of increased risk to the patient.

However, the patients were told that an injection of a cell suspension was planned as a skin test for immunity or response. The patients were also told that within a few days a lump would form and would last for several weeks and gradually disappear. The patients were observed for several weeks after the injection of July 16, 1963. As expected, the lump developed and disappeared within an average period of from six to eight weeks.

The hospital and the doctors in charge of the experiment claim that each patient gave his oral consent. Petitioner, however, claims that the patients were either incompetent to give their consents or that they did not understand to what it was they were being asked to consent.

On December 2, 1963 this article 78 proceeding was instituted by petitioner to obtain the hospital records which are involved in this proceeding, as well as the hospital's financial and administrative records. The application was granted at Special Term on the ground: (a) that, regardless of his motives, a director of a membership corporation, as well as a director of a business corporation, has the absolute and unqualified right to inspect corporate records; and (b) that the disclosure of the patients' medical records to a member of the hospital's board of directors is not within the doctor-patient privilege because it is a disclosure to a member of the hospital's administration—one who has a legitimate interest in the contents of the patients' records.

In our opinion, the determination of the Special Term was improper for several reasons:

(1) Although the experiments were not conducted for the purpose of diagnosis or treatment of the patients, the results of such experiments nevertheless comprised part of the medical charts of the patients and, therefore, come

within the physician-patient privilege (*Matter of New York City Council v. Goldwater*).<sup>\*</sup> Since the patients have concededly not waived the privilege, petitioner is not entitled to an inspection of such records.

(2) It has become a well settled rule that a director of a stock corporation is entitled to an inspection of the corporate books in order to keep himself informed as to the corporation's policies, business and activities so that he may carry out his duty to direct its affairs (*Matter of Cohen v. Cocoline Products, Inc.*).<sup>†</sup> The rule also applies to a membership corporation (*Matter of Davids v. Silcox*).<sup>‡</sup> However, by this rule it was never intended to permit a member of the board of directors or of the board of trustees of a hospital to inspect the medical charts of hospital patients (*Munzer v. State*, Ct. Cl., 41 N.Y.S. 2d 98; *Munzer v. Blaisdell*, 49 N.Y.S.2d 915).

(3) A corporate director is also entitled to an inspection of the corporation's books because he may be subjected to liability for improper management during his term of office (*Matter of Cohen v. Cocoline Products, Inc.*, 127 N.E. 2d 906). However, the possibility of petitioner's liability is here non-existent. Section 46 of the Membership Corporations Law provides that, in the absence of fraud or bad faith, the directors of a membership corporation are not personally liable for its debts, obligations or liabilities. It is only when a director personally participates in a wrongful act that he is personally liable (*Hinkle Iron Co. v. Kohn*, 128 N.E. 113). There is no claim of bad faith or fraud or of personal participation on the petitioner's part.

(4) The petitioner does not have the right, in his capacity as trustee or director, to act for the hospital's patients. It is only the patient or his physician who can act for the patient. No proceeding by any patient to obtain the information in question has been instituted.

(5) The hospital's future policy will be in accordance with petitioner's contention that experiments such as the one here involved should be done only with the patient's written consent after the patient has been properly informed. On September 7, 1963 the hospital's grievance committee approved the experiment. On September 30, 1963 its board of directors approved its grievance committee's report. On January 27, 1964 the hospital's research committee approved

continuance of the cancer immunization studies, but only upon the written, informed consents of the patients. Therefore, no further need for the inspection exists. It should be noted that petitioner is now in possession of the facts as to the manner in which the experiment was conducted on July 22, 1963; as to what information was given to the patients; and as to what information was not given to them.

Accordingly, the order, insofar as appealed from, should be reversed on the law and the facts, with costs; and the petitioner's application should be denied insofar as petitioner seeks the disclosure of the: (a) charts and records of patients who had been subjected to the experimental injection of live cancer cells; (b) the death certificates of any such injected patients who later died; and (c) the pathological studies, slides and laboratory data relating to all of said injected patients.

The inspection under the order (insofar as the order has not been reversed) shall proceed on twenty days' written notice or on such other date as the parties may mutually fix by written stipulation.

\* \* \*

c.

**Hyman v. Jewish Chronic Disease Hospital**  
15 N.Y.2d 317, 206 N.E.2d 338 (1965)

DESMOND, CHIEF JUDGE.

Special Term was correct in its holding that petitioner, being a director of a hospital corporation, is entitled as matter of law to an inspection of the records of the hospital to investigate into the facts as to alleged illegal and improper experimentation on patients (*Matter of Cohen v. Cocoline Products*, 309 N.Y. 119, 127 N.E. 2d 906; *Matter of Martin v. Martin Foundation, Inc.*, 32 Misc. 2d 873, 224 N.Y.S. 2d 972).

It is argued that the data as to such experiments on patients are privileged (CPLR 4504 [a]) and that the patients have not waived the privilege. Any such confidentiality could be amply protected by inserting in the court's order a direction that the names of the particular patients be kept confidential. Actually, the supposed strict secrecy does not really exist as to qualified persons since these records have been seen, read and copied by numerous staff members and employees of the hospital and of the co-operating institution.

We are told that, since this petitioner direc-

<sup>\*</sup> 31 N.E.2d 31.

<sup>†</sup> 127 N.E.2d 906.

<sup>‡</sup> 79 N.E.2d 440.



tor would not be personally liable for the wrongdoing of the hospital, he does not need such an inspection. However, the possibility of liability of the corporation of which he is a director entitles him to learn the truth about the situation on which such alleged liability may be predicated. Again, it is said that a director should not be allowed to act on behalf of the patients without their authority. We do not understand the petitioner to claim any such right of representation. He is carrying out his own duties as a director—to direct the affairs of the corporation.

It is argued, again, that an inspection is unnecessary since newly enacted rules of the hospital now require that written and informed consents of the patients be obtained before experiment. This fact, however, cannot be an obstacle to this director's effort to learn the full truth as to what has been done in the past.

No one seriously questions the right and obligation of a membership corporation director to keep himself informed as to the corporation's policies and activities so that he may do his duties and carry his responsibilities. Any necessary safeguards and protections can, in the discretion

of the Special Term, be provided by its order, including appropriate arrangements for concealing the names of individual patients if that appears to be necessary or proper. The order appealed from should be reversed, without costs, and the matter remitted to Special Term for further proceedings not inconsistent with this opinion.

SCILEPPI, JUDGE (dissenting).

I would affirm especially on the unique facts of this case: (1) The State Department of Education is inquiring into the matter; (2) the Kings County district attorney has been alerted to the situation; (3) the petitioner already knows the facts underlying his contention that the injections were given without the informed consent of the subject patients; (4) the informed consent of the patients is now required; and (5) all administrative and financial records have been ordered turned over to the petitioner. Since petitioner is already in possession of the facts as to the manner in which the experiments were conducted, no further need for the inspection exists.

## C.

### How and by Whom Should the Consequences of Research Be Reviewed?

#### 1.

##### **Informing the Board of Regents Grievance Committee for Decision**

#### a.

##### **Louis J. Lefkowitz, Attorney General of the State of New York Petitioner's Post-Hearing Memorandum**

##### **IN THE MATTER of the**

Application for the revocation of the authorization and license heretofore granted to EMANUEL MANDEL, M.D. and CHESTER SOUTHAM, M.D. to practice medicine in the State of New York, and for the cancellation of their registrations as such, and for such other relief as the premises warrant.

##### **THE STATUTES**

The applicable provisions of the Education Law are as follows:  
Section 6514. Revocation of Certificates.

2. The license or registration of a practitioner of medicine . . . may be revoked, suspended or annulled or such practitioner reprimanded or disciplined in accordance with the provisions and procedure of this article upon decision after due hearing in any of the following cases:

2 (a) That a physician . . . is guilty of fraud or deceit in the practice of medicine. . . .

2 (g) That a physician is or has been guilty of unprofessional conduct. As implemented and defined by the Rules of the Commissioner, filed pursuant to Statute in the Office of the Secretary of State under Title 8, part 60.1, subd. (d) 7 of the Official Compilation Codes, Rules and Regulations of the State of New York, i.e. "immoral conduct of a physician in his practice as a physician."

1. A VALID AND INFORMED CONSENT WAS NOT OBTAINED SINCE THE PATIENTS WERE NOT FULLY INFORMED OF THE NATURE AND DETAILS OF THE EXPERIMENT.

At the outset, it should be firmly understood that while we are dealing with 22 patients in a



hospital, what was done to them, in the experimentation involved herein, was not done in the care or treatment of whatever illnesses or infirmities they had; and the respondents so admit.

It should also be remembered that, [all patients] had a right to expect and to demand from those charged with the administration of the hospital in its care and treatment of patients, that only those procedures and administrations of drugs that were a necessary part of their care and treatment be given and administered.

. . . An analysis of the patients selected amply illustrates that a substantial number of them had not sufficient mental or physical ability to comprehend what was being told to them or what was being done to them; and those who may have had the capacity to understand were not given the full and true nature of the experiment.

*As to patient #18:* Leichter had testified this patient had Parkinsons; developed lung abscess; was always running and falling against wall; had difficulty in communicating; that patient did not understand what was being explained and his speech was unintelligible. Leichter had treated this patient during the years from 1959 to 1963 and stated the patient's condition worsened with respect to July 16th. He further stated as his opinion this patient was unable to understand what an experiment of the type performed would mean.

Rosenfeld testified this patient was in a vegetative state and incommunicative the last year at the hospital; and could not have given a consent.

Southam testified this patient was in complete possession of his senses to extent he nodded agreement to permit examination at site of injection; that each time Southam saw the patient he was ambulatory, had marked shuffling gait, drooled considerably; that he did not regard the patient to be in a vegetative state, but was fully capable of understanding.

This testimony of Southam's is based upon observations made *after* injections were given. An examination of the record of this patient reveals Southam saw patient first time July 19th, then August 13, August 20 and finally October 1st.

Further, what probative weight should be given to Southam's testimony—based as it is on four visits—when it is compared to that of Leichter or Rosenfeld, doctors who have been constantly in attendance at Blumberg Building for many years prior to date of injections, and

who have seen this patient countless numbers of times, examined him and treated him?

Mandel testified he *did not* see this patient before July 16th; introduced report of psychiatrist made August 16th stating "patient is difficult to understand." Mandel stated he did not consider this a psychiatric report and that the charts were very defective. Stated he saw patient on October 5th, 1964 when the patient walked slowly, needed support, drooled a lot; tried to avoid speaking; found him fully alert and aware of place, time and what was going on and answered intelligently whatever questions were asked. Mandel submitted another psychiatric report dated October 27th, 1964 which stated patient was alert, oriented, denied hallucinations, illusions. It should be recalled that on cross-examination Mandel had testified he saw this patient *prior* to July 16th, but did not recall when; that patient drooled a lot; that patient's condition was essentially unchanged for last two or three years. Mandel could not state whether patient avoided speaking when he saw him prior to July 16th. Again, it should be recalled that Mandel, on direct examination, had testified he first saw the patient around December, 1963 and then again October 5th, 1964, and a week before he was testifying on November 4th.

Custodio testified that he did not agree with Rosenfeld's testimony concerning this patient that he was in a vegetative state and incommunicable. Custodio stated he knew patient since his service in 1961; that although patient had difficulty communicating with others who did not know him, he, Custodio, never had any difficulty and that patient understood him.

Mandel's testimony relative to this patient should be completely disregarded; he has contradicted himself as to when he first saw this patient. Even assuming he had seen the patient before July 16th, he gives no valid testimony concerning the patient's condition. As to Custodio, while he testified he disagreed with Rosenfeld's testimony he was silent as to his thoughts regarding Leichter's testimony, and it must therefore be assumed he agreed with Leichter.

It is respectfully urged, with respect to this patient, that Leichter and Rosenfeld, because of their length of service in Blumberg Building, were in a much better position to see, examine and observe this patient than were Southam (who never saw the patient before July 16th), Mandel and Custodio. Mandel's testimony is contradictory as to when he first saw the pa-

tient; at one time he testified it was October 5th, 1964; at another point in his testimony he fixed the time as at December, 1963; and still another point he stated it was *before* July 16th, but could not recall when. Custodio at least agreed the patient had difficulty communicating with others who did not know him.

It is submitted that the testimony of Leichter and Rosenfeld as to this patient should be accepted by the committee; and that a finding be made declaring this patient was incapable of understanding and thus could not have given a valid consent to participate in this experiment.

While only the records and testimony pertaining to a few patients have been shown to illustrate that the believable and probative proof established the absence of ability to understand fully the scope of the experimentation and thus give valid consent, it is by no means conceded that, in those patients not shown, there was present ability to understand and give consent.

The procedures adopted by the respondents in their conduct in pursuing the same give rise to certain compelling and important questions:

Was there any attempt on the part of Mandel, the director of medicine at JCDH, to in any way help or assist Custodio in the selection of the 19 patients in the Blumberg Building? And the answer, admittedly, was NO! Surely, this was a worth-while project—Mandel had been properly enthusiastic—but only to the extent of selecting the three cancer patients and actually being present when two of them were injected by Southam, and then Mandel left! But what of the patients in the Blumberg Building—should they have been placed at the beck and call of Custodio, or, more logically, should not this selection have been made by others more qualified, such as Rosenfeld, head of the Blumberg Building or Leichter who was not only familiar with the patients but was in charge of a cancer research project sponsored by the NIH. Was Custodio fully competent to participate in this project? On his own admission, he had never participated in such an experiment, nor had he read any literature relating to it. All he knew was what Mandel had told him. And what did Mandel know of this project? Only what Southam had told him, and the gist of what Southam told Mandel is that there was no risk to the test, that it was being done regularly at Memorial and that oral consent was sufficient with no knowledge to the patient that cancer cells were to be injected!

Why wasn't a careful screening done by both Mandel and Custodio, with a careful

scrutiny of the hospital records which were available to them prior to July 16th? Why wasn't, prior to July 16th, a detailed statement prepared concerning the test, detailing each and every step of the procedures, the purposes for which the test was to be given and the names of the patients to be selected to participate? And, most important of all, why wasn't each patient informed that the injectable material was cancer cells? Why all the secrecy concerning cancer cells being injected if Mandel and Southam were so sure no deleterious effect could befall the patients? Yet Mandel had the gall to state in an affidavit submitted to the Supreme Court that everything was open and above-board!

Where was consideration shown to the patients with respect to their comfort; their freedom from unnecessary molestation and their absolute right to expect only such procedures and administrations necessary to their care and treatment? How dared Mandel introduce strangers to his hospital and to his patients and to permit these strangers to go through the various wards of the hospital, in open view of other patients? Oh, yes, those strangers were dressed like doctors—they had the long white coat commonly worn by visiting doctors; this then perhaps justified the intrusion as far as Mandel was concerned!

• The haphazard method of selecting patients; the almost complete disregard of their comfort; the slipshod manner in which the entire project was conceived and conducted, is evident throughout the record.

Southam was not concerned with whether Mandel had the right to proceed with this project without sanction or authority. Nor was he concerned with what what patients were selected; whether they were informed, and whether they were capable of giving consent. Mandel was evidently flattered that his hospital had been selected; he made no independent investigation concerning Southam or the project; he took Southam's word that no risk was involved, *although this was the first time they were engaged in performing this test upon debilitated patients*; he failed or refused to select a more capable and experienced participator than Custodio; and failed to assist and supervise the selection of patients.

And the greatest sin of all was the deliberate and willful failure on the part of the respondents herein, to inform each of the 22 patients that they were going to be injected with live cancer cells.

We are dealing with a project which admit-

tedly was in no way therapeutic. It was, rather, an experiment relating to cancer research which had as its ultimate intention the benefit of humanity. This being the fact, it was then incumbent upon the respondents to have seen to it that ALL information connected with the experiment was given, since the patients at JCDH were being asked to become volunteers.

Respondents both admit they sanctioned and counselled the withholding from each patient the fact that the cell suspension to be used was indeed "live cancer cells." Their reasoning? They state that to release this information to the patients would cause a phobia, make them frightened, cause fear and anxiety—and this they wanted to avoid!

Every human being has an inalienable right to determine what shall be done with his own body. These patients then had a right to know what was being planned—not just the bald statement that an injection was to be given, but also the contents of the syringe; and if this knowledge was to cause fear and anxiety or make them frightened, they had a right to be fearful and frightened and thus say NO to the experiment.

Petitioner's exhibit #19, an article entitled "Problems of Informed Consent May Be Unsolvably" cites that Nuremberg Code—"the voluntary consent of the human subject is absolutely essential."

Petitioner's exhibit #6 and Resp. Southam's exhibit AA are entitled "The Normal Volunteer Program of the NIH Clinical Center" and is published by the U.S. Department of Health, Education and Welfare. Under the heading "Definitions" a distinction is made between the "normal volunteer"—a person who is judged to be in excellent health, etc., and "volunteer"—one who offers himself for a service of his own free will. Concededly, the patients at JCDH would come under the second classification. "Informed Consent" is defined as follows:

A formal, explicit, free expression of willingness to serve as a subject for research after the values and effects of such participation have been explained by the investigator and are sufficiently understood for the Volunteer to make a mature judgment.

At page 3 of the exhibit, under "Informed Consent" appears the following language:

The principal investigator *personally* provides the assigned volunteer, *in lay language* and at the level of his comprehension, with information about the proposed research project. He outlines its purpose, method, demands, inconveniences and discomforts, to enable the volunteer to make a *mature judgment* as

to his willingness and ability to participate. When he is fully cognizant of all that is entailed, the volunteer gives his *signed consent* to take part in it. (Emphasis supplied.)

It should be remembered that one of the sponsors for Southam's project and experimentation was the NIH!

How then did Southam discharge his duties and obligations to the volunteers as the principal and chief investigator in this experiment? Again, do we not see the careless and absolute disregard for the rights of the patients who were chosen to participate? While it may be argued that Southam was a stranger to JCDH and its patients and therefore relied upon Mandel, it nevertheless remains the undisputed fact that the 22 patients selected were volunteers in this project, and as to them in that capacity, Southam owed them every consideration and obligation as described by the NIH (*supra*). His was the duty *personally* to provide the volunteer *in lay language* at the level of his comprehension with information about the proposed research project; outlining its purpose; methods; demands; inconveniences and discomforts; so as to enable the volunteer to make a mature judgment as to his willingness and ability to participate; and only when the volunteer is fully cognizant of *all that is entailed*, does he give Southam his *signed consent*. And how did Southam discharge this duty and obligation? First, he said he left it to Mandel to decide the question of "consent" and the manner by which it was to be obtained, albeit he stressed to Mandel the method of obtaining oral consents at Memorial which, in Southam's opinion, were sufficient although the recipient of the injection was not told that cancer cells were being injected. Secondly, he said he was satisfied to have Custodio as his collaborator, despite the fact that he saw Custodio for the first time on the day of the experiment and knew nothing whatever of the latter's ability, experience or knowledge in projects of this kind. This, it is strongly urged, Southam had no right to do. As a scientist engaged in research he had the duty and responsibility for ascertaining the quality of the consent, which may not be delegated to another with impunity. This was his project, and, if it was to serve any useful purpose he should have taken and assumed full and complete authority; by having carefully screened, with Mandel, the patients that were to be selected; by having, with Mandel, spoken, in advance of the injections, to each patient, explaining in lay language at the level of the patient's comprehension, the purpose, methods,

demands, inconveniences and discomforts of the proposed project.

For the record is replete with contradictory statements as to the manner by which "consents" were obtained; Mandel wasn't sure whether he had obtained so-called oral consents from one or two patients; he wasn't sure of the language he used in speaking of the project. Custodio likewise is not sure of just what words were used when speaking to the patients stating that interchangeably he used words as "immunity," "resistance" or "immunological response."

But the salient factor remains that at *no time* and to *no volunteer patient* was information given that, in truth and in fact, the cell suspension mentioned contained *live cancer cells*.

This then is the nub of the entire case. These volunteers, the 22 debilitated patients at JCDH, were not each made "*fully cognizant of ALL that is entailed*" in the proposed project. There was *missing*, deliberately and wilfully so, any statement to the effect that the injectable material contained live cancer cells. As was stated in SCIENCE, petitioner's exhibit 9, in the article entitled "Medical Ethics" and that portion under the chapter heading "Procedures not of direct benefit to the individual" found on page 1025 of the exhibit:

The common feature of this type of investigation is that it is of no direct benefit to the particular individual and that, in consequence, if he is to submit to it he must volunteer in the full sense of the word. It should be clearly understood that the possibility or probability that a particular investigation will be of benefit to humanity or to posterity would afford no defense in the event of legal proceedings. The individual has rights that the law protects and nobody can infringe those rights for the public good. In investigations of this type it is, therefore, always necessary to ensure that the true consent of the subject is explicitly obtained.

It is therefore respectfully submitted that the respondents herein failed to secure a valid and informed consent from each of the 22 patient-volunteers to participate in the experimentation conducted at JCDH.

## 2. THE RESPONDENTS ARE EACH GUILTY OF EACH SPECIFICATION OF THE CHARGES.

The failure of each respondent to reveal ALL that was entailed in the experimentation to each of the volunteer debilitated patients that were selected to participate was fraudulent and deceitful. As illustrated supra, the licensees herein had no right, moral or legal, to with-

hold any information relating to the experimentation. By so doing they violated the absolute right of each patient to determine what shall be done with his own body. By withholding the fact that live cancer cells were to be injected in this experiment, they deprived each patient of their inalienable right to refuse such an injection. *No choice* was given to these volunteers.

The conduct of each respondent was unprofessional, immoral and shocking to one's sense of fairness. Mandel has testified that patients do not question procedures that are done to them in a hospital because they have confidence in the doctors and that patients tend to accept what doctors say to them. It is submitted this confidence was misplaced; that all of these patient-volunteers were duped and misled by Southam and Mandel. Surely, the image of the medical profession must be sullied in the eyes of the public, if the conduct of the respondents herein was to be sanctioned and blessed with innocence.

Again and again it must be repeated and emphasized that a human being has rights and privileges that may not be trespassed upon to any degree. How shocking indeed it would be if a person were to realize that he had no rights or privileges as to what should be done to his body, and that he was a mere "guinea pig" in the eyes of any doctor, whether scientist or researcher, who desired to perform some experimentation on him!

Such a fantastic and gruesome thought could never withstand the indignation and denial of the public.

Upon the entire case therefore it is respectfully submitted each licensee is guilty of each specification contained in the charges.

b.

### **Morris Ploscowe, Esq. Brief on Behalf of Dr. Emanuel E. Mandel**

THE CHARGE THAT DR. MANDEL IS GUILTY OF FRAUD AND DECEIT BECAUSE THE PATIENTS WERE NOT ADVISED "THAT LIVE CANCER CELLS WERE TO BE INJECTED IN THEIR BODIES" CANNOT BE SUSTAINED.

The reasons why the patients were not told that the injections contained tissue cultured cancer cells are not found in fraud or deceit. This is apparent from the following:

Dr. Southam was asked why he deliberately refrained from describing (the injected cells) as "cancer cells." He testified as follows:

For two reasons really. First, I saw no reason why we

should use such a word because it is not pertinent to the phenomenon which is going to follow. We are not doing something which is going to induce cancer. We are not going to do something which is going to cause them any harm; it is not going to produce a transplanted cancer. We are going to observe the growth and rejection of these transplanted cancer cells.

The fact then that they are cancer cells does not mean that there is any risk of cancer to this patient.

Now, the second point is simply that the word, "cancer," has a tremendous emotive value, disvalue, to everybody, not only to the cancer patients but to you and me. What the ordinary patient, what the non-medical person, and even many doctors whose competence in clinical medicine is great but whose knowledge of the basic science behind transplantation is not great—to them the use of a cancer cell might imply a risk that it will grow and produce cancer, and the fear that this word strikes in people is great, and I don't think I have to argue the point to make the point. I think we all recognize it. If we use words like neoplastic; if we use words like tumor, we have no problem.

... Many of these patients undoubtedly know deep down that they have cancer, but the great majority of them have either suppressed this knowledge from the surface or at least they are not talking about it

and they don't welcome conversation that brings it up. So, it is our firmly established and I feel very sound policy *not to use the word "cancer"* with the cancer patients. . . .

The position taken by Dr. Mandel and Dr. Southam in not mentioning the fact that the injections contained cancer cells is justified by medical ethics and current medical practice. Medical ethics do not require the full disclosure to a patient of all conceivable risks and all relevant information as a basis for obtaining patients' consent to a medical procedure. The amount of information imparted to a patient must bear some relation to the risk of a particular procedure. Where there is no substantial risk of harm to a patient, the information imparted to him may be kept at a minimum. We submit that in the instant case it was not necessary to tell the patient that the injections involved tissue cultured cancer cells since there was no possibility that harm could come to the patients from the said cells.[\*]

It should also be noted that the amount of information which should be imparted to a patient as a prerequisite for consent to a medical

[\*] CROSS-EXAMINATION OF DR. CHESTER M. SOUTHAM BY MR. CALANESE.

Q: Doctor, in Vol. 143 of *Science* which is issued February 1964 at page 551 you wrote that there was no theoretical likelihood that the injections would produce cancer. Yet, in the same article, Doctor, you stated that you were unwilling to inject yourself or your colleagues, and you stated, and I quote, "But, let's face it, there are relatively few skilled cancer researchers, and it seemed stupid to take even the little risk."

A: I deny the quote. I am sure I didn't say, "let's face it."

Q: Did you make any statement similar to that?

A: I think the philosophy is an accurate statement.

Q: What was your statement, do you recall?

A: What I am objecting to is the phrase, "let's face it." The statement that I see no reason why a doctor should necessarily serve as a recipient, this is valid, that is, this statement may validly be attributed to me.

Q: The statement, Doctor, was published by Elinor Langer in *Science* of February 7, 1964, Vol. 143, and I quote from her statement that I want to find out from you whether or not what she is quoting as coming from you is correct or not. "Southam, however, who ought to know, said in an interview with *Science* that, although there was no theoretical likelihood that the injections would produce cancer, he had nonetheless been unwilling to inject himself or

his colleagues, when there was a group of normal volunteers at the Ohio Penitentiary fully informed about the experiment and its possible risks and nonetheless eager to take part in it. 'I would not have hesitated' Southam said, 'if it would have served a useful purpose. But,' he continued, 'to me it seemed like false heroism, like the old question whether the General should march behind or in front of his troops. I do not regard myself as indispensable—if I were not doing this work someone else would be—and I did not regard the experiments as dangerous. But, let's face it, there are relatively few skilled cancer researchers, and it seemed stupid to take even the little risk.' "

Did you make that statement?

A: As I said before, the philosophy is correct. I do not know if I made that statement. This is reported—I remember the interview very well. I am still saying that the quotes are not necessarily correct; the philosophy is correct.

Q: That part of the quoting concerning the, "stupid to take even the little risk," do you recall that?

A: No, I don't, and this is one of the reasons that I question whether it is a true quote.

Q: Do you recall the statement that you made that there was no theoretical likelihood that injections would produce cancer?

A: This is, in other words, exactly what I have said earlier this afternoon. [From transcript of proceedings before a Subcommittee of the Committee on Grievances, Department of Education of the State of New York, September, 1964, pp. 636-638.]

or surgical procedure may be left to the sound discretion of a conscientious physician. [\*] This is the import of the rules concerning the testing of drugs which is in evidence as respondent's Exhibit B, and which state that while consent should be obtained for testing of investigational drugs, the laws and the regulations make it clear

[\*] EXAMINATION OF DR. EMANUEL E. MANDEL BY MR. RASHKIS, INVESTIGATOR, NEW YORK STATE DEPARTMENT OF EDUCATION.

Q: Each patient was told that an experiment to determine his immunity was to be conducted. Was each patient told that cell tissue was to be injected?

A: Yes, cell suspension was to be injected.

Q: Each patient was told this?

A: Yes, each patient was told.

Q: Did any patient ask you what a cell suspension is?

MR. PLOSCOWE: If you can recall.

A: I can't.

Q: No one asked you?

A: (No response.)

Q: Did you actually have a conversation with the patients that you spoke to them and they answered you?

A: Yes.

Q: Every patient?

A: Every patient. I asked them if they have—each and every one of them has any objection to us doing the test and they said no.

Q: Did any patient answer anything other than yes or no; that he would agree—

A: No.

Q: No patient questioned any of the terms that you used?

A: I don't remember. I don't think anyone asked.

DR. MANDEL: May I add to that?

MR. RASHKIS: Yes.

DR. MANDEL: I will say almost every day doctors come into situations where they have to ask a patient for permission to do a certain procedure, say a bone-marrow aspiration, a spinal tap, what not.

Most patients don't question these procedures; the patients have confidence in the doctors.

MR. RASHKIS: What is the purpose for the tests?

DR. MANDEL: Diagnostic nature.

MR. RASHKIS: For that particular patient?

DR. MANDEL: For that particular patient.

MR. RASHKIS: Would these patients have understood these tests were to be diagnostic in nature?

DR. MANDEL: No.

MR. RASHKIS: Is there any relevancy in the statement you made about the bone-marrow test?

DR. MANDEL: Only in terms of conversation with patients. Ordinarily they listen and tend to accept what the doctor says to them. [From transcript of proceedings before a Subcommittee of the Committee on Grievances, Department of Education of the State of New York, September, 1964, pp. 96-100.]

that if in the professional judgment of the investigator "it is not feasible or in the best interests of the subject to obtain permission, *the investigational nature of the drug need not be disclosed.*" This concept of patient consent is not new, but has been part of the Code of Ethics of the American Medical Association for many years.

If, in the judgment of a conscientious physician, the investigational nature of a drug need not be disclosed, when it is tested, then there appears to be no reason why the nature of the injected material should have been disclosed to the patients at JCDH, since there was no hazard to the patients from the injections.

The following statements made by distinguished physicians in affidavits submitted on behalf of Dr. Southam, support our contention that a proper consent was obtained from the patients at JCDH in the tests conducted at JCDH and that it was not fraud or deceit not to tell the patients that the injected material contained cancer cells:

Dr. Michael J. Brennan, physician in charge of the division of oncology, Henry Ford Hospital, Detroit, Michigan, stated as follows:

... The need to enter into detailed description of the source and nature of a test material cannot be shown to be a part of our moral and legal duty unless it would be objectively helpful to the patient in coming to a rational and knowledgeable conclusion about the real risks of the procedure to his health.

... He [Southam] did not speak to these patients of giving them a treatment. He asked permission to do a test of considerable scientific import. He then faithfully explained to them the sequence of reactions which they could expect and rightly and correctly assured them of their innocuous character. He hid nothing from the patients which would have been useful to them in making a rational decision regarding the real risks of the test.

He did not mention that the test solutions were made from tissue cultures of cancer cells. This now proves to have been imprudent because of the emotional character of the response which followed revelation of that fact and the opening it gave for accusations of dishonesty and duplicity on his part. There is a difference between withholding information and giving false information but it is often overlooked. However, *the information he withheld was not needed by the patients for judging rightly that his test was safe.*

The real test of the adequacy of his description to the patients of what would happen is whether it corresponded with what did in fact happen.

It was the compassion of the good physician, not the deceit of the charlatan or the calculation of the cold experimentalist, which has laid him open to his present troubles. ...



Dr. George E. Moore, director and chief of surgery of Roswell Park Memorial Institute of Buffalo, New York, stated as follows:

... For the past 4 years, I have been engaged in a similar type of project at Roswell Park Memorial Institute. My research involves the homotransplantation to patients with cancer of tissue-cultured cells derived from human cancer tissue. In my view these tests are of vital importance in the field of cancer research and it is my hope that, through the resulting increased knowledge of immunological factors relating to cancer, important strides may be made leading to possible immunization against, or treatments of, cancer. To the best of my knowledge, *there has been no practical risk of any deleterious effects upon any of the patients who served as subjects in the tests performed by me.*

The question of the type of information to be furnished a patient incident to obtaining his consent to participation in these tests was carefully explored by me in conjunction with other officials of my hospital. It was our decision that the patients would be told that their consent was sought to participation in an investigation involving the injection of live cells derived from a tumor. The word "cancer" was not ordinarily employed. In some instances the phrase "cultured cells from human tumors" was used.

While our procedures thus differ from those employed by Dr. Southam, I believe that Dr. Southam was motivated solely by his concern for the welfare of his patients; *it is clear that this is an area in which fully informed doctors acting solely for the benefit of their patients may arrive at different conclusions as to the best approach to take.* I am aware of the potentially traumatic effects which the use of the word "cancer" may produce, and for the most part, I share Dr. Southam's view that the word should be avoided in such studies since, from a scientific standpoint the general term "cancer" does not accurately reflect the nature of the biologic materials being used.

I do not believe that the differences in the procedures employed by Dr. Southam and by me cast any reflection upon the professional integrity or judgment of Dr. Southam. I believe that the factors which I know were weighed by Dr. Southam prior to making his decision make it preposterous to assert that there was any modicum of "fraud," "deceit" or "immorality" involved in his actions. ...

Dr. Alvin L. Watne, associate professor of surgery and cancer coordinator at West Virginia Medical Center, stated as follows:

... In his affidavit, Dr. Southam describes the procedures which he has employed in his project relating to the study of the relationship between immunological research and cancer. I am engaged in a comparable research project. The information that we present to the patient is that this is a research project that we are conducting here in the department and that their participation is entirely voluntary. If there is any re-

luctance on their part, we do not press the issue. We do not use the words "cancer" or "tumor" in describing the possible transplantation. We do say that we will test the patient's ability to respond to the stimulation and that we are interested in knowing more about their particular tumor problem and that this will give us some information along that line.

... *I believe that the procedures described by Dr. Southam in connection with the obtaining of consents are in accord with the highest standards of the medical profession and I subscribe to the reasons given by him for the adoption of such procedures.*

Dr. I. S. Ravdin, professor of surgery and vice-president of medical affairs at the University of Pennsylvania, stated as follows:

... It is the considered opinion of many investigators that research in the field of host response and immune reactions is likely to provide the first important breakthrough in the treatment of malignant diseases. It is men like Dr. Southam who are best prepared to accomplish this highly desirable breakthrough.

Physicians are constantly concerned as to whether they should tell a patient that he is suffering from a malignant disease. Dr. William T. Fitts, Jr., and I studied this matter some years ago. We sent a questionnaire to members of the Philadelphia County Medical Society in order to ascertain what they did under these circumstances. *Only the dermatologists did this with any frequency.*

The question of whether a proper consent was obtained from the patients in the Southam research at JCDH was also presented to three distinguished physicians who appeared as witnesses on behalf of Dr. Mandel. Each of these physicians was asked a hypothetical question based on the facts brought out at the hearing herein which include the assumption that the patients were not told that the injections contained live cancer cells. Each of these physicians was asked the basic question, "In your opinion as an experienced and conscientious physician, do you believe that a proper consent was obtained to the aforementioned (tissue cultured cancer cells) injections?" Each of the physicians testified affirmatively that a proper consent was obtained to the injections herein.

Dr. David Kershner testified as follows:

Well, in my experience in handling surgery cases for about 40 years and the problems which we have to decide on the Malpractice and Defense Board of the State Society as well as the equal level of the county society, we talk in terms of informed consent and what is informed consent and much is made of it. I don't think any law can be laid down. I don't think we can strictly say this is informed consent and this is not informed consent and this is what you must not tell the patient. I think it has to be individualized. Pa-

tients are not all the same; they don't react the same way. But by and large, I think we can safely say that if a patient is going to be operated upon or any work is going to be done involving malignancy, and we use the word "cancer," it throws a horrible fear into the patient. . . .

Dr. Charles E. Rogers testified as follows:

The reason I say that I believe informed consent was obtained was because I don't think there was any risk involved here. It is well known that we have been trying hard to transplant tissues for a number of years and we have been failing miserably. As far as I can see, what occurred here is they wanted to find out whether there was immune response or to what degree the immune response was engendered in patients who had debilitating diseases, and since we know that we can't transplant these tissues unless we have identical twins or unless we pretreat the patient with radiation or other toxic substances, I would feel that informed consent was obtained. There wasn't a risk involved.

I base that on literature and my knowledge of the immunology such as it may be and the general knowledge I have. I just don't think there is any doubt in my mind these tissues could have possibly survived in these patients. Obviously, they didn't.

On cross-examination, Dr. Rogers was asked the following questions and gave the following answers:

MR. CALANESE: All those volunteers at Jewish Chronic Disease Hospital were not told that cancer cells were being injected.

DR. ROGERS: That isn't germane to the problem, Sir.

MR. CALANESE: As far as you're concerned?

DR. ROGERS: Yes, Sir. That is my opinion.

MR. CALANESE: Further they were told there would be no risk involved with the test they were going to be subjected to at that time.

DR. ROGERS: I think that is true in my opinion.

MR. CALANESE: That is not important as far as the patient or the volunteer is concerned?

DR. ROGERS: No, Sir. I think that you would be causing the patient undue anxiety and undue concern over a procedure that doesn't have a risk.

\* \* \*

MR. CALANESE: It has been established here, Doctor, by the testimony so far, that all that was told to these patients is that an injection was going to be given to determine their resistance to disease and that a lump would form within a few days which would disappear within

2 or 3 weeks; that is all that was told. In your opinion, is that sufficient?

DR. ROGERS: Yes, sir, in this particular case. But, I wish to emphasize every case must be decided on its own merits. In this particular case, there was no risk and there was no need to advise the patients unnecessarily and alarm them and say these are cancer cells, you could get cancer, because the volunteer in a situation like that would be worried.

Dr. Irving Hirschleifer testified as follows:

MR. PLOSCOWE: . . . Was a proper consent obtained from the patients to the cancer injections involved in the instant proceeding?

DR. HIRSHLEIFER: Yes, sir.

MR. PLOSCOWE: Would you tell the panel the reason why you came to that opinion.

DR. HIRSHLEIFER: Well, having been in clinical investigation for many years and also having served in a teaching and training capacity and a medical school affiliated institution for many years, and having helped train many interns and residents since 1946, these were the practices which were performed in no other manner in all my experience.

MR. PLOSCOWE: When you state these were the practices, Doctor, would you be more specific, the technique of obtaining consent. That's right.

DR. HIRSHLEIFER: Yes.

MR. PLOSCOWE: Well, does it make any difference that in this particular proceeding what was done here was for the purpose of experimentation, the making of a test rather than for the therapeutic benefit of the patient?

DR. HIRSHLEIFER: These are the usual practices in hospitals where interns and residents are trained.

MR. PLOSCOWE: Can you tell me, for example, in the hospitals with which you have been associated, . . . universities and teaching institutions, are frequent tests performed on patients which have nothing to do with the therapy or treatment of the patient?

DR. HIRSHLEIFER: Yes, sir.

MR. PLOSCOWE: And is the method of consent obtained in that framework any different from the method of consent obtained here?

DR. HIRSHLEIFER: Sometimes, not to the degree that was obtained here.

MR. PLOSCOWE: Does that mean that we were more formal here?

DR. HIRSHLEIFER: Yes.

\* \* \*



MR. PLOSCOWE: With respect to the specific project, there has been criticism of the fact that the word "cancer" was not used prior to the injection of these patients. Do you find that this is a proper subject of criticism in this particular framework?

DR. HIRSHLEIFER: No, I don't. I attempt never to use that term when conversing with a patient.

[\*] CROSS-EXAMINATION OF DR. EMANUEL E. MANDEL BY MR. CALANESE.

Q: Now, before the injections were made by Dr. Southam, did you personally secure the consent of any of these three patients?

A: I believe I spoke to at least one of them and this is something I cannot remember and haven't been able to remember, whether it was I or Dr. Custodio who spoke to these patients explaining what the objective was, what we were planning to do. Dr. Custodio thought it was I. It may well be.

Q: Let's take your statement that you may have spoken to at least one. Can you tell this committee exactly, to the best of your recollection today, what you told that particular patient . . . concerning this experiment?

A: I remember talking to the one that had the leukemia. I think I mentioned that earlier today, and I mentioned this morning that the patient indicated some resentment over being stuck with needles over a period of his hospitalization without evidence he was really getting better.

Q: That is what I was trying to bring out, Dr. Mandel; . . . I want you to tell this committee and this record what was said by you to this patient and what if anything was said by the patient to you.

A: It is impossible for me to do that. I can't remember it.

Q: To the best of your recollection.

A: Well, I only know I spoke to him and I recall vaguely he indicated his—the fact he was unhappy over having so many forms of treatment and diagnostic procedures and didn't think he was getting better. He showed me how he had lost weight. He showed me he had an enlarged abdomen. He had what is called ascites, free fluid in the abdomen, and I recall that I tried to reassure him.

Q: As to what?

A: As to eventual improvement, that he was going—getting better; that various procedures that have been planned for him and that have been carried out will eventually bring about his ultimate recovery.

Q: Had he told you in any manner, shape or form as to the numbers of time that tests had been made on him over a short period of time before July 16th?

A: I don't—

Q: A number of tests had been made, he was sick and tired of it, he said?

It is apparent from the aforementioned discussion that the respondents, Dr. Mandel and Dr. Southam, by failing to disclose to the patients that the cell suspension injections were cancer cells, were not guilty of fraud or deceit, but were acting in the best interests of the patients and according to accepted standards in the field of medicine.[\*]

A: I am sure he didn't use that expression. He was very well mannered and quite a quiet sort of fellow.

\* \* \*

Q: When you spoke to him concerning this test, just exactly what did you tell him this test was and what it comprised, what the expectations were?

A: I cannot tell you exactly. I can only tell you what I told him, what has been stated a number of times; that the test was planned for the determination of his immune response or his resistance and that it would result in a lump which would disappear after a period of time, after some weeks.

\* \* \*

Q: Did he ask you for any further particulars concerning the test?

A: I don't believe so.

Q: Despite the fact he made a complaint to you about too many tests having been made upon him before, you say he made no further complaint or asked you no information concerning this test?

A: He did this more in a way of general complaint and his complaint was directed primarily towards not getting better. In other words, he compared his lack of improvement with the number of procedures being applied.

Q: Was any statement made to him, Doctor, by you that this test might be of benefit to him?

A: Did I indicate that to him?

Q: Yes.

A: I am certain I didn't.

Q: Did you say anything to him, Doctor, that as part of this test, in addition to the injection and the lump that would form, that would disappear within a short period of time, that blood tests would also be taken and made?

A: I would think that I did.

Q: Don't you know?

A: I don't remember. I am quite certain this was done in every instance.

Q: You are quite sure.

A: Yes.

Q: We are speaking with respect to this specific instance, this leukemia patient. Did you tell him there would be blood tests taken?

A: I don't recall. [From transcript of proceedings before a Subcommittee of the Committee on Grievances, Department of Education of the State of New York, September, 1964, pp. 960-965.]

Even if it should be maintained that Dr. Mandel and Dr. Custodio should have told the patients that "live cancer cells" were being used in the injections despite their fear of instilling cancer phobias in the patients, there can be very little doubt that their failure to do so was an honest medical error. An honest medical error cannot be deemed fraud or deceit or "immoral conduct" of a physician.

## 2.

### The Board of Regents Grievance Committee Makes Its Recommendations

#### a.

#### Report of the Subcommittee of the Committee on Grievances

To the Committee on Grievances:

The undersigned, subcommittee of the COMMITTEE ON GRIEVANCES duly designated to hear the charges against DR. CHESTER M. SOUTHAM and DR. EMANUEL F. MANDEL hereinafter referred to as respondents, pursuant to Section 6515 of the Education Law of the State of New York, and to report its findings and recommendation in respect to the said charges, do hereby, after due deliberation, unanimously report its findings and recommendations as provided by law as follows:

\* \* \*

*The findings and recommendation of Dr. Lawrence Ames, chairman of the subcommittee is as follows:*

The above two physicians are charged with fraud or deceit, as well as unprofessional conduct, in the practice of medicine within the purview and meaning of the Education Law and as implemented and defined by the Rules of the Commissioner. . . .

\* \* \*

Sitting as chairman of the subcommittee of the medical grievance committee hearing this case, I had full opportunity to hear all the testimony and evidence introduced by the attorneys for the respondents and by the attorney general for the petitioner.

Every opportunity was afforded both respondents and the petitioner to present their cases completely and thoroughly and there was no attempt on the part of the committee to impede or curtail the introduction of any evidence or testimony pertinent to the case. I have re-

viewed all the testimony and evidence and after a great deal of study I have come to the following conclusions:

This experiment or research project was not done for the care or treatment of any of these individuals, but rather as a non-therapeutic clinical research project.

All the patients chosen were in a very debilitated condition for that was a necessary prerequisite for this experiment.

Dr. Southam, the chief investigator, was working partly under a grant from the United States Department of Health, Education and Welfare of the Public Health Service and was governed by their rules and regulations regarding experimentation and research.

Dr. Southam was aware of the rules and regulations as set down by the Public Health Service for research and experimentation under these grants. It specifically states, "The principal investigator personally provides the assigned volunteer in lay language and at the level of his comprehension, with information about the proposed research project. He outlines its purposes, methods, demands, inconveniences and discomforts, to enable the volunteer to make a mature judgment as to his willingness and ability to participate. When he is fully cognizant of all that is entailed, the volunteer gives his signed consent to take part in it."

Dr. Southam as chief investigator and Dr. Mandel as chief of medicine of the Jewish Chronic Disease Hospital are both equally responsible for whatever took place and share equal responsibility for these acts.

The 19 patients who were chosen by Dr. Custodio were not given sufficient facts on which to base their judgment of whether or not to give consent. It is admitted that at no time were the words "cancer cell injection" ever used. Many of these 19 patients, in my opinion based on the evidence introduced, were not physically or mentally capable of understanding what was involved and therefore incapable of giving informed consent, even if such information were given to them by Dr. Custodio. The manner in which Dr. Custodio elected to choose the cases for the experiment, the very morning of the injections, and the total time consumed in giving all these injections, convinces me beyond reasonable doubt that proper informed consent could not have been obtained. It is my considered opinion that he was more interested in getting his name on a research project, than in protecting the interests of these debilitated people placed in his

care and trust as senior resident at the Jewish Chronic Disease Hospital.

I find that Dr. Southam, as chief investigator, and Dr. Mandel, as chief of medicine at the Jewish Chronic Disease Hospital, did not fulfill their obligations to the people involved in this investigation, in that they did not obtain or see that the proper informed consent was obtained from these patients or those qualified to give the proper consent for them. [\*]

Every human being has an inalienable right

[\*] CROSS-EXAMINATION OF DR. CHESTER M. SOUTHAM BY MEMBERS OF THE COMMITTEE ON GRIEVANCES.

DR. HELLER: . . . the question in our minds, I believe, the committee's mind, is whether or not patients, whether they are terminal patients, patients who are socially adjusted and could be right here in a social gathering, would know the difference between a test or a treatment and whether or not they could construe, in a setting such as we have described, a test as a routine procedure within a hospital revolving about themselves and their betterment, their welfare, care and treatment?

In other words, the patients, as we have it, were not asked, "Do you know what research is? Do you know what an experiment consists of?" and we don't know what they might have answered to the question "Do you know what research is? Do you want to become a research subject?" These are the points, though they are points of semantics, yet relate to understanding of the patient. It would seem to us that cooperation depends upon the recognition of a doctor, his confidence, but not upon his understanding that this was a research project. What we are concerned with is the method of obtaining the consent, primarily. That is why I raise that question, and I would like your comment on it.

A: One of your key points, I think, is whether these patients in saying, all right, I will have a test, interpreted in this sense of something out of the ordinary, not a routine matter, test that might have been done, but a research project, an experiment.

Obviously, I cannot speak for the patients, but I think there is no question but—I am speaking now over the period of time that these tests were carried on rather than at the particular moment about which I was being questioned previously—certainly when doctors come in, two doctors known not to be associated with their hospital, it certainly was clear to most of these people, I would guess, that they know that this was something out of the routine; that this was a research, and I would not doubt at all that we used such words as research and experiment. Some patients were quite able to converse. As others, as you heard, had impaired ability to converse, but certainly those who were able to talk better, I feel confident, knew that not only that this was research involved, they probably knew that we were from a cancer re-

search hospital. This is obviously opinion. This is not a statement of facts.

DR. HELLER: That is the point I am making. The ability to converse is no measure of understanding whatsoever. The most conversant patients can have the least understanding or the least competency to understand that they are being used for an experiment; that they are volunteering to do so; and that this is research; and certainly in the presence of a doctor whom they are familiar with and other doctors in white coats, confidence is automatically generated. They need no other.

My question is, isn't it an assumption on your part that these patients had understanding; they can communicate even by the visual observations of a syringe, doctor's bag, injection, they—this is part of hospital procedure. So, the communication is taken for granted. My question was, didn't you have to make an assumption that these patients understood, had understanding of the request that you were making, a request of them to volunteer as subjects for an experiment?

A: Yes. I certainly agree both in this specific instance that I was making an assumption—I think that Drs. Mandel and Custodio may be able to make a better answer to this particular point, because they know the patients better. I think it is true, also, as I think you have indicated, that we assume an understanding also when we communicate with patients, that is, I don't know if I said that clearly, but in any doctor-patient relationship there is this quality that you mentioned of the patient in a setting where he recognizes that the doctor is doing things to and for him. Undoubtedly, they associate this with what is proper. He accepts, essentially, things as being proper because they are being done under this total picture of medical doctor-patient relationship. I think that all we can do in such situations is to explain that what we are doing is not for your treatment, if necessary, to say that it makes no difference whether you have such a procedure or not. This will not influence your disease, and it will not influence your proper treatment.

DR. WIENER: Was there any deviation, as far as obtaining patients' consents here, was there any deviation from the long-standing practice of obtaining consents?

A: No, sir. There was—this was the reason that I believe that Dr. Mandel accepted this method. I had assured him that this was our established method of obtaining consents at Memorial and Ewing. [From transcript of proceedings before a Subcommittee of the Committee on Grievances, Department of Education of the State of New York, September, 1964, pp. 784-789.]

ileges must be accorded him. If they be so mentally or physically affected that they be incapable of making decisions, then the nearest of kin must be afforded the right to make this decision.

I therefore find the respondents, Dr. Southam and Dr. Mandel, both guilty beyond reasonable doubt of the charges and specifications as charged.

In considering the degree of punishment, I am considering the outstanding records of both these doctors, the high esteem by which they are held by the medical profession and scientists in general throughout the world. I also take into consideration the nature of the experiment and its purposes and I therefore recommend that they both be given a censure and reprimand.

*The findings and recommendation of Dr. Saul I. Heller, member of the subcommittee is as follows:*

It is my finding that Chester Southam, M.D. and Emanuel Mandel, M.D. are both guilty of each specification contained in the charges.

Just because such a project is worthy, and just because terminal patients were readily available, who were deteriorating anyhow, does not, in my opinion, warrant deceiving such patients into believing that they were submitting to ordinary and customary hospital procedure, intended to aid in the diagnosis of, or the alleviation of their particular illnesses.

This project was not even experimental therapy, although there are many inferences that it might be. In experimental therapy, patients and volunteers are selected who are able to clearly comprehend beforehand the full nature and details of the experiment, which generally are outlined on a printed form on which the patient or volunteer is asked to sign his consent. A volunteer consents after he is fully informed in lay language, that is, language that he can comprehend and this usually involves considerable thought and much discussion, with dozens of questions being asked, and fully answered over a period of time.

In my contacts with various investigators, especially during the past seven years, I was impressed by the fact that the National Institute of Health always advised the investigators to follow the above procedure in experimental therapy.

The project of Dr. Southam and Dr. Mandel at Jewish Chronic Disease Hospital was experimental research on a group of human beings, who were told that an injection was being given to them to test their immunity or resistance to

disease, and that a nodule would form and disappear. However, they were not asked to become volunteers and participate in an experiment on human beings for the purpose of furthering Dr. Southam's cancer research project. These patients and their relatives had the human right to decide what should be done with their bodies, except in a dire medical emergency.

These patients were entrusted to the care of the Jewish Chronic Disease Hospital by their relatives, who visited the patients and spoke to their doctors, and even the relatives were not informed of this research project. I cannot understand why the relatives of these patients were never informed of this experiment prior to the patients' having received the experimental injections of live cancer cells. This omission can only imply deceit, especially when one considers the procedure in any hospital, as, the unrefuted testimony of Dr. Leichter that in regard to patient No. 18, he had secured written consent from the patient's family to tap the patient's chest, and further that before administering the antibiotic drug, Terramycin 401 to patient No. 18, he also secured signed consent from the family. These procedures antedated the injections of live cancer cells.

It is only reasonable to conclude, if you must secure written consent from the family and disclose the true nature of an antibiotic, to give an antibiotic, you must disclose the true nature of the cellular material injected in this experiment, both to the patient and his family, in order to obtain informed consent, as was done by a competent resident in the case of the antibiotic, Terramycin 401. I am referring to Dr. Leichter, who in 1960 had been placed in charge of a research project sponsored by the National Institute of Health, by Dr. Goldner, the director of Jewish Chronic Disease Hospital, at that time. The competent residents at Jewish Chronic Disease Hospital were deliberately by-passed by Dr. Mandel, director at this time because he knew they would only adhere to the procedure of informed written consent.

Dr. Mandel asked Dr. Custodio, a resident who had just returned after a year's absence, if he were interested in participating in a research project which had been brought to Dr. Mandel's attention by Dr. Southam a week or so earlier. When Dr. Custodio indicated that he was interested, he was told by Dr. Mandel, "if we can get oral consents we can go ahead." He directed Dr. Custodio to prepare a list of non-cancer patients. It appears that there was no specific discussion between Dr. Mandel and Dr. Custodio

of any need for their terminal patients nor their families to understand that the patients were to be used as volunteers in cancer research.

Dr. Custodio testified that he chose 19 non-cancer terminal patients, in his mind, at random, the night before the experiment; and the next morning led Dr. Southam and his assistant, Dr. Levin, to the rooms of these patients.

Dr. Southam says that an explanation of some kind was made by Dr. Custodio regarding a test to study the patient's immune reactions and that a nodule would form and later disappear.

Dr. Mandel, himself, chose three cancer patients and states he was familiar with these cases. One of these cases was scheduled for elective surgery on July 18, 1963, and such elective surgery, according to Dr. Mandel, is arranged about five days in advance, and written consent was obtained for this operation. Nonetheless, this patient was used for human experimentation just two days before this scheduled operation and died on July 19, 1963, the day after the operation.

It is obvious that this patient would never have been subjected to this experiment, had any of the 4 doctors involved in the experiment known, or cared about knowing, the status of this patient.

Another of the three cases selected by Dr. Mandel was suffering from leukemia. Dr. Mandel testified that he tried to reassure this patient that he was getting better, because the patient expressed resentment in "being stuck with needles without getting better," and compared his lack of improvement unfavorably with the number of tests. Moreover, Dr. Mandel does not remember if he explained to this leukemia patient that there would be follow-up blood tests. It does seem that this leukemia patient was led to assume that his part in this procedure was therapy for his own illness, or a diagnostic aid to help him.

I thought it necessary, in forming an opinion, to review the 19 non-cancer terminal patients, who were selected at random by Dr. Custodio in his mind the night of July 15, 1963.

The mental and physical condition of the patients renders it impossible for them to give informed consent, in terms of forming a mature judgment in a matter of one to five minutes, on a complex scientific subject. Actually they were subjected to an injection by doctors in hospital attire, and were deceived into believing that this injection was of direct therapeutic benefit to them, or was essential for their treatment. This

was evidenced by Dr. Mandel's statement that patients do not question procedures which are done in a hospital, because they have confidence in the doctors, and tend to accept what they are told. In this instance, I believe their confidence was misplaced.

I would like to re-emphasize the procedure of the experiment which illustrates that informed consents could not have been obtained, because the patients nor their families were never told the truth; in that they were being asked to submit themselves as volunteers for human experiment in the field of cancer research, on a purely research basis, and not for a direct benefit of their particular illnesses. Nor were they told the true nature of the material to be injected.

Dr. Custodio greeted the patient and in the few minutes that the doctors prepared the injections, he told the patient that this was an injection to test their immune reactions and that a nodule would form in a few days and disappear in a few weeks.

During this time, Dr. Southam was sterilizing the skin of the thigh with cotton and alcohol. If the patient appeared apprehensive, Dr. Southam would verbally reassure the patient by such remarks as, "this is cotton, this is alcohol, this is novocaine, it doesn't hurt, you've had it before." Then he would proceed with the subcutaneous injections of live cancer cells. This procedure was repeated with remaining patients, selected by Dr. Custodio. Neither Dr. Southam nor Dr. Mandel knew which patients were selected. Could this be construed as informed consent?

Dr. Mandel, medical director of Jewish Chronic Disease Hospital, was not even present at the experiment of these 19 patients, which indicates that he has shunned his responsibility to the patients entrusted to his care.

In arriving at my decision in this matter, I am extremely concerned with the fact that these chronic, debilitated, sick patients were hurriedly and unexpectedly confronted with a verbal description of a technical procedure, which, even to a normal, educated, intelligent and healthy person, would have been inadequate and untruthful. This is fraud and deceit. I also believe that the omission on the hospital charts that these patients were injected with cultured live human cancer cells constitutes fraud and deceit.

I further believe that the rights of these patients and their families were violated by the respondents in this matter; who resorted to

trickery, false statement, deliberate deception. The respondents by acting in such a manner as to omit and conceal the facts of this experiment involves a breach of duty, trust and confidence to these patients, their families, and their fellow-man.

*The findings and recommendation of Dr. Morris F. Wiener, member of the subcommittee is as follows:*

The allegation that fraud and deceit had been perpetrated upon a group of patients in the Jewish Chronic Disease Hospital by the respondents is based upon: (a). Inadequate consents having been obtained for clinical investigation in not fully disclosing the cancer-origin of the material used in certain immunologic tests, and (b). The assumption that these injections were harmful and may produce cancer.

Although no fact of personal greed on the part of either respondent was revealed, nor was any appreciable injury to any of the subject-patients clearly demonstrated, the respondents failed to obtain written or meaningful consent consistent with appropriate directive governing research projects.

The problems of informed consent are considered nebulous and insoluble by a large segment of competent medical authority. The emotional reaction to the word "cancer" very often justifies its concealment. The blind patient whose sight is restored is not informed that his or her new cornea was transplanted from a cancerous eye removed from another patient. There are other instances where significant facts are concealed from patients, concealment tolerated or condoned by both medical and civil authority.

The injection of material obtained from a culture of cancer cells is not known to cause human cancer. These diseases are the result of autonomous new-growths which develop from an unrestrained proliferation of the individual's own native body cells. Total clinical experience, notably that of surgeons and pathologists who have frequent direct physical contact with cancerous tumors, further supports the principle that cancer is not a disease that is transferable from one individual to another. The universal acceptance of pooled plasma and blood-transfusions since World War II has offered a wide experience for the possible development of cancer from one person to another, and yet not one single case has ever been recorded.

In a recent case of purported transplantation of cancer to a noncancerous patient from a

cancerous patient, no analogous inference can be made. This instance was published in the *Journal of the American Medical Association*, Vol. 192: 752, 1965, the article entitled "Cadaveric Renal Homotransplantation with Inadvertent Transplantation of Carcinoma."

This article refers to the recipient of a homotransplanted kidney obtained from a patient who died of cancer and which apparently was present in the grafted organ. In order to negate the usual homograft rejection and enable the grafted kidney to survive, the patient was treated with immunosuppressive drugs for 5 months from the time of operation: Azathioprine, 100 to 300 mg. doses and Prednisone, 30 to 100 mg. daily were administered, and in addition, the grafted kidney was treated with x-radiation.

A deliberate calculated effort was made by drugs and x-ray to depress the known immune response mechanism that causes the rejection reaction. The treatment was continued until two days before death. It is obvious, and not surprising, that the immune suppression resulting from the treatment to prevent the rejection of the homotransplanted kidney also prevented rejection of the occult cancer cells within the grafted kidney.

It should be noted that the cancer cells in this case were directly transmitted as a part of a vital organized active tumor tissue from the donor to the recipient. On the contrary, in the experiment at issue, the suspension of cells used had been derived from cancer tissue which had been grown in artificial culture media for a period of 5 to 12 years. Considerable experience has shown that this artificially cultured material represents a "standardized biological," and not a biologically active organized tumor with known aggressive determinants.

With regard to the one instance of axillary metastasis following an injection of suspended tissue cultured cancer cells into the arm of one of the patients, the following points may very well be considered. The finding of extrinsic cells in lymph nodes which are not cancer and do not behave as cancer is known. In the case at issue, the presence of cancer cells in the lymph nodes may actually be a result of their passive transportation from the point of inoculation to the node. This type of passive transportation is commonly found in cases of eczematoid skin conditions, tattoo and other pigmentation. In view of the comprehensive experience involving injections of tissue cultured cells, not one case

is known that resulted in cancer. The known scientific principles and abundant evidence militate against such a possibility. Delayed hypersensitivity response in healthy and in sick individuals indicates that both groups of non-cancer ill patients and healthy individuals respond immunologically like healthy patients and not like patients afflicted with advanced cancer. In other words, it was reasonable to assume confidently that prompt rejection of homographs in the aged non-cancer patients could be predictive.

This meaningful term, "practice of medicine," is well proven by the test of time since it was first mentioned by Hippocrates over 2000 years ago. It is within the profound concept of this particular specific designation that medical science has evolved. Without this functioning concept scientific medical research is imperiled. The practice of medicine throughout the centuries has been, to a great extent, a matter of trial and error, and thereby inevitably connoting clinical experimentation. The doctor-patient relationship, which is the core of the practice of medicine, is not altered by hospital practice.

If an error has, in fact, been committed, it is in an area of judgmental vagueness. "The reverse of error is not truth, but error still; truth may lie in between." The public interest may be better served by constructive suggestions aimed at greater clarification of more specific guidelines in clinical research.

However, in view of the apparent current unacceptable method of pursuing the highly laudable purpose of the research program, the respondents are found guilty of the charges herein.

I dissent from the majority opinion as to the measure of discipline. The record shows that both respondents are exceptionally well-trained and highly regarded clinical investigators, and strongly endorsed by the highest local and national medical authorities.

I, therefore, recommend no further action as to discipline be taken.

b.

#### **Recommendations of the Medical Grievance Committee—June 10, 1965**

To the Board of Regents:

I, the undersigned, secretary of the MEDICAL GRIEVANCE COMMITTEE duly appointed pursuant to the Education Law of the State of New York, do hereby certify:

1. That charges, in writing, were duly pre-

ferred and filed against Dr. CHESTER M. SOUTHAM and Dr. EMANUEL E. MANDEL, duly licensed physicians of the State of New York, hereinafter referred to as respondents, wherein each respondent was charged with fraud or deceit and unprofessional conduct in the practice of medicine within the purview and meaning of Section 6514, subdivisions 2(a) and 2(g) of the said Education Law; that a copy of the said charges with notice of hearing were duly served upon each respondent, and hearings duly held thereon before a subcommittee composed of Drs. Ames, (chairman) Heller and Wiener and its written report of findings and recommendations together with a transcript of the evidence were duly transmitted to me.

2. That the said report of findings and recommendations, with the transcript of evidence, wherein it was recommended that each of the respondents, CHESTER M. SOUTHAM, M.D. and EMANUEL E. MANDEL, M.D., be found guilty of each specification of the charges herein, and further, Drs. Ames and Heller recommended that each respondent shall receive a censure and reprimand, Dr. Wiener recommends that no further action be taken as to discipline, were duly submitted to the members of the committee at a regular meeting held on June 10, 1965.

3. That, after due consideration and discussion, the vote of each member of the committee present was duly recorded as follows:

#### **RECORD OF VOTE**

MEMBER	DETERMINATION	RECOMMENDATION
DR. LAWRENCE AMES	guilty—both charges	censure and reprimand
DR. SAUL I. HELLER	same	same
DR. MORRIS F. WIENER	same	no further action
DR. FRANCIS M. BENEDETTO	same	censure and reprimand
DR. PASQUALE CARONE	same	same
DR. IRVING L. ERSHLER	same	same
DR. HENRY I. FINEBERG	same	same
DR. FRANCIS O. HARBACH	same	same
DR. SYDNEY M. KANEV	same	same
DR. JAMES C. POTTER	same	no further action
DR. SAMUEL SANES	same	same
DR. ALFRED A. SCHENONE	same	censure and reprimand
DR. SOLOMON SCHUSSEIM	same	same
DR. HERMAN B. SNOW	same	same
DR. MILTON S. WEINBERG	same	same
DR. WILLIAM L. WHEELER, JR.	same	same
DR. FREDERICK A. WURZBACH, JR.	same	same

4. That, as appears by the aforesaid tabulation of vote, the findings and recommendations



of the subcommittee as to GUILT was thereby adopted and made the findings, recommendation and determination of the committee; and it further appears by the said tabulation of vote as to the measure of discipline that the committee recommended to the Regents by a majority vote that each respondent be given a censure and reprimand on each specification of the charges.

I FURTHER CERTIFY that annexed hereto is a true copy of the record and proceedings taken herein as follows:

1. Transcript of the evidence
2. Report, findings and recommendation of the subcommittee
3. Report, findings, determination and recommendation of committee

All of which is respectfully submitted.

*Henry I. Fineberg*  
SECRETARY

### 3.

#### **The Board of Regents' Discipline Committee Reviews the Recommendations**

We are of the opinion that there are certain basic ethical standards concerning consent to human experimentation which were involved in this experiment and which were violated by the respondents. When a patient engages a physician or enters a hospital he may reasonably be deemed to have consented to such treatment as his physician or the hospital staff, in the exercise of their professional judgment, deem proper. Consent to normal diagnostic tests might similarly be presumed. Even so, doctors and hospitals as a matter of routine obtain formal written consents before surgery, and in a number of other instances, and whether or not a specific consent is required for a specific act must be decided on the facts of the particular case.

No one contends that these 22 patients, by merely being in the hospital, had volunteered their bodies for any purpose other than treatment of their condition. These injections were made as a part of a cancer research project. The incidental and remote possibility, urged by Dr. Mandel, that the research might have been beneficial to a patient is clearly insufficient to bring these injections within the area of procedures for which a consent could be implied. Actual consent was required.

What form such an actual consent must take is a matter of applying common sense to the particular facts of the case. No consent is

valid unless it is made by a person with legal and mental capacity to make it and is based on a disclosure of all material facts. Any fact which might influence the giving or withholding of consent is material. A patient has the right to know he is being asked to volunteer and to refuse to participate in an experiment for any reason, intelligent or otherwise, well-informed or prejudiced. A physician has no right to withhold from a prospective volunteer any fact which he knows may influence the decision. It is the volunteer's decision to make, and the physician may not take it away from him by the manner in which he asks the question or explains or fails to explain the circumstances. There is evidenced in the record in this proceeding an attitude on the part of some physicians that they can go ahead and do anything which they conclude is good for the patient, or which is of benefit experimentally or educationally and is not harmful to the patient, and that the patient's consent is an empty formality. With this we cannot agree.

In his testimony before the subcommittee, Dr. Mandel took the position that he regards these experiments as beneficial to the patients both because the experiment might result in a diagnosis of an advanced cancer which had not been discovered by the hospital, and also because the participation in the experiment would result in extra medical attention to the patients involved and possibly other patients in the hospital. The record indicated that the only additional medical care any of these patients received as a result of this experiment was that the injections were made and they were occasionally checked thereafter as to the progress of the growth and disappearance of the nodule. The inference that participation in the experiment benefited the patients because of such additional medical care is without foundation in the record. Since the purpose of the experiment was to obtain verification of Dr. Southam's hypothesis that diseased patients would reject the implant in the same manner as healthy patients and that their rejection would not be delayed as was that of patients suffering from an advanced cancer, it is somewhat inconsistent for Dr. Mandel to say before the experiment was completed that he authorized it as a diagnostic measure. In any event, it was clearly not treatment, not experimental therapy, and not a diagnostic test which would reasonably be given to these particular patients. Nevertheless, from the manner in which they were asked for their consent and from



the statement made to them that this was a test to determine their immunity or resistance to disease, the patients could naturally assume that it was being given to help in the diagnosis or treatment of their condition. They were not clearly and unequivocally asked if they wanted to volunteer to participate in an extraneous research project.

There is one point which is undisputed, namely, that the patients were not told that the cells to be injected were live cancer cells. From the respondents' standpoint this was not considered to be an important fact. They regarded the experiment as medically harmless. There was not appreciable danger of any harmful effects to the patients as a result of the injection of these cancer cells. It is not uncommon for a doctor to refrain from telling his patient that he had cancer where the physician in his professional judgment concludes that such a disclosure would be harmful to the patient. The respondents testified that they felt that telling these patients that the material did consist of live cancer cells would upset them and was immaterial to their consent. They overlooked the key fact that so far as this particular experiment was concerned, there was not the usual doctor-patient relationship and, therefore, no basis for the exercise of their usual professional judgment applicable to patient care. No person can be said to have volunteered for an experiment unless he has first understood what he was volunteering for. Any matter which might influence him in giving or withholding his consent is material. Deliberate nondisclosure of the material fact is no different from deliberate misrepresentation of such a fact. The respondents maintain that they did not withhold the fact that these were cancer cells because they thought that some of the patients might have refused to consent to the injection of live cancer cells into their bodies. This was, however, a possibility and a decision that had to be made by the patients and not for them. Accordingly, the alleged oral consents that they obtained after deliberately withholding this information were not informed consents and were, for this reason, fraudulently obtained.

Although there is conflicting testimony and evidence in this point, it is our opinion that some of these patients were in such a physical and mental condition that they were incapable of understanding the nature of this experiment or of giving an informed consent thereto. We agree with the discussion of this aspect of the case in the report of findings of Dr. Heller. We note that

in no case were any relatives of any of these patients told about the experiment nor were any of these patients asked if they wished to think the matter over or discuss it with their relatives. It is noteworthy that one of these same patients was operated on two days after the injections and that prior to making the operation, which was a part of the patient's treatment, the hospital obtained two separate written consents each signed by both the patient and a relative. If there was any doubt at all concerning a patient's ability to fully comprehend and consent to this experiment, it was the duty of the physicians involved to resolve that doubt before proceeding further. Even if we accept the testimony of Drs. Mandel and Custodio as to the condition of these patients, it is still clear that there was at least a doubt as to whether or not some of them fully understood what was going on and were mentally competent to consent. We do not say that it is necessary in all cases of human experimentation to obtain consents from relatives or to obtain written consents, but certainly upon the facts of this case and in view of the fact that the patients were debilitated, the performance of this experiment on the basis of alleged oral consents from those particular patients falls short of the ethical standards of the medical profession.

We now come to the question as to the ethical responsibility of Dr. Southam for the improper conduct of this experiment. In addition to his argument that the consent obtained was proper in all respects, Dr. Southam takes the position that he was not responsible for the internal practices at this hospital. He does not remember very well exactly what was said by Drs. Mandel and Custodio while they were obtaining the consents. He realized, however, that these patients were being approached for the first time. He also knew that they were all in a debilitated condition. As a physician in charge of the experiment, it was his duty to pay enough attention to what was going on to make sure that he was dealing with persons capable of being volunteers and sufficiently informed to consent to the use of their bodies for the experiment and not merely with people who were too confused or too sick or too resigned to object to the injection. He could not avoid responsibility for the procedure followed by Drs. Mandel and Custodio when he could see and hear what was going on. He accepts responsibility for the fact that the patients were not told the material to be injected consisted of live cancer cells. He

clearly indicates in his testimony that in such experiments he regards it as important to make it clear to the patients that what is being done is an experiment and is not for the treatment or diagnosis of their own condition, yet he was present, this was not adequately done, and he did not complain. A physician may not shirk his ethical responsibility or violate basic human rights so easily.

As the director of medicine at the hospital Dr. Mandel is directly responsible for the determination of the procedure followed in this experiment. His commendable desire to encourage research in the hospital cannot excuse his indifference to the rights of the patients. Although Dr. Mandel denied it, three of the physicians on his staff at the time testified that before this experiment was carried out he had discussed it with each of them and they had all individually told him that in their opinion he would be unable to obtain an informed consent from the patients. Dr. Mandel subsequently designated Dr. Custodio to carry out most of the details of the experiment and did not discuss it with those three physicians or with the staff physician who was responsible for making the normal rounds in the pavilion where the 19 non-cancer patients were housed. Dr. Mandel attempted to explain away the testimony of these four physicians by stating that they were all hostile to him. With respect to one physician who had been on the staff of the hospital for over 15 years and who held a responsible position under Dr. Mandel for over two years, and who had testified that many were physically or mentally incapable of giving an informed consent, Dr. Mandel testified that he never thought much of that doctor's ability. We believe the testimony of the other four physicians and agree with the statements of Dr. Heller in his report of findings that "the competent residents at Jewish Chronic Disease Hospital were deliberately bypassed by Dr. Mandel . . . because he knew they would only adhere to the procedure of informed written consent." Furthermore, Dr. Mandel was himself present while the first three patients were questioned and injected. The record indicates that the consents obtained from those three patients were defective in all of the respects discussed above except that they were apparently competent to have given an informed consent if they had been properly apprised of all the material facts. Dr. Mandel is equally responsible for failing to give adequate instructions to Dr. Custodio or to take any measures to assure that the other 19

patients were capable of giving an informed consent and in fact gave such consent.

An opportunity to appear before this committee was accorded to the respondents on October 5, 1965. Both respondents appeared in person. Dr. Southam was also represented by Philip Scott, John R. Hupper, and Gerald Oscar, his attorneys. Dr. Mandel was presented by Morris Ploscowe and by Irving Lattimer, his attorneys. John J. Calanese, assistant attorney general, appeared for the petitioner. This committee has given careful consideration to the entire record and to the briefs submitted to it and statements made before it.

After due deliberation and for all of the reasons discussed above it is the unanimous recommendation of this committee that the Board of Regents accept the findings of the medical committee on grievances that both of the respondents are guilty of fraud or deceit in the practice of medicine and of unprofessional conduct in the practice of medicine. It is also our unanimous recommendation that the Board of Regents modify the recommendation of said committee as to the measure of discipline, and that the medical license of each respondent be suspended for a period of one year on each specification, but that the execution of such suspensions be stayed, and each respondent be placed on probation for a period of one year upon the following terms and conditions:

1. That each respondent shall conduct himself in all ways in a manner befitting his professional status and shall conform fully to the moral and professional standards of conduct imposed by law and by his profession.
2. That so long as there is no indication of any further misconduct, each respondent may continue to practice as a physician, but that the department, upon receipt of satisfactory evidence of any such further misconduct, may forthwith terminate the stay of execution and order that the stay be vacated and the medical license of the respondent or respondents involved be suspended for a period of one year from the date of said order.
3. That any such action by the department vacating the stay of the suspension as to either or both respondents shall in no way bar further disciplinary action based upon additional misconduct.
4. That each respondent shall notify the department of any change of address or employment.
5. That upon full compliance with these

conditions for a period of one year each respondent may apply to the department for discharge from probation.

We trust that this measure of discipline will serve as a stern warning that zeal for research must not be carried to the point where it violates the basic rights and immunities of a human person.

Respectfully submitted,  
JOSEPH W. MCGOVERN, CHAIRMAN  
JOSEPH T. KING  
CARL H. PFORZHEIMER, JR.

#### 4.

### The Board of Regents Decides

#### Board of Regents of the University of the State of New York Licenses Suspended, Suspensions Stayed, Respondents Placed on Probation\*

Upon the report of the Regents Committee on Discipline, made in accordance with the provisions of section 211 of the Education Law, it was

*Voted*, That the determination of the Medical Committee on Grievances in the matter of Chester M. Southam . . . and Emanuel E. Mandel . . . be accepted, but that the recommendation of said Committee be modified and license No. 71055 and license No. 37359 respectively, issued under date of March 21, 1951, to said Dr. Southam and December 1, 1939, to said Dr. Mandel, and their registration or registrations as physicians, wherever they may appear, be suspended for a period of 1 year on each specification, said suspensions to run concurrently from the date of the service of the order effecting such suspensions, but that the execution of

such suspensions be stayed, and each respondent be placed on probation for a period of 1 year upon the following terms and conditions:

1. That each respondent shall conduct himself in all ways in a manner befitting his professional status and shall conform fully to the moral and professional standards of conduct imposed by law and by his profession;

2. That so long as there is no indication of any further misconduct, each respondent may continue to practice as a physician, but the Department, upon receipt of satisfactory evidence of any such further misconduct, may forthwith terminate the stay of execution and order that the stay be vacated and the medical license of the respondent or respondents involved be suspended for a period of 1 year from the date of said order.

3. That any such action by the Department vacating the stay of the suspension as to either or both respondents shall in no way bar further disciplinary action based upon additional misconduct;

4. That each respondent shall notify the Department of any change of address or employment;

5. That upon full compliance with these conditions for a period of 1 year each respondent may apply to the Department for discharge from probation; and

that the Commissioner of Education be empowered to execute, for and on behalf of the Board of Regents, all orders necessary to carry out the terms of this vote.

### NOTES

#### NOTE 1.

ELINOR LANGER

HUMAN EXPERIMENTATION—NEW YORK  
VERDICT AFFIRMS PATIENT'S RIGHTS\*

\* \* \*

[The] lawyers for Mandel and Southam raised two technical points of some interest. First, they claimed that, because "no clear-cut medical or professional standards were in force or were violated" by the two physicians, the attempt to find them guilty had an *ex post facto* quality. They also argued that the charges did

\* 34 *Journal of a Meeting of the Board of Regents of the University of the State of New York* 787 (1965). [The Board of Regents consists of 15 individuals elected by joint resolution of the two houses of New York's legislature for terms of 15 years. The Regents have jurisdiction over all education in the state, public and private, and over all licensed professions excluding the law. The three Regents most intimately involved in this decision were the three members of a special committee on discipline: Joseph W. McGovern, a lawyer; Joseph T. King, a lawyer; and Carl H. Pforzheimer, Jr., an investment banker. The remaining Regents, who concurred in the decision, are drawn from a variety of business and professional interests, including law, banking, education, and philanthropy.]

\* 151 *Science* 663, 665-666 (1966). Reprinted by permission. Copyright 1966 by the American Association for the Advancement of Science.

not accurately fit the case. Testimony was introduced from well-known cancer and other professional researchers, including I. S. Ravdin, vice president for medical affairs of the University of Pennsylvania, and George E. Moore, director of Roswell Park Memorial Institute, to the effect that Southam's practices did not differ dramatically from those of other researchers. "If the whole profession is doing it," one of the lawyers remarked in an interview, "how can you call it 'unprofessional conduct'?" The lawyers also argued that the "fraud and deceit" charge was more appropriate to low-brow scoundrels, such as physicians who cheat on insurance, supply illegal narcotics, or practice medicine without a license, than to their respectable and well-intentioned clients.

To all arguments of humane motivations, extenuating circumstance, conflicting testimony, or legal ambiguities, the final answer of the Regents was very simple: It is no excuse. There was never any disagreement on the principle that patients should not be used in experiments unrelated to treatment unless they have given informed consent. But in the Regents' decision, two refinements of that principle are heavily stressed. The first is that it is the patient, and not the physician, who has the right to decide what factors are or are not relevant to his consent, regardless of the rationality of his assessment. "Any fact which might influence the giving or withholding of consent is material," the Regents said. . . .

The second principle stressed by the Regents is that the physician, when he is acting as experimenter, has no claim to the doctor-patient relationship that, in a therapeutic situation, would give him the generally acknowledged right to withhold information if he judged it in the best interest of the patient. In the absence of a doctor-patient relationship, the Regents said, "there is no basis for the exercise of their usual professional judgement applicable to patient care." Southam, in an interview, disagreed. "An experimental relation has some elements of a therapeutic relationship," he said last week. "The patients still think of you as a doctor, and I react to them as a doctor, and want to avoid frightening them unnecessarily." Mandel takes a similar position. In a letter to the editor of a medical affairs newspaper he stated: "In accordance with the age-old motto—*primum non nocere*—it would seem that consideration of the patient's well-being may, at times, supersede the requirement for disclosure of facts if such facts lack

pertinence and may cause psychologic harm." But on this point, the Regents are clear: "No person can be said to have volunteered for an experiment unless he had first understood what he was volunteering for. Any matter which might influence him in giving or withholding his consent is material. Deliberate nondisclosure of the material fact is no different from deliberate misrepresentation of such a fact."

In closing their case, and acknowledging that the penalties imposed were severe—they might have just authorized a censure and reprimand—the Regents were pointed and succinct: "We trust that this measure of discipline will serve as a stern warning that zeal for research must not be carried to the point where it violates the basic rights and immunities of a human person."

What the impact of the case will be is by no means clear. The Regents' decision outlines clear rules for a very narrow situation and attempts to set out some broad principles as well. But it is by no means binding, and it by no means covers the variety of situations with which researchers seeking to use human subjects are faced. The question is, What will cover these situations? Codes and declarations, of which there are already several, are too general to offer specific guidance. Researchers and patients alike are too vulnerable to await a slow case-by-case accretion of specific rulings. One alternative is the development within each hospital or research institution of "ethical review committees" that could define the consent-and-disclosure requirements for each proposed experiment and see that they were adhered to. In theory, this is already taking place. During the Southam-Mandel hearings, the state attempted to prove that Southam, a recipient of an NIH grant, had violated regulations of the Public Health Service. In fact, the regulations in question govern only the normal volunteer program of the NIH Clinical Center in Bethesda. The PHS response to an inquiry from New York's Attorney General made clear that the rules were not generally applicable and stated that, "in supporting extramural clinical investigations, it is the position of the Public Health Service that proper ethical and moral standards are more effectively safeguarded by the processes of review and criticism by an investigator's peers than by regulation."

That is the theory, but the trouble is it is not yet being done. And, given the tremendous growth and variety of medical research involving human beings, if it is not done by the sci-

entific community, someone else will start to do it. The New York Regents may be only the beginning.

## NOTE 2.

AMERICAN ASSOCIATION FOR CANCER RESEARCH  
MINUTES OF THE 58TH ANNUAL MEETING\*

The Annual Business Meeting of Members was called to order at 5:05 P.M., April 14, 1967, at the Sherman House, Chicago, Illinois by President Kaplan. . . . Dr. Kaplan said that the two candidates for Vice-President, as selected in the recent mail balloting by members of the Association, were Drs. Leon Dmochowski and Chester M. Southam; he appointed tellers and asked them to conduct the balloting for Vice-President.

\* \* \*

Dr. Kaplan announced that the tellers had informed him that Dr. Chester M. Southam had

\* 9 *Proceedings of the American Association for Cancer Research* 87-89 (1968). Copyright 1968 by Cancer Research, Inc. Reprinted by permission.

been selected as Vice-President of the Association for 1967-68. . . .

\* \* \*

## NOTE 3.

AMERICAN ASSOCIATION FOR CANCER RESEARCH  
MINUTES OF THE 59TH ANNUAL MEETING†

The Annual Business Meeting of Members was called to order at 5:10 P.M., April 12, 1968, at Haddon Hall, Atlantic City, New Jersey by Vice-President Southam. . . .

\* \* \*

Dr. Southam announced that the tellers had informed him that Dr. Abraham Cantarow had been selected as the Vice-President of the Association for 1968-69. The Secretary-Treasurer said that the Board recommended that Dr. Chester M. Southam be elected President for 1968-69. When no additional nominations were made from the floor, it was moved that these two officers be declared duly elected. . . .

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† 10 *Proceedings of the American Association for Cancer Research* 110-111 (1969). Copyright 1969 by Cancer Research, Inc. Reprinted by permission.



## CHAPTER TWO

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# The Wichita Jury Recording Case

In 1954 a group of law professors and social scientists, with the approval of judges of the Tenth Judicial Circuit, recorded the deliberations of juries in six civil cases in the United States district court in Wichita, Kansas. The investigators did not inform the jurors that microphones had been concealed in the jury room. The litigants were also unaware of the research project, although their attorneys had consented to the recordings.

A year later the Internal Security Subcommittee of the Senate Committee on the Judiciary held public hearings in order to assess the impact of this experiment "upon the integrity of the jury system [which is protected by] the seventh amendment of the Constitution." These hearings led to the promulgation of a law which prohibited any recording of jury deliberations.

The duties and obligations of law professors, social scientists, judges, attorneys, and legislators towards jurors, clients, science, and society are major issues raised by the Wichita Jury Recording case. In examining these materials consider the questions raised in the introduction to the Jewish Chronic Disease Hospital case. In addition, also ask:

1. What value preferences guide the actions of the participants in the two cases toward their co-participants? What assumptions do they make about the research process?
2. Do these two cases pose different issues and, if so, why?

## A.

## How and by Whom Should Research Policy Be Formulated?\*

## 1.

**University of Chicago Law School  
Application to Ford Foundation for Support  
of Research on Law and the Behavioral  
Sciences—June 1952**

The subject matter of law is human behavior. The law deals with such behavior either in problem situations or where, for one reason or another, customary behavior without the added sanction of formal rules is deemed insufficient. The law builds on assumptions about human behavior. These assumptions are important in terms of the conduct to be regulated and in terms also of the effect of the regulation. Quite apart from all this, the institution of law is to be understood and evaluated in terms of the techniques and knowledge of the behavioral sciences. Moreover, the law can furnish to the behavioral sciences a coherent set of problems which can be clearly defined and which can provide the basis for interdisciplinary research. The discipline of the law, its selection of problems and its insistence upon solutions thus can be helpful to the behavioral sciences, and anthropology, social psychology, sociology, and economic theory can aid in the realistic study of the legal system.

In the work which is set forth, legal problems are to be selected as the basis for factual research which will be helpful to the development of the law and at the same time to the development of the behavioral sciences. The specific proposal is that (a) up to three studies be selected from a limited number herein set forth for the purpose of immediate study and that (b) the staff work with an advisory group to plan a more detailed set of studies which might be undertaken. The proposal is that an adequate grant be made to the University of Chicago Law School for a 2-year period to make possible the work on the 3 studies and the planning of a more detailed program.

\* Except as otherwise indicated, all materials in this chapter are reprinted from *Recording of Jury Deliberations, Hearings Pursuant to S.Res. 58 before the Subcommittee to Investigate the Administration of the Internal Security Act and Other Internal Security Laws of the Senate Committee on the Judiciary*, 84th Congress, 1st Session (1955).

\* \* \*

Although trial by jury in both criminal and civil cases is guaranteed by the Federal and most State constitutions, the jury system has long been under attack. The continuing criticism of an institution as basic as the jury underscores the desirability of studying the actual operation of the institution. The actual impact of many legal rules depends on their application by juries. The development of many procedural rules has been profoundly influenced by the existence of the jury system and by assumptions as to how juries operate. The appropriateness of both the substantive and procedural rules thus often depends on whether the assumptions made by the law concerning the jury system are warranted. Yet most of these assumptions have not been subjected to any empirical test. The justice of the legal system as a whole and justice or injustice in particular cases often depend on the successful operation of the jury system according to the assumptions made about it.

Procedures are available for the study of the jury system. Information can be obtained as to the actual operations of the system through systematic interviews with jury members after the close of a case concerning their individual and collective patterns of decision-making, the social structure of the jury and its influence on the decisions, the role of general personality and social determinants in the patterns of cognition and judgment of jury members operating as a group in the box and in the jury room. This is one way to determine what the decision-making process and its causes have been. It is possible to test actual jurors, after the actual trial (and members of a simulated jury), to discover the impressions created by particular types of evidence, comprehension of the instructions given, and the ability to recall as influenced by the length of the trial, the order of presentation, the techniques of the lawyers, etc. Such research could determine to what extent (a) the jury conceives of its function in the same way that the formal law conceives it; (b) the jury's concept of the issues deviates from the legal concepts; (c) the jury comprehends the judge's instructions; (d) the jury comprehends the evidence in the case; (e) the jury was moved by "rational" or emotional factors rooted in personality, social background, and the social situ-



ation of the courtroom, the jury box, and the jury room discussions. This is an area also where the trial and jury system can be closely simulated; that is, mock trials created with selected juries, and the behavior of such a simulated jury can be observed under controlled conditions such as Bales has been developing in his studies of small groups. It might be added in passing that realistic studies, of discussions about real issues by groups of 12 would contribute far more to our knowledge of small group behavior in dealing with important decision-making tasks than we gain from the observation at great expense of the decision-making process of groups of students coming together for no more than an hour solving chess and arithmetic problems. There is no doubt that a sufficient number of judges and lawyers would be willing to participate in such simulated trials as to make the conditions sufficiently close to reality to be useful.

The results of such a study can have far-reaching effects on the use of the jury system and the reform of the rules of evidence. Among other problems it can help clarify are these: (1) What is the effect of interrogation of jurors by the judge rather than the attorneys; (2) what is the effect of a judge's comment on the evidence; (3) what are the advantages of oral versus written instructions to jurors; (4) to what extent are detailed regulations of admissibility frustrated by inadequate regulation of the lawyer's arguments; (5) to what extent is there an identifiable group of cases for which the jury is particularly unsuitable or for which provision should be made for special juries composed of jurors with the requisite special skills or certain personality qualities.

\* \* \*

## 2.

### **Bernard Meltzer A Projected Study of the Jury as a Working Institution\***

The proposed study of the jury system, which I have been asked to describe, is one of four projects in a research program in law and

the behavioral sciences to be undertaken at the University of Chicago Law School. This research, which has been made possible by a grant from the Ford Foundation, will be conducted over a 3-year period. The program "will represent a major effort to bring to bear on the problems of law the research techniques of the behavioral sciences and, at the same time, to enrich the behavioral sciences by a study of legal institutions."

Trial by jury is a many-sided and controversial institution, which is a promising subject for interdisciplinary study. The jury plays an important role in the administration of justice. It is also a powerful symbol of our democratic faith, with an important place in our political theory. Furthermore, the jury, as an ad hoc collection of amateurs who generally must act unanimously to resolve a dispute, is a distinctive group which can be of special interest to students of small group interaction.

The jury has so often been called the "palladium of our civil rights" that this phrase has found a place in our dictionaries. At the same time, there has been continuous and lively criticism of the fitness of jury trial for some types of modern litigation. As a result, there have been significant reforms, both in this country and in England, and proposals for additional reforms.

The discussion has often involved untested assumptions about the actual workings of the institution and community attitudes toward it. In addition to such matters, which can be illuminated by empirical investigation, the debate involves fundamental and pervasive issues, such as the proper role of the expert and the amateur, the proper distribution of power between the official and the citizen, and the extent to which particular values represented by basic institutions should override any operational inefficiencies which they may involve.

Such issues cut too deep into our social fabric to be resolved by even the most comprehensive study. But a study could give us a store of reliable information about the actual workings of the jury in various contexts and the rules and usages which tend to promote or frustrate the various purposes ascribed to the jury. It could, for example, shed light on the criteria which should control eligibility for and exemption from jury service; on those rules of evidence which are based on assumptions about jury behavior; on the proper application of elastic concepts, such as "reversible error," which are also

\* 287 *The Annals of the American Academy of Political and Social Science* 97-100, 102 (1953). Reprinted by permission. [Professor Meltzer was then Director of the Jury Project at the University of Chicago Law School.]

based on such assumptions. A study in addition could make clearer the actual impact of particular rules of substantive law when they are to be applied in the end by a more or less random group drawn from the community.

From the many facets of the jury system which might be examined, the following have been selected for initial emphasis in the study:

1. The functions assigned and imputed to the jury.

2. The principles, if any, on which jury trial is or is not made available to litigants; the assumptions behind the decisions of litigants to elect or to waive a jury; identifiable classes of cases in which jury trial appears to involve special inconvenience.

3. The legal rules, the administrative practices, and the litigants' choices, which determine the composition of particular juries.

4. The decisional process—the determinants of the jurors' individual judgments; the nature and effect of group deliberation.

5. Workable criteria for appraising particular verdicts.

6. The rules and usages which promote, or interfere with, informed and rational jury determinations or the efficient use of jury in adjudication.

7. The significance of the jury as a form of democratic participation—community attitudes regarding the jury.

8. The social and individual costs of the jury system in various classes of litigation.

9. A re-examination of the functions of the jury in the light of data regarding its operations.

\* \* \*

... Since the jury room is not open to direct observation and since the general verdict, which is the usual form of a jury verdict, discloses nothing more than a general conclusion, it will be necessary to attempt to reconstruct the operations of the jury indirectly.

A variety of techniques may prove useful for this purpose. First, cases will be classified according to various criteria, which cannot be spelled out here. These cases will be studied prior to trial and observed in court by a lawyer and a social psychologist. After the jury has returned its verdict, the individual jurors will, with the permission of the courts involved, be interviewed under conditions assuring anonymity and with appropriate safeguards worked out with the courts.

These interviews will be based on two types of questions: (1) a general set of questions reflecting general presuppositions about the determinants of jury behavior in particular types of cases, and (2) a set of questions prepared by the lawyer-sociologist team based on the specific events of the trial.

These interviews should reveal the response of individual jurors to the trial; their capacity to follow and organize evidence and to understand and apply the judge's instructions; the nature and effect of the group deliberations; and the determinants of, or influential factors in, particular verdicts. These interviews should also yield data regarding the general personality and skills of the individual jurors and the impact of jury service on their attitudes regarding the jury and the administration of justice.

Some of the difficulties raised by the techniques described above deserve mention. . . .

\* \* \*

[I]ndividual jurors may not be wholly aware of, or may not be able to reconstruct accurately, either their personal responses or the group activity. . . .

\* \* \*

The data produced by intensive case studies of jury behavior will be supplemented by data designed to disclose the differences between judge and jury determinations. To facilitate such comparison, judges presiding over jury cases will be asked to keep records of such cases; to indicate, prior to the return of the jury verdict, the verdicts they would have reached if they had absorbed the jury function; to suggest explanations of significant differences between their "as-if" and the real verdict. In order to deepen the study of particular cases, the judges' parallel verdicts will be sought in cases which will be the subject of interviews with the jurors. In some cases, the judge may, of course, wish more time for reflection than is taken in the jury room. A record of such cases and the judge's subsequent "verdict" would be instructive.

Judges on their own initiative have in the past undertaken such comparisons, and we look forward to their cooperation in this and other phases of the study. We recognize that the judge's as-if verdict may diverge from the verdict which he might have reached if he were in fact disposing of a man's liberty or property. But the fact that judges often sit without a jury reduces this difficulty. The traditions and habits

which produce the real verdict will presumably not be abandoned for the simulated verdict.

\* \* \*

It is tempting to speculate more concretely on the final form of the study and possible impact both on the administration of justice and on legal education and research. But there has already been too much prophecy in this paper. It seems more appropriate to turn to some of the difficulties presented by the study which have general implications.

Not the least of these is a more or less explicit feeling that ignorance about the jury may be bliss. This is a curious notion in a society which is based on free inquiry and yet which is relatively uninformed about the operations of its key institutions. There is a related notion, that the examination of an important legal institution is necessarily animated by hostility to the institution, which is completely inapplicable to the projected study.

Neither of these notions should or will, we believe, be accepted by the bar, the judiciary, or the other interested disciplines. Because of their common interest in a better understanding and a continual improvement of the administration of justice, we are confident of getting their indispensable cooperation.

Any description of a plan for research is more an exercise in prophecy than in reporting. Accordingly, this prospectus is presented with the caveat that it is tentative and that my colleagues have saved their rights. This prospectus also carries with it an unrestricted invitation for critical comment. Indeed, it is the hope for such comment from lawyers and nonlawyers alike which is the primary justification for this premature delivery.

### 3.

#### **Letter from Paul R. Kitch, Esq.\* to Professor Bernard D. Meltzer—May 1, 1953**

Your statement as to the projected study of the jury as a working institution reached my desk this week and because I have always been particularly interested in the projected research in the indicated field I am taking this opportunity to make a definite suggestion.

\* Paul R. Kitch is a graduate of the University of Chicago Law School and an attorney in Wichita, Kansas.

For a good many years I participated in a substantial number of jury cases. As is the case with the most trial lawyers I was always interested in attempting to find out the various factors which would underlie a jury's verdict.

This office for many years consistently carried on the practice of interviewing a substantial number of the jurors who sat on each case. There was always a twofold purpose to these interviews. One, of course, was in a situation of an adverse verdict to discover if there were any grounds for charging misconduct of a jury so as to obtain a new trial. The other was to ascertain the receptiveness of jurors to certain arguments and to attempt to better evaluate the effectiveness of various approaches to a jury.

There is one conclusion which I long ago arrived at and from which I have never wavered and that is that jurors as a class are seldom accurate in their recollections as to actually what transpired in a jury room and many times intentionally deceive the interviewers in an effort to be all things to all men. In several instances we have actually examined jurors on post trial motions where there has been substantial conflict in the testimony as to what did and did not happen in the jury room. These are all matters covered by reported cases. Jurors are seldom conscious of the underlying psychological factors which have motivated their decisions.

The above paragraph is preliminary to my criticism of a so-called research project which basically is dependent upon testing methods which are unscientific to say the least. I do not mean to be critical. I only want to be constructive. Your entire paper leaves me with the clear impression that all your faculty committee has outlined is a better coordinated and obviously more efficient use of the same testing techniques that have been used by the profession for generations.

Need for true research in the indicated field is so great that it seems a shame to miss the opportunity to really make a thrilling and valuable contribution to the profession when at last some funds have been provided for the endeavor.

If the university would use the first allotment of funds for the purpose of first obtaining exact information as to what goes on in a jury room and then organizing the next step of your project on the basis of interpreting such information I am sure that you would end up with something of considerably more value than what is presently indicated.

I am certain that you could get the coopera-

tion of various courts in permitting you to install secret transcribing devices in jury rooms so that over a comparatively short period of time you could accumulate a substantial number of actual verbatim case histories. Adequate safeguards can be arranged with the courts for the protection of the identity of the individuals involved.

Once you accumulated a sufficient body of factual information the possible uses which you

could make of the material would be of inestimable value to the profession and to the public. I do not think such a study has ever been made but from the standpoint of its future public interest and public contribution I am sure that its value will be many times greater than the value of the project outlined.

\* \* \*

## B.

### How and by Whom Should the Research Process Be Administered?

#### 1.

**Letter from Paul R. Kitch, Esq. to  
Orie L. Phillips, Chief Judge, Circuit Court  
of Appeals—November 23, 1953**

You will recall that while you were in Wichita I discussed with you in a preliminary way a research project for the study of the jury system, which project is being carried out by the faculty of the University of Chicago Law School under the terms of a very liberal grant from the Ford Foundation.

A general description of the project is outlined in a pamphlet by Bernard D. Meltzer, chairman of the committee supervising the project. A copy of the same is enclosed with this letter.

The committee recognized that if a significant job is to be accomplished that it will be necessary to obtain specific and concrete information as to what actually goes on in a jury room. The only means by which the committee can assimilate scientific, accurate information concerning the actual functioning of a jury is to obtain the assistance of trial courts to the extent that actual recordings are made of a substantial number of actual jury deliberations. It is the hope of the research committee that a minimum of 500 transcriptions can be assembled over the next 3-year period.

The committee would prefer to obtain these records in the various Federal courts because of their belief that a testing program in an area where juries are generally considered to be of the highest quality would more accurately reflect the true worth of the jury system than would recordings taken from some jurisdictions where the administration of the jury system has been questioned.

The committee is quite anxious to start this project in the 10th circuit because of a belief on their part that the states represented in the 10th

circuit provide more of a typical cross section of community life than most of the other circuits.

As I previously informed you Judge Hill has already indicated his willingness to assist in the proposed project provided that the matter has your preliminary approval. Once a satisfactory plan is worked out we then propose to ask the assistance of other trial judges of the circuit. We are not asking any trial judge to assume such a responsibility unless he personally desires to give his aid. However, we do feel that the project is so meritorious that we will have little difficulty in gaining the assistance of the trial judge.

Naturally it is not intended any testing methods be made of jury deliberations in criminal cases nor is it intended that such transcriptions be made of any particular case without the consent of trial counsel for the respective litigants.

It is proposed that the experimental work be carried out under very definite rules designed to fully protect all jurors against any identification.

The committee has already made preliminary experiments with various types of recording equipment. In the committee's opinion the technical aspects of this problem will be easily handled.

A formal set of rules for use of the equipment will be approved by each trial judge and, if you desire, by yourself before the project is commenced in any court. In order to be specific we have prepared the following proposed rules with the expectation that there will be suggestions made for their improvement. I will state these proposed rules specifically so that you will have something concrete to consider. They are as follows:

1. A recording microphone will be placed in each jury room. The recording instrument

with a satisfactory locking device will be placed in the judge's office or at such other place as the trial judge may designate. The trial judge will be the sole custodian of the key. The operation of the instrument will be the responsibility of the court reporter or such other person as the trial judge may designate.

2. No recordings will be made in criminal cases. No recordings will be made in civil cases without the consent of counsel for each party.

3. When a recording is taken of a jury deliberation the record will be sealed and will remain in the custody of the trial judge or such other person as he may designate until final judgment has been entered and all appeals have been terminated. When this time arises the recording will be forwarded to a person to be designated by the research committee or to such person as the circuit court may prefer to designate. A single transcription of the record will then be made. Thereafter the original recording will be destroyed. Thereafter the research committee will supervise the editing of the transcript so that all personal names or geographical references and all other identifying statements will be edited in such a way as to avoid any identification of the persons or controversies involved. The edited transcript, together with the original transcript, will then be forwarded to the clerk of the circuit court of appeals or to such other person as your court may designate for review. If such officer is satisfied that the record has been appropriately edited he will then destroy the original transcript and will return to the research committee the edited transcript. If such officer feels that further editing is necessary he shall accomplish the same, destroying both the original transcription and the suggested edited transcription, and will return to the committee the transcript as edited by such officer.

4. The entire project insofar as it involves the recording of jury deliberations will receive no publicity from any source until after the project is completed.

5. In the event that services of court reporters are required by designation of trial courts or in the event that any person is appointed by the circuit court to perform any of the duties called for hereunder such persons shall be compensated from the funds made available to the research committee in such amounts and at such times as the trial court or the circuit court may fix.

I again state the above rules are only suggestive in form and it is the desire of the committee to carry out the project under any addi-

tional restraints which the trial judges or yourself may see fit to require.

I have intentionally been very brief in the presentation. I have made no effort to outline the possible advantages of this study to the legal profession and the courts at large. Certainly the study is approached with an open mind by the research committee and there is absolutely no basis for predicting any given conclusions which can be established by the study.

If there is any reluctance on your part to approve the court's participation in this program I would be very happy to arrange for the presence in Denver of those persons who will be responsible for the carrying out of the actual project at any time which you may suggest. We are all extremely enthusiastic concerning the possible contributions to be made by this proposed study and we are very hopeful of obtaining your approval.

## 2.

### **Letter from Chief Judge Orie L. Phillips to Paul R. Kitch, Esq.—November 25, 1953**

\* \* \*

I have no objection to the project being carried out in the tenth circuit, subject to the conditions and rules set forth in your letter, with one exception.

I think it should be made plain to the jurors that while a recording is to be made of their deliberations in the jury room, it will only be released in a form which is wholly impersonal and will not disclose to the public the identity of the jurors or of the trial in which the recording was made. I think a rule should be included covering the suggestion I have made.

\* \* \*

## 3.

### **Letter from Paul R. Kitch, Esq. to Delmas C. Hill, United States District Judge, District of Kansas—January 20, 1954**

You have the original of Judge Phillips' letter giving his approval to the proposal . . . to make transcriptions of jury-room deliberations under certain conditions. . . .

However, in giving his approval to the plan Judge Phillips stated that he thought in addition to the protective measures outlined in

our letter that the jury should also be advised that a transcription might be made of their jury-room deliberations.

Those in charge of the research project feel that if the jury is aware that a transcription is being made that the deliberations may be affected thereby and the accuracy of any conclusions drawn from the transcriptions will be subject to serious challenge.

They are willing to take any further measures which Judge Phillips feels to be necessary to positively guarantee the protection of the identity of the jurors. If it becomes necessary to actually advise the jury of the taking of a transcription it is doubtful if the advantages of getting the information on such a basis will justify the tremendous expense involved in the undertaking. However, I do not believe that any final decision has been reached on this point pending our efforts to obtain Judge Phillips' consent to the original plan or some modification thereof.

In addition to the above point a very realistic problem is presented and that is that few lawyers would express their consent to the making of such a transcription if the jury was to know that the same was being made.

As long as the jury does not know a transcription is being made then there is no possible basis for belief that the jury's deliberations will be affected by the making of a transcription.

If the jury is advised that a transcription is to be made of its deliberations many lawyers will feel that the jurors may not carry on their deliberations in the same manner as they would in the absence of such a transcription. Consequently any lawyer who felt he had a particularly weak case would be extremely reluctant to consent to the procedure. If the jury does not know the recording is being made we do not anticipate any difficulty in obtaining the consent of counsel in each case to the making of the transcription.

If this matter can be cleared with Judge Phillips I think that Mr. Meltzer, Mr. Kurland, and Mr. Levi are ready to take immediate steps to launch the program.

#### 4.

#### **Letter from Victor J. Stone to Judge Delmas C. Hill—March 18, 1954**

I want to thank you most sincerely for the hospitality and cooperation that you extended

to us yesterday. We were, of course, gratified to learn that you are as enthusiastic about our project as we are.

We are proceeding with our plans on the following understanding. Mr. Arents will arrange with Mr. Clark, the building supervisor, to install sound equipment necessary to do the job we have in mind. If it appears advisable, we shall have someone in Wichita to attend the machine.

It is also my understanding that we can order a copy of the trial transcript if we wish. You prefer to retain the sound tape of a deliberation until you have disposed of motions and have entered final judgment in the case. Thereupon, the tapes are to be shipped to us in Chicago, where they will be transcribed. The transcriptions will then be edited to remove any possibility of identifying the particular case. Only the edited version will ever be revealed to persons outside our immediate staff, and originals will be disposed of in accordance with your directions.

\* \* \*

#### 5.

#### **Letter from Judge Delmas C. Hill to Chief Judge Orie L. Phillips— August 26, 1954**

Pursuant to your request for information concerning the jury project conducted in this court during the past few months by the Law School of the University of Chicago I am happy to give you the following information.

The law school provided new equipment for the project including microphones and the recording machine itself. At project expense the microphones were installed in well-concealed places in the jury room and the machine was placed in the coat closet of my private office with proper wiring connections.

A project team was sent to Wichita by the dean of the law school with Mr. Ab Mikva, an able Chicago attorney and former law clerk to Mr. Justice Minton, as the supervisor. There were two keys to the closet which was kept locked at all times during the period the project was being carried on with one key remaining in my hands and the second key being turned over to Mr. Mikva, and at no time during the recordings was anyone permitted to listen to the same—this including myself. Mr. Mikva operated the recording machine and made such mechani-

cal adjustments or changes on the same during the recordings as was necessary to insure proper recording but did not at any time listen to the deliberations for any other purpose.

In addition the following precautions were taken:

1. The recordings were made only in civil cases and at no time was any recording made of any criminal proceeding.

2. In each case recorded, prior to the commencement of the trial, counsel on both sides were called into my chambers and were fully advised concerning the project, and the consent of all counsel in each particular case was received before the recording of the jury deliberations was made. At this same time counsel were advised that the recordings would not be heard by anyone until after all posttrial matters were disposed of by the court and that the same would not be available to anyone for purposes of appeal.

3. No publicity of any kind or character was given to the project through the medium of the newspapers.

4. No juror had knowledge that recordings of the jury deliberations were being made so that that fact could not in any way affect the jury deliberations.

Recordings were made of jury deliberations in six civil jury trials. Two of them being automobile damage suits. Two were government land condemnation suits. One a breach of promise to marry suit and one a damage suit arising from the alleged underground seepage of water from a privately owned water reservoir.

As to all of these cases the project team was instructed by the court to do nothing with any particular recording until that case was finally disposed of by the court. At present transcripts have been made in two cases, both of which have been finally disposed of. In the two condemnation cases motions for new trials were filed and no questions raised which could in any manner be affected by these recordings as they were purely legal questions passed upon by the court during the course of the trials, and the team was permitted to make transcripts of those recordings. In the fifth case notice of appeal was given and nothing was done on this case. In the sixth case a motion for new trial is still pending and likewise nothing has been done by the team with this recording.

All of the recordings were taken back to Chicago by Mr. Mikva and placed under lock in the office of Dean Levi, but after receipt of your telephone call, I requested that they

return to me all of the recordings except in the two civil cases finally completed, and also all other material they had concerning any of the incomplete cases. I have also requested copies of the transcripts in the two completed cases so that I could forward the same to you for your perusal and consideration.

In connection with these cases I permitted the team to go out and personally interview the jurors, but not until such jurors had been finally discharged from jury duty and even then without the jurors learning that any recordings had been made. I am also advised that copies of these interviews will be furnished to me so that I may forward them on to you.

Undoubtedly you also want to know the reaction of the lawyers involved in these various cases to this project and I can assure you that they were all wholeheartedly in favor of it and had a feeling that a great deal could be learned from such an experiment from which improvements could be made in our jury system.

After you called me I asked Mr. Kitch to come to my office in order that I could find out exactly what had transpired in the committee room. He stated to me that his purpose in going to the meeting was to secure the cooperation of other judges so that the project could be extended and carried on in other districts. That he did not refer to any particular district in which the project had operated but did say that it was in the 10th circuit. He also asked me to tell you that he would be very glad to fly out to Denver and talk further with you about the same if you so desired. Likewise, I would be very happy to talk with you personally about it and, of course, will give you any other desired information, and when I have received the two completed transcripts and the writeups of juror interviews, I will forward the same to you.

## 6.

**Letter from Judge Arthur F. Lederle,  
American Bar Association, Section of  
Judicial Administration, to Paul R. Kitch,  
Esq.—September 22, 1954**

I appreciated very much the courtesy of your attendance at our council meeting. As you perhaps know, your visit stirred up some controversy, but that is one of the things that one must expect if he is to do anything out of the regular, day-to-day routine.

I hope you will continue to maintain your



enthusiasm for your project and wish that you would continue to keep in touch with Judge Holtzoff, who is chairman of the section jury committee.

## 7.

**University of Chicago Law School  
Application to Ford Foundation for Extension  
of Grant—December 21, 1954**

On August 18, 1952, the Ford Foundation granted \$400,000 to the law school of the University of Chicago "to support a research program in law and the behavioral sciences for a period of 2½ years, including about one-half year for organization and recruitment." At that time the foundation stated of the research program, "We feel that it promises to be particularly significant for the development of both law and the behavioral sciences." The research program is fulfilling the hopes and expectations held for it. This is an application for an adequate grant to insure the continuation and development of the program for a 5-year period. . . .

The program of research in law and the behavioral sciences provides for three studies to be selected for immediate and intensive study. The three studies selected and now in process are the nature and operation of the jury system; . . . The program called also for the preparation, with the help of an advisory group, and with the benefit of the experience of the present program, of an additional detailed set of possible studies. The members of the advisory group are David Cavers, Harvard Law School; Abe Fortas, Esq., Arnold, Fortas & Porter; Harold Lasswell, Yale School of Law; Robert K. Merton, Columbia University; John Ritchie III, University of Wisconsin Law School; Hon. Walter Schaefer, Illinois Supreme Court; and Hans Speier, the Rand Corp. As planned, with the help of this group, an additional detailed set of possible studies is now in preparation. The present on-going research program has been presented to this advisory group.

\* \* \*

. . . The jury project has wide scope. Most emphasis has been placed on (1) problems of jury selection; (2) the effect upon the juror of the manner of his reception into the trial system; (3) the impact of the trial and of the instructions of the judge upon the jurors; (4) the nature of the jury's deliberation; and (5) a com-

parison of jury trials with alternative adjudicative procedures. We have been interested also in analyzing jury behavior, partly in terms of types of jurors, their beliefs, origin, economic and educational status. A considerable amount of historical, legal, and comparative law material has been prepared, both as a way of framing problems and as an aid to understanding and evaluating data.

The jury project has used a variety of techniques. Among them, it has used intensive interviews with actual jurors; the experimental jury; and questionnaires. This arrangement of technique has made it possible to try out observations from actual juror interviews by way of an experimental jury and through questionnaires. We have full files on 18 cases where we have had an observer present throughout the trial followed by intensive interviews with jurors. We have used experimental juries by recording a version of a trial, playing this version or significant modifications of it to different juries drawn from regular jury pools and under the discipline of court personnel, recording these deliberations and receiving the various verdicts. By this means it is possible to obtain data on the impact in the deliberation and on the verdict of particular evidence, modes of trial, and instructions. . . . The deliberations on the same case by different juries have provided material on the variety and recurring patterns of jury discussions with respect to the ordering of evidence and issues and the considerations of factors outside the evidence of the case. We are using a questionnaire (the judge-jury questionnaire) to determine the extent to which judges agree or disagree with jury verdicts. The questionnaire is now being filled out by more than 800 Federal and State judges in connection with jury cases as they come before the judge. Questionnaires or interviews are being used to determine further relationships between juror characteristics and votes (the criminal-juror questionnaire), the attitude of jurors toward jury service, reasons for jury waiver, and the damage expectations for jury cases in different areas of the country (regional consistency of jury verdicts).

The present work on the jury project has opened up many avenues for further investigation. We wish to continue our inquiry into the meaning to jurors of particular instructions in different type situations as, for example, the impact of various instructions on the jury, where insanity is pleaded as a defense; the effect of



contributory negligence instructions, or the effect of judicial admonitions to disregard improper evidence, or to consider evidence only as impeachment testimony, or to disregard the failure of the defendant to testify in his own behalf, or to consider a confession only after it has been found by the jury to be voluntary. These are areas where the jury's felt sense of justice and its evaluation of behavior may create an application of law more meaningful than formal rules. The material on jury deliberation, now being obtained through both experimental means and the interviewing of actual jurors, will help in the present analysis of the extent to which (1) issues are understood and deliberated upon; (2) data not presented in the formal trial enter into the deliberation; (3) different types of evidence are critically evaluated; (4) the indoctrination of jurors prior to or during the trial can change juror behavior. We have prepared an experimental case to help reveal the effect upon deliberation and verdict of special interrogatories and special verdicts as means of controlling the deliberation process. The analysis of the relationship between individual juror awards and the characteristics of the jurors will give more meaning to our inquiry into the methods and the effect of the methods of jury selection. We wish to develop further the comparison to bench trials and to those relevant phases of arbitration and administrative hearings, and to obtain additional material on the impact of the jury trial in creating an image of the justice of the legal system among the jurors, the litigants, and various segments of the public.

\* \* \*

## 8.

### **Letter from Chief Judge Orie L. Phillips to Paul R. Kitch, Esq.—April 14, 1955**

Judge Hill talked with me on the telephone day before yesterday and advised that he would have to get consent from the lawyers in the case before the recording could be made public. He thought there would be no trouble in getting that consent.

Judge Hill also said he thought it might be better to keep anonymous the court in which the recording was made. I told him I would leave that to you and him. The important thing is to make quite clear at the outset the safeguards that

are thrown around the taking and use of the recording.

## 9.

### **Excerpts from Proceedings at the Annual Conference of the Tenth Judicial Circuit held at Estes Park, Colorado—July 7, 1955\***

JUDGE HILL: [L]adies and gentlemen, one of the most precious rights possessed by the people of Anglo-Saxon nations is the right to trial by jury. That right was acquired only after years of bloodshed and is considered by all of us as one of the greatest safeguards of our individual liberties. Do we lawyers and judges follow the responsibility of the administration of the jury system as it exists today? That responsibility should never be taken lightly, but we should constantly strive to better the system by improving the efficiency of its administration. We will all agree, I am sure, that the ultimate purpose of trial by jury is the ascertainment of the truth. If we fail in any particular instance to achieve that purpose, it is not the fault of the jury system, but, rather, a deficiency in its administration, and the lawyers and judges involved must take the blame.

Down through the years the jury system has been taken for granted by both lawyers and judges, and little if no effort has been made to study the system with the view of improving its efficiency. Something over a year ago a group of Kansas lawyers in preparation for a program at the 1954 Tenth Circuit Conference carried out a most worth-while project regarding the jury system within the Federal courts of this circuit. You will all recall that project. Personally, I received a great benefit from that project and as a result changed some of my own administrative practices which I am sure has to some extent been an improvement.

The approach to this problem should be in a scientific manner and not in a hit-and-miss fashion. A study should be conducted on a nationwide basis including local, State, and Federal courts and in all geographical areas. Such a study must necessarily take a considerable time, and in the interim we should withhold our opinions, criticisms, or suggestions until such time as the study is completed. It would take

\* Judge Delmas C. Hill presided and approximately 100 judges and lawyers attended this conference.

the full cooperation of the bench and bar as well as laymen and organizations interested in bettering the efficiency of the system.

In this connection we must bear in mind that the sole objective of such a study would be to better the system and not to tear it down. One of the most significant jury studies being conducted is a research project under the sponsorship of a well-known American university which includes among its activities the actual recording of jury room deliberations. Naturally, such a project must be conducted with extreme caution and with consideration for the full protection of litigants and jurors. I have requested a representative of that university to furnish me with a statement concerning the safeguards used in this connection, and I am advised by him that the following constitute a list of minimum safeguards strictly adhered to:

1. The consent of the trial judge is first obtained, and the project then conducted under such restrictions and limitations as he sees fit to use.

2. The consent of the litigants through their attorneys must be obtained.

3. That the recordings be made only in civil cases.

4. That the recordings be made only under the close personal supervision of the trial judge and not to be heard or used by anyone including the judge until after the judgment in the case becomes final.

5. The recordings when finally transcribed, then edited by the project staff to remove all geographical, personal, and other identifying references, and the original recording is then destroyed.

6. That care is exercised to prevent adverse results from such investigation and premature publicity be avoided.

7. That the product of this work be used only for scientific purposes for which it was intended.

By special arrangement with the university I referred to and with the cooperation of the project staff our program this afternoon will consist of the reproduction of one of its recordings. Before playing the reproduction we will present an abbreviated resume of the case from which the recording was made so that you may have the proper background to understand the jury deliberations as you hear them. In this connection two members of the Wichita bar will assist me. Mr. Malcolm Miller will state the case for the plaintiff, and Mr. William Tinker will state

the case for the defendant. After that I will read you in substance the instructions actually given in the case which were also furnished to me by the project staff.

Let me stress the fact that the only purpose of this program is to stimulate interest amongst the bench and bar of this circuit in some sort of an effort to study scientifically the workings of the jury system toward the end that some improvement in the efficiency of the jury system in this circuit may be achieved. There are many methods by which such a study could be conducted. We are only attempting to illustrate one such method and have selected that particular method believing that it might be of interest to you.

In conclusion, let me say that as of now there are no materials available to judges, lawyers, or students of the law on the process of the workings of jury deliberations. The results of all such studies should be made available to lawyers, judges, and students of the law to the end that the efficiency of the jury system may be improved and thus our entire judicial system strengthened. . . .

\* \* \*

JUDGE HILL: Anyone else?

JUDGE PHILLIPS: Judge Hill, I would like to have a word: It happened to be revealed at a meeting of the American Bar Association that these experiments were being carried on and a very distinguished member of the Federal Judiciary was quite critical. In fact, he condemned it severely and even went so far as to intercede with the foundation that was making some grants in aid of this project and suggested that they should withdraw their grant. As a result of that event the members of this committee asked me to present my views about the desirability and the propriety of carrying on this experiment. At first blush I had some doubts. But after carefully going over the safeguards that were thrown around the experiment I reached the conclusion that it could not do any harm, that the jury involved in the case would have no intimation or even curiosity or any idea that the recording was being made of their deliberations and that no publicity would be given to it so that juries generally might get the notion that their deliberations were being recorded so they might have some inhibitions.

I had some doubts at the beginning as to what good might come out of it after realizing they reached a conclusion that it could not be

harmful. I would like to say I think there are two results that are on the credit side: One is proving, I think demonstrating, that juries generally do a good job. I think it demonstrates that. As a matter of fact, sometimes, if you ascertained the reasons or the line of reasoning that a jury goes through in reaching its verdict, you wonder what in the world they were thinking about, how they got off on this tangent or that tangent. But the thing about it is that when they get through and bring in a verdict in a vast majority of cases I think the trial judge will say the jury did the right thing. . . .

\* \* \*

The other thing is important that Judge Murrah suggested, instructing a jury. As my good friend Mr. Rooney indicated, it is a difficult problem. No matter how you couch your language it is difficult to present a charge that will be clear and plain and intelligent to the jury. If

we discover in a particular case from one of these recordings that the jury didn't understand the charge, that on a critical issue the charge was misleading, we will avoid those mistakes in the future.

\* \* \*

MR. GARWOOD: I would like to inquire whether these experimenters are aware of the fact that eavesdropping on juries is a criminal offense.

VOICE: May I say that eavesdropping is when you seek to use information against the individual you are eavesdropping on. It is not eavesdropping when you gather general information as to general conduct without any desire to use it against the person from whom you got the information.

JUDGE HILL: Anyone else? We will stand adjourned.

\* \* \*

## C.

### How and by Whom Should the Consequences of Research Be Reviewed?

#### 1.

##### Informing the United States Congress for Decision

SENATOR JAMES O. EASTLAND, chairman: This hearing has been called to make a public record of the facts behind the reports respecting the recording of the deliberations of juries, allegedly in connection with a research project financed by a grant from the Ford Foundation.

The purpose of getting the facts of this matter on record is to permit assessment of the impact of this activity upon the integrity of the jury system, as a basis for decision respecting what legislation may be necessary to protect the jury system. The Congress has an obligation in this regard, for the Congress has the duty of making all laws necessary and proper for carrying into full effect the provisions of the Constitution, and the seventh amendment of the Constitution specifically requires preservation of trial by jury, which necessarily must mean the kind of trial by jury which was in force under the common law at the time that amendment was written.

The jurisdiction of the Internal Security Subcommittee in this matter arises from the fact that anything which undermines or threatens the integrity of the jury system necessarily

affects the internal security of the United States.

The Internal Security Subcommittee has been criticized in the past for giving too much attention to matters involving subversion and subversives, and critics of the committee have pointed out that the field of internal security is not limited to matters of subversives and subversion. I suppose there may be some criticism of the subcommittee in this instance for venturing into a matter which does not appear to involve subversives or subversion. Nevertheless, I am clear in my own mind that this subcommittee does have jurisdiction to go as far as we propose to go.

\* \* \*

My views have been especially well expressed, perhaps better than I could have done it myself, by the *Washington Post* in an editorial appearing in the issue of October 7, under the head "Jury Tapping." The *Post* said:

A jury imperatively needs to carry on its deliberations in private. When it retires to consider the evidence and arguments in a case which has been argued before it, its members must be free from any outside pressure or fear of reprisal. They must be free also to discuss the case with full confidence that what they say will not go beyond the walls of the jury

room. Any impairment of this privacy not only destroys the detachment with which they ought to deliberate, but effectually deprives the litigants of their right to a fair trial. Uninhibited discussion becomes very difficult if there is fear of a concealed microphone.

It is significant, I think, that Attorney General Brownell has declared that—

We in the Justice Department are unequivocally opposed to any recording or eavesdropping on the deliberations of the jury under any condition, regardless of purpose.

\* \* \*

a.

**Testimony of Edward H. Levi, Dean,  
University of Chicago Law School—  
October 12, 1955**

MR. SOURWINE, chief counsel: Would you give the reporter your full name, Dr. Levi?

MR. LEVI: My name is Edward H. Levi. I am professor of law and dean of the law school at the University of Chicago.

Senator, I have a statement which I think might be helpful to the committee, which I would like the privilege of making.

THE CHAIRMAN: We will put it in the record. We will put it in the record at the conclusion of your testimony.

MR. LEVI: Well—

THE CHAIRMAN: Mr. Sourwine, proceed, sir.

MR. SOURWINE: How many years have you been at the University of Chicago?

MR. LEVI: I have been at the University of Chicago on the faculty since 1936.

MR. SOURWINE: And how long have you been dean of the law school?

MR. LEVI: Since 1950.

\* \* \*

MR. SOURWINE: Dean Levi, do you have any connection with the Ford Foundation?

MR. LEVI: I have no connection with the Ford Foundation other than that I am administering, that is, I am conducting a research program for which they gave the funds.

MR. SOURWINE: What was that grant from the Ford Foundation?

MR. LEVI: It was a grant of \$400,000 given for research in the area of law and social sciences.

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MR. SOURWINE: What is the nature of the project or projects under that grant?

MR. LEVI: There are three projects under that grant. One relates to the jury system. . . .

\* \* \*

MR. SOURWINE: Sir, what was it hoped to learn through this project with respect to juries?

MR. LEVI: It was hoped to learn how juries operate, to what extent they understand the instructions of the judges, to what extent they are able to handle difficult problems of evidence, to what extent it might be possible to speed up the jury system, so as to avoid the clogging of the courts in the urban centers and at the same time to preserve the jury system.

THE CHAIRMAN: Now, would not the proper way to handle that, Doctor, be to confer with the jury after the verdict is rendered?

MR. LEVI: We have done so.

THE CHAIRMAN: Before it had been discharged?

MR. LEVI: We have done so. One part—

THE CHAIRMAN: Answer my question.

MR. LEVI: I am sorry, sir.

THE CHAIRMAN: Is that not the proper way to handle it?

MR. LEVI: I think that is one proper way to handle it.

THE CHAIRMAN: That is the proper way to handle it; is it not?

MR. LEVI: Senator, if what you are asking me, is, is that the only proper way, I think my answer is No, not in my judgment.

THE CHAIRMAN: Now, do you not realize that to snoop on a jury, and to record what they say, does violence to every reason for which we have secret deliberations of a jury?

MR. LEVI: Senator, on that I think that reasonable men differ. I do not.

THE CHAIRMAN: You do not think so?

MR. LEVI: I do not.

THE CHAIRMAN: You do not think that a member of a jury would frankly express his opinion? Would he hesitate to frankly express his opinion if he thought there might be a microphone hidden, taking down what they said?

MR. LEVI: I think that if he were conscious of the fact that there was a microphone, while he was conscious of it, this might result in some inhibitions.

THE CHAIRMAN: Yes. But if those things secretly happen and then it is released and the records are played before people, how is he going to know whether in that particular case there is a microphone there or not?

MR. LEVI: I agree with what you are say-

ing, Senator. In this instance—in these instances—the records are not to be played publicly. There was to be no public discussion of it.

THE CHAIRMAN: Were they?

MR. LEVI: They were played, as I understand it—I was not there—at the conference of the Tenth Judicial Circuit by the judges. Our understanding was that this was a session at which the judges were using these recordings in order to determine whether their instructions are understood by the jury. I was told that Judge Phillips wished to have these recordings released to the judges for that purpose.

THE CHAIRMAN: Well, then, they were played before groups of people, were they not?

MR. LEVI: Senator, the answer is "Yes," they were, but not by us.

THE CHAIRMAN: Well, now, what is the reason for secret deliberations of a jury?

MR. LEVI: The reason for secret deliberations of a jury is so that the jury shall not be disturbed in its discussions, so that the discussions can be orderly and that they can state their opinions. But, Senator—

THE CHAIRMAN: Now, that is it; so that they can frankly state their opinions. That is the reason, is it not?

MR. LEVI: I believe that is right, Senator. But—

THE CHAIRMAN: Certainly. Now, that is a principal reason, is it not?

MR. LEVI: It is certainly one of the major reasons.

THE CHAIRMAN: Yes. Now, you violated that, did you not?

MR. LEVI: Senator, I beg to differ. Our recording of these deliberations was under an arrangement with the consent of the trial judge and the chief judge of the circuit—

THE CHAIRMAN: Regardless, now, of who agreed to it; regardless of who agreed to it, you still violated it, did you not?

MR. LEVI: Senator, I was trying to answer that precise point.

THE CHAIRMAN: Well, I think you go off on a question of attempting to justify it by saying—

MR. LEVI: No, sir.

THE CHAIRMAN: By saying that a judge agreed to it.

MR. LEVI: No sir. I was about to—

THE CHAIRMAN: The fact is that you violated the very reason that we have secret deliberations by juries.

MR. LEVI: What I was about to say, Sena-

tor, was that this was done under an arrangement which provided that there should be no publicity; that the recording, when transcribed, should be changed so as to remove all identifying statements, and that there shall be no release—

THE CHAIRMAN: No publicity. And practically everyone in the United States who reads the newspapers knows about this.

MR. LEVI: Yes, sir. That is what happened.

SENATOR JENNER: Who had custody of the transcriptions after they were made?

MR. LEVI: The court had custody of the transcriptions, and we had custody from the court.

THE CHAIRMAN: You had custody then?

SENATOR JENNER: Who had physical possession of the transcriptions?

MR. LEVI: I answered as I did because there was a period of time when we had custody, physical custody, and a period of time when the court had physical custody.

SENATOR JENNER: But you gave up physical custody so that they could be played at a public hearing in Estes Park, Colo.; is that right?

MR. LEVI: Not quite, sir. We gave up physical custody at the request of Judge Phillips for their playing at what we understood to be a closed session of the judges of the 10th circuit.

THE CHAIRMAN: Now, describe and explain just exactly what you did to record the deliberations of the jury in Wichita?

MR. LEVI: Senator, in order to answer that question, I have to say that the arrangements for the recordings, as I have indicated before, were set up in Wichita.

THE CHAIRMAN: By whom?

MR. LEVI: By Mr. Paul Kitch, who worked, as I understand it, with Judge Hill and obtained the consent of Judge Phillips. I was not and have never been in Wichita, but I was notified that these arrangements had been made, and after I was notified that these arrangements had been made, I sent Mr. Mikva to Wichita to carry out the arrangements with the instructions that he was to work under the direction of the trial judge.

\* \* \*

MR. SOURWINE: Could you state succinctly for the record, Dean, what you hope to prove by this project?

MR. LEVI: We hope to prove, and I believe we will be able to prove, that the jury is an efficient method of deciding cases, despite the clog-

ging of the dockets, that it can be strengthened and preserved, and that the doubts that have arisen about the jury system in various parts of this country and in the world, as for example, in England, where the jury system has been much limited, are not well established.

We wish to strengthen the jury system by this project.

\* \* \*

THE CHAIRMAN: Now, did you ever discuss this in any way with the representatives of the Ford Foundation?

MR. LEVI: The representatives of the Ford Foundation? Yes, I did discuss this with the representatives of the Ford Foundation after the recordings had been made.

THE CHAIRMAN: Yes, sir. You did not before?

MR. LEVI: No, sir.

\* \* \*

MR. SOURWINE: Have additional funds been requested?

MR. LEVI: Additional funds have been requested and they have been obtained. The Ford Foundation, although it has not been announced, has made a grant of \$1 million, made it in August, for the continuation of the general program.

MR. SOURWINE: And that was made in August of this year?

MR. LEVI: That is right, sir.

MR. SOURWINE: Did the Ford Foundation at that time have knowledge of the fact that this project included the recording of deliberations in jury rooms?

MR. LEVI: Yes, sir.

MR. SOURWINE: It is anticipated that a report will be made of this project to the Ford Foundation?

MR. LEVI: Well, we made reports to the Ford Foundation, yes, sir.

\* \* \*

MR. SOURWINE: Do you contemplate that partial disclosure of the results of this study will be permitted at any time?

MR. LEVI: You mean, of the Wichita operation?

MR. SOURWINE: Yes.

MR. LEVI: That has not been decided. The arrangement was—and I hope I may be permitted to put this in, because I want to answer it explicitly—the arrangement was that the trans-

cripts were to be so changed that there would be no identifying statements, but we have not decided whether, even though that has been done—and it has been done—this material could be disclosed, that is, in its changed form. We have never made that determination.

MR. SOURWINE: Had that editing been done, either wholly or partially, prior to the time that the recordings or portions of them were played back at Estes Park, Colo.?

MR. LEVI: I believe so.

MR. SOURWINE: Who made the selection with respect to the portion or portions to be played at Estes Park?

MR. LEVI: I do not know the answer to that question.

MR. SOURWINE: You had nothing to do with that?

MR. LEVI: No, I had nothing to do with it, and the project had nothing to do with it. We were put in a position where we were told that the chief judge of the circuit wanted the recording back, and that presumably the court had jurisdiction, and we were returning the tapes for that purpose. It was not our plan.

\* \* \*

MR. SOURWINE: Do you know, sir, in how many cases the proceedings of juries or the deliberations of juries have been recorded?

MR. LEVI: You mean by us?

MR. SOURWINE: By you or anybody else, if you know of any other—

MR. LEVI: I know of no recordings by anyone else, and I am told that the Wichita operation resulted in the recording of 5 or 6 cases. I would say 6 cases, although I am told that 1 of the cases had 2 parts to it, and that makes the sixth.

\* \* \*

MR. SOURWINE: All right. Was there to your knowledge any agreement that the transcription or recording of the jury's deliberations would be sealed up until all time for appeal in any individual case had passed?

MR. LEVI: I think that Mr. Kitch's letter has a statement to that effect.

MR. SOURWINE: Doesn't that statement necessarily imply the belief that what was done would have constituted ground for appeal or exception, if not concealed until the time for appeal had passed?

MR. LEVI: I should not think so, but—

MR. SOURWINE: Then why was it stipulated that it would be concealed?

MR. LEVI: I do not know. That is Mr. Kitch's letter. I would say that you were raising a legal question as to whether the consent of counsel would not operate, so that no matter whether they were looked at before or not, it would make no difference. I think the point was rather a different one, namely, that the trial judge should not be in a position of having heard the deliberations at the time when he might still be passing on motions which might relate to the case. This is my understanding.

\* \* \*

#### NOTE

STATEMENT BY EDWARD H. LEVI, DEAN OF  
THE LAW SCHOOL OF THE UNIVERSITY OF  
CHICAGO—OCTOBER 12, 1955

I am happy to appear before this committee to discuss some of the aspects of the jury project now being conducted by the University of Chicago Law School and in particular that segment of the project which has recorded a limited number of actual jury deliberations.

Three points should be made. The first of these is this: A number of distinguished and able leaders of the bar believe a study of the jury system can make a substantial contribution to the administration of justice, and many of these leaders believe that such a study may properly use recordings of actual jury deliberations. The second point is that the recordings of actual deliberations were carried out under safeguards to preserve the integrity of the jury system. The third point is that the jury system is a suitable and important subject for basic study, and the study as carried out should contribute to the strengthening—and not the weakening—of this important American institution.

The leaders of the American bar who have expressed their approval of the study of the jury system include leaders in the field of legal education and research, and judges and lawyers whose distinguished positions testify to their knowledge and understanding of the need for basic research in order to improve the administration of justice.

The president, the president-elect, and five of the living past presidents since 1947 of the Association of American Law Schools have authorized the issuance of a statement in which they declare that they see great possible gain to our knowledge, to our teaching of law, to our

legal system and to our machinery of justice in a study of the jury system which includes, under safeguards, the recording of actual jury deliberations. The statement is signed by Edwin D. Dickinson, of the University of Pennsylvania; Karl N. Llewellyn, of the University of Chicago; F. D. G. Ribble, dean of the University of Virginia; Robert E. Matthews, of Ohio University; Wesley N. Sturges, of Yale University; Maurice T. van Hecke, of the University of North Carolina, and Charles B. Nutting.

The statement of the president, president-elect, and former presidents of the Association of American Law Schools is as follows:

In our opinion, a much more accurate knowledge of the actual workings of our jury system has long been needed. Such knowledge ought not only to make clearer the solid values of that system but also to suggest measures for making the system work more smoothly and effectively to achieve those values. Research into the course of actual jury deliberations is key material and the most fruitful material to further these ends. Certainly if such research is done only under supervision of the court, and if it is done only by consent of counsel on both sides, and if it is done only under the seal of professional secrecy, and if, furthermore, not even excerpts are in any event to be published without the careful and tested cutting out of each name, date, locality, or other identifying mark of any sort at all, we then see, in the recording of a body of actual jury deliberations, not only no harm or danger, but on the contrary great possible gain to our knowledge, to our teaching of law, to our legal system and to our machinery of justice.

Five judges of the Illinois Appellate Court have authorized a statement expressing their confidence that the results of the jury study should contribute to the administration of justice. Judges Hugo Friend, John G. Lewe, Roger Kiley, John V. McCormick and Ulysses S. Schwartz have authorized the issuance of this statement:

We have long been aware and have in fact worked with the jury study now being conducted by the University of Chicago Law School. We believe this to be a most important study which should have the support of both the bench and bar. In our judgment, the study is being ably conducted by able people. It deals with a most important American institution and the results of the study should contribute to the administration of justice.

\* \* \*

Five former presidents of the National Conference of Commissioners on Uniform State Laws and the present chairman of the executive



committee have sent messages approving of the jury study and the recording under safeguards of actual jury deliberations. These are men who have had wide experience and high position in the organized bar and who know particularly well the relationship between basic research and improvements in the administrations of justice. . . .

These statements are as follows:

HOWARD L. BARKDULL: Recording of actual jury deliberations under careful supervision of court and with consent of counsel constitutes a useful research tool in improving administration of justice. Successful operation of American courts depends on effectiveness of jury system and requires constant study for means of correcting all points of weakness or abuse. Benefits of recording greatly outweigh objections.

JAMES C. DEZENDORF: If our system of government is to be maintained and preserved we must conduct research on all phases of the judicial process. Research concerning the functioning of the jury cannot be conducted intelligently unless actual jury deliberations can be studied and reviewed.

JOE C. BARRETT: Information relating to process of jury deliberations would be of great help to trial lawyers and judges in pointing up areas wherein improvements would be made in presentation of evidence and in the court's instructions on rules of law governing the case. Recordings of actual jury deliberations should play a significant part in such a study. I am impressed with this study as a means of pointing the way for substantial improvement in the administration of justice. To be of maximum value the study should have a base sufficiently broad to demonstrate whether there is any pattern of weakness that needs correction.

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b.

**Testimony of Harry Kalven, Jr., Professor  
of Law, University of Chicago—  
October 12, 1955**

\* \* \*

MR. SOURWINE: Do you know anything of a condition that, in the case where the juries were deliberating, that the names of the participants in those cases were not to be disclosed?

MR. KALVEN: Yes, sir; I understand that is a very important condition they have that they not be disclosed, which would be our wish not to disclose those.

MR. SOURWINE: Who did make that condition? Did you make it, or was it made by a judge?

MR. KALVEN: I have no direct information on that. I imagine that idea would occur very quickly to both sides; that that would be quite appropriate.

MR. SOURWINE: Has that condition been adhered to?

MR. KALVEN: Very strictly, sir.

\* \* \*

MR. SOURWINE: Do you know whether the attorneys for the parties of record in these cases gave their consent to the bugging of the jury room?

MR. KALVEN: Again, sir, I am repeating somewhat second-handed knowledge, but it was my understanding in each case they did, and they gave their consent to the judge.

MR. SOURWINE: Do you know who those various attorneys were?

MR. KALVEN: I do not of my own knowledge. Again that would seem to be an item going to identifying the cases.

MR. SOURWINE: Would you think that if you gave those names you would be identifying the cases?

MR. KALVEN: I would think so, sir.

\* \* \*

MR. SOURWINE: [C]an you tell us briefly what you sought to learn through this project?

MR. KALVEN: Well, as I say, the answer to that is along this line. As we were making a study of the jury as such, we have our lines of inquiry, including the experimental jury routine Mr. Levi mentioned this morning, including the interviewing of jurors after trial, and getting an impression of what the trial was like; what their impressions were like, and so forth. And the chief need that we felt that we had—and we think we have now—is to have some final way of corroborating the reality, both of the experimental work and completeness and reality of the interview after trial.

The point, sir, was that a limited number of real recordings kept with all proper precautions would give us in a sense a final test, as I say, of the realism of what we are doing.

MR. SOURWINE: You were actually primarily concerned with testing the realism of these moot juries that you had set up?

MR. KALVEN: I think that is the easiest way to put it.



MR. SOURWINE: Whose deliberations you had recorded?

MR. KALVEN: That is right; you can recognize that a device like this quickly meets the objection that it looks interesting, but do real juries act like that.

MR. SOURWINE: Did the moot jury cases contain the same components of the case which were actual, and which you recorded?

MR. KALVEN: To some degree; not in fact as much as I wished they had, just as a matter of method, but they contain the same kinds of problems.

MR. SOURWINE: How did you accomplish that? Were you able to select cases which did contain those problems, or did you construct your moot cases, so as to match?

MR. KALVEN: We constructed our moot cases to—the matching is not very close, sir. I would rather not come too close to the content of the actual cases, but I think you can anticipate it would not be hard to have some common issues on the civil side of jury cases.

\* \* \*

MR. SOURWINE: Do you know of the recording of the deliberations of juries in any courts, except in these 5 or 6 cases in Wichita?

MR. KALVEN: No, sir; I do not.

MR. SOURWINE: Were these five recordings in the Federal court in Wichita the only recordings of jury proceedings which were planned as a part of your project?

MR. KALVEN: Yes, sir. This is the ambiguity about whether it is 5 or 6 Mr. Levi mentioned this morning.

MR. SOURWINE: You had no plan at any time to record a larger number?

MR. KALVEN: Well, there is the original correspondence, sir, from which Mr. Kitch—perhaps Mr. Kitch can talk to that point later—in which he proposed the matter to Judge Phillips; he mentioned a large number of cases, but it was a much larger number, I think, sir, than anyone on our side had committed themselves of doing; and it was at that time, with the understanding that the juries would be told about it, and Judge Phillips had no objection to that.

MR. SOURWINE: Whose side was Kitch on?

MR. KALVEN: I think on his own side, sir. He was interested in this as an independent lawyer.

MR. SOURWINE: Was he ever on the payroll of the University of Chicago?

MR. KALVEN: No, sir, not to my knowledge.

MR. SOURWINE: Ever on the payroll of this project?

MR. KALVEN: No, sir.

MR. SOURWINE: Did he receive any compensation for the work he did, so far as you know?

MR. KALVEN: Just our thanks, I think, sir.

\* \* \*

THE CHAIRMAN: Mr. Kalven, do you believe in the American jury system?

MR. KALVEN: I do, very much, sir. That is the reason why I am interested in the study.

THE CHAIRMAN: Yes, sir. Do you believe that juries should deliberate in secret?

MR. KALVEN: I think on the balance, it is better, sir, if they deliberate with an assurance of privacy.

THE CHAIRMAN: Do you believe that the American jury system is one of the greatest safeguards of human liberty?

MR. KALVEN: I think I would say, "Yes, sir," to that.

\* \* \*

THE CHAIRMAN: What would happen to that great safeguard if a jury's deliberations were "bugged"?

MR. KALVEN: Well sir, I think it makes a good deal of difference who does the bugging, under what circumstances. I agree that if all juries were conscious of the fact that their deliberations might be recorded, if you will permit the phrase, by anyone whomsoever and for any purpose—

THE CHAIRMAN: It would affect them?

MR. KALVEN: I think that is right. I think that is almost irrelevant.

THE CHAIRMAN: And yet you say, for a jury, if they were conscious of the fact—well, do you not know that if it is done in some cases, that every juror is going to think, well, maybe there is a microphone in this room?

MR. KALVEN: I do not think so, sir. May I make one statement?

THE CHAIRMAN: You do not?

MR. KALVEN: The statement is this—

THE CHAIRMAN: Answer my question.

MR. KALVEN: The answer is, no, sir.

THE CHAIRMAN: You do not think that?

MR. KALVEN: No, sir. May I amplify my answer?

THE CHAIRMAN: Certainly.

MR. KALVEN: I think the only two points to be made here are these: The first point, it

was not contemplated it would be having the degree of publicity there presently is about this. As Mr. Levi said this morning, it was not clear what final disposition would be made of these.

THE CHAIRMAN: Do you not realize that a procedure, so at variance with the American system of government, is bound to be widely known?

MR. KALVEN: I do not think so, sir. This procedure—this was done 15 months ago—it seems to me it has been kept a pretty good secret until recently.

THE CHAIRMAN: Everybody in the country knows it now, do they not?

MR. KALVEN: Not through our efforts.

THE CHAIRMAN: Sir?

MR. KALVEN: Not through our efforts.

THE CHAIRMAN: Regardless of whose efforts, it is known, it is not?

MR. KALVEN: In any event, I would like to get to my second point, which, I think, is more relevant. What is now known is that, with the consent of the attorneys and with the consent of the judge, and for scientific purposes, a few juries may from time to time be recorded. I see no reason why that should strike any fear in the heart of any juror in America. And that seems to me, at the maximum, the threat that has been created thus far.

THE CHAIRMAN: What is the seventh amendment to the Constitution of the United States?

MR. KALVEN: It is the amendment providing for jury trial in civil cases.

THE CHAIRMAN: Certainly.

MR. KALVEN: In cases involving the Federal Government.

THE CHAIRMAN: Certainly. That means secret deliberations?

MR. KALVEN: I am not completely sure about that, sir.

THE CHAIRMAN: You are not?

MR. KALVEN: Not as a matter of technical constitutional law. As I understand it—and I do not profess to be an expert on this point—it was rather unclear at the time the Constitution was adopted just how much jury secrecy there was. And as I understand the constitutional test for the meaning of the seventh amendment, it goes back to the institution as of the time the amendment was adopted. I am not making that as a firm proposition of law, sir. It is just my impression on that point.

THE CHAIRMAN: Let me ask you this question—have you finished your answer?

MR. KALVEN: Yes, sir.

THE CHAIRMAN: Did you write a letter to President Truman, asking clemency for the atomic spies, Rosenbergs?

MR. KALVEN: I did, sir. I have the letter with me.

THE CHAIRMAN: You say you have the letter with you?

MR. KALVEN: Yes, sir.

THE CHAIRMAN: Give us a copy of the letter.

MR. KALVEN: I would be pleased to read the letter.

\* \* \*

THE CHAIRMAN: Sir?

MR. KALVEN: I wrote this letter as a private citizen to the president. No copies of this letter have been given out. This copy is from my file.

THE CHAIRMAN: Do you not know that the *Daily Worker* knew about it?

MR. KALVEN: The only way I know is that Fulton Lewis, Jr., had it on his broadcast the other night. I have no idea how the *Daily Worker* knew about it.

THE CHAIRMAN: Have you read the *Daily Worker*?

MR. KALVEN: No, sir; I have never read the *Daily Worker*.

\* \* \*

SENATOR JENNER: Going back to the seventh amendment to the Constitution, Mr. Kalven, you expressed an opinion, I believe, to the chairman of the committee on that a while ago.

I want to ask you: Is there any question in your mind that the right of jury trial guaranteed by the seventh amendment encompasses all of the attributes of a jury trial as it was known at the time that amendment was written?

\* \* \*

MR. KALVEN: I agree with you; that is the legal test of the meaning.

SENATOR JENNER: Then that being true, did not this include the absolute freedom of deliberation of the jury?

MR. KALVEN: Well, sir, I am not an expert on the law on that, as I was suggesting before. The law, so far as I know—and I hesitate to offer a judgment on the question like this—but so far as I know until immediately prior to that date—that is, the date of the adoption of the

amendment—the law had been the other way. It appears to have been changed within a few years of that time.

I would gather that, if there was a question raised seriously today, there would be a real doubt at common law, that the jury had secrecy of deliberations. I do not want to make a point of that. I think it is agreed, since the amendment was adopted, it has been the spirit and principle of the amendment that jury deliberations are basically secret, with the qualification, sir, that you know this morning that, of course, it is permissible, if not—well, let me say it is permissible and certainly customary practice almost everywhere for the lawyers and newspapers to talk to jurors after cases, in that sense to find out what happened during the deliberations, and in that sense equally to upset the jurors' serenity of mind, and so forth, in terms of disclosure of the content of the deliberations being made after the deliberation is over.

SENATOR JENNER: After the deliberations have reached the final conclusion, but you know of no case except the case that you cited out in Wichita where a jury's free deliberation has been fettered by bugging a room?

MR. KALVEN: Sir, I want to again say I think this is an important distinction, and I think it is not being fully appreciated here, that the jury's free deliberation was not being fettered in that case, because the jury did not know about it. And in future cases, the jury's free deliberation is being fettered only in the sense that the juror would be concerned because again some judge, a judge who is primarily the custodian of the jury, would decide that it would be appropriate, with the consent of counsel, and for impartial scientific purposes, to again permit a recording to be made.

SENATOR JENNER: Well, since this has been so widely publicized—

MR. KALVEN: Well, sir—

SENATOR JENNER (*continuing*): What effect do you think it will have on the effect of free deliberation of the jury in the future?

MR. KALVEN: I think that is relatively easy to answer.

If it was correctly publicized, I think it would have virtually no effect.

SENATOR JENNER: Go ahead.

THE CHAIRMAN: What effect would bugging 500 or 1,000 jury deliberations have?

MR. KALVEN: I think it—sir, I will agree that if again the auspices are different—if the judge does not consent—I think this is a quite

improper practice, and I should think there was no doubt about it.

THE CHAIRMAN: Whether the judge consented or not, the effect on the jury would be the same, would it not?

MR. KALVEN: I should think it would not be. The judge is, as I say—the jury is regarded as an appendage of the court. It seems to me whether or not the judge consents, makes the enormous and crucial difference here.

THE CHAIRMAN: Do you not know, whether the judge consents or not, the pressure against free deliberation would be on that juror, would it not?

\* \* \*

MR. KALVEN: Sir, it depends entirely on whether the court has consented for impartial, scientific purposes, or not.

THE CHAIRMAN: Is it not true—is it not true that you have planned to bug the deliberations of juries in 500 to 1,000 cases?

MR. KALVEN: No, sir; it is clearly not. I understand—

THE CHAIRMAN: That is not true?

MR. KALVEN: That is not true, sir. Let me explain the answer. There is this proposal. Mr. Kitch can talk to this, as I say. Mr. Kitch, I think, will indicate more ambition about this in some ways than we had, but the proposal was that—stating a number as large as 500, with the condition that the jury know about it. At no time, when that condition was removed, was it ever contemplated to do more than a handful of cases.

THE CHAIRMAN: No more than a handful of cases?

MR. KALVEN: That is right, sir; and to do them for the sake of evaluating our other observation work on the jury.

THE CHAIRMAN: I am going to read to you from a speech made today in St. Paul, Minn., by the Assistant Attorney General, Warren E. Burger. I want you to state then whether his statements are true or false:

A Justice Department official charged today that the University of Chicago Law School plans to eavesdrop on 500 to a thousand juries during its research project into the American jury system. The planting of the microphone in Wichita, Kans., jury room was only the first step in a study of "very sweeping proposals."

MR. KALVEN: Sir, I would say that the statement is truly false. The Department of Justice has good reason to know what the facts are.

SENATOR JENNER: May I ask a question, Mr. Chairman?

THE CHAIRMAN: Yes, sir.

SENATOR JENNER: Why is your study only concerned with civil procedure? Are you not interested in a scientific study of criminal procedure?

MR. KALVEN: We are studying criminal cases, too.

SENATOR JENNER: Why did you only bug civil trials; why did you not bug some criminal trials?

MR. KALVEN: I will confess, sir, because of a generally recognized difference between criminal and civil cases; and because of the generally recognized importance of the jury on the criminal side we, perhaps, had some additional hesitation about that.

SENATOR JENNER: Then your scientific study would be lopsided, in any event; would it not? You would not know what a jury was thinking on a criminal trial, but you would know everything connected with the civil trial—would that not be one-sided—this is to be an objective study, was it not; you are supposed to bring in all juries, both criminal and civil.

MR. KALVEN: That is right, sir. And we had not decided definitely the question raised about whether it would be appropriate to do it in a criminal case. Personally, I would think so.

\* \* \*

SENATOR JENNER: Do you plan to go on with that in criminal procedures?

MR. KALVEN: We have had no immediate plans for some time now to go ahead. We are digesting the material that we have. I would not say that we are foreclosing the possibility of doing additional work like this, if the opportunity, with the consent of the court, with appropriate safeguards, arises.

SENATOR JENNER: In other words, then, everybody is on notice in this country that, sometime in the future, a criminal jury may be bugged for the scientific study of the research fund granted to the University of Chicago; am I to understand that?

MR. KALVEN: Sir, again they are on notice that if a court deems it appropriate, and if we deem it worthwhile, from the point of view—from our research at that time, that the possibility is considered.

SENATOR JENNER: That is all I want to know. Go ahead.

MR. SOURWINE: Will you clarify your tes-

timony of a moment ago. Did I hear you correctly? I thought I heard you state that, at no time after the condition that juries should be advised was withdrawn, was there any consideration of eavesdropping on more than a very few, a handful, of cases; is that right?

MR. KALVEN: That is my understanding, sir.

\* \* \*

I am not sure that everyone on the project sees exactly as I do on this, since I am making two statements. One is that in terms of our actual plans there were no plans to do this again in the immediate future. Secondly, my own point of view about this, which I am not sure is, as I say, completely a unanimous one, is that I would be in favor of doing this in a very limited additional number of cases.

MR. SOURWINE: But you are testifying now that, after the Wichita cases, there might have been consideration to doing it in some other cases?

MR. KALVEN: That is right, sir. And that remains true today.

\* \* \*

THE CHAIRMAN: That is over the next year, you say?

MR. KALVEN: No. Over the next year, sir, if our plans had gone as projected, I think it is very doubtful we would have done any more cases. There were no concrete plans for doing any.

THE CHAIRMAN: I will guarantee you that you will not do any bugging after Congress passes some legislation.

MR. KALVEN: I admit, sir, the situation has changed somewhat.

THE CHAIRMAN: I will say it has.

MR. SOURWINE: Professor Kalven, if I heard you correctly, you used the phrase, "the generally recognized importance of the jury in criminal trials"?

MR. KALVEN: That is right.

MR. SOURWINE: Would you say the importance of the jury in civil cases is not generally recognized?

MR. KALVEN: No, sir. I would say that the importance of the jury in criminal cases is recognized more widely; that I really have an eye to the fact that in many areas of civil litigation, as you know, the jury is very frequently waived now by the parties. We have the phenomenon in England of having the jury almost disappear in

personal-injury litigation. And as I read the literature, it seems to me that while some people have been willing to say that the jury may not be the most effective institution on the civil side, I have never seen anyone make that statement on the criminal side, because I think its role is different.

I think, in that sense, it has a more important role on the criminal side, and that is the difference I was referring to.

\* \* \*

MR. SOURWINE: Mr. Kalven, where are the recordings that were made of the jury deliberations in Wichita; do you know?

MR. KALVEN: Not precisely, sir. To my knowledge, they are stored in two rooms in the basement of the law school. Mr. Strodtbeck is the direct custodian of this, and I think could testify more directly to that.

MR. SOURWINE: Do you know who had access to them?

MR. KALVEN: Only two people—Mr. Strodtbeck's staff—Mr. Strodtbeck himself and, of course, I would have, whenever I would want to see them. They are under lock and key, the recordings themselves. And I think Mr. Strodtbeck could describe a little more vividly, sir, than I can, the precise security measures that have always been taken with these materials.

MR. SOURWINE: Can you tell us when and where any of these recordings have been played?

MR. KALVEN: Sir, except for performances to ourselves, for our own purposes, the only occasion I know of is the Estes Park meeting of the judges of the Tenth Judicial Circuit.

MR. SOURWINE: Who do you mean, "to ourselves"?

MR. KALVEN: The immediate—not more than a group of 5 or 6 people.

MR. SOURWINE: Who are those persons?

MR. KALVEN: I think it would be fair to say anyone on the staff at this point might, if circumstances warranted, hear the materials. I do not know that they have. They have all had contact with them.

MR. SOURWINE: How many persons are on the staff at this moment?

MR. KALVEN: I think 13. I thought you might ask that.

MR. SOURWINE: You said 5 or 6 persons a moment ago. Did you mean different groups of 5 or 6 now, and 5 or 6 later?

MR. KALVEN: I mean, sir—no, sir; I mean on this count, sir, there are 10 people at the moment. The problem about counting them, sir, is

that we have a fair amount of part-time help that is at a relatively junior level, and it is a question whether you include those on the staff or not, but any of the senior members of the staff, I am sure, would have access to this material.

\* \* \*

MR. SOURWINE: Do you know, Mr. Kalven, about an agreement that transcripts of recordings of jury's deliberations would be sealed up until all time had elapsed for appeal from the individual case?

MR. KALVEN: I know, sir; there is some provision for that. Then, I am not familiar as of my own knowledge with that. Again, I think, Mr. Strodtbeck could talk—

MR. SOURWINE: Do you see in such a provision any implication that what was done would have constituted a ground for appeal or exception if it had not been sealed until the time of appeal had passed?

MR. KALVEN: I think, as Mr. Levi said, that since the counsel had consented to the whole arrangement, anyway, it would be a peculiar ground for appeal; that the judge, perhaps—I think, this is his own idea—perhaps was a little uneasy about his having access to this material until that had happened.

MR. SOURWINE: Have you stated, sir, publicly, that the United States attorney had agreed to the jury bugging in each instance?

MR. KALVEN: I did, sir, in the one public statement we have made, make a statement saying that all the parties to the case, I mean, that counsel for all parties to the case had agreed, including the United States attorney. I believed that to be true at the time, sir. I understand, sir, that might have been a mistake. The man actually in charge of the case was an assistant United States attorney, sir.

The intention of our statement was that the lawyer handling the Government's case had consented on behalf of the government.

MR. SOURWINE: You were quoted as having said that the thing had been cleared with the United States attorney, and I am just asking you whether you had said that.

MR. KALVEN: I think my statement is unfortunately a little ambiguous on that, sir, but I think the clear intention of it was that whoever was the lawyer for the government in the case involved had consented, and I understand that to be the fact.

MR. SOURWINE: Who was the lawyer for the government who consented?

MR. KALVEN: I am not sure I know the name, sir. I think it was Mr. Cowger.

\* \* \*

MR. SOURWINE: The Senator mentioned a moment ago, or you mentioned, Mr. Vishinsky. Have you ever read the *Law of the Soviet State* by Andre Vishinsky?

MR. KALVEN: No; I have never. . . .

MR. SOURWINE: Do you consider it an authoritative legal work?

MR. KALVEN: Not having read it, sir, I really do not have an opinion on it.

MR. SOURWINE: Do you agree with this quotation from Vishinsky's book, page—

MR. KALVEN: I doubt it.

MR. SOURWINE: (reading):

The classical form of the bourgeois court is the "court with jury" (a court with participation of sworn assessors) existing in the capitalist countries that preserve the bourgeois democratic forms of state order. In such a court the jurors (ordinarily 12) decide the guilt of the accused, and on the basis of that verdict, permanent judges (appointed by state authority) apply the law, and designate the punishment.

In its modern form, such a court was created by the bourgeoisie in consequence of its victory over feudalism and was progressive as compared with bureaucratic and caste courts of the noble landowner state. While bourgeois democracy flourished, such a court undoubtedly served as a bulwark of the political freedoms proclaimed by the bourgeoisie at the time of its triumph over the power of the feudal monarchy. But jurors are now, as they were formerly, the bulwark of that order of social relationships which rests on private capitalist property. The class character of such a court is an index as well of the class direction of the justice to which the jury gives effect.

Chosen chiefly from the circles of the middle and petty bourgeoisie, and predisposed by their own social position to see the buttressing of the existing social order as their function, jurors are captivated by the views even as to concrete matters, enunciated by the press, which is in the hands of the biggest capitalists.

MR. KALVEN: What was the question, now, sir?

MR. SOURWINE: The question is whether you agree with that statement or not?

MR. KALVEN: Insofar as I understand it, I do not agree with it.

MR. SOURWINE: You do know whether the Soviet courts have a jury system?

MR. KALVEN: I know absolutely nothing about the jury system in the Soviet courts.

MR. SOURWINE: Do you believe that the American jury system is superior to the Soviet court system?

MR. KALVEN: Yes; I certainly do, sir.

MR. SOURWINE: Do you agree with Engels as quoted by Vishinsky on page 507, that—"the English court of jurors, as the most developed, is the culmination of juridical falsehood and immorality"?

MR. KALVEN: No, sir.

MR. SOURWINE: Were you, Professor Kalven, ever a member of the committee to secure justice for the Rosenbergs?

MR. KALVEN: No, sir; I was not.

MR. SOURWINE: Were you a member of the Chicago Committee to Secure Justice for the Rosenbergs?

MR. KALVEN: No, sir; I was not.

MR. SOURWINE: Did you attend a meeting of that committee in November 1952?

MR. KALVEN: I am not sure, sir, whether one of those occasions might not have been sponsored in part by that. I would not say to that, categorically, no.

\* \* \*

MR. SOURWINE: . . . Are you familiar or have you read the case of *McDonald and United States Fidelity and Guaranty Company v. Pless*?

MR. KALVEN: I am familiar with the general ruling in that case, sir, although—

MR. SOURWINE: Did you know that the court said in that case:

If evidence thus secured could be thus used, the result would be to make what was intended to be a private deliberation (by the jury) the constant subject of public investigation—to the destruction of all frankness and freedom of discussion and conference (in the jury room).

For while it may often exclude the only possible evidence of misconduct, a change in the rule would open the door to the most pernicious arts and tampering with jurors. The practice would be replete with dangerous consequences. It would lead to the grossest fraud and abuse and no verdict would be safe.

MR. KALVEN: I think there are two points to make about that, sir. One is, that is not the unanimous rule in the United States on the point of whether a jury verdict may be impeached by testimony as to what went on in the deliberations.

The second point is, that it seems to be quite a different question than the one we are involved with here, because it involves the adversary use by the parties themselves of this material to

harass the jurors, to prolong the litigation, et cetera.

MR. SOURWINE: I only asked if you were familiar with that as a preface to asking you whether you agree with—

MR. KALVEN: I think if the question, sir, is whether the jury deliberations should be totally open to counsel thereafter, on any grounds whatsoever, to seek to impeach the verdict, I certainly agree with it, sir. It is a question whether, for example, quotient verdicts should be impeachable by testimony that this was the way in which the verdict was arrived at in the deliberations. I am not sure that I have made up my mind on that question. As you know, there are jurisdictions in which that is the rule.

MR. SOURWINE: It was the sense of the court in this particular case, was it not, that the inviolability of the proceedings in the jury room was so important that even, for the sake of helping to do justice to the litigants, a juror should not be permitted to testify as to what took place in the jury room?

MR. KALVEN: I think, sir, that the court also had in mind the point that has no relevance to our study, and that is that they were interested to putting an end to litigation, and the peculiar way we put the jury under surveillance, if the parties themselves could interrogate them as to what went on in the deliberations, and seek to make an adversary use of it.

The general policy that is indicated by the court in that sense, I agree with.

MR. SOURWINE: Are you familiar with the case of *Clark v. United States* (289 U.S.)?

MR. KALVEN: I have seen references to the case, sir. I am not directly familiar with it. I know it is another case in this general area.

MR. SOURWINE: In which it was held that—freedom of debate might be stifled and independence of thought checked if jurors are made to feel that their arguments and ballots were to be freely published to the world.

MR. KALVEN: Sir, is this not the opinion of Judge Cardozo?

MR. SOURWINE: Yes.

MR. KALVEN: To pay for one quotation with another, I understand that he goes on with an example, which I admit, of a possibility of evidence being the juror was bribed. This evidence was not available. This is paying too high for a juror for serenity of mind.

MR. SOURWINE: My question was whether you agree with the passage which I read to you.

MR. KALVEN: Yes, sir; in general we have

no dispute with you, sir, as to the importance of generally keeping the deliberations of juries confidential. We do not regard what we did as violating that principle.

MR. SOURWINE: I will ask you just about one more case:

Are you familiar with the case of *Remmer v. The United States* in title 347, United States Reports?

MR. KALVEN: No, sir.

MR. SOURWINE: In that case it was held that—a juror must feel free to exercise his functions without the FBI or anyone else looking over his shoulder. Would you agree with that?

MR. KALVEN: Yes, sir.

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c.

**Testimony of Logan Green, Esq.,  
Garden City, Kansas—October 12, 1955**

\* \* \*

MR. SOURWINE: Mr. Green, were you ever involved in a case in which you were asked to consent to the recording, unbeknownst to the jury, of deliberations in the jury room?

MR. GREEN: Yes, sir; I was.

MR. SOURWINE: How was that matter first broached to you?

MR. GREEN: Well, we were preparing to try a case in Federal court, in Wichita, and the court called us in chambers and informed us.

THE CHAIRMAN: What court was that?

MR. GREEN: That was Judge Delmas C. Hill.

MR. SOURWINE: When you say he called "us in chambers"—you mean he called you and opposing counsel about the matter, both?

MR. GREEN: He called counsel for both parties to the lawsuit; yes, and informed us that there was a team of some kind present from the University of Chicago, as I recall, who desired to place a recording machine in the jury room during their deliberations in that case, and he asked us if any of us objected to that procedure.

Do you want me to go ahead?

MR. SOURWINE: Did you both say that you did not object?

MR. GREEN: Counsel for both sides said that they had no objection.

MR. SOURWINE: Did you feel, sir, speaking just for yourself, that you could afford to object to a project which the judge told you he had already approved?



MR. GREEN: Well, frankly, I didn't give the matter too much thought right at that time. We were preparing to select a jury, and I had a lawsuit to try, and I didn't give it as much thought as I would now.

MR. SOURWINE: Have you given the matter thought since that occasion, sir?

MR. GREEN: Yes, sir; I have since the publicity has arisen.

MR. SOURWINE: If a similar proposal should be made to you now, would you consent to it?

MR. GREEN: I do not think I would.

MR. SOURWINE: Mr. Chairman, I will state that I have been in telephonic contact with a number of the attorneys who were counsel in cases of this nature in Wichita. They all tell substantially the same story. I have suggested that Mr. Green be subpoenaed, and I think that we can establish from the testimony of Mr. Kitch when he comes to the stand that this was the practice uniformly followed in each of the cases.

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d.

**Testimony of Fred L. Strodtbeck, Associate Professor of Sociology, Law School, University of Chicago—October 13, 1955**

MR. SOURWINE: Give your full name, your business or profession and your address.

MR. STRODTBECK: Fred L. Strodtbeck. I am now employed as an associate professor of sociology in the law school, in the department of sociology, of the University of Chicago.

MR. SOURWINE: How long have you been there employed?

MR. STRODTBECK: Since September 1953.

MR. SOURWINE: Mr. Strodtbeck, will you tell us what your connection has been with the project for investigation of the jury system which involved the recording of the deliberations of some juries in Wichita?

MR. STRODTBECK: I think I should first state that I [came] to the project with a primary responsibility for the experimental study of jury processes. Now related to the experimental study of jury processes was this corroboratory operation which involved the recording of an actual deliberation.

\* \* \*

As a part of our specialty in the study of small groups, we very, very frequently record many types of conferences and deliberations.

That is the objective of our work, to capture the ongoing interaction process between persons, and then study them.

In doing this we have come to learn that it is very difficult to identify individually the voices of a large number of persons when they do participate in the type of deliberation which characterizes a jury deliberation.

MR. SOURWINE: How did you solve that problem in the jury room in Wichita?

\* \* \*

MR. STRODTBECK: In the operation of making the transcription we, by piecing together the information we have, by listening to the way in which they refer to one another by name, identify the seat positions, and list them by numbers 1 through 12 around the table. These numbers are then given names, which are alphabetically discriminated.

The first man's name always begins with A, the last person's name begins with the twelfth letter from A.

In all of our subsequent transcriptions, our identifications of persons are carried out by these pseudonyms, and obviously we have in our files a key which would relate the pseudonyms to the interviewed protocols, so that the full set of information may be collated in our ultimate scientific study.

MR. SOURWINE: So that you do know and would be able to tell actually who the juror was who said a certain thing?

MR. STRODTBECK: Without question.

MR. SOURWINE: Do you see anything wrong in that?

MR. STRODTBECK: No.

MR. SOURWINE: Did you ever have anything to do with securing permission for the placing of these microphones in jury rooms?

MR. STRODTBECK: I definitely did not.

\* \* \*

I knew that permission had been received from Mr. Cowger. I did not know that his position was assistant in contrast to some other rank. I did know that he was a representative of the government.

MR. SOURWINE: How did you know that permission had been secured from him?

MR. STRODTBECK: I talked to him personally.

MR. SOURWINE: And what did you say to him?

MR. STRODTBECK: After the trial, we were



particularly interested to find out something of the nature of the problems confronting the government attorney, in a case of the sort that he had engaged in.

The full scientific utilization of this material required that we know something of the strategy for the various acts which are a part of the presentation of the government's case, in an action of this type. He was very helpful in discussing the difficulties of the work and the reasons for his objecting at certain times and not having objected at other times. This was all a part of our investigation.

MR. SOURWINE: Do you mean that you discussed with him the recording of the proceedings of several juries in several different cases?

MR. STRODTBECK: I did not discuss with him, at any time, the recordings of any of the deliberations which we had taken. I discussed with him his tactics in the particular case which we had observed, which we subsequently recorded the deliberations of, and which we subsequently interviewed the jurors who had participated in it.

Now, concerning the number of cases, some confusion arose in his testimony, and I believe that I can understand why it was. There were 5 actions involved in the two cases which occurred simultaneously. The 2 cases, in order to simplify the task for the jury, were broken in 2 sets, such that 3 actions occurred in 1 of the cases, and 2 actions in a subsequent case.

Mr. Cowger was very helpful working with Judge Hill to incorporate an experiment of this sort into the two sets of cases.

In one instance, the instructions were sent into the deliberation room, and in the other instance they were not. And, although I have not processed the materials in my own files, I believe that, in one instance, instructions concerning the quotient verdict were given, whereas in other instances the instructions concerning the quotient verdict were not given.

And I believe that this instruction is one which may or may not be given in such cases, in the Kansas jurisdiction.

\* \* \*

MR. SOURWINE: These microphones were concealed, were they not?

\* \* \*

MR. STRODTBECK: I understand, in keeping with our desire simply to not identify the cases, that it is possible for us to describe that the

microphones were placed on the walls and were disguised as a part of the heating apparatus in the room.

MR. SOURWINE: Where were your wires run to your recorder?

MR. STRODTBECK: The deliberation room is immediately above the judge's chamber. The wires were run through the walls into a cloak closet off of the judge's chamber.

MR. SOURWINE: While these proceedings were being recorded, was the recorder attended?

MR. STRODTBECK: No. As soon as a deliberation was begun, we had our materials arranged in such a way that the recorder would run for a long period of time. The judge would admit perhaps one of our representatives to his chambers. They would unlock the closet in which the machine was located. The machine would be started, and then the chambers would be vacated. The person would go upstairs, and the deliberation would begin.

MR. SOURWINE: Then, nobody heard these deliberations?

MR. STRODTBECK: Not at the time they were being recorded, there was no monitoring whatsoever.

\* \* \*

MR. SOURWINE: . . . I want to find out if there was any general procedure for editing either the original or the duplicate.

MR. STRODTBECK: No.

MR. SOURWINE: Have all the originals been preserved intact?

MR. STRODTBECK: Yes.

MR. SOURWINE: Have all of the so-called "duplicates" been preserved intact?

MR. STRODTBECK: Oh, yes.

MR. SOURWINE: Did you in some instances make further rerecordings from the duplicates?

MR. STRODTBECK: Yes.

MR. SOURWINE: In how many instances was that done?

MR. STRODTBECK: Just one.

\* \* \*

MR. SOURWINE: . . . Will you tell us what security provisions you have been applying to the recordings, the duplicate recordings, the second duplicate recordings played at Estes Park, and the transcriptions and protocols that you have told us about?

MR. STRODTBECK: Yes; I believe I can outline the system in general.

I have treated these materials which come

from our interviews and from the recordings, as if they were confidential materials. By that I mean that whenever they are withdrawn from our files, care is taken to see that the person who withdraws the materials has a limited and specific use to make. The materials which carry identification of places are always separated from those materials which have been masked for the purposes of our subsequent study.

In all instances, our materials are kept under lock, and under conditions of surveillance, which would insure that they would not circulate.

There is one exception to this, and this was a part of the presentation of the Estes Park material. In order to have as realistic as possible a background for the appreciation of the deliberation, the masked protocols of this particular case were made available to Judge Hill who, in turn, permitted them to be read by the two lawyers who made the facts statement prior to the presentation of the deliberation.

MR. SOURWINE: They were lawyers who had had nothing to do previously with the case?

MR. STRODTBECK: That is right—nothing whatsoever.

MR. SOURWINE: And were the protocols shown to them—the protocols which had been edited some?

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MR. STRODTBECK: Exactly.

MR. SOURWINE: So that you could not identify the case?

MR. STRODTBECK: Exactly.

\* \* \*

MR. SOURWINE: Are the recordings kept in a safe?

MR. STRODTBECK: No; they are kept in a locked file.

MR. SOURWINE: In your office?

MR. STRODTBECK: Not in my office, but in offices which are a part of my operation.

THE CHAIRMAN: Why did you use the name "Mr. X?"

MR. STRODTBECK: At the Estes Park presentation, and this question is very important, sir, we did not identify the university. We did not identify the locale in which the recordings had been made.

And, in view of the fact that I was completely unknown to members attending the judicial conference, I could appear there, could make this presentation without in any way com-

promising the security that we have thrown around this entire operation.

THE CHAIRMAN: Why did you use the name "Mr. X?"

MR. STRODTBECK: I did not use the name "Mr. X." Judge Hill, in introducing me, indicated that there was a representative of the project here. And he, jokingly, as an aside, said, "Perhaps we will call him Mr. X," and that name was given to me. I did not use it. . . . The importance of maintaining the security of the location of these materials is an absolute essential for their scientific use. We do not want these materials at any time to become a part of adversary differences between the participants, and the notion of maintaining this security is a part of just standard scientific caution which would be made in using materials of this sort. It would be the same if there were confidential materials from medical records, sir.

\* \* \*

MR. SOURWINE: Do you know anything about any plan under consideration at any time by the University of Chicago to record 500 or any other larger number of jury deliberations?

MR. STRODTBECK: I do know this, I have at some point seen a letter—I believe you requested this letter the other day—in which, in the original negotiations between Mr. Kitch and Judge Phillips, the reference to some number of observations was made. I do know that that letter occurred probably in November of 1953.

This particular project came to my attention and began to have serious consideration by our research staff after January 1954. As soon as this more serious consideration by our research staff began, it then became apparent that unless we had an observer present to see the trial and unless we take pains to interview all 12 of the jurors, then it would not be possible to make the full scientific exploitation of the recording.

As soon as this was clear, the notion of any large number of transcriptions of actual deliberations was obviously and physically impossible. And so I am certain that at no time since my own active participation in this project has the notion been present among any of our research staff that anything more than a very limited number of recordings would be made.

You can appreciate that, given the interviews with 12 jurors, given the text of the deliberation, given a digest of the trial, you have there, by the time this is synthesized and written

out, more than enough for a regular-sized volume, and it is impossible to digest and assimilate more than a very, very small number of such sets of empirical materials.

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c. **Testimony of Paul Richard Kitch, Esq.,  
Wichita, Kansas—October 13, 1955**

SENATOR JENNER: Will you state for the record your full name?

MR. KITCH: My full name is Paul Richard Kitch.

SENATOR JENNER: And your business or profession?

MR. KITCH: I am a lawyer.

SENATOR JENNER: Where do you reside, sir?

MR. KITCH: I reside at Wichita, Kans.

\* \* \*

MR. SOURWINE: Are you connected in any way with the University of Chicago?

MR. KITCH: I am not, other than that I am a graduate of the university, if that constitutes a connection, of which I am very proud.

MR. SOURWINE: Have you ever been employed by the University of Chicago?

MR. KITCH: I have never been employed by the University of Chicago except in the capacity of a waiter, if I may be frank about it.

MR. SOURWINE: Have you, sir, ever acted for the University of Chicago?

MR. KITCH: I have acted only in a way you would have to let me explain because there are factors involved in it.

In looking back over the correspondence, I understand why you talk about my being an agent for the university.

This matter originated with a bar committee. The original idea was our idea, and I happened to be interested enough, as a fellow who went to Chicago, having heard that they had funds available, to see the project.

MR. SOURWINE: What was that bar committee, Mr. Kitch?

MR. KITCH: It started off originally as a public relations committee of the Wichita Bar Association. . . .

\* \* \*

There had appeared a series of articles in the local paper written, ghost written, at least, for several well-known trial attorneys in the

country, in which the feature of these articles was the tricks which these attorneys used from time to time, as though the trial of a lawsuit before a jury were a matter of trickery, and that the litigant who eventually won the lawsuit was the man who was able to hire a lawyer that could outtrick the jury.

MR. SOURWINE: Where did you say this appeared?

MR. KITCH: This appeared in a local Wichita paper, and as to which one, it is my recollection it was the *Wichita Beacon*. That is what precipitated the meeting of the public relations committee.

\* \* \*

[T]he upshot of this committee action was the fact that, if there could be some way to combat this kind of publicity, to tell the American public how good the jury system was, instead of how bad it was, that it would be a fine thing, and that was the origin and nucleus of this particular survey.

That led up to this proposition. Mr. Stanley first took it up with some persons on the university staff—as to who, I do not know—at least, the dean reported to me that he had taken up the idea and suggested that I come up and talk about it. And I came up, and I talked to Mr. Levi.

He was a classmate of mine. And I told Mr. Levi that here was an opportunity to do some practical research where the definite advantage that would come from it could be pinpointed, in a practical utility to the profession, instead of dealing with my old criticism of law professors of getting unreal and theoretical, that here was a chance to sink their teeth into something.

He was very reluctant. In fairness to Ed, I have to say that I sold Ed on the possibilities, and this public relations committee sold him on the possibilities.

To get out from under any commitment on it, he said, "Well, I can see that it has got merit," but he said, "How are you going to get permission to do this sort of thing?"

He passed the buck back on that. I was ready. I said, "The bar association will back me on it, and I will guarantee to you that we will find judges in Kansas who will give you this kind of consent."

The next thing I knew, one of the staff members wrote me and said that if we could get the consent, they would be interested.

Now, there were numerous negotiations.

This was not a one-letter proposition or an overnight proposition or a one-day proposition. I talked to numerous of the trial lawyers in Wichita about it. The nucleus of the bar in Wichita has been solidly behind this. There has been no secretiveness about it. It has simply been information that has been kept by the profession.

MR. SOURWINE: You mean the bar of Wichita generally was consulted about the recording of jury deliberations?

MR. KITCH: Well, that portion of the bar of Wichita that engages in trial practice to any extent at all were all consulted, with the mass effort originating there.

MR. SOURWINE: You consulted every trial lawyer in Wichita?

MR. KITCH: I would not make that broad a statement, of every trial lawyer, because I know some fellows that may think they are trial lawyers, and I may not think they were.

MR. SOURWINE: That is why I asked the question, because I think your previous statement made it pretty clear that you had consulted everyone who participated in trial work at all. That would mean all trial lawyers.

MR. KITCH: Well, in any substantial amount. Let us say that.

MR. SOURWINE: I thought that was what it was.

MR. KITCH: Yes, that is what I mean by it. Now, that is where it started.

MR. SOURWINE: Did they know how many cases had been recorded?

MR. KITCH: Well, now, you understand, none had been recorded at the time I am talking about. We were talking about getting an opportunity to get somebody to finance this project for us.

MR. SOURWINE: You said it has been common knowledge in Wichita. Do the substantial trial lawyers in Wichita all know now that it was done?

MR. KITCH: Oh, yes.

MR. SOURWINE: Did they know before it broke in the newspapers?

MR. KITCH: Yes. That is what I am telling you.

MR. SOURWINE: Did they know in how many cases it was recorded?

MR. KITCH: They would not know the exact number, and I did not, either, because once we set it up—

MR. SOURWINE: Did they know the project had been ended?

MR. KITCH: Had been ended?

MR. SOURWINE: Yes; that there would be no more recordings.

MR. KITCH: No, because we still hope there will be.

MR. SOURWINE: All right. Go ahead.

MR. KITCH: And I will make clear on that point that, for a minute, I am not receding from the desire of the bar, and I may say it is now the 10th circuit. I think I can speak for the great majority of the trial lawyers in the 10th circuit, that we want this carried on.

Now, as to any specific number, we do not profess to be experts in research, but we want it done in a sufficient number of cases that it can be said to be a fair sampling of what the jury system actually can accomplish.

MR. SOURWINE: How many cases would that take?

MR. KITCH: In my opinion, it would take a minimum of 100 cases. I do not think you can sample six jury deliberations and arrive at any final conclusion, and I do not think you do, either. I think you would criticize a study that was based on the six juries.

Now, there is one more conclusion about this agency matter that I have got to get in here, because, having taken this up, not only with Judge Phillips but with several others of the circuit judges who expressed enthusiasm, and, from your experience with the bar, you know what trouble you get into if you ever have an idea—the next thing you do is, someone else has an idea by which you work for them.

Judge Phillips came up with the idea that while we were waiting on this, instead of waiting, we had better get to work. And the next thing I knew, and before this project, the recording project got under way, I was heading up the 10th circuit committee for a study of the jury system then and there, which resulted in sending out rather extensive questionnaires to 1,400 jurors, every juror who had served in the circuit during the past year, and then putting those results together in order to report to the judicial conference.

That report was printed. But when you start pinning my expressions of opinion down to Mr. Levi or to Judge Phillips, you have got me in so many different capacities that at any given moment, very frankly, I don't know—I mean, it would be unfair to reflect my opinion at any time to those gentlemen or to put my words in their mouths if you get the point.

\* \* \*

Now, yesterday, the question was asked by the Senator. He said, "Well, now, do you know of any immediate threat to the jury system?"

Naturally we do not know of any immediate threat to the jury system. But if you will study the figures, you will find that institutions do not die by some sudden attack. They die by the gradual lack of confidence, the gradual lack of appreciation, the very thing I am talking about—these newspaper articles, that there is always trickery.

They lose confidence in it, and you have to offset that. And to document that, in this very survey that I am talking about, not conducted by the law school, but conducted by our committee, the surprising thing that we find in there, and not having it with me today I do not want to quote the exact figure, but somewhere from 30 to 40 percent of those jurors who filled out that questionnaire reported that if they themselves were involved in litigation, they would prefer to have their cases tried through the judges.

Fifty years ago, if you had put that kind of questionnaire out, you gentlemen know yourselves what kind of response you would have gotten, because at that time the confidence in the jury system was more dominant, and our Constitution was aimed at the fact of preserving that right.

And here you have got a substantial number of jurors that have come in and experienced their service in the courts and they go out and they say next, "If I have litigation of my own, I prefer to have the judge try it."

So we cannot deal up high. We have got to get down to these concrete problems of the profession to make this jury trial so that it actually accomplishes for the jury system what I might say the Federal rules have accomplished for Federal procedure.

That is the best analogy that I know of.

\* \* \*

[W]ith 1 or 2 exceptions, every judge that first heard it was violently opposed to it. They would say, "Why, we can't possibly do that." And with one exception, every judge that I have presented it to since, after study, changed their minds and have given tentative approval to it.

To get the background of this, I want you—I mean, you were talking about legislation about it. I would like to have you come out to the 10th circuit, and not talk just to one judge, but talk to every judge in the 10th circuit about it.

\* \* \*

MR. SOURWINE: Mr. Kitch, you stated quite clearly and quite convincingly your own purpose in all this as being to defend and improve the jury system. Are you quite sure that the project as undertaken and carried out by the University of Chicago and as financed by the Ford Foundation has exactly the same purpose and not others?

MR. KITCH: I am 100 percent convinced of that, and I have seen these boys every day work on it. There is no question in the world in my mind about that.

MR. SOURWINE: You have worked with them closely on this matter all the time?

MR. KITCH: When I say I have worked with them closely, I have kept in contact, and the boys that have come down in Wichita, for instance, I have tried to make some effort to see that they were entertained and some kind of attention given to them from the graduates of the school.

\* \* \*

MR. SOURWINE: Now, did you have anything to do with the preparation of any presentation made by the university or in their behalf to the Ford Foundation seeking additional funds for this project?

MR. KITCH: No, not at any time; I never had any participation in that.

MR. SOURWINE: Did you ever have any contact, directly or indirectly, with the Ford Foundation about this project?

MR. KITCH: None whatever, and I know generalities only, what I read in the paper.

MR. SOURWINE: Now, when you went out to secure permission for the project—and you did that, did you?

MR. KITCH: Yes; I did.

MR. SOURWINE: Did you do that with the understanding you were seeking this permission for the University of Chicago?

MR. KITCH: No; I didn't, I did it—the idea I was going to prove to them was it could be done.

MR. SOURWINE: Now, did you understand that if you got their permission, that they would do it?

MR. KITCH: Not with any finality.

MR. SOURWINE: Had they told you that if they got the permission they would go ahead?

MR. KITCH: No, after I got—you could check the lapse of time, there was still quite a

problem, and I had had considerable trouble getting them to move. . . .

\* \* \*

MR. SOURWINE: Have you been connected in an attorney-client relationship in a case in which the proceedings of a jury were recorded?

MR. KITCH: My firm was counsel in one of the particular cases. I didn't—the fact of the matter is, I didn't know about it at the moment, but one of the men in the office reported to me that he had been requested to give his consent and he had given it.

MR. SOURWINE: You say you didn't know at the time?

MR. KITCH: That it was going to be recorded?

MR. SOURWINE: You did not know at the time your firm was connected with that case?

MR. KITCH: Well, I didn't know what cases were going to be recorded. Once the project was set up, I stayed clear away from it.

MR. SOURWINE: Are you presently connected in an attorney-client relationship with any of the cases in which the jury deliberations were recorded?

MR. KITCH: Well, yes, we still, the case—I don't know whether it is on appeal before the circuit court or not, but I was—my firm represented the plaintiff in the case.

MR. SOURWINE: Does the question of the jury's deliberations having been recorded have any place in the appeal procedure in that case?

MR. KITCH: In the what—appeal procedure?

MR. SOURWINE: Appeal procedure, yes.

MR. KITCH: No.

MR. SOURWINE: Was that fact mentioned at all in the appeal?

MR. KITCH: None whatever.

MR. SOURWINE: It was not disclosed?

MR. KITCH: Well, now, wait a minute. What fact mentioned—that I was connected with the case?

MR. SOURWINE: No, the fact that the jury in that case had its deliberations—

MR. KITCH: No.

MR. SOURWINE: (continuing): Listened in upon?

MR. KITCH: No. The circuit court wouldn't know that.

MR. SOURWINE: Does the client of either counsel in that case, as far as you know, know that?

MR. KITCH: You had Mr. Green yesterday.

I did not ask him whether he had taken it up with his counsel or not. I talked with him briefly—

MR. SOURWINE: Were you his adversary in that case?

MR. KITCH: Yes. Of course, I, a while ago, inadvertently identified one case. I am sorry I have not hid that, but as long as—

MR. SOURWINE: Do you know if he has advised his client that was the case?

MR. KITCH: No, I don't know, but Mr. Newkirk told me he had taken it up with his clients.

MR. SOURWINE: He did take it up with his client?

MR. KITCH: Or, my client—don't get me wrong—it is the firm's clients—

MR. SOURWINE: Well, but it is your firm's client; he doesn't know about it?

MR. KITCH: That is right. The point I want to make is that Don Newkirk is a member of my firm. I wasn't trying to create the impression I was in any way trying to shift it over to him.

MR. SOURWINE: Was there any understanding in connection with the permission granted for this project on the question of whether clients would be advised with respect to the recording of the jury's deliberations?

MR. KITCH: No, there was not.

MR. SOURWINE: That was left in each case to the discretion of counsel?

MR. KITCH: That is—I assume so, because I did not cover that on the proposed rule—

MR. SOURWINE: Counsel were not cautioned to keep this confidential?

MR. KITCH: Oh, yes; they were cautioned to keep this confidential.

MR. SOURWINE: Well, would keeping it confidential permit them to tell their clients?

MR. KITCH: Yes, it would. There is no such thing as matters—I mean, I have never heard of a matter between an attorney and a client that was confidential.

MR. SOURWINE: Did you hear the testimony this morning of Mr. Mikva, that in his opinion if a client did not know that counsel had agreed to the recording of the jury's deliberations, and subsequently learned it he would, nevertheless, be bound by the attorney's waiver and would not be able to secure any—

MR. KITCH: I heard that.

MR. SOURWINE: Would you agree with that?

MR. KITCH: Definitely.

MR. SOURWINE: Do you think that counsel

could foreclose the client from his rights in a case like that?

MR. KITCH: Definitely.

MR. SOURWINE: I take it you do not think that agreement in any way constitutes improper conduct by counsel.

MR. KITCH: None whatever.

MR. SOURWINE: You think that the right of the client to free and full and unfettered deliberation in the jury room is a right that counsel may waive, and he can do that without his client's consent and without his knowledge, without constituting misconduct on the part of counsel?

MR. KITCH: I do.

Of course, you have stated that in an argumentative way. Counsel can waive jury, and I can give you all kinds of cases on it, and once counsel waives the jury, he gives away a right entirely, and there is no recourse merely because he did not consult the client; the law is very clear on that. I am talking about civil cases—you understand, there is a difference between criminal and civil.

\* \* \*

MR. SOURWINE: Is there any connection between this project for recording the deliberations of juries and any project you know of approved by the bar association, relating to it?

MR. KITCH: Yes; there is a connection.

MR. SOURWINE: What is the connection?

MR. KITCH: You have got various bar groups that want to be kept informed on the progress, as to whether you are able to do it, make advances in it—

MR. SOURWINE: You mean, there are varied bar groups that know of this project for recording proceedings of juries?

MR. KITCH: Well, I wouldn't want to go quite so strong—

MR. SOURWINE: Well, your answer, sir, was—

MR. KITCH: The judicial section of the American Bar is quite familiar—that is the one that asked to be kept advised as we went along.

Now, when you talk about a bar group, the Tenth Circuit Judicial Conference is very definitely a bar group, and they are quite interested in these results and they do know about it.

In addition to that, we have a rather semi-permanent committee called the jury study committee of the tenth circuit, in which I am involved and all of those members are very interested in knowing, at the appropriate time,

whether we have made progress and what they can do to help carry it on.

\* \* \*

MR. SOURWINE: All right, sir.

I have no further questions.

MR. KITCH: Mr. Chairman, may I make a statement to explain background on the Estes Park—I think I can do it in a limited time, I mean, that has come into this hearing—

THE CHAIRMAN: Oh, sure.

MR. KITCH: We have not covered it, and a lot of responsible names have been involved.

I would like to state briefly that the origin of the Estes Park meetings arrived by reason of the fact that under the statute the Federal judges—

THE CHAIRMAN: Now, what judges?

MR. KITCH: All right—can't I just—I think I could save time if I could, if I could get off one point and then if you direct me, I will stop and start over—I mean, if you want me to start that way, I will.

THE CHAIRMAN: Well, go ahead.

MR. KITCH: A judicial conference is required by statute once a year, at which time you meet and consider ways and means of improving the administration of justice. That is just the background of it.

Judge Phillips, knowing about this project, asked me if—last summer, the request came here ahead of the meeting—if I could not get this research group to demonstrate the recording for the meeting the following summer, to pick out one of the recordings made and show the safeguards that we used and show the method of editing and then show the results that could be obtained and then let the judges discuss the whole project, the whole recording project.

I told him that I would endeavor to do so. I took that up with Dean Levi and he objected to it on the grounds he thought it was premature, that it would interfere with the research nature of the project.

I told Judge Levi—Dean Levi—that it put me in a very bad position, that if we had the cooperation of the 10th circuit and if they wanted this program for the purpose, that he was not in any position to deny it and he finally said that what he would do would be what Judge Phillips requested, that he would release an edited recording for the program but that he would have no connection with it whatever.

I went up there—runs in my mind in May,

because the program was scheduled in June, to see definitely that the prepared recording would be suitable for playing here—I mean, to be heard, and at that time they played it for me and I heard the edited version of this tape; it had been reduced to 30 minutes.

I heard it in a small room and it sounded adequate and so we put on the program.

\* \* \*

f.

**Testimony of Irving Ferman, Washington  
Office Director, American Civil Liberties  
Union—October 13, 1955**

MR. SOURWINE: Your full name, sir, your business or profession, and your address, please?

MR. FERMAN: My name is Irving Ferman. I speak in behalf of the American Civil Liberties Union as its Washington, D.C., director. I am also a member of the bar of the State of Louisiana.

\* \* \*

The union is a private organization devoted to the preservation of our personal liberty. Accordingly, we are vitally interested in the matter before this committee today.

At this point I wish to emphasize my appreciation for what the lawyers and social scientists in Chicago are trying to do in bringing greater understanding to the functioning of our legal institutions.

\* \* \*

The right to jury trial is a right immemorial to freedom. Upon it rests much of our Anglo-American judicial traditions. This right is firmly established in our Constitution in article III and the sixth and seventh amendments to the Constitution.

A basic distinction between our free society and the totalitarian societies of communism and fascism is the independence with which our society guards its judicial system.

\* \* \*

Justice William O. Douglas in his *Almanac of Liberty* expresses the essential character of the jury as follows:

A jury reflects the attitudes and mores of the community from which it is drawn. It lives only for the day and it does justice according to its light. The

groups of 12 who are drawn to hear a case make the decision and melt away. It is not present the next day to be criticized. It is the one governmental agency that has no ambition. To preserve this kind of status for our jury system, it is essential that we keep completely private what transpires in a jury room during the deliberative stage.

The fact that no coercion of a jury took place in the instant case at hand is of no significance. The fact that consent was obtained to the jury bugging from counsel of both sides as well as the court is of no significance, and, further, the fact that the present law, in a good number of jurisdictions, does not recognize merely communication with the jury even during a deliberation as presumption of improper influencing of a jury verdict is of no significance.

The fact is that the right to jury trial must mean all that it can possibly mean. This should involve the greatest assurance that the jury's impartiality will be protected from any kind of surveillance, embarrassment, or possibility of coercion.

When consent was obtained by the judge as well as counsel, little litigious opportunity was left for objections to be properly raised. Thus out of this consensual relation, there evolved the public policy limiting the secrecy of the jury room, and thus derogating from the basic fundamental right of a jury trial.

In short, any action which tends to establish even a climate injurious to the preservation of a particular personal right guaranteed by our Constitution must be challenged. To suggest the fact that a particular individual in a given situation has not been harmed is completely irrelevant, and likewise irrelevant is the fact that possible harm from future infringements resulting from practices of this kind may be outweighed by the good that could come from such practices.

We plead today that the jury room remain tightly closed forever.

MR. SOURWINE: Mr. Ferman, is there any question in your mind but that the right of jury trial guaranteed by the seventh amendment encompasses all of the attributes of a jury trial known at the time the amendment was written?

MR. FERMAN: There is no doubt in my mind.

MR. SOURWINE: Have you made any researches to determine what the attributes of a jury trial were at that time?

MR. FERMAN: No, I have not specifically.



\* \* \*

THE CHAIRMAN: [I]s it your judgment that the judge in this case, Judge Hill, who permitted that, did not perform his duty?

MR. FERMAN: Well, Mr. Chairman, you could appreciate my feelings as a member of the bar in criticizing a gentleman on the bench.

THE CHAIRMAN: I know how we feel about criticizing judges. But—

MR. FERMAN: I think that I could appreciate the motivation of a judge and his interest in experimentation that might lead to an improvement of the jury system, but I think, on the other hand, that the duty to have remain private jury deliberations should have prevailed.

THE CHAIRMAN: Do you not think it was his duty to prohibit the bugging of these juries?

MR. FERMAN: Yes; I do.

\* \* \*

### NOTES

#### NOTE 1.

STATEMENT BY CHIEF JUDGE ORIE L. PHILLIPS, JUDGE DELMAS C. HILL, AND DEAN EDWARD H. LEVI—OCTOBER 25, 1955

Because certain statements have appeared in the public press with respect to the recording of the deliberations of juries in six cases in the United States district court at Wichita, Kans., which are inaccurate and misleading, Orie L. Phillips, chief judge of the Court of Appeals for the 10th Judicial Circuit, Delmas C. Hill, judge of the United States District Court for the District of Kansas, and Edward H. Levi, dean of the University of Chicago Law School, have prepared the following statement, which statement reflects the pertinent facts:

\* \* \*

Early in February 1954, Mr. Kitch discussed with Judge Phillips the waiver of the condition that the jury be informed. Judge Phillips stated that he still was of the opinion the jurors should be informed, but that he might not object if the experiment were restricted to a limited number of cases. Thereafter, Mr. Kitch informed Judge Hill of this conversation had with Judge Phillips. On February 12, 1954, while Judge Hill was sitting in the Colorado district, he discussed the matter with Judge Phillips. Judge Phillips reiterated that he thought the recording should not be made without the

knowledge of the jurors. Judge Hill replied that Mr. Kitch was not willing to adopt the suggestion Judge Phillips had made in his letter because he thought informing the jurors might interfere with the freedom of their deliberations. Judge Hill asked Judge Phillips if he would object if a few cases were recorded following the procedures that Mr. Kitch had set forth in the rules, but without prior communication to the jurors. Judge Phillips replied that while he still adhered to the opinion the jurors should be informed, he would not object to it being tried out on an experimental basis in a limited number of cases.

\* \* \*

In August 1954, in a conversation with Judge Phillips, Judge Hill again affirmed what he had said in July 1954. Judge Phillips stated that if any requests were made to make further recordings in the 10th circuit, it was his intention to present such requests to the Judicial Council of the Court of Appeals of the Tenth Circuit, composed of the five circuit judges, for full and careful consideration. No further requests for permission were made and no further recordings were made.

The recording of a limited number of actual jury deliberations was undertaken as an experiment in a serious manner to further an important study of a basic legal institution with the objective of strengthening and improving the jury system, and with a purpose to observe essential safeguards to protect the jurors and litigants from injury.

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#### NOTE 2.

LETTER FROM CHIEF JUDGE ORIE L. PHILLIPS TO SENATOR JAMES O. EASTLAND—  
OCTOBER 27, 1955

\* \* \*

As chief judge of the Court of Appeals of the 10th Circuit, I have no supervisory powers over district judges or district courts, except the power to assign a district judge to sit in the court of appeals, in a district other than his own in the 10th circuit, and in statutory three-judge courts.

Such supervisory powers as exist are vested in the Judicial Council of the circuit, composed of the five circuit judges. Accordingly, I conceived that I could only express my opinion and

give advice, I certainly had no power officially to authorize that a recording be made or direct that it not be made.

The reasons I took and adhered to the position that the jurors involved should be advised in advance that the recording was to be made were twofold:

1. I felt that if the recordings were made, especially on the scale originally planned but not carried out, the fact that recordings were being made would inevitably become known to the public, but that the limitations and safeguards to be observed would in all probability not be fully known or understood by the general public.

As a result, future jurors might enter the jury room to deliberate on their verdict conscious of the fact that a recording might be made of the deliberations, but without any real understanding of the limited purpose for which the recording would be used and the abridgments that would be made thereof; and that such fact might inhibit full and frank discussion in the jury room.

On the other hand, if such jurors knew that such recordings were only made when that fact was made known to the jurors in advance of the making, they would not fear that such a recording might be made of their deliberations, and they would not thus be inhibited.

2. The second reason is based on what I conceive to be fair and decent.

While a recording of the jury's deliberations in a particular case, wholly unknown to the jurors, would not affect their deliberations or their verdict in that particular case, it seems to me that when jurors enter the jury room to deliberate on their verdict, rightfully believing that their deliberations and discussions will be effectively closed to any outside ears, that it would be grossly unfair to permit a recording to be made of their deliberations, even if made for what is believed to be for a worthy and useful purpose.

\* \* \*

After the experimental recordings had been completed in the Federal district court at Wichita, Judge Hill and I reached a firm understanding that no further recordings would be permitted in that court.

By that time I had reached the conclusion that if any further requests were made for permission to make recordings in the courts of the 10th circuit, I would ask that matter be sub-

mitted to the Judicial Council of the circuit, with the request that it give the matter careful consideration and take such action as it deemed appropriate. I regret that I did not follow that course in the first instance. Hindsight is better sometimes than foresight. However, no requests have been made and no further recordings have been made in the 10th circuit.

I did not suggest the program that was put on at Estes Park in July 1955. Mr. Kitch's committee had participated in the program on another phase of the jury study at the Judicial Conference of the circuit in July 1954. It was well received and highly commended. When I came to prepare the program for the 1955 Conference, I asked Mr. Kitch if he desired to have his committee participate in the program. I definitely did not have the recordings in mind. However, Mr. Kitch suggested the presentation of a portion of one of the recordings and the building of a program around it. I told him it would be all right, providing the portion of the recording produced had been cut so as to render it wholly impersonal, and if the procedures followed in the making of the recording were fully stated. Mr. Kitch prepared the program and directed its presentation.

May I add that I have a high regard for Judge Hill. I am sure that it was his desire to proceed with the experiment cautiously and not to do anything that would injure litigants, the jurors involved, or the jury system, and that he was actuated by a desire to make a worthwhile contribution to the study being carried out by the law school of the University of Chicago. Moreover, he limited the recordings in his court to six cases.

\* \* \*

## 2.

### Congress Decides

Eighty-Fourth Congress, Second Session  
An Act to Further Protect and Assure the  
Privacy of Grand and Petit  
Juries in the Courts of the United States  
While such Juries are Deliberating or Voting\*

\* \* \*

Whoever knowingly and willfully, by any means or device whatsoever—

\* Act of August 2, 1956, Ch. 879, §1, 70 Stat. 935; see 18 U.S.C. §1508 (1964).

(a) records, or attempts to record, the proceedings of any grand or petit jury in any court of the United States while such jury is deliberating or voting; or

(b) listens to or observes, or attempts to listen to or observe, the proceedings of any grand or petit jury of which he is not a member in any court of the United States while such jury is deliberating or voting—

shall be fined not more than \$1,000 or imprisoned not more than one year or both.

\* \* \*

### NOTES

#### NOTE 1.

WALDO W. BURCHARD  
LAWYERS, POLITICAL SCIENTISTS, SOCIOLOGISTS—  
AND CONCEALED MICROPHONES\*

\* \* \*

... The fact that the persons who made the recordings were competent social scientists pursuing a serious study of an important American institution appeared to make no difference to the critics.

The attitudes of these critics reflect a basic distrust of social science and social scientists that should be of concern to persons in the field, and particularly to sociologists, who engage more extensively in research making use of techniques of this type than most other social scientists. If social research is to continue, it must have public support, which means, in part, that it must have newspaper support. The fact that the adverse criticism of editors and commentators went almost entirely unanswered in the public press<sup>4</sup> probably means that social scientists have not yet found an effective way of convincing the public and perhaps more important, the moulders of popular opinion of the propriety, utility, and most of all, the sincerity of their efforts. The fact that nearly three months elapsed between the playing of the recordings and the public announcement of the event implies that those members of the legal profession who heard

\* 23 *American Sociological Review* 686, 687 (1958). Reprinted by permission.

4. Members of the research team, of course, defended their actions before the Senate subcommittee, in interviews, in speeches, and in articles. At least one popular newspaper columnist, Fred Othman, had a good word to say for the use of concealed microphones. Nonetheless, the overwhelming bulk of commentary was unfavorable.

the records were not overly disturbed by them. . . .

\* \* \*

#### NOTE 2.

TED R. VAUGHAN  
GOVERNMENTAL INTERVENTION IN SOCIAL  
RESEARCH—POLITICAL AND ETHICAL  
DIMENSIONS IN THE WICHITA JURY RECORDINGS\*

\* \* \*

The failure to accomplish satisfactorily the stated objectives of the Hearings seems to lie in the political and ethical orientations of the Subcommittee members themselves. A principal purpose, closely related to the failure to see the objectives and its accomplishments was the initial failure to view the issue problematically. To the Subcommittee members, unaccustomed as they were to seeing things in the relative terms of science, the case presented no problem in the sense of an issue to be resolved. The only problem was that project members had violated a set of norms, and a set assumed by the senators to be superordinate to all others. And that problem could be satisfactorily solved through chastising the errant researchers and erecting obstacles to its recurrence.

\* \* \*

A second source of the Hearings' shift in purpose, closely related to the failure to see the issue as problematical, was the failure to differentiate between scientific inquiry directed toward the acquisition of knowledge and non-scientific investigation of or intervention in the jury process. Committed as the Subcommittee members were to the supremacy of one set of values and norms, they could see no need for making such a distinction. They failed to consider that science is itself a system of norms with the same claims to legitimacy as other normative systems. It is true that the norms of science are not always apparent to those engaged in scientific pursuits, much less to laymen. But to assume that scientific objectives are "naturally" subordinate to other normative systems and values is to beg the crucial question.

\* \* \*

The result of this failure to discriminate

\* Gideon Sjoberg, ed.: *Ethics, Politics and Social Research*. Cambridge, Mass.: Shenkman Publishing Co. 50, 60-75 (1967). Reprinted by permission.

between scientific and nonscientific investigations of the jury system was that the scientific position did not receive a fair hearing. All the potential disadvantages and possible abuses of nonscientific intrusion on the deliberative process were assumed to hold for scientific research as well. If this position were consistently taken by political authorities, a considerable amount of current social science research would be terminated.

The third source of the Subcommittee's failure to achieve its stated objectives is a natural consequence of the preceding ones. The one possibly legitimate role representatives of the state could have in the research process would be that of mediating between conflicting systems of values and norms. Not only did the Subcommittee fail in this respect, the members did not even entertain such a role as a legitimate possibility. Although the entire Hearing operated behind the facade of concern for the privacy of jurors while deliberating a case, every indication was that this was not the major concern. The Subcommittee failed to treat the matter either as an ethical concern or as a political problem in institutional relations. Certainly, the matter was not fully anticipated in the ambiguous legal traditions the Senators apparently thought they were defending.

\* \* \*

The fact that political intervention in social research is a paramount issue here should not obscure the significance of the ethical dimension of the case. In this particular case, as is true of much research, political and ethical questions are intertwined in a very complex manner. . . .

. . . We neither posit simple solutions nor attempt to establish a code of ethics for scientists. We do, however, question some of the solutions that have been utilized in similar circumstances. To posit a hierarchy of values, for example, and then operate exclusively on that basis is to be unrealistic about the issue. To argue that in the final analysis one's conscience should be his guide is hardly any better, because one's decision is inevitably based on a pre-existing value system. . . .

The arguments in defense of unrestricted freedom of scientific inquiry are fairly well known to the social scientist, if only because they receive some prominence in textbooks on methodology. . . .

The immediate issue, of course, is the freedom of the scientist to investigate on-going ac-

tivities of an operating jury without pressure, constraint, or control by nonscientific agencies. Justification of this action is usually predicated on the broader norm that there should be unrestricted freedom of scientific inquiry, a norm which, in turn, rests on the value premise that men have the inherent right to know, i.e., that knowledge is always superior to ignorance and that nothing external to the object of investigation itself should influence the acquisition of knowledge. If knowledge is a value, any other circumstance which endangers its achievement, or influences its outcome in any way, is undesirable. The scientific enterprise cannot make significant advances without maximal freedom from external control. The pernicious effects of the intrusion of the special interests of political, religious, or other groups into the research process are well documented in the literature. Scientists could and should oppose actual or potential attempts by nonscientific interests to control or influence social research.

More specifically, the conventional textbook view would have it that investigation of the actual operations of the jury system, or any other basic social institution, is justified by the value of knowledge itself. It is better to know about the workings of the jury system than not to know. The same legitimacy that attaches to knowledge in a general sense legitimates the quest for knowledge in the particular case. From this perspective, the jury system has no special claim to inviolability from scientific investigation. More pragmatically, the investigation can also be legitimated on the grounds that any decision to alter or revise an institution such as the jury system should be based on the most accurate, precise information available. Such information would be available only through direct inquiry into the mechanisms of jury operations. . . . And scientific investigation poses no real threat to the integrity of the jury system because the scientist is not concerned with the action of individual jurors, only with patterns of behavior. Data obtained are treated as classes, not as individual attributes. As a professional scientist, furthermore, the social investigator is already bound to a long-standing set of rules that proscribe the revelation of anything that might identify a person and thereby possibly embarrass, harass, or otherwise do him injury or harm.

What such textbook methodology usually ignores is that the scientific ethic (values and norms) is only one in a system of many conflicting, competing ethics making up the social fab-

ric. It tends, instead, to assume there is a hierarchy of ethics with the scientific ethic at the pinnacle. But in actual practice, especially in field research, as the Wichita case reveals, ethical issues are much more problematic. There is no natural law to which the scientist can appeal. Like others, he bases his decisions on personal predilections and particular values. But one person's predilections—be he scientist or saint—are not perforce more natural than another's. The proclivity among scientists to minimize, if not ignore, competing social ethics, and to define science in some independent sense, is quite widespread. In short, the scientist typically subscribes to the notion that the end of knowledge justifies the scientific means.

Counterposed to the scientific ethic are the values and norms of the judicial process. Advocates of this position contend that no circumstances justify the invasion of the traditional privacy and confidence of jury deliberations, for, normatively, there should be inviolable privacy of such deliberations. . . . Anything that undermines in any way the impartial nature of the jury should not be permitted. And, the argument continues, the jurors' impartiality is influenced by the actual or possible invasion of the jury room irrespective of the purpose. Any actual or potential surveillance undermines impartiality because it raises the possibility of considerations extraneous to the merits of the case itself. Obviously, impartiality is threatened if there are attempts to influence the verdict. Even if scientific investigation is not directed toward influencing verdicts, it still threatens impartiality to the extent that it introduces any question of possible embarrassment, coercion, or other such considerations into the minds of actual jurors. A jury is impartial to the extent that nothing but the consciences of the individual jurors influence its decision. Even if anonymity is assured or the members are unaware of immediate surveillance, impartiality is threatened unless the norm of inviolable privacy prevails. In terms of this principle, the matter of safeguards in scientific research is largely irrelevant. To protect the opportunity for an impartial and just verdict, secret deliberations must be preserved regardless of the ultimate value that might result from the scientific investigation of the jury system. . . .

\* \* \*

Our own present position is that if certain general principles are accepted, the conflict of ethics neither poses a hopeless dilemma nor re-

stricts action to the alternative responses already reviewed. These basic premises include the denial of a natural ordering of ethics and the affirmation that the merits of the individual case determine the ascendancy of particular ethics for that particular case. If the assumption is granted that science is a part of the social system, an institution with values and norms not inherently superior to others, then ethical conflicts are inevitable. And they cannot satisfactorily be ignored, avoided, or suppressed.

To re-emphasize the need for a self-conscious sense of ethical responsibility in social research, we can conclude by very briefly indicating two general procedures which should be included in decision-making by social science researchers. First, a self-conscious and serious sense of reflection is needed, including detailed awareness and consideration of the points of view of respondents or other participants. Genuine understanding of another's point of view, including the basic values upon which his argument rests, may, of course, convince one completely of the merits of one's own position. But the decision is made on the basis of comparison not abstraction. Simply to assert scientific prerogatives as abstractions is to attempt an easy solution to a difficult problem. When contradictory positions are thoroughly understood, a decision can be based on the merits of the case. Such understanding need not necessarily alter research objectives, although this may well be the case, but it may force a reconsideration of means. Such a reassessment may necessitate more creative, innovative research procedure.

\* \* \*

#### NOTE 3.

REGINALD ROSE  
TWELVE ANGRY MEN\*

[The setting is a jury room in New York City.]

\* \* \*

#4: I think it's customary to take a preliminary vote.

#7: Yeah, let's vote, Who knows, maybe we can all go home.

FOREMAN [From opposite end of the table.]: It's up to you. Just let's remember we've

\* William I. Kaufman, ed.: *Great Television Plays* 183-191, 196-197, 201-202, 227-228, 232 (1969). Reprinted by permission of International Famous Agency. © 1965 by Reginald Rose.

got a first-degree murder charger here. If we vote guilty, we send the accused to the electric chair. That's mandatory.

#4: I think we all know that.

#3: Come on, let's vote.

#10: Yeah. Let's see who's where.

\* \* \*

FOREMAN: . . . nine . . . ten . . . eleven. That's eleven for guilty. Okay. Not guilty. [#8 *slowly raises his hand.*] One. Right. Okay, eleven to one, guilty. Now we know where we are. [#8 *lowers his hand.*]

#10: Boy-oh-boy. There's always one.

[#8 *doesn't look in his direction.*]

#7: So what do we do now?

#8: Well, I guess we talk.

\* \* \*

#10: All right, then you tell me. What are we sitting here for?

[#8 *looks at him, trying to phrase the following. They wait.*]

#8: Maybe for no reason. I don't know. Look, this boy's been kicked around all his life. You know, living in a slum, his mother dead since he was nine. He spent a year and a half in an orphanage while his father served a jail term for forgery. That's not a very good headstart. He's a wild, angry kid and that's all he's ever been. You know why he got that way? Because he was knocked on the head by somebody once a day, every day. He's had a pretty terrible nineteen years. I think maybe we owe him a few words. That's all. [*He looks around the table, #9 nods slowly.*]

#10: I don't mind telling you this, mister. We don't owe him a thing. He got a fair trial, didn't he? What d'you think that trial cost? He's lucky he got it. [*Turning to #11.*]: Know what I mean? [*Now looking across table at #'s 3, 4, 5.*] Look, we're all grown-ups in here. We heard the facts, didn't we? [*To #8.*] Now you're not going to tell us that we're supposed to believe that kid, knowing what he is. Listen, I've lived among 'em all my life. You can't believe a word they say. You know that. [*To all.*] I mean they're born liars.

\* \* \*

#3: Okay. Now here's what I think, and I have no personal feelings about this. I'm talking about facts. Number one: let's take the old man who lived on the second floor right underneath the room where the murder took place. At ten minutes after twelve on the night of the kill-

ing he heard loud noises in the apartment upstairs. He said it sounded like a fight. Then he heard the kid shout out, "I'm gonna kill you." A second later he heard a body fall, and he ran to the door of his apartment, looked out, and saw the kid running down the stairs and out of the house. Then he called the police. They found the father with a knife in his chest. . . .

FOREMAN: And the coroner fixed the time of death at around midnight.

#3: Right. I mean there are facts for you. You can't refute facts. This boy is guilty. I'm telling you. Look, I'm as sentimental as the next guy. I know the kid is only nineteen, but he's still got to pay for what he did.

\* \* \*

#10 [*Impatiently in #8's direction*]: Listen, what about that woman across the street? If her testimony don't prove it, nothing does.

#11: That's right. She was the one who actually saw the killing.

FOREMAN: Let's go in order here.

[#10 *raises, handkerchief in hand.*]

#10 [*Loudly.*]: Just a minute. Here's a woman . . . [*He blows his nose.*] Here's a woman who's lying in bed and can't sleep. [*He begins to walk around the table, wiping his tender nose and talking.*] She's dying with the heat. Know what I mean? Anyway, she looks out the window and right across the street she sees the kid stick the knife into his father. The time is 12:10 on the nose. Everything fits. Look, she's known the kid all his life. His window is right opposite hers, across the el tracks, and she swore she saw him do it. [*#10 is now standing behind #6 and looking across table at #8. #10 wipes his nose.*]

#8: Through the windows of a passing elevated train.

#10 [*Through the handkerchief.*]: Right. This el train had no passengers on it. It was just being moved downtown. The lights were out, remember? And they proved in court that at night you can look through the windows of an el train when the lights are out and see what's happening on the other side. They proved it!

#8: [*To #10.*]: I'd like to ask you something. You don't believe the boy. How come you believe the woman? She's one of "them" too, isn't she?

[#10 *is suddenly angry.*]

#10: You're a pretty smart fellow, aren't you?

[*He takes a step toward #8. #8 sits calmly*

there. #10 strides toward #8. The FOREMAN rises in his seat. #3 and 5 jump up and move toward #10.]

FOREMAN [*Nervously*.]: Hey, let's take it easy.

[#3, 5, and 10 stand behind #7. #3 and 5 have reached #10, who looks angrily at #8. #3 takes #10's arm.]

#10 [*Angrily*.]: What's he so wise about? I'm telling you . . .

#3 [*Strongly*.]: Come on. Sit down. [*He begins to lead #10 back to his seat.*] What are you letting him get you all upset for? Relax.

\* \* \*

FOREMAN [*To #7*.]: Okay. How about you?

#7: Me? [*He pauses, looks around, shrugs, then speaks.*] I don't know, it's practically all said already. We can talk about it forever. It's the same thing. I mean this kid is five for oh. Look at his record. He was in children's court when he was ten for throwing a rock at his teacher. At fifteen he was in reform school. He stole a car. He's been arrested for mugging. He was picked up twice for knife-fighting. He's real swift with a knife, they said. This is a very fine boy.

#8: Ever since he was five years old his father beat him up regularly. He used his fists.

#7 [*Indignantly*.]: So would I! A kid like that.

[#3 walks over from the water fountain toward #7. He stands behind #7, talks to #8.]

#3: And how. It's the kids, the way they are nowadays. Listen, when I was his age I used to call my father "sir." That's right, Sir! You ever hear a boy call his father that anymore?

#8: Fathers don't seem to think it's important any more.

#3: No? Have you got any kids?

#8: Three.

#3: Yeah, well I've got one, a boy twenty-two years old. I'll tell you about him. When he was nine he ran away from a fight. I saw him. I was so ashamed I almost threw up. So I told him right out: "I'm gonna make a man outa you or I'm gonna bust you in half trying." Well, I made a man outa him all right. When he was sixteen we had a battle. He hit me in the face! He's big, y'know. I haven't seen him in two years. Rotten kid. You work your heart out . . . [*He stops. He has said more than he intended and more passionately than he intended it. He is embarrassed. He looks at #8, and then at all of them. Then loud.*] All right. Let's get on with it. [*He turns and walks angrily around the table to*

*his seat. He sits down. #4 looks at #3 and then across the table.*]

#4: I think we're missing the point here. This boy, let's say he's a product of a filthy neighborhood and a broken home. We can't help that. We're here to decide whether he's guilty or innocent, not to go into the reasons why he grew up this way. He was born in a slum. Slums are breeding grounds for criminals. I know it. So do you. [*#5 reacts to the following.*] It's no secret. Children from slum backgrounds are potential menaces to society. Now, I think . . .

#10 [*Interrupting*.]: Brother, you can say that again. The kids who crawl outa those places are real trash. I don't want any part of them, I'm telling you.

[*The face of #5 is angry. He tries to control himself. His voice shakes.*]

#5: I've lived in a slum all my life . . .

[*#10 knows he has said the wrong thing.*]

#10: Oh, now wait a second . . .

#5 [*Furious*.]: I used to play in a back yard that was filled with garbage. Maybe it still smells on me.

#10 [*Beginning to anger*.]: Now listen, sonny . . .

[*FOREMAN has risen.*]

FOREMAN [*To #5*.]: Now, let's be reasonable. There's nothing personal . . .

[*#5 shoots to his feet.*]

#5 [*Loud*.]: There is something personal! [*He looks around at the others, all looking at him. Then, suddenly he has nothing to say. He sits down, fists clenched. #3 gets up and walks to him pats him on the back. #5 doesn't look up.*]

\* \* \*

[*The door opens. The GUARD enters carrying a curiously designed knife with a tag hanging from it. #4 walks into the shot and takes the knife from the guard. He turns and moves back to his seat as the GUARD exits. He stands behind his seat holding the knife.*]

#4 [*Leaning over to #8*.]: Everyone connected with the case identified this knife. Now are you trying to tell me that it really fell through a hole in the boy's pocket and that someone picked it up off the street, went to the boy's house and stabbed his father with it just to be amusing?

#8: No. I'm saying that it's possible that the boy lost the knife, and that someone else stabbed his father with a similar knife. It's possible.

[*#4 flicks open the blade of the knife and*



jams it into the table. Jurors #2, 5, 10, 11, 12 get up and crowd around to get a better look at it.]

#4: Take a look at that knife. It's a very unusual knife. I've never seen one like it. Neither had the storekeeper who sold it to the boy. Aren't you trying to make us accept a pretty incredible coincidence?

#8: I'm not trying to make anyone accept it. I'm just saying that it's possible.

[#3, standing next to #4, is suddenly infuriated at #8's calmness. He leans forward.]

#3 [Shouting.]: And I'm saying it's not possible.

[#8 stands for a moment in the silence. Then he reaches into his pocket and swiftly withdraws a knife. He holds it in front of his face, and flicks open the blade. Then he leans forward and sticks the knife into the table next to the other. The two ornately carved knives stuck into the table, side by side, are each exactly alike. There is an immediate burst of sound in the room.] [Simultaneous]

#7: What is this?

#6: What is it?

#12: Where'd that come from?

#2: How d'you like that!

[The jurors cluster around the knives. #8 is standing away from the table, watching. #3 looks up at him.]

#3 [Amazed.]: What are you trying to do?

#10: [Loud.]: Yeah! What's going on here? Who do you think you are?

[#6 has taken the knife out of the table and is holding it.]

#6: Look at it! It's the same knife!

[#8 watches them closely, a few steps back from the group. The ad lib hubbub still goes on.]

#4: Quiet! Let's be quiet!

[The noise begins to subside. #4 takes the knife from #5's hand and speaks to #8, who stands at left of frame.]

#4: Where'd you get it?

#8: I was walking for a couple of hours last night, just thinking. I walked through the boy's neighborhood. The knife comes from a little pawnshop three blocks from his house. It cost two dollars.

#4: It's against the law to buy or sell switch-blade knives.

#8: That's right. I broke the law.

\* \* \*

#3 [Shouting.]: . . . Now, listen to me, you

people! I've seen all kinds of dishonesty in my day . . . but this little display takes the cake! [#3 strides swiftly toward #8. He reaches him, waves his hand in #8's face.] You come in here with your heart bleeding all over the floor about slum kids and injustice, and you make up some wild stories, and all of a sudden you start getting through to some of these old ladies in here! Well, you're not getting through to me! I've had enough! [To all.] What's the matter with you people? Every one of you knows this kid is guilty! He's got to burn! We're letting him slip through our fingers here!

#8: [Calmly.]: Slip through our fingers? Are you his executioner?

#3 [Furious.]: I'm one of 'em.

#8: Maybe you'd like to pull the switch.

#3 [Shouting.]: For this kid? You bet I'd like to pull the switch!

#8: I'm sorry for you . . .

#3: Don't start with me now!

#8: What it must feel like to want to pull the switch!

#3 [Raging.]: Listen, you shut up!

#8 [Baiting him.]: Ever since we walked into this room you've been behaving like a self-appointed public avenger!

#3 [Loud.]: I'm telling you now! Shut up!

#8: You want to see this boy die because you personally want it, not because of the facts.

#3 [Roaring.]: Shut up!

#8: You're a sadist . . .

[The jury groups around #3 and #8.]

#3 [Roaring.]: Shut up! [And he lunges wildly at #8. #8 holds his ground as #3 is caught by many hands and held back. He strains against the hands, his face dark with rage.] Let me go! I'll kill him! I'll kill him!

#8 [Calmly.]: You don't really mean you'll kill me, do you?

\* \* \*

#### NOTE 4.

HARRY KALVEN, JR. AND HANS ZEISEL  
THE AMERICAN JURY\*

\* \* \*

The jury study has had one special burden to bear along with all the customary difficulties of large-scale research. At one point one of its research approaches generated a national scandal. As one of several lines of approach, it

\* Boston: Little Brown and Co., vi-vii (1966). Reprinted by permission.



was decided to obtain recordings of actual jury deliberations, partly to learn whether post-trial interviews with jurors permit reconstruction of the events of the jury room. The move was undertaken, with the consent of the trial judge and counsel, but without the knowledge of the jurors, in five civil cases in the federal district court in Wichita, Kansas. Although extensive security measures were taken to insure the integrity of the effort, when the fact became public in the summer of 1955, there followed public censure by the Attorney General of the United States, a special hearing before the Sub-Committee on Internal Security of the Senate Judiciary Committee, the enactment of statutes in

some thirty-odd jurisdictions prohibiting jury-tapping, and for a brief, painful moment, widespread editorial and news coverage by the national press.

None of the Wichita data are included in this book, nor will they be included in future books. We note the episode here simply to make clear to that man who would say, "That's all very interesting, professors, but did you ever hear a real jury deliberate?" that the answer is Yes, and to point out that one of the distinctions of the jury study is that it is a research project that has a Purple Heart.

\* \* \*



## CHAPTER THREE

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# The Impact of Societal Dynamics on Human Experimentation— Extending Knowledge, Risking Life, and Relying on Professional Authority

The Jewish Chronic Disease Hospital and Wichita Jury Recording cases provoked intense, though short-lived, debates about the nature and extent of the authority of investigators to intervene in subjects' lives, to expose them to risks, and to tamper with the fabric of professional and societal institutions. In partial defense to considerable criticism, the investigators argued that the need to acquire knowledge for the benefit of individuals and society required such interventions. More specifically, the scientists believed that the injection of "cancer cells" in debilitated subjects and the recording of jury deliberations were the next logical experiments that needed to be performed and that, as the experts in these fields, they had no choice but to carry out their research despite the risks involved.

The materials in this part are designed to explore these assertions by focusing on three issues which surfaced repeatedly in the arguments about the propriety of the two experiments: (1) the need of individuals and society continually to extend the frontiers of knowledge and technology; (2) the inevitability of injury to life and limb in all human endeavors; and (3) the need and wish to rely on the expertise of professionals. As the case studies sug-

gest, however, three opposing motivations which also have deep roots in man and society brake these forces: (1) man's fear of the unknown and his desire to perpetuate the status quo; (2) man's belief in the paramount value of every human life; and (3) man's desire to control those decisions which affect his life.

These conflicts, between man and man as well as man and society, are presented in this section in their broadest possible context. They will recur, in varying guises, throughout this volume. The tension between self-determination and delegation of authority to experts, for instance, underlies much of the analysis of the role and authority of experimental subjects presented in Part Three. The three forces are highlighted in this chapter because a better perspective on the remainder of the book can be achieved through a prior appraisal of society's overall attitudes toward the acquisition of knowledge, the balancing of risks and benefits, and the reliance on professional authority.

## A.

### Man's Quest for Knowledge and Mastery

The assertion has been made that two basic characteristics underlie the origin and growth of science and technology: "the need to control the workings of nature for our welfare and the simple, irreducible need to understand the world about us and ourselves."\* This proposition in turn raises a fundamental philosophical question: "What must nature, including man, be like in order that science be possible at all?"†

The materials in this section suggest that science and technology have always advanced in a relentless fashion, driven by scientists' "curiosity" and "urge for discovery" and by technologists' wish to satisfy human "wants" and "needs." Moreover, the concept of progress, which has had a powerful impact on the Western world ever since the seventeenth century, has reinforced the quest for the extension of knowledge and technology which is so manifest in contemporary society. These assumptions raise questions about the rationality of attempting to impose controls on science and technology in general and on human experimentation in particular.

In studying these materials, consider the following questions:

1. What values does man seek to maximize by scientific and technological advances? Are these values in conflict with other individual and societal values?
2. Can and should science and technology be controlled?
3. What are the distinctions between science and technology, and how relevant are they to resolving the problems raised by human experimentation?
4. What consequences should follow once investigations move from theory to studies on inanimate objects and then to experimentation with animals, investigators themselves, other individuals, groups, or society at large?

\* J. A. Mazzeo: *The Design of Life*. New York: Pantheon Books xiii (1967).

† T. S. Kuhn: *The Structure of Scientific Revolutions*. Chicago: The University of Chicago Press 172 (1962).

## 1.

**Curiosity and Necessity**

## a.

**Basic Human Characteristics?**

George Sarton  
*A History of Science\**

When did science begin? Where did it begin? It began whenever and wherever men tried to solve the innumerable problems of life. The first solutions were mere expedients, but that must do for a beginning. Gradually the expedients would be compared, generalized, rationalized, simplified, interrelated, integrated; the texture of science would be slowly woven. The first solutions were petty and awkward but what of it? A *Sequoia gigantea* two inches high may not be very conspicuous, but it is a *Sequoia* all the same. It might be claimed that one cannot speak of science at all as long as a certain degree of abstraction has not been reached, but who will measure that degree? When the first mathematician recognized that there was something in common between three palm trees and three donkeys, how abstract was his thought? Or when primitive theologians conceived the invisible presence of a supreme being and thus seemed to reach an incredible degree of abstraction, was their idea really abstract, or was it concrete? Did they postulate God or did they see Him? Were the earliest expedients nothing but expedients or did they include reasonings, religious or artistic cravings? Were they rational or irrational? Was early science wholly practical and mercenary? Was it pure science, such as it was, or a mixture of science with art, religion, or magic?

Such queries are futile, because they lack determination and the answers cannot be verified. It is better to leave out for the nonce the consideration of science as science, and to consider only definite problems and their solutions. The problems can be imagined, because we know the needs of man; he must be able to feed himself and his family, to find a shelter against the inclemencies of the weather, the attacks of wild beast or fellow men, and so on. Our imaginations are not arbitrary, for they are guided by a large number of observed facts. To begin with,

archaeologic investigations reveal monuments which help us to realize the kind of objects and tools that our forefathers created and even to understand their methods of using them, and to guess their intentions. The study of languages brings to light ancient words which are like fossil witnesses of early objects or early ideas. . . .

In order to simplify our task a little, let us assume that the primitive men we are dealing with have already solved some of the most urgent problems, for otherwise their very existence would have remained precarious, not to speak of their progress, material or spiritual. Let us assume that they have discovered how to make a fire and have learned the rudiments of husbandry. They are already—that is, some of them are—learned people and technicians, and they may already be speaking of the good old days when life was more dangerous but simpler and a man did not have to remember so many things. . . .

\* \* \*

Let us consider rapidly the multitude of technical problems that early men had to solve if they wished to survive, and, later, to improve their condition and to lighten the burden of life. They had to invent the making of fire and experiment with it in various ways. Not only the husbandman but also the nomad needed many tools, for cutting and carving, flaying, abrading, smoothing, crushing, for the making of holes, for grasping and joining. Each tool was a separate invention, or rather the opening up of a new series of inventions, for each was susceptible of improvements which would be introduced one by one. In early times there was already room for key inventions, which might be applied to an endless group of separate problems and which ushered in unlimited possibilities. For example, there was the general problem of how to devise a handle and how to attach it firmly to a given tool. Many different solutions were found for that problem, one of the most ingenious being that of the Eskimos and Northern Indians, namely, the use of babiche (strings or thongs of rawhide) by means of which the tool and handle are bound together; as the hide dries it shrinks almost to half its length and the two objects are inseparable. A tighter fit could hardly be obtained otherwise.

The husbandman had to discover the useful plants one by one—plants to use as food, or as drugs, or for other domestic purposes—and this implied innumerable experiments. It

\* Cambridge: Harvard University Press 3-5, 16-17 (1952). Reprinted by permission. Copyright 1952, by the President and Fellows of Harvard College.

was not enough for him to discover a plant; he had to select among infinite variations the best modalities of its use. He had to capture animals and to domesticate the very few that were domesticable, to build houses and granaries, to make receptacles of various kinds. There must have been somewhere a first potter, but the potter's art involved the conscious or unconscious coöperation of thousands of people. Heavy loads had to be lifted and transported, sometimes to great distances. How could that be done? Well, it had to be done and it was done. Ingenious people invented the lever, the simple pulley, the use of rollers, and later, much later, that of wheels. A potter of genius applied the wheel to his own art. How could a man cover his body to protect it from the cold or the rain or the burning sun? The use of hides was one solution, the use of leaves or bark another, but nothing equaled the materials obtained by the weaving of certain fibres. When this idea occurred to a great inventor, the textile industry was born. . . .

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Some readers will object that whatever knowledge there was, was purely practical, empirical, too raw and rough to deserve the name of science. Why should we not call it science? It was a very poor science, very imperfect, yet perfectible; our science is decidedly deeper and richer, yet the same general description applies to it—it is very imperfect yet perfectible. Or one might say: There was no pure science. Why not again? How pure must science be to be called pure? If pure science is disinterested science, knowledge obtained for its own sake without thought of immediate use, surely the early astronomers were, or might be as pure as our own. It is possible that astrologic fancies had already developed, but it is equally possible that they had not, for that would have implied a degree of sophistication which those astronomers had not yet reached. Their main reason for observing the strange behavior of certain planets may have been simply curiosity.

Curiosity, one of the deepest of human traits, indeed far more ancient than mankind itself, was perhaps the mainspring of scientific knowledge in the past as it still is today. Necessity has been called the mother of invention, of technology, but curiosity was the mother of science. The motives of primitive scientists (as opposed to those of primitive technicians and shamans) were perhaps not very different from those of our contemporaries; they varied consid-

erably from man to man and time to time and then as now covered the whole gamut from complete selflessness, reckless curiosity, and spirit of adventure down to personal ambition, vainglory, covetousness.

If research had not been inspired and informed from the beginning by a certain amount of disinterestedness and adventurousness, and by what its enemies would later call indiscretion and impiety, the progress of science would have been considerably slower than it was. The amount of knowledge attained by some primitive men can be deduced from anthropologic records and also from the amount observable in the most ancient civilizations. When man appears on the scene of history, we find him already a master of many arts, expert in many crafts, as full of lore as of cunning.

Then as now the true scientist, even as the true artist, was likely to be or to seem a bit queer and secretive; it is highly probable that his more practical neighbors already made jokes about his absent-mindedness. Of course, he was not more absent-minded than they were, but their minds were focused on different interests. He was engrossed in his own reflections; his motives being less tangible, his life seemed mysterious. Sometimes he may have wished for praise and recognition, or he may already have discovered that such praise was futile and that it was better not to try for it. If he were selfish and jealous, the primitive inventor might prefer to keep his new idea—say a better hook, or a better ax, or better materials for the making of either—to himself and his family. In almost every case the scientist or the inventor tended to be reticent. The growth of science was always entangled in psychologic and social accidents.

Not only was the development of primitive invention somewhat confidential and secret, it was also of necessity antagonistic to the regular habits and traditions that it tended to subvert. Every invention, however useful it may turn out to be (and it cannot be useful before it is used), is disturbing, and the more pregnant it is, the more disturbing. There were vested interests in prehistoric times as well as now, though they could not be described in exactly the same way and were perhaps less blatant. There was, then as now, a strong inertia impeding progress, the inertia of habit and complacency, distrust and contempt of everything novel or foreign. That inertia, however, was not simply a hindrance, but a necessity, like a flywheel or a brake, to steady and warrant mankind's invasion of the

unknown. Men's resistance to new tools or new-fangled ideas was useful, because novelties should be thoroughly tested before being adopted. Every accepted tool was the fruit of a very long process of trial and error, of a very long tussle between inventors, innovators, reformers at one end and conservatives at the other. The latter were far more numerous; the former were more enthusiastic and aggressive.

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## NOTES

### NOTE 1.

SIGMUND FREUD  
THREE ESSAYS ON THE THEORY OF  
SEXUALITY (1905)\*

\* \* \*

At about the same time as the sexual life of children reaches its first peak, between the ages of three and five, they also begin to show signs of the activity which may be ascribed to the instinct for knowledge or research. This instinct cannot be counted among the elementary instinctual components, nor can it be classed as exclusively belonging to sexuality. Its activity corresponds on the one hand to a sublimated manner of obtaining mastery, while on the other hand it makes use of the energy of scopophilia. Its relations to sexual life, however, are of particular importance, since we have learnt from psychoanalysis that the instinct for knowledge in children is attracted unexpectedly early and intensively to sexual problems and is in fact possibly first aroused by them.

It is not by theoretical interests but by practical ones that activities of research are set going in children. The threat to the bases of a child's existence offered by the discovery or the suspicion of the arrival of a new baby and the fear that he may, as a result of it, cease to be cared for and loved, make him thoughtful and clear-sighted. And this history of the instinct's origin is in line with the fact that the first problem with which it deals is not the question of the

distinction between the sexes but the riddle of where babies come from. . . .

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### NOTE 2.

A. V. HILL  
EXPERIMENTS ON FROGS AND MEN\*

Man is an inveterate experimenter. Those of us who have been small boys ourselves, or indeed are still small boys, will know what joy is found in taking an old alarm clock to bits or a bicycle to pieces, in seeing how fast we can run a hundred yards, in breeding rabbits, pigeons, or canaries, in fixing wireless apparatus together, or, when we are older, in trying a new kind of oil or petrol or even a new medicine. Boys and men, however, also girls and women, are not the only experimenters, as any who have watched a kitten or a parrot will know; and experiments made by monkeys have been scientifically studied. Man, however, is the chief experimental animal, both as experimenter and as subject. Indeed, in many of man's most joyful adventures he acts in both capacities; he makes experiments upon himself, often to his own great danger or discomfort.

To run in a Marathon race or to try to swim the Channel, to see how far one can ride a bicycle in 24 hours, to climb to 20,000 feet, to set out to walk (or to fly) to the South Pole, to make a height record in an aeroplane, to dive under the sea, all these involve trials and experiments upon oneself; which is one reason why so many apparently useless feats are performed. Every new adventure on which man has embarked throughout the ages, every change in his social, economic and political condition, has meant experiments upon his bodily frame and organization, experiments sometimes successful but often followed by disaster.

In learning the use, treatment, and preservation of food he must, unwittingly often, have made millions of experiments upon himself, thousands of them extremely unpleasant, many of them fatal. Without these experiments, however, the present order of civilization, depending as it does upon a regular supply of food, would have been impossible. When he set out to journey on the sea he experimented on sea-sickness, and later on, as his journeys lengthened, on scurvy and the need of vitamins. When he deserted a natural diet and gathered together in

\* James Strachey, ed.: *The Standard Edition of the Complete Psychological Works of Sigmund Freud*. London: The Hogarth Press and the Institute of Psychoanalysis 194-195 (1953). Reprinted by permission of Sigmund Freud Copyrights Ltd., The Institute of Psychoanalysis, the Hogarth Press, Ltd. and Basic Books, Inc.

\* 2 *The Lancet* 261 (1929). Reprinted by permission.

cities he experimented on nutrition and the physiological effects of radiation (or its absence), with rickets as a curious result. When he began to dig deep tunnels, or to work in diving bells or diving suits, he discovered that the physical solubility of gases in his blood and tissues may affect his well-being, and he invented caisson disease. When he climbed high mountains, or went up in balloons, he discovered mountain sickness, and acclimatization to it. When he took to rapid manoeuvres in aeroplanes he found out that the human factor is a limiting one, that violent acceleration—"centrifugal force"—may play havoc with his circulation and render him suddenly unconscious. Labouring in hot mines, in extremes of climate, with excess or deficiency of sunlight; living on sterilized, preserved, or purified food; breathing quartz dust or carbon monoxide; working with materials which exert a chronic irritation on the skin, or with ultra-violet light, or with X rays and radium; in all such experiments he found limitations to his independence of his external environment; he made experiments upon himself and others, experiments involving ill-health, disaster, and death to many. Even apart from disease, from the experiments which Nature wantonly insists upon making on us, we cannot avoid making experiments on ourselves if we are to do anything new; and, even if we do nothing new, we shall probably find we must make experiments still to discover how to remain as we are.

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## NOTE 3.

DEREK J. DE Solla PRICE  
SCIENCE SINCE BABYLON\*

\* \* \*

[T]he motivation for research may be an intellectual itch—indeed, the purpose of education has been defined as the business of making people uncomfortable, making them itch—but a deeper and more specific urge may have made these persons into scientists. By far the most common inner reason is that as youngsters they have wanted to be a Mr. Boyle of the Law. They seek an immortal brainchild in order to perpetuate themselves. In an age of teamwork amongst scientists, of little men working on big machines, this hallowed form of eponymic immortality is becoming insecure, and the image of really great

men and their theories has become more precious. If, however, this is becoming a problem, there is surely all the more reason to examine the process that made it possible, during the Scientific Revolution, for men to fashion bricks of science inscribed with their own names and build up, faster than ever before, an imposing edifice and superstructure of theory and experiment.

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## NOTE 4.

DEREK J. DE Solla PRICE  
THE SCIENCE OF SCIENTISTS\*

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[S]cience seems to be its own sweet beast. It has a life and an order all of its own, intransigent to human will and desire, impervious to national origins and philosophical frameworks, unresponsive to the wishes and fears of society, sensitive only in the shortest perspective to happy accidents and creative geniuses, and responsive only parochially and temporarily to most stimuli of support and lack of support. I have, you will realize, exaggerated all these things for dramatic effect, but the general line permeates all we know of the history of science and many of the working judgments of scientists are predicated along these lines.

To put it all in another way, there is only one world to explore and discover. Moving to another analogy, there is only one proper way (at any given time) to fit together the pieces of the jigsaw puzzle. And for a last piece of picturesque theorizing about the particular go of science, one has to pick the fruit of the tree of knowledge, piece by piece, in its due ripeness.

To substantiate these generalities a little, consider the peculiar supranationality of science: Budapest and Delhi must produce the same physics as Moscow and Boston and seek the same ultimate scientific Nirvana, however different their tastes in music and philosophy and their goals in politics or economics. Temporary local deviances may exist from time to time at the research front, as when newtonians and cartesians do battle, or when a school follows Lysenko, but in the end it turns out that either you do science the one right way or you cannot pursue it at all.

Consider also the strong anticonstructionist feeling one has about scientific laws: that there is only one world there. If Crick and Watson had

\* New Haven: Yale University Press 48 (1962). Reprinted by permission.

\* 1 *Medical Opinion and Review* 88, 89-90 (1966). Reprinted by permission.



not existed, the same work would have to have been done by others; if Planck had not found his constant, it would merely have borne another name. Admittedly, there are some difficult questions—if oxygen was an element that had to be discovered early in the game, is it also true that phlogiston was similarly necessary at the time?

But in the rest of the world there is none of this simplicity: if Beethoven had not been, a unique contribution would have been lost and music might have taken a quite different path, and if Cleopatra had had a long nose or the Japanese had developed an atomic bomb, the entire course of world history could have been different.

Science seems to be so strongly ordered in all its objective certainty that one has very little chance to decide what shall be done next. One cannot, of course, predict at any time, but with all the dangers of 20/20 hindsight one can see that we can affect the order of the God-given sequence of discoveries but a little and can make only local effects in deciding that the law was due to Boyle and not to Hooke or Marriotte. Such things may be very important to Messrs. Boyle and Marriotte, or to their countries, but on the whole the juggernaut of science is little swayed.

Precisely because of this heavily impersonal inevitability of the scientific machine, and not in spite of it, we historians of science have been able to say meaningful things about the behavior of individual scientists and the forces that move them. One can see that Planck has deeper problems in attaching his name to a constant than Beethoven to a symphony. If the jigsaw puzzle is not yet ready for the piece you want to fit, no amount of inducement and support will fit it.

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b.

### **Can Science and Technology Be Distinguished?**

[i]

*Jerome B. Wiesner  
Technology and Society\**

\* \* \*

I would like to clear up . . . the ambiguity that exists between technology and scientific research, their objectives and their methods. This confusion is by no means surprising since modern technology has become highly dependent

upon basic scientific knowledge for much of its progress. In turn, scientific research in many fields is only possible because of the elaborate and sensitive tools that technology has made possible. The vast and powerful particle accelerators, the electron microscope with which to explore the world of cells and viruses, and the electronic computer to calculate problems which only a few years ago were beyond the scope of human comprehension, are but three of a large number of scientific tools which have extended enormously our ability to measure, observe, and understand the world around us.

This close alliance between science and technology, though relatively new, is so complete that the average person, and indeed many scientists and engineers as well, fail to distinguish any difference between them.

In the beginning, technology did not depend upon science. The inventions that provided the basis for the industrial revolution—the steam engine, the loom, the lathe, and many other machines—were invented by practical men and based upon art, observation, and common sense. In the first stage of industrialization man was exercising his ingenuity in the exploitation of the things he found around him. The factory with its power machines, its use of unskilled or semi-skilled labor doing simple repetitive operations, the utilization of raw materials like iron, coal, copper, etc., improvements in transportation growing from the development of the railroad and the steamboat are all examples of this inventiveness. Most important, of course, was that with the introduction of machines he had begun a continuing process of extending human capabilities, first by augmenting muscle power through harnessing the almost limitless energy sources found in nature, later by speeding communications by electrical means, and most recently by augmenting mental activities by the introduction of computing machines to replace human effort in menial, repetitive activities and to assist in difficult or lengthy calculations.

The fact that scientific research had little or no effect on early technology does not mean that scientists did not exist or were not working. They did and were, and during the period of the industrial revolution the foundations were laid for modern physics, chemistry, and biology. However, it was not until the middle of the nineteenth century that extensive practical use was made of the accumulating scientific knowledge. Only then did men begin to exploit the available knowledge of chemistry and electricity for useful purposes.

\* Harry Woolf, ed.: *Science As a Cultural Force*. Baltimore, Md.: The Johns Hopkins Press 37-40 (1964). Reprinted by permission.

Chemists learned to synthesize organic materials and set up research laboratories for obtaining the new knowledge required to meet their applied objectives. It was in the field of chemistry that research methods were first applied in a systematic manner to develop new products. The application of electricity was more haphazard in the beginning. The scientific observations of Gilbert, Henry, and Maxwell were seized upon by the inventors of the electric motor, the electric generator, the telegraph, telephone, and other devices. Not until the end of the nineteenth century were research methods applied to the exploitation of electrical phenomena, first by Thomas Edison who, in reality, was more of an inventor than a scientist, and later by many technologists in the laboratories of such industries as the General Electric Company and the predecessors of the American Telephone and Telegraph Company. Thus, it was in these fields—chemistry and electricity—that the merger of scientific inquiry and technology first occurred, that the power of the scientific methods was applied to solving useful problems, and that the great value of the thorough understanding of physical phenomena was demonstrated. In exploiting electrical phenomena technologists deal with fields and electrons and waves which can only be observed indirectly and understood through scientific research. It is not surprising, therefore, that scientifically based industry, like the electronics industry which depends very heavily upon basic research, should be the sponsor of much fundamental research.

Modern technology still requires invention. The vacuum tube, the transistor, memory devices for computers, and new materials tailored with specific properties are all inventions. But they are inventions made by men with special knowledge who have an understanding of a scientific field and who base their inventions on an intimate familiarity with that field, just as the inventor of old called upon his first hand experience of the world he could see and feel to provide the working substance of his ingenuity.

This is the nature of modern, scientifically based technology. Clearly, the first requirement is the existence of a body of scientific knowledge. To use this knowledge as the basis for an invention in the solution of a specific problem, the technologist must have good understanding of the underlying science. Also, more likely than not, as he converges on the development of a specific device, the technologist will find that he is handicapped by the fact that the scientists who first explored the field that he is exploiting

left vast areas of ignorance which must be filled before his task can be completed. These can only be filled by doing further fundamental research. Because the specific knowledge required to solve a problem is its goal, such work is often called "applied" or "directed research," though it is obvious that in another context it would be regarded as fundamental or basic research.

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[ii]

Norman W. Storer

*The Internationality of Science and the Nationality of Scientists\**

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[W]e are concerned with the fundamental nature of the "energy" that keeps science going and with the ways in which it is channeled so that scientific activity can continue. Because science is essentially an intellectual activity rather than one that depends directly upon the exploitation of physical energy, we must agree in the beginning that this energy must be motivational in character; we bring it to mind when we ask why scientists want to engage in research and the other activities associated with it. And we must agree that its structuring is determined by the special set of norms and values that distinguish science from other sectors of society.

This definition of the problem has provided the framework for much of the "basic" research on science, beginning with Robert K. Merton's pioneering essays. . . .

. . . Merton pointed out . . . that professional recognition, the celebration by colleagues of one's scientific achievements, is the single most appropriate and legitimate reward achievement in science. Through the analysis of a series of disputes over priority in scientific discovery, . . . Merton was able to demonstrate that the receipt of professional recognition, earned by being the first to discover something, is indeed of central importance to the scientist's motivation. Even if he is reluctant to admit it, the scientist yearns for indications that his work has been accepted by his colleagues as valid and significant—indications that may range all the way from being mentioned in a footnote to being awarded a Nobel Prize. This is not to say that all research is done simply in order to gain recog-

\* 22 *International Social Science Journal* 80, 83-85, 89-90 (1970). Reprinted by permission of UNESCO.

nitition, but that without such feedback much of the desire to engage in research would quickly dwindle.

Why the scientist should want professional recognition is a question that has not yet been fully resolved. There are two major hypotheses at present which attempt to explain this. First, there is the proposal that the scientist is trained to want recognition during his apprenticeship in science because it certifies that he has satisfied the demanding requirements of his role: he has advanced our knowledge of some aspect of reality. A complementary hypothesis contends that the desire to create, to produce "meaningful novelty," is a basic human need and that the act of creation is not complete without the receipt of competent response to it from others. The discovery of a regular relationship between physical phenomena is a type of creativity, especially since it must be described in words or mathematical equations if it is to take its place in the body of scientific knowledge, and the person responsible for it needs the affirmation of his peers that his creation is valid and meaningful. In science, a positive response to the product of one's creativity constitutes professional recognition—and even a negative response is to be preferred over no response at all. In other contexts a person may desire affirmation of the beauty or cleverness or practical utility of what he has created, but the basic need for competent response seems to be the same in all forms of creativity.

Regardless of why the scientist desires professional recognition, it is possible now to assert that the desire for it is the normatively appropriate motive of the scientist—even though specific individuals may also find a variety of other rewards for engaging in research. This central assertion is supported by indirect evidence of several types, and the primary objection to it that remains is the problem of its apparent conflict with the idea that the scientist is disinterested, altruistic, and entirely unconcerned with fame.

Again, two different but complementary explanations have been offered for scientists' reluctance to admit their interest in receiving professional recognition. (The numerous incidents of priority conflict, together with other data showing the large proportion of scientists who admit to an occasional worry about "being scooped," are sufficient to dispose of the contention that they don't really care about this.) One explanation is that there is another norm in science which calls for humility and works to make the scientist deny his interest in receiving any

sort of reward for his achievements. The other is that since professional recognition is worthless if it is not objective—it is supposed to represent Mother Nature's judgment of the validity and significance of one's discovery, not the discoverer—the scientist hesitates to admit his interest in professional recognition because this might lead to his colleagues' bestowing it as a favor to him rather than as an impersonal evaluation of his work.

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The description of science given . . . is of course appropriate primarily to what is called basic research—the disinterested quest for new, universally valid knowledge which is sought without regard for its possible relevance to the solution of practical problems. Applied research, on the other hand, is oriented directly or indirectly to solving "real" problems. The importance of this distinction for our purposes is that the empirical problems which beset men as physical beings, in contrast to the theoretical questions which intrigue them as intellects, are usually localized in space and time. They are related to national interests rather than scientific interests.

A problem that exists in one part of the world need not exist in other parts or at other times, so that a solution to it tends to lack the universality that characterizes answers to basic scientific questions. This means that if scientists' interests were to be guided solely by practical concerns, the consensus on what constitutes important scientific questions—which provides the foundation of the international scientific community—would immediately be shattered. And since their problems would not only be peculiar to places and times but also identified by criteria which vary from one culture to the next, the scientists of a given nation would be neither interested in nor able to contribute to the work of scientists in other nations.

Further, empirical problems do not arise in logical sequence as do the theoretical questions that occupy basic scientists, so applied research has relatively little potential for building a cumulative, generalized body of knowledge. Only coincidentally would one scientist's research have meaningful implications for another's research, so the chances for obtaining competent response to one's work from others would be drastically reduced. There is thus a considerably smaller scientific audience for achievements in applied research, and greatly diminished opportunity for the applied researcher to gain the kind

of immortality that can be gained through fundamental scientific discoveries.

This means that the applied researcher must look more to non-scientists for rewards than to his colleagues, and these rewards must be something other than the competent response to creativity which is the normatively appropriate reward. They are usually money and sometimes public adulation, neither of which requires that the giver really understand what the scientist has done. The applied researcher must thus violate the norm of disinterestedness at the same time that he makes it more difficult for him to participate fully in the activities of the scientific community, and he is therefore viewed by basic scientists as threatening the moral rightness and stability of their entire enterprise. For this reason there are appreciable pressures on the young scientist not to engage in applied research. The invidious distinction between basic and applied research is obvious to any advanced graduate student and he is ordinarily trained to seek the more prestigious type of work and the sort of position in which this can be carried out with maximum facility.

Yet society's interest in encouraging and supporting science must rest ultimately on the assumption that this support will be eventually repaid in the form of solutions to pressing problems. Thus, despite the fact that it is basic research which provides the context, or the specialized universe of discourse within which the applied scientist works, there are often demands that the scientist devote himself entirely to applying his skills to one or more of the problems that his society has defined as needing solution. . . .

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## 2.

### The Impact of Values

#### a.

#### Search for Truth?

*Philip Handler  
An Interview\**

NEWS REPORT: Do you feel any constraints should be placed on fundamental research if

there is some reason to believe that the results of that research might be harmful to society?

HANDLER: No. No constraints. Let me give you a dramatic illustration. You may know of the demonstration that one can take a fertilized frog egg, discard its nucleus and insert a nucleus from a somatic cell of some other frog, and the egg develops into a frog which is an absolutely perfect twin of the donor frog—the one that provided the transplanted nucleus. Presumably, by that technique we would make an indefinite number of perfect copies of that donor frog. It's merely a matter of time before we can switch from frogs to mammals. When we have the biological technology, we should be able to make perfect copies of the best bull or greatest cow in the world, in whatever number may be desired. This could go a long way toward improving world food production. Obviously, the next step would be man, and perhaps one day we may be able to make copies of a Bunche, Rabi, or Schirra, or of an Eldridge Cleaver, or Lew Alcindor or of any other genotype identifiable in our population.

I hope that day never comes. I can't imagine any more dangerous tool in the hands of an autocratic, dictatorial, authoritarian government. It would be the most powerful mechanism ever devised—the ultimate despoliation of the human race, degradation of the worst order. We could create an ant-like society that is utterly repugnant. The idea of exploring all of the remarkable variety in the human gene pool is far more attractive. There are all kinds of people we have yet to produce. The human gene pool is colossal in its potential variety. The other idea leads to disaster.

And yet I think there is no alternative but to go down this trail and do the biological experimentation that, one day, may offer this kind of a capability. The idea that, since we can see this end possibility, we should by fiat state, "Thou shalt not in thy laboratory do any experiment which leads down that trail," is an equally repugnant thought. That kind of censorship is as repugnant as censorship of literature in other communities and is as potentially damaging. No constraints. Utilization of scientific information is a political and social decision and we have mechanisms in our society for arriving at such decisions. Let's use those. But no constraints on dissemination of facts, the ideas people are allowed to think, or the search for scientific understanding.

Historically, there isn't much we can do in

\* 19 *News Report of the National Academy of Sciences* 9 (March 1969). Reprinted by permission of the National Academy of Sciences.

the future that can compare to the shocks of the past. Consider the Copernican revolution, which converted this earth from the center of the universe to a tiny planet around a sun that is only a little star in the cosmos. Consider the idea of evolution which traces us to nothingness, in effect, so that man's image of himself as something created in the image of his maker has been destroyed. It is hard to think of anything that we are likely to do in the future which can conceivably be as traumatic an alteration of man's view of himself, his position in the general scheme of things as what has already happened. If we can live with these concepts, we can live with almost any other kind of information. Censorship of physics from now on can never expunge understanding of how to build nuclear weapons; censorship of biology will not abolish the knowledge which is required for biological weapons. The search for truth is man's noblest pursuit. Surely man's mind can live comfortably with the knowledge and understanding so gained without damage to man. And what better basis is there for the moral imperatives which guide our society?

## NOTES

### NOTE 1.

KARL W. DEUTSCH  
SCIENTIFIC AND HUMANISTIC KNOWLEDGE  
IN THE GROWTH OF CIVILIZATION\*

... Science itself depends for its life on the prior acceptance of certain fundamental values, such as the value of curiosity and learning, the value of truth, the value of sharing knowledge with others, the value of respect for facts, and the value of remembering the vastness of the universe in comparison with the finite knowledge of men at any particular moment. Historically, such values have been held by outstanding scientists. One thinks of P. W. Bridgman's well-known dictum that—"in the face of the fact, the scientist has a humility almost religious"; or of Newton's description of his own work as the play of a child with pebbles on the shores of the ocean of knowledge, or his reference to the sharing of knowledge with others by describing his own achievements as being due to

his having stood "on the shoulders of giants." Beyond such evidence, it could perhaps be shown that the cumulative work of science could not go on if any of the values just listed were rejected.

As science rests on certain values, so do almost all values depend on knowledge, and thus to some extent in turn on science, if they are to proceed from the realm of words to that of action. This implies a circular chain of causation or a feedback process, as do many processes of social and cultural development. To act morally is in one sense the opposite of acting blindly. It is acting in the presumed knowledge of what in fact it is that we are doing. Almost every significant action of this kind implies serious assumptions in some field of science. To love one's neighbor requires at the very least that we find out where and who our neighbor is. If we are to respond to his needs we must first ascertain what his needs are and what action in fact is likely to be helpful to him. ...

... If we evaluate an action as good on the basis of our mere surmise of the good will of its doer, therefore, we may find ourselves forced to assume that such subjective good will—as in the Kantian Imperative—must include by implication also the will to gain and apply the best available knowledge of the probable consequences of the action chosen. The duty to have good intentions, in other words, is meaningless without the duty to try to know the facts and try to foresee correctly the consequences of one's deeds, and it is this latter duty which may distinguish in practice the responsible from the irresponsible statesman, or the well-intentioned doctor from the well-intentioned quack.

Attempts at hermetic separations of science from values are thus bound to fail. Science without at least some values would come to a dead stop; ethics without at least some exact and verifiable knowledge would be condemned to impotence or become an engine of destruction. Much of the anxious discussion of international politics between statesmen and atomic scientists, or between the so-called schools of "idealism" and "realism" among political writers, hinges upon the discrepancy between the strength of the moral convictions involved and the poverty of reliable knowledge of the probable consequences of the proposed courses of action.

The relationship of science to values thus implies a double question: the mutual interrelation of science and the general values of a civilization; and the relationship of a specific state of scientific knowledge to the pursuit of

\* Harcourt Brown, ed.: *Science and the Creative Spirit*. Toronto: University of Toronto Press 18-21 (1958). Reprinted by permission of University of Toronto Press. Copyright, Canada, 1958 by University of Toronto Press.

specific purposes or policies. The first of these problems, the general relationship of science and value, and thus to some extent of truth and goodness, leads us close to the heart of every civilization within which it is examined. If conceived as mutually incompatible, science and values may frustrate or destroy each other, dragging their civilization towards stagnation or decline. As a mutually productive and creative partnership, science and values may succeed in strengthening each other's powers in a self-enhancing pattern of growth, rendering their civilization increasingly open and able to learn from the hopes and dreams of the individuals within it, as well as from the universe around it.

This general vision of a mutually beneficial partnership becomes increasingly difficult to retain, however, as we proceed from the consideration of the growth of civilization on the grand scale to the effect of the timing of particular discoveries or innovations upon specific policies at specific times and places. Would it have been better for mankind if Einstein's principle of relativity, or Chadwick's discovery of the neutron, or Hahn's work on uranium fission had all come ten years later than they did, and no atom bomb had been available to drop on Hiroshima? Perhaps the most useful consideration in the face of questions such as these might be to realize the impossibility of foreseeing the ultimate consequences of even the smallest scientific or technological advance, as well as the inexhaustibility of most or all of the great contributions. Benjamin Franklin's answer to the question "What is the use of a scientific discovery?" consisted in asking the counterquestion "What is the use of a baby?" Just as it seems impossible to foretell the eventual good a child may do, so it is impossible to foretell what evil he may do, and our whole attitude to children is in a sense based upon the bet that the good they do will far outweigh the evil. In civilized countries we have long ago abandoned the discussion which sometimes still echoes in mythology, whether a certain child should have been killed at birth in order to forestall the harm he did in adult life. Rather we have come to center our attention on providing a family and an environment for him in which love will outweigh hate, and in which his opportunity for free and friendly growth will be the best.

If there is merit in Benjamin Franklin's argument, we might similarly decide to bet on the potential goodness rather than on the potential evil of knowledge, and concentrate on providing

a human and social environment for science in which its constructive possibilities are likely to be realized. It is possible, of course, to imagine extreme situations for some times and places in which the short-term potentialities for destruction might seem so great in the case of a particular invention or discovery, and the prevailing political régime might seem so unlikely to avoid its suicidal misuse, that a policy of temporarily restricting, delaying, or withholding such knowledge might appear as the least of several likely evils for the time being. Even granting all these assumptions, however, such a policy of fear of knowledge would have to be viewed as extremely transitory and exceptional in any modern technological civilization that is to continue to advance or indeed to survive. A civilization so prone to commit suicide that it could be saved only by concealing from it the means of its own destruction would not endure for long. Rather, for the long run and for most conditions that are likely to occur, we might do better to adopt the opposite assumption: that any modern civilization that is to endure will have to learn how to live with its new knowledge of its vast means of destruction.

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#### NOTE 2.

BENTLEY GLASS  
THE ETHICAL BASIS OF SCIENCE\*

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[S]cience is more than the instrument of man's increasing power and progress. It is also an instrument, the finest yet developed in the evolution of any species, for the malleable adaptation of man to his environment and the adjustment of his environment to man. If the human species is to remain successful, this instrument must be used more and more to control the nature and rate of social and technological change, as well as to promote it. In this sense, at least, science is far more than a new sense organ for comprehending the real relations of natural phenomena and the regularities we call "laws of nature." It is also man's means of adjustment to nature, man's instrument for the creation of an ideal environment. Since it is preeminently an achievement of social man, its primary function

\* 150 *Science* 1254, 1255, 1258, 1260-1261 (1965). Copyright 1965 by the American Association for the Advancement of Science. Reprinted by permission.

is not simply that of appeasing the individual scientist's curiosity about his environment—on the contrary it is that of adjusting man to man, and of adjusting social groups in their entirety to nature, to both the restrictions and the resources of the human environment.

\* \* \*

Those who distrust science as a guide to conduct, whether individual or social, seem to overlook its pragmatic nature, or perhaps they scorn it for that very reason. Rightly understood, science can point out to us only probabilities of varying degrees of certainty. So, of course, do our eyes and ears, and so does our reason. What science can do for us that otherwise we may be too blind or self-willed to recognize is to help us to see that what is right enough for the individual may be wrong for him as a member of a social group, such as a family; that what is right for the family may be wrong for the nation; and that what is right for the nation may be wrong for the great brotherhood of man. Nor should one stop at that point. Man as a species is a member—only one of many members—of a terrestrial community and an even greater totality of life upon earth. Ultimately, what is right for man is what is right for the entire community of life on earth. If he wrecks that community, he destroys his own livelihood. In this sense, coexistence is not only necessary but also right, and science can reveal to us the best ways to harbor our resources and to exploit our opportunities wisely.

. . . From the foregoing description of science as itself an evolutionary product and a human organ produced by natural selection, it may already be guessed that I do not adhere to the view that either the processes or the concepts of science are strictly objective. They are as objective as man knows how to make them, that is true; but man is a creature of evolution, and science is only his way of looking at nature. As long as science is a *human* activity, carried on by individual men and by groups of men, it must at bottom remain inescapably subjective.

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From the beginning the inveterate foe of scientific inquiry has been authority—the authority of tradition, of religion, or of the state—since science can accept no dogma within the sphere of its investigations. No doors must be barred to its inquiries, except by reason of its own limitations. It is the essence of the scientific mind not only

to be curious but likewise to be skeptical and critical—to maintain suspended judgment until the facts are in, to be willing always, in the light of fresh knowledge, to change one's conclusions. Not even the "laws" of science are irrevocable decrees. They are mere summaries of observed phenomena, ever subject to revision. These laws and concepts remain testable and challengeable. Science is thus wholly dependent upon freedom—freedom of inquiry and freedom of opinion.

\* \* \*

The scientist escapes lightly—instead of ten commandments only four: to cherish complete truthfulness; to avoid self-aggrandizement at the expense of one's fellow-scientist; fearlessly to defend the freedom of scientific inquiry and opinion; and fully to communicate one's findings through primary publication, synthesis, and instruction. Out of these grow the social and ethical responsibilities of scientists that in the past 20 years have begun to loom ever larger in our ken.

These may be considered under the three heads of proclamation of benefits, warning of risks, and discussion of quandaries. . . .

\* \* \*

The problem of the future is the ethical problem of the control of man over his own biological evolution. The powers of evolution now rest in his hands. The geneticist can define the means and prognosticate the future with some accuracy. Yet here we enter the third great arena of ethical discussion, passing beyond the benefits of science and the certain risks to the nebulous realm of quandaries. Man must choose goals, and a choice of goals involves us in weighing values—even whole systems of values. The scientist cannot make the choice of goals for his people, and neither can he measure and weigh values with accuracy and objectivity. There is nonetheless an important duty he must perform, because he and he alone may see clearly enough the nature of the alternative choices, including *laissez faire*, which is no less a choice than any other. It is the social duty and function of the scientist in this arena of discussion to inform and to demand of the people, and of their leaders too, a discussion and consideration of all those impending problems that grow out of scientific discovery and the amplification of human power. Science is no longer—can never be again—the ivory tower of the recluse, the refuge of the asocial man. Science has found its social basis



and has eagerly grasped for social support, and it has thereby acquired social responsibilities and a realization of its own fundamental ethical principles. The scientist is a man, through his science doing good and evil to other men, and receiving from them blame and praise, recrimination and money. Science is not only to know, it is to do, and in the doing it has found its soul.

## NOTE 3.

MICHAEL POLANYI  
PERSONAL KNOWLEDGE\*

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[S]cience may be once more discredited, as it was by St. Augustine, if it cannot avoid denaturing our conception of man. The appreciation of natural science is of recent origin and its tradition is rooted in a limited area. It is a single shoot of one civilization among many others of equal antiquity and richness. The Greeks never developed a systematic natural science, nor did Byzantium or China, despite their technological achievements. . . .

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Encircled today between the crude utilitarianism of the philistine and the ideological utilitarianism of the modern revolutionary movement, the love of pure science may falter and die. And if this sentiment were lost, the cultivation of science would lose the only driving force which can guide it towards the achievement of true scientific value. The opinion is widespread that the cultivation of science would always be continued for the sake of its practical advantages. It was expected, for example, that Lysenko's theories, if false, would be soon abandoned by the Soviet Government because they could produce no useful results. This expectation overlooked the fact that such questions cannot be decided in practice. Lysenko's theories are actually the theoretical conclusions which Michurin in Russia and Burbank in the U.S. derived from their substantial successes as plant-breeders. Almost every major systematic error which has deluded men for thousands of years relied on practical experience. Horoscopes, incantations, oracles, magic, witchcraft, the cures of witch doctors and of medical practitioners before the

advent of modern medicine, were all firmly established through the centuries in the eyes of the public by their supposed practical successes. The scientific method was devised precisely for the purpose of elucidating the nature of things under more carefully controlled conditions and by more rigorous criteria than are present in the situations created by practical problems. These conditions and criteria can be discovered only by taking a purely scientific interest in the matter, which again can exist only in minds educated in the appreciation of scientific value. Such sensibility cannot be switched on at will for purposes alien to its inherent passion. No important discovery can be made in science by anyone who does not believe that science is important—indeed supremely important—in itself.

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## b.

## Faith in Progress?

[i]

John B. Bury  
*The Idea of Progress\**

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. . . We now take [the idea of progress of humanity] so much for granted, we are so conscious of constantly progressing in knowledge, arts, organising capacity, utilities of all sorts, that it is easy to look upon Progress as an aim, like liberty or a world-federation, which it only depends on our own efforts and good-will to achieve. But though all increases of power and knowledge depend on human effort, the idea of the Progress of humanity, from which all these particular progresses derive their value, raises a definite question of fact, which man's wishes or labours cannot affect any more than his wishes or labours can prolong life beyond the grave.

This idea means that civilisation has moved, is moving, and will move in a desirable direction. But in order to judge that we are moving in a desirable direction we should have to know precisely what the destination is. To the minds of most people the desirable outcome of human development would be a condition of society in which all the inhabitants of the planet

\* Chicago, Ill.: The University of Chicago Press 181-183 (1958). © Copyright 1958 by Michael Polanyi. Reprinted by permission.

\* New York: The Macmillan Co. 1-7, 35-36, 43-44, 334-335, 346-347, 351-352 (1932). Reprinted by permission of the Macmillan Company. Copyright 1932 by the Macmillan Company, renewed 1960 by the Macmillan Company.



would enjoy a perfectly happy existence. But it is impossible to be sure that civilisation is moving in the right direction to realise this aim. Certain features of our "progress" may be urged as presumptions in its favour, but there are always offsets, and it has always been easy to make out a case that, from the point of view of increasing happiness, the tendencies of our progressive civilisation are far from desirable. In short, it cannot be proved that the unknown destination towards which man is advancing is desirable. The movement may be Progress, or it may be in an undesirable direction and therefore not Progress. This is a question of fact, and one which is at present as insoluble as the question of personal immortality. It is a problem which bears on the mystery of life.

Moreover, even if it is admitted to be probable that the course of civilisation has so far been in a desirable direction, and such as would lead to general felicity if the direction were followed far enough, it cannot be proved that ultimate attainment depends entirely on the human will. For the advance might at some point be arrested by an insuperable wall. Take the particular case of knowledge, as to which it is generally taken for granted that the continuity of progress in the future depends altogether on the continuity of human effort (assuming that human brains do not degenerate). This assumption is based on a strictly limited experience. Science has been advancing without interruption during the last three or four hundred years; every new discovery has led to new problems and new methods of solution, and opened up new fields for exploration. Hitherto men of science have not been compelled to halt, they have always found means to advance further. But what assurance have we that they will not one day come up against impassable barriers? The experience of four hundred years, in which the surface of nature has been successfully tapped, can hardly be said to warrant conclusions as to the prospect of operations extending over four hundred or four thousand centuries. Take biology or astronomy. How can we be sure that some day progress may not come to a dead pause, not because knowledge is exhausted, but because our resources for investigation are exhausted—because, for instance, scientific instruments have reached the limit of perfection beyond which it is demonstrably impossible to improve them, or because (in the case of astronomy) we come into the presence of forces of which, unlike gravitation, we have no

terrestrial experience? It is an assumption, which cannot be verified, that we shall not soon reach a point in our knowledge of nature beyond which the human intellect is unqualified to pass.

But it is just this assumption which is the light and inspiration of man's scientific research. For if the assumption is not true, it means that he can never come within sight of the goal which is, in the case of physical science, if not a complete knowledge of the cosmos and the processes of nature, at least an immeasurably larger and deeper knowledge than we at present possess.

Thus continuous progress in man's knowledge of his environment, which is one of the chief conditions of general Progress, is a hypothesis which may or may not be true. And if it is true, there remains the further hypothesis of man's moral and social "perfectibility," which rests on much less impressive evidence. There is nothing to show that he may not reach, in his psychical and social development, a stage at which the conditions of his life will be still far from satisfactory and beyond which he will find it impossible to progress. This is a question of fact which no willing on man's part can alter. It is a question bearing on the mystery of life.

Enough has been said to show that the Progress of humanity belongs to the same order of ideas as Providence or personal immortality. It is true or it is false, and like them it cannot be proved either true or false. Belief in it is an act of faith.

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It may surprise many to be told that the notion of Progress, which now seems so easy to apprehend, is of comparatively recent origin. It has indeed been claimed that various thinkers, both ancient (for instance, Seneca) and medieval (for instance, Friar Bacon), had long ago conceived it. But sporadic observations—such as man's gradual rise from primitive and savage conditions to a certain level of civilisation by a series of inventions, or the possibility of some future additions to his knowledge of nature—which were inevitable at a certain stage of human reflection, do not amount to an anticipation of the idea. The value of such observations was determined, and must be estimated, by the whole context of ideas in which they occurred. It is from its bearings on the future that Progress derives its value, its interest, and its power. You may conceive civilisation as having gradually

advanced in the past, but you have not got the idea of Progress until you go on to conceive that it is destined to advance indefinitely in the future. . . .

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In this last stage of the Renaissance, which includes the first quarter of the seventeenth century, soil was being prepared in which the idea of Progress could germinate, and our history of its origin definitely begins with the work of two men who belong to this age, Bodin, who is hardly known except to special students of political science, and Bacon, who is known to all the world. Both had a more general grasp of the significance of their own time than any of their contemporaries, and though neither of them discovered a theory of Progress, they both made contributions to thought which directly contributed to its subsequent appearance.

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[Bodin's] work announces a new view of history which is optimistic regarding man's career on earth, without any reference to his destinies in a future life. And in this optimistic view there are three particular points to note, which were essential to the subsequent growth of the idea of Progress. In the first place, the decisive rejection of the theory of degeneration, which had been a perpetual obstacle to the apprehension of that idea. Secondly, the unreserved claim that his own age was fully equal, and in some respects superior, to the age of classical antiquity, in respect of science and the arts. He leaves the ancients reverently on their pedestal, but he erects another pedestal for the moderns, and it is rather higher. . . . In the third place, he had a conception of the common interest of all the peoples of the earth, a conception which corresponded to the old ecumenical idea of the Greeks and Romans, but had now a new significance through the discoveries of modern navigators. He speaks repeatedly of the world as a universal state, and suggests that the various races, by their peculiar aptitudes and qualities, contribute to the common good of the whole. This idea of the "solidarity" of peoples was to be an important element in the growth of the doctrine of Progress.

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In the sixties of the nineteenth century the idea of Progress entered upon the third period of its history. During the *first* period, up to the

French Revolution, it had been treated rather casually; it was taken for granted and received no searching examination either from philosophers or from historians. In the *second* period its immense significance was apprehended, and a search began for a general law which would define and establish it. The study of sociology was founded, and at the same time the impressive results of science, applied to the conveniences of life, advertised the idea. It harmonised with the notion of "development" which had become current both in natural science and in metaphysics. Socialists and other political reformers appealed to it as a gospel.

By 1850 it was a familiar idea in Europe, but was not yet universally accepted as obviously true. The notion of social Progress had been growing in the atmosphere of the notion of biological development, but this development still seemed a highly precarious speculation. The fixity of species and the creation of man, defended by powerful interests and prejudices, were attacked but were not shaken. The hypothesis of organic evolution was much in the same position as the Copernican hypothesis in the sixteenth century. Then in 1859 Darwin intervened, like Galileo. The appearance of the *Origin of Species* changed the situation by disproving definitely the dogma of fixity of species and assigning real causes for "transformism." What might be set aside before as a brilliant guess was elevated to the rank of a scientific hypothesis, and the following twenty years were enlivened by the struggle around the evolution of life, against prejudices chiefly theological, resulting in the victory of the theory.

The *Origin of Species* led to the *third* stage of the fortunes of the idea of Progress. We saw how the heliocentric astronomy, by dethroning man from his privileged position in the universe of space and throwing him back on his own efforts, had helped that idea to compete with the idea of a busy Providence. He now suffers a new degradation within the compass of his own planet. Evolution, shearing him of his glory as a rational being specially created to be the lord of the earth, traces a humble pedigree for him. And this second degradation was the decisive fact which has established the reign of the idea of Progress.

Evolution itself, it must be remembered, does not necessarily mean, applied to society, the movement of man to a desirable goal. It is a neutral, scientific conception, compatible either with optimism or with pessimism. According to

different estimates it may appear to be a cruel sentence or a guarantee of steady amelioration. And it has been actually interpreted in both ways.

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Thus in the seventies and eighties of the last century the idea of Progress was becoming a general article of faith. Some might hold it in the fatalistic form that humanity moves in a desirable direction, whatever men do or may leave undone; others might believe that the future will depend largely on our own conscious efforts, but that there is nothing in the nature of things to disappoint the prospect of steady and indefinite advance. The majority did not inquire too curiously into such points of doctrine, but received it in a vague sense as a comfortable addition to their convictions. But it became a part of the general mental outlook of educated people.

When Mr. Frederic Harrison delivered in 1889 at Manchester an eloquent discourse on the "New Era," in which the dominant note is "the faith in human progress in lieu of celestial rewards of the separate soul," his general argument could appeal to immensely wider circles than the Positivists whom he was specially addressing.

The dogma—for a dogma it remains, in spite of the confidence of Comte or of Spencer that he had made it a scientific hypothesis—has produced an important ethical principle. Consideration for posterity has throughout history operated as a motive of conduct, but feebly, occasionally, and in a very limited sense. With the doctrine of Progress it assumes, logically, a preponderating importance; for the centre of interest is transferred to the life of future generations who are to enjoy conditions of happiness denied to us, but which our labours and sufferings are to help to bring about. If the doctrine is held in an extreme fatalistic form, then our duty is to resign ourselves cheerfully to sacrifices for the sake of unknown descendants, just as ordinary altruism enjoins the cheerful acceptance of sacrifices for the sake of living fellow-creatures. Winwood Reade indicated this when he wrote, "Our own prosperity is founded on the agonies of the past. Is it therefore unjust that we also should suffer for the benefit of those who are to come?" But if it is held that each generation can by its own deliberate acts determine for good or evil the destinies of the race, then our duties towards others reach out through time as well as through space, and our contemporaries are only a negligible fraction of the "neighbours" to whom

we owe obligations. The ethical end may still be formulated, with the Utilitarians, as the greatest happiness of the greatest number; only the greatest number includes, as Kidd observed, "the members of generations yet unborn or unthought of." This extension of the moral code, if it is not yet conspicuous in treatises on Ethics, has in late years been obtaining recognition in practice.

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[If we accept the reasonings on which the dogma of Progress is based, must we not carry them to their full conclusion? In escaping from the illusion of finality, is it legitimate to exempt that dogma itself? Must not it, too, submit to its own negation of finality? Will not that process of change, for which Progress is the optimistic name, compel "Progress" too to fall from the commanding position in which it is now, with apparent security, enthroned? . . . A day will come, in the revolution of centuries, when a new idea will usurp its place as the directing idea of humanity. Another star, unnoticed now or invisible, will climb up the intellectual heaven, and human emotions will react to its influence, human plans respond to its guidance. It will be the criterion by which Progress and all other ideas will be judged. And it too will have its successor.

In other words, does not Progress itself suggest that its value as a doctrine is only relative, corresponding to a certain not very advanced stage of civilisation; just as Providence, in its day, was an idea of relative value, corresponding to a stage somewhat less advanced? Or will it be said that this argument is merely a disconcerting trick of dialectic played under cover of the darkness in which the issue of the future is safely hidden by Horace's prudent god?

[ii]

Thomas S. Kuhn

*The Structure of Scientific Revolutions\**

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With respect to normal science, then, part of the answer to the problem of progress lies simply in the eye of the beholder. Scientific progress is not different in kind from progress in other fields, but the absence at most times of competing schools that question each other's aims and standards makes the progress of a normal-scientific community far easier to see. That, however,

\* Chicago: The University of Chicago Press 162-172 (1964). © Copyright 1964 by the University of Chicago Press. Reprinted by permission.

is only part of the answer and by no means the most important part. We have, for example, already noted that once the reception of a common paradigm has freed the scientific community from the need constantly to re-examine its first principles, the members of that community can concentrate exclusively upon the subtlest and most esoteric of the phenomena that concern it. Inevitably, that does increase both the effectiveness and the efficiency with which the group as a whole solves new problems. Other aspects of professional life in the sciences enhance this very special efficiency still further.

Some of these are consequences of the unparalleled insulation of mature scientific communities from the demands of the laity and of everyday life. That insulation has never been complete—we are now discussing matters of degree. Nevertheless, there are no other professional communities in which individual creative work is so exclusively addressed to and evaluated by other members of the profession. The most esoteric of poets or the most abstract of theologians is far more concerned than the scientist with lay approbation of his creative work, though he may be even less concerned with approbation in general. That difference proves consequential. Just because he is working only for an audience of colleagues, an audience that shares his own values and beliefs, the scientist can take a single set of standards for granted. He need not worry about what some other group or school will think and can therefore dispose of one problem and get on to the next more quickly than those who work for a more heterodox group. Even more important, the insulation of the scientific community from society permits the individual scientist to concentrate his attention upon problems that he has good reason to believe he will be able to solve. Unlike the engineer, and many doctors, and most theologians, the scientist need not choose problems because they urgently need solution and without regard for the tools available to solve them. In this respect, also, the contrast between natural scientists and many social scientists proves instructive. The latter often tend, as the former almost never do, to defend their choice of a research problem—e.g., the effects of racial discrimination or the causes of the business cycle—chiefly in terms of the social importance of achieving a solution. Which group would one then expect to solve problems at a more rapid rate?

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In its normal state, then, a scientific community is an immensely efficient instrument for solving the problems or puzzles that its paradigms define. Furthermore, the result of solving those problems must inevitably be progress. There is no problem here. Seeing that much, however, only highlights the second main part of the problem of progress in the sciences. Let us therefore turn to it and ask about progress through extraordinary science. Why should progress also be the apparently universal concomitant of scientific revolutions? Once again, there is much to be learned by asking what else the result of a revolution could be. Revolutions close with a total victory for one of the two opposing camps. Will that group ever say that the result of its victory has been something less than progress? That would be rather like admitting that they had been wrong and their opponents right. To them, at least, the outcome of revolution must be progress, and they are in an excellent position to make certain that future members of their community will see past history in the same way. . . .

When it repudiates a past paradigm, a scientific community simultaneously renounces, as a fit subject for professional scrutiny, most of the books and articles in which that paradigm had been embodied. Scientific education makes use of no equivalent for the art museum or the library of classics, and the result is a sometimes drastic distortion in the scientist's perception of his discipline's past. More than the practitioners of other creative fields, he comes to see it as leading in a straight line to the discipline's present vantage. In short, he comes to see it as progress. No alternative is available to him while he remains in the field.

Inevitably those remarks will suggest that the member of a mature scientific community is, like the typical character of Orwell's *1984*, the victim of a history rewritten by the powers that be. Furthermore, that suggestion is not altogether inappropriate. There are losses as well as gains in scientific revolutions, and scientists tend to be peculiarly blind to the former. On the other hand, no explanation of progress through revolutions may stop at this point. To do so would be to imply that in the sciences might makes right, a formulation which would again not be entirely wrong if it did not suppress the nature of the process and of the authority by which the choice between paradigms is made. If authority alone, and particularly if nonprofessional authority, were the arbiter of paradigm debates, the out-

come of those debates might still be revolution, but it would not be *scientific* revolution. The very existence of science depends upon vesting the power to choose between paradigms in the members of a special kind of community. Just how special that community must be if science is to survive and grow may be indicated by the very tenuousness of humanity's hold on the scientific enterprise. Every civilization of which we have records, has possessed a technology, an art, a religion, a political system, laws, and so on. In many cases those facets of civilization have been as developed as our own. But only the civilizations that descend from Hellenic Greece have possessed more than the most rudimentary science. The bulk of scientific knowledge is a product of Europe in the last four centuries. No other place and time has supported the very special communities from which scientific productivity comes.

What are the essential characteristics of these communities? Obviously, they need vastly more study. In this area only the most tentative generalizations are possible. Nevertheless, a number of requisites for membership in a professional scientific group must already be strikingly clear. The scientist must, for example, be concerned to solve problems about the behavior of nature. In addition, though his concern with nature may be global in its extent, the problems on which he works must be problems of detail. More important, the solutions that satisfy him may not be merely personal but must instead be accepted as solutions by many. The group that shares them may not, however, be drawn at random from society as a whole, but is rather the well-defined community of the scientist's professional compeers. One of the strongest, if still unwritten, rules of scientific life is the prohibition of appeals to heads of state or to the populace at large in matters scientific. Recognition of the existence of a uniquely competent professional group and acceptance of its role as the exclusive arbiter of professional achievement has further implications. The group's members, as individuals and by virtue of their shared training and experience, must be seen as the sole possessors of the rules of the game or of some equivalent basis for unequivocal judgments. To doubt that they shared some such basis for evaluations would be to admit the existence of incompatible standards of scientific achievement. That admission would inevitably raise the question whether truth in the sciences can be one.

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These last paragraphs point the directions in which I believe a more refined solution of the problem of progress in the sciences must be sought. Perhaps they indicate that scientific progress is not quite what we had taken it to be. But they simultaneously show that a sort of progress will inevitably characterize the scientific enterprise so long as such an enterprise survives. In the sciences there need not be progress of another sort. We may, to be more precise, have to relinquish the notion, explicit or implicit, that changes of paradigm carry scientists and those who learn from them closer and closer to the truth.

It is now time to notice that until the last very few pages the term 'truth' had entered this essay only in a quotation from Francis Bacon. And even in those pages it entered only as a source for the scientist's conviction that incompatible rules for doing science cannot coexist except during revolutions when the profession's main task is to eliminate all sets but one. The developmental process described in this essay has been a process of evolution *from* primitive beginnings—a process whose successive stages are characterized by an increasingly detailed and refined understanding of nature. But nothing that has been or will be said makes it a process of evolution *toward* anything. Inevitably that lacuna will have disturbed many readers. We are all deeply accustomed to seeing science as the one enterprise that draws constantly nearer to some goal set by nature in advance.

But need there be any such goal? Can we not account for both science's existence and its success in terms of evolution from the community's state of knowledge at any given time? Does it really help to imagine that there is some one full, objective, true account of nature and that the proper measure of scientific achievement is the extent to which it brings us closer to that ultimate goal? If we can learn to substitute evolution-from-what-we-do-know for evolution-toward-what-we-wish-to-know, a number of vexing problems may vanish in the process. Somewhere in this maze, for example, must lie the problem of induction.

I cannot yet specify in any detail the consequences of his alternate view of scientific advance. But it helps to recognize that the conceptual transposition here recommended is very close to one that the West undertook just a century ago. It is particularly helpful because in both cases the main obstacle to transposition is the same. When Darwin first published his theory

of evolution by natural selection in 1859, what most bothered many professionals was neither the notion of species change nor the possible descent of man from apes. The evidence pointing to evolution, including the evolution of man, had been accumulating for decades, and the idea of evolution had been suggested and widely disseminated before. Though evolution, as such, did encounter resistance, particularly from some religious groups, it was by no means the greatest of the difficulties the Darwinians faced. That difficulty stemmed from an idea that was more nearly Darwin's own. All the well-known pre-Darwinian evolutionary theories—those of Lamarck, Chambers, Spencer, and the German *Naturphilosophen*—had taken evolution to be a goal-directed process. The “idea” of man and of the contemporary flora and fauna was thought to have been present from the first creation of life, perhaps in the mind of God. That idea or plan had provided the direction and the guiding force to the entire evolutionary process. Each new stage of evolutionary development was a more perfect realization of a plan that had been present from the start.

For many men the abolition of that teleological kind of evolution was the most significant and least palatable of Darwin's suggestions. The *Origin of Species* recognized no goal set either by God or nature. Instead, natural selection, operating in the given environment and with the actual organisms presently at hand, was responsible for the gradual but steady emergence of more elaborate, further articulated, and vastly more specialized organisms. Even such marvelously adapted organs as the eye and hand of man—organs whose design had previously provided powerful arguments for the existence of a supreme artificer and an advance plan—were products of a process that moved steadily from primitive beginnings but toward no goal. The belief that natural selection, resulting from mere competition between organisms for survival, could have produced man together with the higher animals and plants was the most difficult and disturbing aspect of Darwin's theory. What could ‘evolution,’ ‘development,’ and ‘progress’ mean in the absence of a specified goal? To many people, such terms suddenly seemed self-contradictory.

The analogy that relates the evolution of organisms to the evolution of scientific ideas can easily be pushed too far. But with respect to the issues of this closing section it is very nearly perfect. The process described . . . as the resolu-

tion of revolutions is the selection by conflict within the scientific community of the fittest way to practice future science. The net result of a sequence of such revolutionary selections, separated by periods of normal research, is the wonderfully adapted set of instruments we call modern scientific knowledge. Successive stages in that developmental process are marked by an increase in articulation and specialization. And the entire process may have occurred, as we now suppose biological evolution did, without benefit of a set goal, a permanent fixed scientific truth, of which each stage in the development of scientific knowledge is a better exemplar.

Anyone who has followed the argument this far will nevertheless feel the need to ask why the evolutionary process should work. What must nature, including man, be like in order that science be possible at all? Why should scientific communities be able to reach a firm consensus unattainable in other fields? Why should consensus endure across one paradigm change after another? And why should paradigm change invariably produce an instrument more perfect in any sense than those known before? From one point of view those questions, excepting the first, have already been answered. But from another they are as open as they were when this essay began. It is not only the scientific community that must be special. The world of which that community is a part must also possess quite special characteristics, and we are no closer than we were at the start to knowing what these must be. That problem—What must the world be like in order that man may know it?—was not, however, created by this essay. On the contrary, it is as old as science itself, and it remains unanswered. . . .

#### NOTE

BERTRAND RUSSELL  
THE SCIENTIFIC OUTLOOK\*

\* \* \*

Science in the course of the few centuries of its history has undergone an internal development which appears to be not yet completed. One may sum up this development as the passage from contemplation to manipulation. The love of

\* London: George Allen & Unwin Ltd. 269–273 (1931). Reprinted by permission of George Allen & Unwin Ltd. and W. W. Norton & Company, Inc. Copyright 1931 by Bertrand Russell. Copyright renewed 1959 by Bertrand Russell.



knowledge to which the growth of science is due is itself the product of a twofold impulse. We may seek knowledge of an object because we love the object or because we wish to have power over it. The former impulse leads to the kind of knowledge that is contemplative, the latter to the kind that is practical. In the development of science the power impulse has increasingly prevailed over the love impulse. The power impulse is embodied in industrialism and in governmental technique. It is embodied also in the philosophies known as pragmatism and instrumentalism. Each of these philosophies holds, broadly speaking, that our beliefs about any object are true in so far as they enable us to manipulate it with advantage to ourselves. This is what may be called a governmental view of truth. Of truth so conceived science offers us a great deal; indeed there seems no limit to its possible triumphs. To the man who wishes to change his environment science offers astonishingly powerful tools, and if knowledge consists in the power to produce intended changes, then science gives knowledge in abundance.

\* \* \*

Science in its beginnings was due to men who were in love with the world. They perceived the beauty of the stars and the sea, of the winds and the mountains. Because they loved them their thoughts dwelt upon them, and they wished to understand them more intimately than a mere outward contemplation made possible. "The world," said Heraclitus, "is an ever-living fire, with measures kindling and measures going out." Heraclitus and the other Ionian philosophers, from whom came the first impulse to scientific knowledge, felt the strange beauty of the world almost like a madness in the blood. They were men of Titanic passionate intellect, and from the intensity of their intellectual passion the whole movement of the modern world has sprung. But step by step, as science has developed, the impulse of love which gave it birth has been increasingly thwarted, while the impulse of power, which was at first a mere camp-follower, has gradually usurped command in virtue of its unforeseen success. The lover of nature has been baffled, the tyrant over nature has been rewarded. . . . Thus science has more and more substituted power-knowledge for love-knowledge, and as this substitution becomes completed science tends more and more to become sadistic. The scientific society of the future as we have been imagining it is one in which the

power impulse has completely overwhelmed the impulse of love, and this is the psychological source of the cruelties which it is in danger of exhibiting.

\* \* \*

c.

### Survival of Individual or Species?

[i]

Marshall Walker

*The Nature of Scientific Thought\**

\* \* \*

[E]thical behavior for man is that pattern of individual and collective conduct which maximizes the probability of survival of man as individual and species. This definition permits an act to be classified as ethical or unethical by examining its consequences. As time goes on and consequences become clearer, the classification of a given act in the past may change. The nuclear bombing of Hiroshima, which was intended as an ethical act (to save the lives, both Japanese and American, which would be lost in a full-scale invasion), may be classified as unethical by future historians. A classification is never certain because man is not omniscient. A man must judge whether an act is ethical or unethical in advance, hence he must try to predict the consequences. He extrapolates his knowledge of the past to predict the future. This procedure is the domain of science. His classification of acts as ethical or unethical is as reliable as his scientific predictions of future events, no more and no less. Some ethical judgments will be as "certain" as, "The sun will rise tomorrow"; others will be as uncertain as, "The probability of rain tomorrow is three out of ten."

This definition of *ethical behavior* is operationally equivalent to the traditional statement that ethical behavior is conduct according to the will of God. Religious tradition has it that God created man, encouraged him to be fruitful, and forbade suicide. Thus a pattern of behavior which maximizes man's survival is at least part of the will of God as traditionally accepted. The possibility still remains that behavior to encourage survival is not all that is necessary. It is assumed here that such items as courtesy and loving kindness, which at first appear to be op-

\* Englewood Cliffs, N.J.: Prentice-Hall, Inc. 153-156 (1963). © 1963 by Prentice-Hall, Inc. Reprinted by permission.

tional, are positive contributions to survival probability and hence are included in the definition of ethical conduct. From this point of view the behavior of such societies as Nazi Germany does not maximize their probability of survival and leads eventually to their destruction.

Mathematicians will inquire how two different quantities, probability of individual survival and probability of species survival, can be maximized simultaneously. The quantity to be maximized is the sum of the weighted probabilities. The weighting factors must be estimated from empirical observation bearing in mind that the weighting factors have changed in the past and will probably change in the future. Different species at the present time weight the factors quite differently. Among bees and ants the survival of the species seems to be heavily weighted.

Ethical predictions take a long time to test empirically, and observations must be extended over many generations. The ethical advice, "Honor thy father and thy mother," according to tradition was pronounced by Moses acting as spokesman for the God of the Hebrews. In this case the end is stated: "... that thy days may be long. . . ." The conditions are not stated, but we must infer a set of conditions if the statement is to be verifiable. The assumed conditions will specify the procedure necessary for verification. Let us assume that Moses was talking to the Hebrews and that he had in mind the general conditions of Hebrew life. A predictive form of the commandment is then: "I [Moses] predict that those Hebrews who honor their fathers and mothers will increase their probability of living a long time."

This prediction is clearly based on tribal experience handed down by legend. Primitive tribes had little regard for old people; when men and women became too old to look out for themselves, they were left behind to starve when the tribe migrated. The commandment is saying, "If you honor and look after your old parents, you are setting an example for your son to follow when you yourself get old." This example illustrates clearly the concern for continued survival and the fact that obedience to this law automatically brings a probability of reward.

\* \* \*

The prevalence of the verbot form of models of ethical laws has produced the illusion that there is a difference between natural laws and

ethical laws. The difference is commonly stated in this way: a man can disobey the ethical law, "Thou shalt not steal," by committing a theft, but a man cannot disobey Newton's second law by any action whatever. This apparent difference disappears immediately when one regards "Thou shalt not steal" as an abbreviation for "Stealing decreases the probability of survival of man as individual and species . . ."

\* \* \*

An attempt might be made to base ethical classification on the maximization of probability of individual survival alone. It seems obvious that the probability of individual survival would be increased by an individual's behavior which showed concern for the survival of others of his species. This essay does not adopt that postulate because it leads to a classification contradictory to the teaching of many ethical thinkers. Consider a man who does not believe in an afterlife, but "voluntarily" becomes a martyr to aid the survival of others of his society. Ethical teachers tend to regard this act as moral even when they have little approval for the society itself. The individual survival postulate would classify this act as unethical. The empiricist respects the judgments of many ethical teachers because he considers that these judgments are insights based on observation of society. This essay considers that the postulate of maximization of probability of survival of individual and species gives greater correspondence to the consensus of ethical teachers.

On the other hand, one might attempt to base ethical classification on the maximization of probability of species survival alone. Some insect societies appear to approach this procedure. Its success is probably due to the fact that an individual insect is not very imaginative and does not worry about his own individual future. Man, however, is very imaginative and does worry about his individual future. It seems likely that an ethic which required a man to subordinate completely his own powerful drive toward personal survival to concern for species survival alone would lead to such low morale that the survival probability of the species would be decreased by such a policy. The adoption of the postulate specifying maximization of the weighted sum of the probabilities of personal and species survival permits one to adjust the weighting factors to give maximum correspondence to the observed conditions for each species.



[ii]

*United States Atomic Energy Commission  
In the Matter of J. Robert Oppenheimer\**

\* \* \*

DR. OPPENHEIMER: One important point to make is that lack of feasibility is not the ground on which we made our recommendations [about the hydrogen bomb].

Another point I ought to make is that lack of economy, although alleged, is not the primary or only ground; the competition with fission weapons is obviously in our minds. The real reasons, the weight, behind the report is, in my opinion, a failing of the existence of these weapons would be a disadvantageous thing. It says this over and over again.

I may read, which I am sure has no security value, from the so-called minority report, Fermi and Rabi.

The fact that no limits exist to the destructiveness of this weapon makes its very existence and the knowledge of its construction a danger to humanity as a whole. It is necessarily an evil thing considered in any light. For these reasons, we believe it important for the President of the United States to tell the American public and the world that we think it is wrong on fundamental ethical principles to initiate the development of such a weapon.

In the report which got to be known as the majority report, which Conant wrote, DuBridge, Buckley and I signed, things are not quite so ethical and fundamental, but it says in the final paragraph: "In determining not to proceed to develop the super bomb, we see a unique opportunity of providing by example some limitations on the totality of war and thus of eliminating the fear and arousing the hope of mankind."

I think it is very clear that the objection was that we did not like the weapon, not that it couldn't be made.

Now, it is a matter of speculation whether, if we had before us at that time, if we had had the technical knowledge and inventiveness which we did have somewhat later, we would have taken a view of this kind. These are total views where you try to take into account how good the thing is, what the enemy is likely to do, what you can do with it, what the competition is, and the extent to which this is an inevitable step anyway.

\* Transcript of Hearing Before Personnel Security Board, Washington, D.C.: United States Government Printing Office 79-80, 249-251 (1954).

My feeling about the delay in the hydrogen bomb, and I imagine you want to question me about it, is that if we had had good ideas in 1945, and had we wanted to, this object might have been in existence in 1947 or 1948, perhaps 1948. If we had had all of the good ideas in 1949, I suppose some little time might have been shaved off the development as it actually occurred. If we had not had good ideas in 1951, I do not think we would have it today. In other words, the question of delay is keyed in this case to the question of invention. . . .

The notion that the thermonuclear arms race was something that was in the interests of this country to avoid if it could was very clear to us in 1949. We may have been wrong. We thought it was something to avoid even if we could jump the gun by a couple of years, or even if we could outproduce the enemy, because we were infinitely more vulnerable and infinitely less likely to initiate the use of these weapons, and because the world in which great destruction has been done in all civilized parts of the world is a harder world for America to live with than it is for the Communists to live with. This is an idea which I believe is still right, but I think what was not clear to us then and what is clearer to me now is that it probably lay wholly beyond our power to prevent the Russians somehow from getting ahead with it. I think if we could have taken any action at that time which would have precluded their development of this weapon, it would have been a very good bet to take that, I am sure. . . .

\* \* \*

MR. GRAY: [Y]ou don't intend to have this record suggest that you felt that if those who opposed the development of the hydrogen bomb prevailed that would mean that the world would not be confronted with the hydrogen bomb?

DR. OPPENHEIMER: It would not necessarily mean—we thought on the whole it would make it less likely. That the Russians would attempt and less likely that they would succeed in the undertaking.

MR. GRAY: I would like to pursue that a little bit. That is two things. One, the likelihood of their success would we all hope still be related to their own capabilities and not to information they would receive from our efforts. So what you mean to say is that since they would not attempt it they would not succeed?

DR. OPPENHEIMER: No, I believe what we then thought was that the incentive to do it

would be far greater if they knew we were doing it, and we had succeeded. Let me, for instance, take a conjecture. Suppose we had not done anything about the atom during the war. I don't think you could guarantee that the Russians would never have had an atomic bomb. But I believe they would not have one as nearly as soon as they have. I think both the fact of our success, the immense amount of publicity, the prestige of the weapon, the espionage they collect, all of this made it an absolutely higher priority thing, and we thought similar circumstances might apply to the hydrogen bomb. We were always clear that there might be a Russian effort whatever we did. We always understood that if we did not do this that an attempt would be made to get the Russians sewed up so that they would not either.

\* \* \*

MR. GRAY: I am trying to get at at what time did your strong moral convictions develop with respect to the hydrogen bomb?

DR. OPPENHEIMER: When it became clear to me that we would tend to use any weapon we had.

MR. GRAY: Then may I ask this: Do you make a sharp distinction between the development of a weapon and the commitment to use it?

DR. OPPENHEIMER: I think there is a sharp distinction but in fact we have not made it.

\* \* \*

MR. GRAY: Your deep concern about the use of the hydrogen bomb, if it were developed, and therefore your own views at the time as to whether we should proceed in a crash program to develop it—your concern about this—became greater, did it not, as the practicabilities became more clear? Is that an unfair statement?

DR. OPPENHEIMER: I think it is the opposite of true. Let us not say about use. But my feeling about development became quite different when the practicabilities became clear. When I saw how to do it, it was clear to me that one had to at least make the thing. Then the only problem was what would one do about them when one had them. The program we had in 1949 was a tortured thing that you could well argue did not make a great deal of technical sense. It was therefore possible to argue also that you did not want it even if you could have it. The program in 1951 was technically so sweet that you could not argue about that. It was purely the military, the political and the humane

problem of what you were going to do about it once you had it.

\* \* \*

d.

#### **Advancement of Social and Political Goals?**

##### *President's Task Force on Science Policy Science and Technology—Tools for Progress\**

The Task Force recommends that the President explicitly enunciate, as a national policy, the need for vigorous, high-quality science and technology, focusing on our national goals and purposes, and recognizing the cultural and inspirational values in man's scientific progress.

The Task Force also recommends that the President call for—as one national goal—continuing leadership in science and in the technology relevant to our other national goals and purposes.

\* \* \*

Our national progress will become ever more critically dependent upon the excellence of our science and technology. A vigorous, high-quality program aimed at advancing our scientific and technological capabilities (including the social, economic, and behavioral components) is vital to all national goals and purposes. Such a program is especially vital to our national defense and security and to our international posture generally; to our ability to negotiate properly safeguarded arms limitations; to our continued economic growth and development and to our international trade balance; to the health of business, labor, and the professions; to the quality of our environment; to the personal health and welfare of all; to the scope and quality of our educational processes; and to the culture, spirit, and inspiration of our people generally. The effectiveness of essentially all our social institutions, including particularly Government itself, is deeply influenced by the quality of our science and technology.

The Nation, therefore, has a fundamental need for excellence in science and technology. Accordingly, it also needs to insure that the effectiveness of our science and technology is not downgraded or destroyed by the unthinking or the uninformed. That is not to say that the limitations of science and technology should not be recognized. We do not suggest complacent ac-

\* Washington, D.C.: U.S. Government Printing Office 4, 8-9, 38-39 (1970).

ceptance of the unwanted side effects of narrowly motivated or incompletely understood applications of science. Nor do we suggest that technology should dictate social purpose. On the contrary, we wish to emphasize the importance of seeking to optimize utilization of science and technology in the service of social, political, and economic goals.

*Anti-Science Attitudes.* The rapid rise of attitudes disdainful of science and technology, and the disillusionment of many young people with science and technology is of grave concern. The sources of these attitudes include deficiencies in the application of science and technology which should in fact be criticized and should be corrected. Inanimate technology is not of itself the problem; rather the primary need is "to conceive ways to discover and repair the deficiencies in the processes and institutions by which society puts the tools of science and technology to work." (1) The sources of the shift in attitudes toward science and technology also include widespread lack of perspective and understanding of their nature and role in past and future improvement in the human condition. The public and its elected representative must have a better grasp of both the limitations and the promise of science and technology. Priority should be given to presenting this complex matter to the public in a balanced and understandable fashion. The responsibility for achieving this understanding starts with the executive and legislative branches of the Federal government and spreads to include state and local government, universities, business and professional organizations, and other private institutions in positions of leadership.

*Scientific Leadership.* The scientific and technological resources of this Nation are among its most powerful tools for the achievement of our social, political, and economic purposes. The management, strength, and proper allocation of these vital resources are political responsibilities of the highest significance, with not only short-term but also very long-term implications both nationally and internationally. The leadership of today must provide the legacy for tomorrow.

The Task Force believes that one of the important national goals for which this Nation should strive is leadership and excellence in science itself—as a long-range investment in achieving the Nation's other goals, as a precursor to more directly applicable and controllable technology, and as a contribution to the culture, spirit, and inspiration of our people.

\* \* \*

The United States is entering an era of profound problems as we look to the seventies and beyond. This is an era of relative strategic balance with the Soviet Union, of the emergence of Communist China as a nuclear power, of increased unrest among the non-nuclear nations and increased temptation toward confrontation and escalation, of the historic possibility of achieving verified nuclear arms limitation agreements, and of unusually intense budget pressures.

These significant new factors dictate the need for special attention to the following general aspects of science policy for national security purposes:

1. *Avoidance of technological surprise.* Technology will not stand still; on the contrary, it will likely move more rapidly. The penalty for technological surprise can be enormous.
2. *Reducing lead-time for reaction to changed circumstances.* The capability to react quickly to significantly changed circumstances—changes in perception of Soviet intentions, for example—will become even more critical than it has always been.
3. *Increased emphasis on intelligence and reconnaissance information.* In a period of relative strategic balance, it will be more important than ever to have the best possible information on what is happening behind the "Iron" and "Bamboo" curtains. The margin for error will be significantly reduced, and the premium on precision will be increased. Obviously, the need for continuing verification of nuclear arms agreements further emphasizes this point.
4. *Reduction in total costs.* The increased performance requirements for military hardware, the effects of inflation, and the budget pressures all dictate renewed attention to the matter of cost reduction.

All four of these points lead to the need for increased emphasis on research and development in relation to other competing national security activities. In guarding against technological surprise, it is vital that high-risk long-range research and development programs in critical areas be sustained. The greatest single contribution to reducing lead-times for quicker reaction to changed circumstances would be a development program which emphasizes the bringing of critical high-technology sub-elements of new weapon systems to the demonstration phase on a continuing

basis. The significance of research and advanced technology for the purpose of dissolving the "Iron" and "Bamboo" curtains is apparent. Finally, direct research and development projects aimed at cost reduction are indicated: for example, development of "design for low cost" techniques, inclusion of ultimate cost in original research and development specifications, competitive research and development projects where demonstration of low cost is a primary objective.

The impact of the generally rising anti-science and anti-technology attitudes discussed previously in this report could have a particularly important effect on the correct military research and development program for the Nation. The issue of national security research in our universities, for example, has become an irrational one with many students and many faculty members alike. Attacks on the military-industrial complex have, in too many cases, become narrowly self-serving and very short range in perspective. The need for better public and Congressional understanding of both the limitations (e.g., lead-times) and the nature and importance of science and technology for national security purposes is very great indeed.

\* \* \*

#### NOTE

DEREK J. DE Solla PRICE  
THE SCIENCE OF SCIENCE\*

\* \* \*

[I] approach modern science with a mixture of doubt and hope about its analysis. On the one hand, I have grave doubts about the accuracy of our knowledge of the way in which science behaves, develops, and interacts with society; even the most able practitioners of science must surely be misled by many of the myths and idols. On the other hand, I believe that one can effectively pursue hard knowledge in this area and that such knowledge might have a rather attractive and provoking universal validity.

Yet to all this I must adduce the paradox that the new knowledge about modern science seems to be growing in the midst of strong resistance from both within and without the field of the history of science. The resistance from outside is from scientists themselves and is tradi-

tional. It has always been part of the special mystique of the scientist that he and he alone can really know about science. Only an esteemed and successful creative scientist can speak for his peers, criticize the state of science, counsel governments and universities, and guide the policies of laboratories and learned societies. An outsider must be presumed ignorant, not merely of the technical facts, but also of that special knowledge of the life of science that can only be won on its battlefields. This is in spite of the fact that creative artists have their competent art critics who may never have drawn or painted, and that practicing musicians are judged by music critics who cannot play an instrument but who wield nonetheless a pen that contributes much to the progress of their creative industry. . . .

\* \* \*

#### 3.

#### Curiosity and Mastery— The Ever-Widening Net

a.

#### Myron G. Schultz Daniel Carrión's Experiment\*

When Carrión undertook his experiment in 1885 he was 26 years old and in his sixth year of training at the Facultad de Medicina in Lima. To qualify for his medical degree he had to prepare an original thesis, and he had devoted himself with increasing vigor during the preceding three years to a study of the epidemiology and clinical manifestations of verruga peruana. This unusual disease is manifested by multiple, nodular, vascular eruptions of the skin and mucous membranes accompanied by fever and severe rheumatic pains. Verruga peruana has existed for many centuries in the steep valleys of the Peruvian cordillera; in fact, most scholars believe that the depiction of wart-like eruptions on the *huacas*, the anthropoid ceramic artifacts of the Inca dynasty, demonstrates its pre-Columbian existence.

Carrión carefully studied nine patients with verruga peruana who were hospitalized in Lima. It was a work in which he invested his emotions as well as his intellect. As a youngster, he had

\* John R. Platt, ed.: *New Views of the Nature of Man*. Chicago: The University of Chicago Press 49-50 (1965). Reprinted by permission.

\* 278 *New England Journal of Medicine* 1323-1325 (1968). Reprinted by permission.

made frequent trips with his uncle, Manuel Ungaro, through the Peruvian mountains, going to and from school in Lima to his home in Cerro de Pasco. He saw people with verrugas during these trips, and this sight made a deep impression on him. He later told a classmate that through his research he hoped "to make an important contribution to aching humanity." Furthermore, an element of chauvinism crept into his work. When Dr. Izquierdo, a research worker in Chile (the country that had just defeated Peru in the so-called "War of the Pacific"), made what Carrión considered to be superficial observations on verruga peruana Carrión's wounded patriotism was inflamed. He believed that verruga peruana was a Peruvian problem and should therefore be solved by a Peruvian, not by an outsider. Carrión had the noblest of intentions, but they were paving the road to disaster.

\* \* \*

Gradually, Carrión came to the conclusion that the most effective way to study the incubation period and symptomatology of verruga peruana would be to perform an experimental inoculation on himself. . . .

. . . As the months went by his determination to inoculate himself became an *idée fixe*. He repeatedly spoke of his plan to his friends and professors; they repeatedly tried to dissuade him. The final impetus came when a request was received in Lima from some important European investigators for specimens of verrugas and the Academia Libre de Medicina set up a prize competition on the subject of verruga peruana. Carrión could no longer be stopped.

On the morning of August 27, 1885, he was in the Nuestra Señora de las Mercedes ward of the Dos de Mayo Hospital. In bed No. 5 was Carmen Paredes, a 14-year-old boy with a verruga on his right eyebrow. . . . Using a lancet he had brought with him, he tried to inoculate his arm with blood taken from the verruga. . . .

\* \* \*

On September 17 Carrión felt a vague discomfort and pains in his left ankle. He was not bothered greatly until two days later, when fever began. This was accompanied by strong, teeth-chattering chills, abdominal cramp and pains in all the bones and joints of his body (this mode of onset is typical of Oroya fever). . . .

Carrión told his friends that they were too worried by his illness. He said, "The symptoms I feel could not be other than those of the verruga

to which the eruption period soon will follow and all will be over."

Carrión's friends thought differently. They were impressed by the rapidity with which an anemia had developed. This is not surprising, since today it is known that the hemolytic anemia of Oroya fever can be one of the most rapidly developing and severe of any of the anemias of man. . . .

Carrión was indeed failing, but he was thinking clearly enough to say to his friends:

Up to today, I thought I was only in the invasive stage of the verruga as a consequence of my inoculation, that is, in that period of anemia that precedes the eruption. But now I am deeply convinced that I am suffering from the fever that killed our friend, Orihucla. Therefore, this is the evident proof that Oroya fever and the verruga have the same origin, as Dr. Alarco once said.

This remarkable insight expressed the essence of Carrión's experiment. He had not, as is often said, set out to prove the single etiology of verruga peruana and Oroya fever. He had merely intended to study the onset of verruga peruana; yet, when a completely different disease developed, he was able, in spite of his grave state, to grasp the full meaning of his experiment. In addition to demonstrating the unitary etiology of verruga peruana and Oroya fever, he demonstrated the inoculability of the disease.

Up until this time in the course of his illness, Carrión was confined to his rooming house, where he was receiving the attentions of a surrogate mother and his classmates. On October 3 he was visited by a Dr. Flores. When the doctor examined Carrión's blood under the microscope he noted that the red cells showed enlarged and altered morphology. The total red-cell count was only 1,085,000; there was also a leukocytosis. Carrión was urged by Dr. Flores to enter the hospital, where he could be given a blood transfusion, but he refused until the following day. When he finally relented and was transferred to the hospital where all was in readiness for the blood transfusion, a committee of doctors decided, inexplicably, to delay it.

In the evening he was completely delirious and rambled on about the different opinions that existed on the pathology of verruga peruana. On October 5, 39 days after the inoculation, he was in coma. Most of what he uttered was incomprehensible, but his last words were heard clearly by one of his friends. He said, "*Enrique, c'est fini.*"

\* \* \*

## NOTES

## NOTE 1.

A. J. BENATT  
CARDIAC CATHETERISATION\*

\* \* \*

When Werner Forssmann conceived the idea of introducing a catheter into the right heart, his object was to administer emergency drugs on the operating-table in the most rapid and efficient way. He opposed intracardiac injections because of the peril of cardiac tamponade, which might arise through injuring the coronary vessels with the needle, though he does not mention whether in fact he had experienced such an accident. The first experiment was on a human cadaver, and he was amazed at the ease with which the ureteric catheter could be guided up the arm vein into the right auricle. . . . He then carried out an experiment on himself. A wide-bore needle was inserted into an antecubital vein, through which a ureteric catheter of 4-Charrière thickness was introduced and passed with great ease to a length of 35 cm. At this point, however, his colleague who performed the operation flinched and the experiment was abandoned. A week later Forssman took matters literally into his own hands. He infiltrated his left antecubital fossa with a local anaesthetic and dissected the vein. Having passed a catheter into it, he placed himself behind an X-ray screen and watched in a mirror the passage of the catheter, which he himself manipulated into the right heart. The screening plant was obviously attached to the operating-theatre where he carried out the experiment: but to get radiographs taken he had to walk, with the catheter in position, to the radiological department, which was quite a distance away. Though the journey involved climbing stairs he had no discomfort, and he mentions only an occasional sensation of warmth similar to that felt when calcium is injected.

In his report, Forssmann visualised the future applications of his method both for diagnosis and therapy. The first patient to be treated by this procedure was a woman with purulent peritonitis, who received through the catheter one litre of glucose with added adrenalinc and strophanthin. After improving temporarily she relapsed and eventually died with the catheter still in the right auricle. . . .

\* \* \*

\* 1 *Lancet* 746-747 (1949). Reprinted by permission.

Forssmann presented his paper well, performed his experiments in logical sequence, carried them out with zeal and great courage, and provided radiological evidence that the tip of the catheter had reached the right auricle. Moreover, he was the first to inject a radio-opaque substance directly into the right heart; but, though producing exemplary skiagrams of dogs, he did not succeed in obtaining contrast pictures of his own heart when he injected 'Uroselectan' through the catheter. . . .

\* \* \*

## NOTE 2.

G. LILJESTRAND  
NOBEL PRIZE PRESENTATION  
SPEECH FOR PHYSIOLOGY  
OR MEDICINE (1956)\*

. . . The heart is the sun of the microcosm formed by the human body, as stated already by William Harvey in his monumental treatise on the circulation of the blood. Its central role in both healthy and pathologic states is well known and is illustrated, for example, by the fact that cardiovascular diseases are at present responsible for more deaths than any other group of diseases. It is for essentially new contributions in this important field that the Nobel Prize for Physiology or Medicine has been awarded this year.

Two factors are decisive for the work of the heart. One is the pressure conditions in its various chambers. The other is the quantity of blood forced by its right side through the pulmonary vessels to its left side which, in turn, transmits the blood to all the parts of the body, to be returned once more to the right atrium. Exact data regarding these two factors have long been available through animal experiments. It has been possible to measure the pressure, after introduction of catheters connected to suitable recording instruments. . . .

As far as man is concerned, these methods were for a long time only partly applicable. Thus, it was possible to record the pressure in the peripheral arteries—and this is what is usually meant when we speak of the blood pressure—as well as in the superficial veins. These values reflect to some extent the conditions in the left ventricle and the right atrium. But measurements of the right ventricular pressure, which is of

\* *Nobel Lectures, Physiology or Medicine 1942-1962*. Amsterdam: Elsevier Publishing Co. 501-502, 505 (1964). Reprinted by permission. © Nobel Foundation, 1957.

essential importance for the work of the right side of the heart, was impracticable. Similarly, it was possible, for determination of the oxygen content, to take samples of the arterial blood, but not of the mixed venous blood in the right side of the heart, which gives the average value for the body as a whole. It was, in fact, necessary to resort to indirect methods. These have yielded valuable results although they have somewhat undeservedly—as is often the case—been overshadowed by the subsequent conquests. . . .

As late as 1928, there were good reasons for the statement in a textbook that in man one was naturally confined to the use of the indirect methods. Consequently, it was highly surprising when, already in the following year, Werner Forssmann at the surgical clinic in Eberswalde was able to show—by making, with the inexperience of youth, by no means harmless experiments on himself—that a narrow catheter could be advanced from a cubital vein into the right atrium itself, a distance of almost two-thirds of a metre. Obviously, this constituted a remarkable advance. It was thereby demonstrated that, on principle, the methods well known from animal experiments could also be adapted for studies in man.

This was naturally of paramount importance for a study of pathologic changes in the circulatory system, which could be reproduced with difficulty, or not at all, in animal experiments. It also opened up better opportunities for röntgenologic examination of the right side of the heart and the pulmonary vessels, after injection of contrast medium directly into these organs. For this purpose as well, Forssmann made experiments on himself. It must have required firm conviction of the value of the method to induce self-experimentation of the kind carried out by Forssmann. His later disappointment must have been all the more bitter. It is true that the method was adopted in a few places—in Prague and in Lisbon—but on the whole Forssmann was not given the necessary support; he was, on the contrary, subjected to criticism of such exaggerated severity that it robbed him of any inclination to continue. This criticism was based on an unsubstantiated belief in the danger of the intervention, thus affording proof that—even in our enlightened times—a valuable suggestion may remain unexploited on the grounds of a preconceived opinion. A contributory cause in this substance was presumably that Forssmann was working in a milieu that did not clearly grasp the great value of his idea.

\* \* \*

Professor Forssmann. As a young doctor you have had the courage to submit yourself to heart catheterization. As a result of this, a new method was born which since that time has proved to be of very great value. It has not only opened up new roads for the study of the physiology and the pathology of the heart and lungs, it has also given the impetus for important researches on other organs. We are glad to be able to welcome you in this country where once your ancestors worked.

\* \* \*

On behalf of the Caroline Institute I proffer you the hearty congratulations of your colleagues on your brilliant achievements.

I now have the honour of asking you to accept the Nobel Prize from the hands of His Majesty the King.

#### NOTE 3.

WERNER FORSSMANN  
THE ROLE OF HEART CATHETERIZATION  
AND ANGIOCARDIOGRAPHY IN THE  
DEVELOPMENT OF MODERN MEDICINE—  
NOBEL LECTURE (1956)\*

\* \* \*

[In 1929] I carried out my first experiments in angiocardiology. Here for the first time the living heart of a dog was successfully visualized radiologically with the aid of a contrast medium. Even at that time, the complete lesser circulation in the dog could be shown with the cinematographic radiology according to Gottheiner.

Although no results could be attempted with human beings, because no apparatus had been devised, their possibility had at least been demonstrated in principle. Only four months after this publication, Moniz, Carvalho, and Lima were able to disclose rather better results. With them began the immense quantity of writing on angiocardiology.

Further development of technique was impeded not only by the absence of technical essentials and consequent lack of knowledge. To some outsiders, ethical considerations also weighed heavily in the balance against it. And when one thinks how hard men like Cournand

\* *Nobel Lectures, Physiology or Medicine 1942–1962*. Amsterdam: Elsevier Publishing Co. 506, 508–510 (1964). Reprinted by permission. © Nobel Foundation, 1957.



and McMichael had to fight against such people in 1941 and later, one can perhaps understand what difficulties stood in my way twelve years before.

\* \* \*

... Courmand and McMichael, too, ... had strong resistance to overcome, the harder to deal with because people did not hesitate to obstruct practical research work with threadbare ethical and moral objections, such as are still occasionally raised today. But these voices also must fall silent now it has been shown how responsibly this circulation research has been conducted everywhere and with what high moral earnestness it has been applied. . . .

\* \* \*

Angiocardiology, in the form in which it is practised today, is of course still burdened with risks which impose limitations on its use. Its use cannot therefore be justified for examinations which are not strictly necessary, but here, too, new possibilities can be discerned.

Further development will in many cases enable us to dispense with the massive and dangerous quantities of contrast media which at the moment we still need, and to manage instead with smaller, less harmful amounts of radioactive isotopes. . . .

From all this, we can see that modern cardiology has become something much more universal than was originally supposed.

One may compare the art of healing with a work of art, which from different standpoints and under different lighting reveals ever new and surprising beauty.

\* \* \*

b.

**Ronald W. Clark**

**JBS—The Life and Work of J. B. S. Haldane\***

John Burdon Sanderson Haldane was born on November 5, 1892 . . . On both sides of the family tree his ancestors were vigorous, mentally distinguished, and toughly individualist; from them he was to draw a combination of aristocratic self-assurance, intellectual integrity, and almost endearing bloody-mindedness.

\* \* \*

\* New York: Coward-McCann, Inc. 11, 14, 15, 19, 25-26, 60-65 (1968). Reprinted by permission of Coward-McCann, Inc. Copyright © 1968 by Ronald Clark.

His real education came from his father, to whose example in the scientific approach to facts he owed much of his success. The education began young, and at the age of three the father was taking samples of his son's blood for investigation. A year later, both traveled to London, where John Scott Haldane was testing the atmosphere on the Metropolitan Underground. . . .

\* \* \*

JBS learned more than this scientific attitude from his father. John Scott Haldane was one of those very rare men who can train themselves to ignore fear. His son, describing how his father disliked experimenting on animals and "preferred to work on himself or other human beings who were sufficiently interested in the work to ignore pain or fear," explained that his father had achieved a state in which he was almost indifferent to pain. "However," he went on, "his object was not to achieve this state but to achieve knowledge which could save other men's lives. His attitude was much more like that of a good soldier who will risk his life and endure wounds in order to gain victory than that of an ascetic who deliberately undergoes pain. The soldier does not get himself wounded deliberately, and my father did not seek pain in his work, though he greeted a pain which would have made some people writhe or groan, with laughter." . . .

\* \* \*

On the wall [of Cherwell, the Haldane home in Oxford] there was worked in stone the eagle crest of the Haldanes and the single-word family motto—"Suffer." As a symbol in the life of JBS this was to have the significance of *Citizen Kane's* "Rosebud." To suffer and to endure was to seem more a natural part of life to him that it did to most men. . . .

\* \* \*

John Scott Haldane's main industrial work concerned conditions in mines, and to investigate and report on these he was regularly employed by both government and industry. JBS often went, too, partly as a useful experimental animal whose reactions might be interesting, partly to be taught the facts of life. . . . JBS long remembered a pit in North Staffordshire. . . . "After a while," JBS later wrote, "we got to a place where the roof was about eight feet high and a man could stand up. One of the party lifted his safety lamp. It filled with blue flame and went out with a



pop. If it had been a candle this would have started an explosion, and we should probably have been killed. But of course the flame of the explosion inside the safety lamp was kept in by the wire gauze. The air near the roof was full of methane, or firedamp, which is a gas lighter than air, so the air on the floor was not dangerous.

"To demonstrate the effects of breathing firedamp, my father told me to stand up and recite Mark Antony's speech from Shakespeare's 'Julius Caesar,' beginning 'Friends, Romans, countrymen.' I soon began to pant, and somewhere about 'the noble Brutus' my legs gave way and I collapsed on to the floor, where, of course, the air was all right. In this way I learnt that firedamp is lighter than air and not dangerous to breathe."

\* \* \*

At Oxford JBS . . . decided to teach physiology, a decision which would have been recklessness in a lesser man, since he had neither degree nor other qualification in the subject. In fact Haldane, Fellow of the Royal Society and the author of more than 300 scientific papers, never did take any scientific degree, thus following his father, who never took a course in engineering but became president of the Institution of Mining Engineers.

\* \* \*

Haldane's interest in a unified science course reflected his own ability to take up a new subject and worry his way quickly into its essentials. . . . With physiology, to which he now devoted himself, he had the aid of environment, since he had been brought up surrounded by the rules of the discipline. . . . John Scott Haldane had discovered that it was carbon dioxide in the human bloodstream which enabled the muscles to regulate breathing under different conditions. But it was not known whether the carbon dioxide did so by making the blood more acid, as was suspected, or by some other method. Haldane therefore taught his son the technique developed by himself of gas analysis, by which very small amounts of gas can be accurately measured. Then he gave him, together with Peter Davies, a young worker in the physiological laboratory, the task of finding how carbon dioxide did this particular job. The experiments which followed enabled JBS to make a number of useful discoveries, and they encouraged him in the practice of self-experimentation for which he was to become famous.

He and his colleague argued that if acidity of the blood was the vital factor, then an increase in the amount of alkaline sodium bicarbonate in the blood would slow down breathing, since such slowing down would help to retain more carbon dioxide and thereby retain the normal balance. The first task was therefore to discover the amount of sodium bicarbonate already in a normal person's blood. This was not easy. In fact it was three months before Haldane and Davies got their different estimates to agree. . . .

Once Haldane and Davies had finished this first part of their work, they began to use themselves as guinea pigs—for one reason which Haldane was never tired of emphasizing: neither a dog nor a rabbit nor any experimental animal other than man can "tell you if he has a headache, or an upset of his sensors of smell, both of which I obtained as symptoms during these experiments."

They wanted to see if John Scott Haldane's theory about breathing was correct, but they wanted to give a quantitative answer—to be able to state, for instance, how much more one would breathe if the alkaline reserve in the blood were increased by a stated amount. And they wanted to find out if any symptoms of certain diseases could be put down to changes in the alkalinity of the blood.

The first part of the work was fairly easy. Haldane and Davies each ate about an ounce and a half of bicarbonate of soda—and each, as expected, found that his breathing was slowed and that the carbon dioxide in the blood rose to balance the bicarbonate. Here Haldane followed what was to be his universal rule. When reporting experiments on himself, he would rarely if ever note "I felt . . ." or "I began to pant. . . ." Scientific thinking was objective thinking, and the records were couched in the impersonal form of "J. H. panting . . ." or "J. H. finding difficulty in breathing." Explaining this, JBS once wrote: "In fact, I try to think of myself as I would of anyone else. This is the essence of justice."

Getting acid into the blood was more difficult than getting it out. To start with, Haldane began by drinking hydrochloric acid; if neat, this would have been fatal, so he had to dilute it—but he diluted it so much that it failed to have much effect. He then worked out a number of chemical tricks to smuggle the hydrochloric acid into his blood disguised as something else. One method was to drink a solution of ammonium chloride. At the first attempt he dissolved five grams in 100 c.c. of water—and on drinking the

solution was violently sick. He then diluted it still further and tried again; this time the trick worked, although he had to drink less than the carefully calculated amount which he estimated would kill him. The ammonium chloride, absorbed from the intestine, went to the liver, where it was turned into urea, leaving the acid behind. One or two ounces of it, JBS found, was sufficient to make him very short of breath, and after some of the experiments he panted for several days.

It had seemed unlikely that any practical results would spring from this work. However, as Appleton found when radar grew from his discovery of ionized layers in the stratosphere, the "purest" experiments can produce the most utilitarian results. So it was with this work on the acidity of the blood. Soon afterward a Continental doctor discovered that one particular kind of fit, from which some babies suffered and a few died, was caused by the extreme alkalinity of the blood. The ammonium chloride treatment was successfully used by the doctor to cure the condition. . . .

Many similar experiments followed during the next few years, some in the Oxford physiological laboratory, some in John Scott Haldane's own private laboratory at Cherwell. There was a thirteen-day experiment during which JBS drank eighty-five grams of calcium chloride dissolved in water and produced "intense diarrhoea, followed by constipation due to the formation of a large hard faecal mass. There was great general discomfort, pains in the head, limbs and back, and disturbed nights." To discover the change in the pressure of carbon dioxide in the lungs after violent exercise, he ran five times up and down a thirty-foot staircase, repeated the sequence nineteen times, and had samples of his breath taken after each. In the gas chamber of the Cherwell laboratory he recorded his own and other people's reactions to various concentrations of gas. And at Cherwell also he drank quantities of hydrochloric acid, reporting afterward that walking at three miles an hour caused severe panting and that cycling was impossible. "There were occasional slight headaches. A certain exhilaration and irritability of temper were noticed at times by myself and others, but there was no mental confusion, and the experiment was not unpleasant." In the case of the more dangerous carbon monoxide, the symptoms of poisoning were, he wrote, "the same as alcoholic poisoning, except that carbon monoxide goes a bit further. One is that you cannot walk straight

or talk straight, although you feel you are perfectly all right. If you go into a mine full of this gas with a bird in a cage, the bird gets drunk first, and then comes off its perch, and you yourself will probably feel full of beans. That is the great danger, for you tumble over, get unconscious and die."

These experiments, which had to be combined with Haldane's quota of teaching, were frequently uncomfortable, frequently unpleasant, and sometimes both. . . . But a little more information had been acquired about the way the human body works, a satisfactory conclusion for a man who could say, as Haldane did, "You cannot be a good human physiologist unless you regard your own body, and that of your colleagues, with the same sort of respect with which you regard the starry sky and yet as something to be used and, if need be, used up."

However useful it was, and of that there can be no doubt, there was a trace of exhibitionism about the way he spoke of such work, a flamboyance epitomized by his comment that the only way to test a chemical's reaction was to take ten times the dose listed as fatal in the British pharmacopoeia. . . .

\* \* \*

c.

### **B. F. Skinner Baby in a Box\***

*Since the publication of this article in the Ladies Home Journal in October, 1945, several hundred babies have been reared in what is now known as an "Air-Crib." The advantages reported here have been generously confirmed. Although cultural inertia is perhaps nowhere more powerful than in child-raising practices, and in spite of the fact that the device is not easy to build, its use has steadily spread. The advantages to the child and parent alike seem to be too great to be resisted. One early user, John M. Gray, sent a questionnaire to 73 couples who had used Air-Cribs for 130 babies. All but three described the device as "wonderful." The physical and psychological benefits reported by these users seem to warrant extensive research.*

In that brave new world which science is

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\* *Cumulative Record* 419-426 (Enlarged Edition, 1959). Copyright © 1959, 1961, Appleton-Century-Crofts, Inc. Reprinted by permission of Appleton-Century-Crofts, Educational Division, Meredith Corporation.

preparing for the housewife of the future, the young mother has apparently been forgotten. Almost nothing has been done to ease her lot by simplifying and improving the care of babies.

When we decided to have another child, my wife and I felt that it was time to apply a little labor-saving invention and design to the problems of the nursery. We began by going over the disheartening schedule of the young mother, step by step. We asked only one question: Is this practice important for the physical and psychological health of the baby? When it was not, we marked it for elimination. Then the "gadget-eering" began.

The result was an inexpensive apparatus in which our baby daughter has now been living for eleven months. Her remarkable good health and happiness and my wife's welcome leisure have exceeded our most optimistic predictions, and we are convinced that a new deal for both mother and baby is at hand.

We tackled first the problem of warmth. The usual solution is to wrap the baby in a half-a-dozen layers of cloth—shirt, nightdress, sheet, blankets. This is never completely successful. The baby is likely to be found steaming in its own fluids or lying cold and uncovered. Schemes to prevent uncovering may be dangerous, and in fact they have sometimes even proved fatal. Clothing and bedding also interfere with normal exercise and growth and keep the baby from taking comfortable postures or changing posture during sleep. They also encourage rashes and sores. Nothing can be said for the system on the score of convenience, because frequent changes and launderings are necessary.

Why not, we thought, dispense with clothing altogether except for the diaper, which serves another purpose—and warm the space in which the baby lives? This should be a simple technical problem in the modern home. Our solution is a closed compartment about as spacious as a standard crib. The walls are well insulated, and one side, which can be raised like a window, is a large pane of safety glass. The heating is electrical, and special precautions have been taken to insure accurate control.

After a little experimentation we found that our baby, when first home from the hospital, was completely comfortable and relaxed without benefit of clothing at about 86°F. As she grew older, it was possible to lower the temperature by easy stages. Now, at eleven months, we are operating at about 78°, with a relative humidity of 50 per cent.

Raising or lowering the temperature by more than a degree or two produces a surprising change in the baby's condition and behavior. This response is so sensitive that we wonder how a comfortable temperature is ever reached with clothing and blankets.

The discovery which pleased us most was that crying and fussing could always be stopped by slightly lowering the temperature. During the first three months, it is true, the baby would also cry when wet or hungry, but in that case she would stop when changed or fed. During the past six months she has not cried at all except for a moment or two when injured or sharply distressed—for example, when inoculated. The "lung exercise" which so often is appealed to to reassure the mother of a baby who cries a good deal takes the much pleasanter form of shouts and gurgles.

How much of this sustained cheerfulness is due to the temperature is hard to say, because the baby enjoys many other kinds of comfort. She sleeps in curious postures, not half of which would be possible under securely fastened blankets.

When awake, she exercises almost constantly and often with surprising violence. Her leg, stomach, and back muscles are especially active and have become strong and hard. It is necessary to watch this performance for only a few minutes to realize how severely restrained the average baby is, and how much energy must be diverted into the only remaining channel—crying.

A wider range and variety of behavior are also encouraged by the freedom from clothing. For example, our baby acquired an amusing, almost apelike skill in the use of her feet. We have devised a number of toys which are occasionally suspended from the ceiling of the compartment. She often plays with these with her feet alone and with her hands and feet in close co-operation.

One toy is a ring suspended from a modified music box. A note can be played by pulling the ring downwards, and a series of rapid jerks will produce *Three Blind Mice*. At seven months our baby would grasp the ring in her toes, stretch out her leg and play the tune with a rhythmic movement of her foot.

We are not especially interested in developing skills of this sort, but they are valuable for the baby because they arouse and hold her interest. Many babies seem to cry from sheer boredom—their behavior is restrained and they have

nothing else to do. In our compartment, the waking hours are invariably active and happy ones.

Freedom from clothes and bedding is especially important for the older baby who plays and falls asleep off and on during the day. Unless the mother is constantly on the alert, it is hard to cover the baby promptly when it falls asleep and to remove and arrange sheets and blankets as soon as it is ready to play. All this is now unnecessary.

Remember that these advantages for the baby do not mean additional labor or attention on the part of the mother. On the contrary, there is an almost unbelievable saving in time and effort. For one thing, there is no bed to be made or changed. The "mattress" is a tightly stretched canvas, which is kept dry by warm air. A single bottom sheet operates like a roller towel.\* It is stored on a spool outside the compartment at one end and passes into a wire hamper at the other. It is ten yards long and lasts a week. A clean section can be locked into place in a few seconds. The time which is usually spent in changing clothes is also saved. This is especially important in the early months. When we take the baby up for feeding or play, she is wrapped in a small blanket or a simple nightdress. Occasionally she is dressed up "for fun" or for her play period. But that is all. The wrapping blanket, roller sheet, and the usual diapers are the only laundry actually required.

Time and labor are also saved because the air which passes through the compartment is thoroughly filtered. The baby's eyes, ears, and nostrils remain fresh and clean. A weekly bath is enough provided the face and diaper region are frequently washed. These little attentions are easy because the compartment is at waist level.

It takes about one and one-half hours each day to feed, change, and otherwise care for the baby. This includes everything except washing diapers and preparing formula. We are not interested in reducing the time any further. As a baby grows older, it needs a certain amount of social stimulation. And after all, when unnecessary chores have been eliminated, taking care of a baby is fun.

An unforeseen dividend has been the contribution to the baby's good health. Our pediatrician readily approved the plan before the baby was born, and he has followed the results enthu-

siastically from month to month. Here are some points on the health score: When the baby was only ten days old, we could place her in the preferred face-down position without danger of smothering, and she has slept that way ever since, with the usual advantages. She has always enjoyed deep and extended sleep, and her feeding and eliminative habits have been extraordinarily regular. She has never had a stomach upset, and she has never missed a daily bowel movement.

The compartment is relatively free of spray and air-borne infection, as well as dust and allergic substances. Although there have been colds in the family, it has been easy to avoid contagion, and the baby has completely escaped. The neighborhood children troop in to see her, but they see her through glass and keep their school-age diseases to themselves. She has never had a diaper rash.

We have also enjoyed the advantages of a fixed daily routine. Child specialists are still not agreed as to whether the mother should watch the baby or the clock, but no one denies that a strict schedule saves time, for the mother can plan her day in advance and find time for relaxation or freedom for other activities. The trouble is that a routine acceptable to the baby often conflicts with the schedule of the household. Our compartment helps out here in two ways. Even in crowded living quarters it can be kept free of unwanted lights and sounds. The insulated walls muffle all ordinary noises, and a curtain can be drawn down over the window. The result is that, in the space taken by a standard crib, the baby has in effect a separate room. We are never concerned lest the doorbell, telephone, piano, or children at play wake the baby, and we can therefore let her set up any routine she likes.

But a more interesting possibility is that her routine may be changed to suit our convenience. A good example of this occurred when we dropped her schedule from four to three meals per day. The baby began to wake up in the morning about an hour before we wanted to feed her. This annoying habit, once established, may persist for months. However, by slightly raising the temperature during the night we were able to postpone her demand for breakfast. The explanation is simple. The evening meal is used by the baby mainly to keep herself warm during the night. How long it lasts will depend in part upon how fast heat is absorbed by the surrounding air.

One advantage not to be overlooked is that the soundproofing also protects the family from the baby! Our intentions in this direction were

\* The canvas and "endless" sheet arrangement was soon replaced with a single layer of woven plastic, which could be cleaned and instantly wiped dry.

misunderstood by some of our friends. We were never put to the test, because there was no crying to contend with, but it was never our policy to use the compartment in order to let the baby "cry it out."

Every effort should be made to discover just why a baby cries. But if the condition cannot be remedied, there is no reason why the family, and perhaps the neighborhood as well, must suffer. (Such a compartment, by the way, might persuade many a landlord to drop a "no babies" rule, since other tenants can be completely protected.)

Before the baby was born, when we were still building the apparatus, some of the friends and acquaintances who had heard about what we proposed to do were rather shocked. Mechanical dish-washers, garbage disposers, air cleaners, and other laborsaving devices were all very fine, but a mechanical baby tender—that was carrying science too far! However, all the specific objections which were raised against the plan have faded away in the bright light of our results. A very brief acquaintance with the scheme in operation is enough to resolve all doubts. Some of the toughest skeptics have become our most enthusiastic supporters.

One of the commonest objections was that we were going to raise a "softie" who would be unprepared for the real world. But instead of becoming hypersensitive, our baby has acquired a surprisingly serene tolerance for annoyances. She is not bothered by the clothes she wears at playtime, she is not frightened by loud or sudden noises, she is not frustrated by toys out of reach, and she takes a lot of pommeling from her older sister like a good sport. It is possible that she will have to learn to sleep in a noisy room, but adjustments of that sort are always necessary. A tolerance for any annoyance can be built up by administering it in controlled dosages, rather than in the usual accidental way. Certainly there is no reason to annoy the child throughout the whole of its infancy, merely to prepare it for later childhood.

It is not, of course, the favorable conditions to which people object, but the fact that in our compartment they are "artificial." All of them occur naturally in one favorable environment or another, where the same objection should apply but is never raised. It is quite in the spirit of the "world of the future" to make favorable conditions available everywhere through simple mechanical means.

A few critics have objected that they would

not like to live in such a compartment themselves—they feel that it would stifle them or give them claustrophobia. The baby obviously does not share in this opinion. The compartment is well ventilated and much more spacious than a Pullman berth, considering the size of the occupant. The baby cannot get out, of course, but that is true of a crib as well. There is less actual restraint in the compartment because the baby is freer to move about. The plain fact is that she is perfectly happy. She has never tried to get out nor resisted being put back in, and that seems to be the final test.

Another early objection was that the baby would be socially starved and robbed of the affection and mother love which she needs. This has simply not been true. The compartment does not ostracize the baby. The large window is no more of a social barrier than the bars of a crib. The baby follows what is going on in the room, smiles at passers-by, plays "peek-a-boo" games, and obviously delights in company. And she is handled, talked to, and played with whenever she is changed or fed, and each afternoon during a play period which is becoming longer as she grows older.

The fact is that a baby will probably get more love and affection when it is easily cared for, because the mother is not so likely to feel overworked and resentful of the demands made upon her. She will express her love in a practical way and give the baby genuinely affectionate care.

It is common practice to advise the troubled mother to be patient and tender and to enjoy her baby. And, of course, that is what any baby needs. But it is the exceptional mother who can fill this prescription upon demand, especially if there are other children in the family and she has no help. We need to go one step further and treat the mother with affection also. Simplified child care will give mother love a chance.

A similar complaint was that such an apparatus would encourage neglect. But easier care is sure to be better care. The mother will resist the temptation to put the baby back into a damp bed if she can conjure up a dry one in five seconds. She may very well spend less time with her baby, but babies do not suffer from being left alone but only from the discomforts which arise from being left alone in the ordinary crib.

How long do we intend to keep the baby in the compartment? The baby will answer that in time, but almost certainly until she is two years old, or perhaps three. After the first year, of

course, she will spend a fair part of each day in a play-pen or out-of-doors. The compartment takes the place of a crib and will get about the same use. Eventually it will serve as sleeping quarters only.

We cannot, of course, guarantee that every baby raised in this way will thrive so successfully. But there is a plausible connection between health and happiness and the surroundings we have provided, and I am quite sure that our success is not an accident. The experiment should, of course, be repeated again and again with different babies and different parents. One case is enough, however, to disprove the flat assertion that it can't be done. At least we have shown that a moderate and inexpensive mechanization of baby care will yield a tremendous saving in time and trouble, without harm to the child and probably to its lasting advantage.

d.

**David M. Rorvik and Landrum B. Shettles**  
**You Can Choose Your Baby's Sex\***

\* \* \*

Interest in choosing sex remains as high among prospective parents today as it ever was, despite the almost universal unconcern of baby doctors. And failure to produce the desired sex still creates as much anguish as it did in the past—perhaps more, since we have come to expect so much from modern medical science. . . .

One doctor who does understand the anguish of such parents is Landrum B. Shettles, M.D., Ph.D., D.Sc. (He is an assistant attending obstetrician-gynecologist at Columbia-Presbyterian Medical Center and an assistant professor of clinical obstetrics and gynecology at Columbia College of Physicians and Surgeons.) Sitting in his office, he recalls . . . when he made the discovery that may help millions select the sex of their offspring.

\* \* \*

[A]fter examining more than 500 sperm specimens, he is convinced that . . . small, round-headed sperms carry the male-producing chromosomes, and the larger, oval-shaped type carry the female-producing X chromosomes. [I]n most cases, the round sperm far outnumbered the oval-shaped sperm.

\* \* \*

After making his discovery, Dr. Shettles published his findings in the scientific journal *Nature* and suddenly found himself in the middle of a controversy.

Not everybody has agreed with his findings, and he does not claim scientific infallibility. But he does stand on his record, on observations he has made in the laboratory and, most important, on his results to date. Other researchers have provided some impressive corroboration of Dr. Shettles' work.

As soon as he had made his initial discovery, Dr. Shettles had only one thing in mind: to find some means of exploiting this new knowledge to help parents choose the sex of their children. Since there definitely seemed to be a difference in the overall size of the two types of sperm, he reasoned, there must be other differences as well. Perhaps one type was stronger than the other or faster—or both. Perhaps one type could survive longer in a certain environment than the other. . . .

It seemed fairly certain that the larger, female-producing sperm (now called gynospers) must be more resistant than the other type. Why should there be nearly twice as many of the smaller, boy-producing variety (known as androsperms) in the ejaculate of the average male if not to compensate for some inferiority in coping with the environment beyond the male reproductive tract? There may be as many as 170 boys conceived for every 100 girls, and for every 100 female births, there are about 105 male births. . . .

What accounts for the greater slaughter of androsperms within the womb? To find out, Dr. Shettles began studying the environment that exists inside the vagina and uterus at about the time of conception. . . .

\* \* \*

Acid inhibits both gynospers and androsperms, but it harms the androsperms first and most, cutting them out of the herd and thus out of competition. The gynospers' greater bulk seems to protect them from the acid for much longer periods than their little brothers are able to survive.

Alkaline secretions are kind to both types of sperm and generally enhance the chances for fertilization. But in the absence of hostile acids, the androsperms are able to use the *one* advantage they have over their sisters: the speed and agility that their small, compact heads and long tails give them.

\* Look 86-98 (April 21, 1970). Reprinted by permission.

\* \* \*

As a result of these findings, Dr. Shettles has formulated two procedures—one to be used if a female child is desired, the other if a male is wanted. These procedures can be used in the home *without* prior semen analysis.

#### *The procedure for female offspring*

1. Intercourse should cease two or three days before ovulation. Timing is the most important factor.
2. Intercourse should be immediately preceded, on each occasion, by an acidic douche consisting of two tablespoons of white vinegar to a quart of water. The timing might be enough to ensure female offspring, but the douche makes success all the more likely, since the acid environment immobilizes the androsperms.
3. If the wife normally has orgasm, she should try to avoid it. Orgasm increases the flow of alkaline secretions, and these could neutralize or weaken the acid environment that enhances the chances of the gynosperms.
4. The face-to-face, or "missionary," position should be assumed during intercourse. Dr. Shettles believes that this makes it less likely that sperm will be deposited directly at the mouth of the cervix, where they might escape the acid environment of the vagina.
5. Shallow penetration by the male at the time of male orgasm is recommended. Again, this helps make certain that the sperm are exposed to the acid in the vagina and must swim through it to get to the cervix.
6. No abstinence from intercourse is necessary, until after the final intercourse two or three days before ovulation. A low sperm count increases the possibility of female offspring, so frequent intercourse, prior to the final try two or three days before ovulation, cannot hurt and may actually help. This may be why Dr. Shettles says that "having girls is more fun."

#### *The procedure for male offspring*

1. Intercourse should be timed as close to the moment of ovulation as possible.
2. Intercourse should be immediately preceded, on each occasion, by a baking-soda douche, consisting of two tablespoons of baking soda to a quart of water. The solution should be permitted to stand for 15 minutes before use. This allows the soda to become completely dissolved.
3. Female orgasm is not necessary but is desirable. If a woman normally has orgasm, her hus-

band should time his to coincide with hers or let her experience orgasm first.

4. Vaginal penetration from the rear is the recommended position. This, Dr. Shettles says, helps ensure deposition of sperm at the entrance of the womb. This is desirable because the secretions within the cervix and womb will be highly alkaline, more so even than in the vagina, in spite of the alkaline douche, and an alkaline environment is most favorable to androsperms.
5. Deep penetration at the moment of male orgasm will help ensure deposition of sperm close to the cervix.
6. Prior abstinence is necessary; intercourse should be avoided completely from the beginning of the monthly cycle until the day of ovulation. This helps ensure maximum sperm count, a factor favoring androsperms.

\* \* \*

Some observers believe that our new ability to choose the sex of our children will result in a bumper crop of boys, but Dr. Shettles is personally convinced that parents will not use his techniques to produce either mostly males or mostly females. "Over the years, parents have expressed only one desire," he says, "and that is to have families that are well balanced in terms of sex. Most find an equal number of boys and girls ideal."

Many couples have told Dr. Shettles they had initially planned for a family of two children, hoping for one of each. But when both offspring turned out to be of the same sex, they made a third attempt and so on. So it is not too far-fetched to envision sex-selection making a significant contribution in the effort to control the population explosion. How much better it would be to achieve the ideal family balance in two tries instead of three or four or more or never. The advantages of sex-selection are manifest: parental satisfaction, balanced families, very possibly smaller families and, *healthier* families.

Sometimes health—or lack of it—is attached to our sex chromosomes. Only males, for example, suffer from hemophilia, the grim and often fatal "bleeder's disease." Similar hereditary, sex-linked diseases include one type of muscular dystrophy and numerous enzyme-deficiency disorders that can kill, cripple and retard for life.

Though most of these diseases remain incurable, they can be prevented if carriers of sex-linked diseases could simply avoid conceiving children of the vulnerable sex.

\* \* \*



. . . For the first time in all time, parents have the opportunity to make a scientific attempt at choosing the sex of their children and to make that attempt with a high expectation of success.

e.

**Sir Bernard Lovell**  
**Man Moves into the Universe\***

\* \* \*

The scientific reasons for a manned Mars expedition are, of course, immense. . . . If replicating organisms are found to exist in the atmosphere or on the surface of the planet, unqualified support would thereby be produced for the belief that widespread development of organisms has occurred elsewhere in the universe.

The immense importance of this issue demands that the world as a whole should share the responsibility for the investigation. Contamination by human species, or by earlier rockets or satellites before the manned flights, could prejudice the entire investigation. The Soviets have not, so far, expressed their agreement with the standards suggested by the National Aeronautics and Space Administration and the Committee on Space Research of the International Council of Scientific Unions. Indeed their planetary technique, as exhibited by their investigations of Venus, allows the carrier bus to plunge to destruction in the planet's atmosphere.

The urgency of renewing attempts at a clear international understanding on these investigations arises also because of the danger of contamination on Earth by returning manned planetary probes. Even though the moon is an arid body, the Apollo program involves strict quarantine arrangements for the returning astronauts. The risks of contamination are probably negligible; nevertheless NASA has quite rightly taken

every reasonable safeguard. With Mars the risk must be far greater. The only safe assumption to make is that a spacecraft returning from Mars would probably convey entirely foreign organisms to the terrestrial environment. The consequences could be disastrous to crops or animal life unless the necessary controls can be exercised. This is manifestly an international problem, and international agreement is essential on the biological investigations to be made before a manned flight is attempted, and on the quarantine and other biological safeguards to be applied to the returning spacecraft.

\* \* \*

**NOTE**

HANS JONAS  
PHILOSOPHICAL REFLECTIONS ON  
EXPERIMENTING WITH HUMAN SUBJECTS\*

\* \* \*

[P]rogress is an optional goal, not an unconditional commitment, and . . . its tempo in particular, compulsive as it may become, has nothing sacred about it. Let us also remember that a slower progress in the conquest of disease would not threaten society, grievous as it is to those who have to deplore that their particular disease be not yet conquered, but that society would indeed be threatened by the erosion of those moral values whose loss, possibly caused by too ruthless a pursuit of scientific progress, would make its most dazzling triumphs not worth having. Let us finally remember that it cannot be the aim of progress to abolish the lot of mortality. Of some ill or other, each of us will die. Our mortal condition is upon us with its harshness but also its wisdom—because without it there would not be the eternally renewed promise of the freshness, immediacy, and eagerness of youth; nor, without it, would there be for any of us the incentive to number our days and make them count. . . .

\* 25 *Bulletin of the Atomic Scientists* 4, 6 (1969). Copyright © 1969 by the Educational Foundation for Nuclear Science. Reprinted by permission of Science and Public Affairs, the *Bulletin of the Atomic Scientists*, and by permission of Basic Books, Inc., publishers of Eugene Rabinowitch and Richard S. Lewis, eds.: *Man on the Moon*. New York: Basic Books, Inc. (1969).

\* 98 *Daedalus* 245 (1969). Reprinted by permission of *Daedalus*, Journal of the American Academy of Arts and Sciences, Boston, Massachusetts.



## B.

## Man's Willingness to Risk Human Lives

"[W]e have become accustomed to the fact that many activities are permitted, even though statistically we know they will cost lives, since it costs too much to engage in these activities more safely or to abstain from them altogether. We have grade crossings, even though we know that with grade crossings a certain number of people will be killed each year and even though grade crossings could be eliminated relatively easily. We use automobiles—knowing that they cost us fifty thousand lives each year—because to use safer, slower means of transport would be far too costly in terms of pleasure and profits foregone. Worse even than that, we use automobiles with relatively cheap (but relatively dangerous) control systems, and so on *ad infinitum*. And we do this because we deem the lives taken to be cheaper than the costs of avoiding the accidents in which they are taken."\*

The materials in this section examine risktaking in the endeavors of science and technology. The debate over the introduction of safety provisions for the coal mining industry illustrates society's refusal to eliminate known, avoidable risks because the benefits to society outweigh the estimated harm. Beyond this "calculated risk," the oral contraceptive case which appears in Chapter Eleven suggests that society is also willing to take "blind" risks with only a very rough idea of the relative benefit and harm. Another case study in this section, reconstructed from the early days of interspace exploration, illustrates the problems of making decisions about risks when neither the benefits nor the harm of an endeavor are adequately known.

Over the centuries similar dilemmas have arisen in many areas of human endeavor. "As we in our day can smile condescendingly at the primitives and ancients who practiced human sacrifice for what they considered to be the general good of the tribe or nation, future generations may ask whether we could make human sacrifice more acceptable in our day by calling it 'social cost.'"<sup>†</sup> Perhaps the materials are proof of "man's inhumanity to man," or perhaps they only provide evidence of inherent limitations in man's capacity or willingness to curtail risktaking beyond a certain point.

In studying these materials, consider the following questions:

1. What risks are, and should be, acceptable to society?
2. What values do we seek to maximize by accepting or rejecting certain kinds of risks?
3. Are these values in conflict with other individual and social values?
4. Should the fact that risks pervade all human activity make risks which occur in the pursuit of science and technology more or less acceptable?
5. What part in risk-decisions should the various participants play?
6. When should the balancing of risks and benefits be left to the person affected, and when, if ever, should society impose limits on risktaking?<sup>‡</sup>

\* Guido Calabresi: "Reflections on Medical Experimentation in Humans," 98 *Daedalus* 387 (1969).

† Edmond Cahn: "Drug Experiments and the Public Conscience," Paul Tatalay, ed.: *Drugs in Our Society*. Baltimore: The Johns Hopkins Press 260-261 (1964).

‡ The mechanisms by which society may exercise collective control over risktaking are merely touched upon here and will be enlarged upon in Part Four.

## 1.

**The Health and Safety of Coal Miners\***

## a.

**Statement by President Harry S. Truman  
upon Signing an Amendment to the Federal  
Coal Mine Safety Act—July 16, 1952**

I have today signed S. 1310, a bill relating to the prevention of major disasters in coal mines.

This measure is a significant step in the direction of preventing the appalling toll of death and injury to miners in underground mines. These totally unnecessary and preventable accidents result in grief-stricken families as well as a shocking loss and waste of skilled manpower.

Under Public Law 49, 77th Congress, the Secretary of the Interior has been authorized to inspect coal mines to the end of making them safer places in which to work. In reporting on their inspections of coal mines, the Federal coal mine inspectors have recommended measures to correct unsafe conditions and practices, but there has been no authority to enforce these recommendations. Disaster has, in many instances, followed in the wake of repeated and unheeded warnings of impending danger.

S. 1310 will, in part, correct this situation. The measure seeks to help prevent major disasters in coal mines from five causes—explosion, fire, inundation, mantrip, or manhoist accidents. Nevertheless, the legislation falls short of the recommendation I submitted to the Congress to meet the urgent problems in this field. In particular, the bill has the following deficiencies:

1. Coal mines in which less than 15 persons are regularly employed underground are exempted from compliance with any of the mine safety provisions regardless of whether a major disaster might be imminent. This exempts a large

group of mines, many of which are hazardous and need a great deal of safety improvement. Inspections of these mines will continue under the earlier statute, but compliance with recommendations of the Federal inspectors will be on a purely voluntary basis.

2. The provisions of the legislation are directed solely toward the prevention of major disasters from the five causes mentioned heretofore. Such disasters accounted for only approximately 7 percent of the coal mine fatalities during the last 20 years. The broad phase of accident prevention in general remains the responsibility of the States in which coal is mined, despite the record to date indicating either the inability or unwillingness of the States to meet this responsibility.

3. The legislation contains several exemptions to the safety provisions particularly with regard to replacement of dangerous electrical equipment and faulty ventilation systems which have been the causes of most recent major disasters. I am advised that these exemptions were provided to avoid any economic impact on the coal mining industry, but they are so worded that the unsafe conditions and practices could continue for years before the mines would be required to comply with the law.

4. The measure contains complex procedural provisions relating to inspections, appeals, and the postponing of orders which I believe will make it exceedingly difficult if not impossible for those charged with the administration of the act to carry out an effective enforcement program. I believe that it is possible to draft simpler and more effective procedural provisions which would not adversely affect the rights of any of the parties concerned with the prevention of mine injuries and deaths.

5. The measure vests the mine safety enforcement functions directly in the Director of the Bureau of Mines. This violates the principle now established for most executive departments that functions should be vested in the department head in order to provide the flexibility of organization and clear lines of authority and accountability essential for effective administration.

... We will do our very best to prevent mining disasters with the authority granted in this bill but the Congress eventually will have to meet its responsibility for enacting legislation which provides tools fully adequate to prevent the great loss of life and the thousands of crippling injuries due to mine accidents.

\* Except as otherwise noted, all materials in this section are reprinted from the *Hearings on Bills to Improve the Health and Safety Conditions of Persons Working in the Coal Mining Industry of the United States before the Subcommittee on Labor of the Senate Committee on Labor and Public Welfare*, 91st Congress, 1st Session (1969). Members of the Subcommittee were: Senators H. A. Williams, chairman; J. Randolph; C. Pell; G. Nelson; W. F. Mondale; T. F. Eagleton; A. Cranston; J. K. Javits; W. L. Prouty; W. B. Saxbe; H. Bellmon; and R. S. Schweiker.

b.

**Recommendations by President Lyndon B. Johnson Urging the Enactment of the Federal Coal Mine Health and Safety Act of 1968—September 11, 1968**

When President Harry Truman signed the Coal Mine Safety Act sixteen years ago, he declared that, "the legislation falls far short of the recommendation I submitted to the Congress to meet the urgent problems in this field."

The record shows just how far short that measure fell. Since 1952, over 5,500 miners have been killed on the job. Another 250,000 were seriously disabled. No one knows how many thousands more have died, their lungs blackened by the ravages of coal dust disease—pneumoconiosis.

Today, despite the safety measures on the books, coal mining remains the most dangerous and hazardous occupation for the American worker. The National Safety Council reports that of the forty major industries in this country, coal mining ranks highest in frequency and severity of death and injury.

We have succeeded in preventing many of the major coal mine disasters that took dozens of lives at a time. But coal miners are still crushed by cave-ins, burned by explosions, maimed by antiquated and unsafe equipment. They still pay with their health for the right of earning a living because the air they breathe is thick with coal dust. At the very least, one out of every ten active miners—and one out of every five retired miners—suffers from a serious respiratory disease. For the tens of thousands of miners so afflicted, the shortness of breath may shorten their lives.

Consider some of the tragedies of just the past few months:

- A massive landslide at the face of a mine in West Virginia crushed three workers to death.
- A major explosion in a Kentucky mine snuffed out the lives of a nine-man crew. The cause: the dangerous practice of hauling dynamite on a drilling machine.
- Miners in West Virginia inadvertently drilled into an abandoned water-filled mine shaft, and four were drowned.

There was nothing inevitable about these disasters. They happened because our coal mine safety laws are inadequate, and because even existing laws are all too frequently ignored.

At the present time, Federal inspectors have too little jurisdiction over the working face of

the mines, where nearly half of the fatal accidents occur. They cannot tell a mine owner to shore up a sagging roof in this area. They cannot require the replacement of a potentially hazardous machine. They cannot require a reduction in the level of coal dust in the air to safe limits because the laws do not even touch on the problem of health standards. They have no jurisdiction at all over the nation's 2,250 surface mines, which account for almost 40 percent of our coal production.

Our inspectors are not even backed by effective enforcement penalties where the law does apply. It is a measure of this weakness that last year more than 80 percent of the nation's nearly 6,000 underground coal mines were in violation of one or more federal safety standards.

Today, I urge the Congress to remedy these defects. I recommend the Federal Coal Mine Health and Safety Act of 1968.

It is time that an enlightened and progressive nation give its coal miners a new charter of health and safety as they toil for the comfort of us all.

This Act will, for the first time:

- Extend federal enforcement to the face of mine, the area where so many deaths and injuries occur, as well as correcting 18 other specific safety omissions in the present law.
- Abolish the "grandfather clause" which allows old and unsafe electrical equipment to be used.
- Give the Secretary of the Interior authority to develop and issue safety standards as the need arises.
- Provide a way to reduce the human devastation of coal dust disease by requiring the Secretary of Health, Education and Welfare to develop health criteria, and the Secretary of the Interior, following such criteria, to issue health standards and enforce them.
- Impose meaningful and effective sanctions for failure to comply with the terms of the law: criminal penalties and higher fines for willful violations, civil penalties and injunctions to deter and stop unsafe practices.
- Apply the law's reach to surface coal mines.
- Create simplified and streamlined enforcement procedures to require quick correction of hazardous conditions.

The cost of this measure will be small. Its

benefits will be large, not only in terms of the lives it can save and the injuries it can prevent, but in practical terms of dollars and cents. Last year alone, over 1.8 million man-days were lost to the nation and the mine owners as a result of job-related deaths and injuries. Many millions of dollars in workmen's compensation payments were awarded to injured and disabled miners.

\* \* \*

**c. Statement of Senator Jennings Randolph,  
West Virginia—February 27, 1969**

\* \* \*

. . . I am a prime sponsor of two of the measures on coal mine health and safety before us. On January 10, 1969, I introduced the then administration's proposed Federal Coal Mine Health and Safety Act of 1969, S. 355, a bill carefully developed over months of study and effort and refined further as a consequence of the tragic November 1968, fire and explosion at Mountaineer Mine No. 9 between Farmington and Mannington, W. Va. S. 355 is combined coal mine health and safety legislation.

On January 21, 1969, I introduced, at the request of President Boyle of the United Mine Workers of America, another measure—S. 467, proposed with the title, "Coal Mine Health Act," and considerably broader in scope than the provisions of S. 355 relating to the purpose of elim-

ination of health dangers to coal miners resulting from the inhalation of coal dust.\*

\* \* \*

Coal miners must have the safest attainable working conditions short of the destruction of their jobs. There must be improvement of existing conditions in the interest of arresting and, hopefully, of preventing black lung and other diseases incident to coal mining. I underscore the word "preventing" if for no other reason than to emphasize that legislation relating to compensation for claims growing out of the occupation is generally a State matter, the responsibility of the legislatures of the States.

\* \* \*

**d. Statement of Senator Harrison A. Williams, Jr.,  
New Jersey—February 27, 1969**

\* \* \*

The beginnings of coal mining in this country go back to 1730; our Federal Bureau of Mines was established in 1910. Yet, it was not until 1941 that the first Federal coal mine safety legislation was enacted into law . . . [Senator Randolph] was one of the chief architects of that beginning accomplishment.

In 1942, the Russell Sage Foundation published a study regarding the prevention of fatal explosions in coal mines. The authors of that

\* At the time I introduced the Johnson Administration bill (January 10, 1969), I said I believed coal mine safety and coal mine health should be handled separately, but I introduced the Johnson Administration measure as a single bill, with the subject matter divided into separate titles. There seemed, on January 10, to be much closer but not complete agreement on the part of producers and miners to the safety provisions of the Administration proposal than to the health (dust control) provisions. The United Mine Workers differed sharply with the Administration's proposed legislative approach to the dust control (anti-black lung) problem. That is why the UMW proposed and why I introduced a dust control bill.

\* \* \*

There are probably opponents of health provisions of these bills (on medical and/or other technical grounds) who are not against the safety provisions.

All persons will not agree with this evaluation, but it seems to me that the prime target of all this legislation is first to recodify and modernize the mine

safety laws to mitigate against or, if possible, totally obviate, the catastrophic coal mine explosions and fires which kill and seriously injure miners and cause intensive property damage. If we can do this without encumbering the objective and delaying its fulfillment with a long and tedious controversy over dust control procedures and technology, it seems to me that we should do it.

\* \* \*

Of course the "safest" mine and the "healthiest" mine is the closed one—but it does not produce the coal that is so vital to much of our country's economy—and it does not provide payrolls and livelihood for miners. The search must be for feasible ways to achieve safety and improved occupational health in active mining operations where jobs are provided. To be too reckless or too little, too late in any direction in coal mine safety and coal mine health would be catastrophic. To achieve the feasible and the "proper balance" and, at the same time, the effective will not be done easily. . . . [Letter to the Editor, *St. Louis Post-Dispatch* from Senator Jennings Randolph, February 24, 1969.]

report, commenting on title I of the Federal Coal Mine Safety Act, wrote:

... Undoubtedly, the record of explosions in 1940 was influential, if not the determining factor in bringing about this legislation. In this there was no departure from the usual pattern; dead miners have always been the most powerful influence in securing passage of mining legislation.

I believe the foundation referred to the great difficulty in getting the Bureau of Mines established in 1910, as well as the long history of unsuccessful efforts to enact legislation before the first breakthrough by the 1941 act.

Here we are, 27 years later, and yet another mining disaster has occurred—the Mannington-Farmington, W.Va., No. 9 mine explosions, which killed 78 men in November 1968. The stark reality contained in the Russell Sage Foundation 1942 study is as true today as it was then. Our antidisaster actions are always after the fact.

In a report from the Bureau of Mines, Department of the Interior, called "Coal Mine Fatalities of 1968," there is documented a total of 309 work fatalities in 1968. By dividing 78—the number of men killed in the Mannington explosion disaster—into 309, there was the equivalent of four Mannington disasters that year.

In 1967, the Bureau reported 220 mine-worker fatalities. Despite such a large number of fatalities, the report provided the reassuring statement that there were no "major disasters" in coal mines for that year. This is because the legal definition of a major disaster means that five or more men are killed simultaneously.

As far as this Senator is concerned, these hearings must take great care to look behind and deeper than the legal definition. We must come to understand the human definition of a mine disaster. My starting point is that one man killed equals one disaster. And it seems to me that is the only definition we can in good conscience offer the widows and children of the 309 mine-workers killed in 1968.

\* \* \*

... The largest number of underground deaths in a year are caused by roof falls in the mines. The second ranking cause is gas and dust explosions; other leading causes are haulage accidents, electrical, and machinery. Deaths from these five ranking causes of fatal accidents made up 93 percent of the total deaths in underground workings in 1968.

We have recently been made aware of the seriousness and the magnitude of the miner's

occupational respiratory disease, pneumoconiosis, or "black lung," which is caused by inhalation of coal dust. Dr. William Stewart, the Surgeon General of the United States, has said that "black lung" conservatively affects more than 100,000 soft-coal workers.

This information may be news to us but it is an old story to the coalminers. According to Dr. Lorin E. Kerr, assistant to the executive medical officer, United Mine Workers of America welfare and retirement fund, what we now know as "black lung" was called "miner's con" years ago. The first medical term was "miner's asthma"; the cause of miner's spitting, coughing, and breathlessness was unknown, so doctors used the phrase "miner's asthma" to label the condition.

After years of study and research we now call it pneumoconiosis. But call it what you will—it disables and kills.

Men die from this disease, but before they die they suffer extreme pain and shortness of breath for 15 to 20 years—many continuing to go down to the mines to work with their affliction.

The mechanization of our coal mining industry has increased the miner's chances for contracting the disease. High-powered drills, mechanized loaders, electrical cables—the whole mechanized process—creates more coal dust.

The seriousness of this problem is underscored when we consider the Surgeon General's estimate that as many as 70 percent of the 144,000 coal miners in this country are suffering from "black lung." Furthermore, pneumoconiosis is not recognized as a compensable occupational disease except in the States of Pennsylvania, Virginia, and Alabama. We just heard from Senator Randolph that it appears as though this is going to be increased by West Virginia shortly.

\* \* \*

... It is obvious that we must have legislative standards and technical methods for controlling coal dust at safe levels.

In this otherwise dark picture there is one pleasant thought: If I read correctly the coal industry's production and fiscal reports, these hearings will not be complicated by the "bankruptcy" arguments so frequently leveled at this type legislation.

The National Coal Association calls its product "The Fuel of the Future."

In a July 1968 press release, the NCA stated

that the bituminous coal industry has recovered from its recent lean years. In 1967 it produced 551 million tons of coal and prospects were bright. Major research efforts were then underway to convert coal to competitive gasoline and pipeline gas. Major oil companies, seeing their supplies dwindling, are investing heavily in coal. The release concluded: "As America's main reservoir of energy in the years ahead, coal's prospects are glittering."

\* \* \*

e.

**Statement of W. A. Boyle, President,  
United Mine Workers of America—  
February 27, 1969**

\* \* \*

The Federal Mine Safety Act that was passed in 1941 at the insistence of the senior Senator of West Virginia, and people like him, what happened? We had to take it. It was a watered-down version of what the United Mine Workers wanted. And we accepted the 1941 law because it was better than no law at all. We had no law. And that law, Mr. Chairman, if you care to review it, provides that the Federal coal mine inspectors under the direction of the Secretary of Interior . . . had no authority whatsoever under the 1941 act except to go in and find, if the coal operator would let him go in, violations of the law, and make recommendations.

And then in 1952 we came to the Congress session after session after session during the interim period, and in 1952, after the disasters occurred in the State of Illinois and elsewhere, the Congress of the United States said it was high time that we gave these Federal inspectors some authority. They vested their men with the authority in 1952 to not only make inspections and recommendations, but to close mines and withdraw men if imminent danger existed. So we had been living with those things.

Then year after year we tried to get all mines in the United States covered by some legislation. Union mines and nonunion mines. Mines that employed from one man to 15 men were excluded. We tried to get them covered. We failed, miserably failed, before the Congress of the United States to get any consideration on that until 1966, when I appeared before both Houses.

They passed legislation then that included and protected those men who worked in mines employing less than 15 men, and it has elimi-

nated and cut down the accident rate in coal mines.

Contrary to what the coal operators who opposed that legislation at that time, contrary to what they had to say, and they tried to impress upon the Congress of the United States that the mines would close down because they were in no position to operate under those restrictive laws—I am happy to relate to you that I know of no coal mines that have been closed down by the passage of that law. But to the contrary, new mines have been opened up, both large and small.

\* \* \*

As I indicated earlier, we have exercised our judgment and concluded that, if both health and safety standards are included in one bill, there will be no bill. Why do I say this? Because there are opponents who are opposed to some features of the health bill, and there are opponents who are opposed to some features of the safety bill, and, if you allow your opposition to concentrate against one bill, your prospects of success are diminished, if not completely destroyed. For these reasons, we proceeded in two directions, both with the same basic motivation, to improve the health and safety of the coal miners of the United States.

\* \* \*

I direct my remarks to S. 467, the health bill pending before your committee. Before I discuss the specifics of the bill, I think it advisable to provide a historical background, particularly with comments involving coal miners' chest diseases, in general. The medical profession had concluded that, in its opinion, coal dust was not harmful to the lungs. It was their view that only exposure to silica dust was damaging to the lungs. However, in recent years, medical evidence has revealed that coal dust in the lungs does cause a major disabling disease. In fact, in recent years, there has been a rapid rise in the incidence of chest disease among coal miners and, more importantly, this incidence has developed in greater percentage among men in their forties and fifties, a most unhappy situation.

In our opinion, the increasing incidence of coal miners' dust disease has developed because of the introduction of mechanical machinery in the mines. This is particularly true when one is aware of the fact that more than 50 percent of the underground coal produced in the United States is mined by continuous-mining machin-

ery. The coal industry was one of the first basic industries in the United States to be completely mechanized.

The union did not oppose mechanization . . . It was the consensus of those, both on management's and on labor's side, that if there were not mechanization, there would be total elimination of coal mining in America.

However, we were not fortunetellers, nor could we look through crystal balls to realize that the introduction of machinery would increase the innumerable dust diseases which now exist. Nor did our crystal ball or our fortunetellers inform us of the other problems which would come into play, particularly involving ventilation, roof control, and methane emissions. These are things which result from experience. . . .

\* \* \*

. . . The health bill which we offered set standards of 3 milligrams of respirable dust in a cubic meter of air. Under its provisions, the Bureau of Mines would be empowered to close a mine, or a section of a mine, where the 3 milligrams standard was being exceeded. The Bureau would inspect these mines at least once each 60 days and would take samples of the atmosphere at various locations with an instrument approved by the Bureau. The coal operators would be required to continuously monitor the coal dust levels in their mines. A permanent record of such tests would be kept for the information of the Bureau inspector.

Further, and of greatest importance, the bill sponsored by the United Mine Workers of America imposes penalties for operators who exceed the dust standards, or who fail to maintain accurate records.

\* \* \*

. . . To our delight, we have observed that the Federal mine safety law has been enforced more effectively in the past several months than ever before in this history of the act. The credit for such enforcement belongs to the Director of the Bureau of Mines, Mr. John O'Leary. It is our fervent prayer that President Nixon will continue his appointment as Director, so that his services which he has so capably demonstrated in a short time will be continued.

\* \* \*

Let me call to the attention of this subcommittee what happened to this coal industry of recent days. You confirmed the appointment of

a director by the name of Dr. Hibbard. He served for a short period of time with an expert on safety by the name of James Westfield—the only man in my judgment in the U.S. Bureau of Mines who was qualified on these explosions and these hazards that are found in these coal mines. To lead these rescue crews into these mines it required James Westfield.

What happened? The family became disturbed in the Department of the Interior for some reason or other. Dr. Hibbard could take it no longer, Jim Westfield could take it no longer, and they both resigned. And I was knocking on the Secretary's door repeatedly, by telephone and otherwise, asking him to appoint a new director of the Bureau of Mines. And let him tell me that I wasn't knocking on his door asking him to get a head of that department because of the condition in the coal mines of this country. And what did he do? Seven months elapsed before we had a head of that department down there. Seven months went by without a head.

\* \* \*

SENATOR PROUTY: How effective have the State mining agencies been?

MR. BOYLE: Here is a former director of a State bureau of mines. Maybe he can answer. I refer the question to him. He will tell you.

MR. EVANS: Senator, what I am going to say I don't mean to be an indictment of every state mining department in the United States, because there are some good state mining departments in the United States. But by and large they are under the complete domination of the coal industry.

SENATOR PRUTY: Thank you.

SENATOR BELLMON: Do you mean to imply that the mine operators do not want safety?

MR. EVANS: I did not say that, Senator. I haven't indicted every coal operator in the United States. There are coal operators in the United States who are vitally interested in safety. But I am afraid that there are far too many who are not interested to the extent that they should be interested. I think that many coal miners are injured and die in mine accidents and die in mine explosions through carelessness, through neglect of mine officials, through greed, through the operators paying too much attention to production and not enough to mine safety, the general welfare of the people who work for him. I don't mean that to be indictment of every coal company in this country.

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f.

**Statement of Senator Jennings Randolph,  
West Virginia—March 7, 1969**

\* \* \*

Occupational health in coal mining is not likely to be as readily improvable as is the safety of operations. But in this area of concern we must make an all-out effort to find ways to prevent diseases growing out of exposure to the pollution that is an inescapable part of the industry of coal mining, at least as of the present.

We can dictate standards by law, but achieving them in practice, Mr. Secretary, will not be easy. In fact, I am not sure that the difficulties of achievement of substantially better occupational health conditions in coal mines and the cost thereof will permit coal to stay competitive in some of its present markets.

I qualify that statement—I say I am not sure. This, however, is not a reason in itself for timid or time-consuming approaches to the search for effective disease-preventing practices and equipment in the business of coal mining and in the writing of laws to require their use.

But we must look at the broad picture of humanity, technological feasibility, and economics in all of these measures. Mr. Secretary, I say to you, as I have said to others on numerous occasions, the safest coal mine and the disease-free coal mine is the one that is closed. But it is not a producer for the economy or a source of payrolls to sustain miners and their families.

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g.

**Testimony of Walter J. Hickel, Secretary of  
the Interior—March 7, 1969**

\* \* \*

The President has asked that a bill be introduced in Congress, which I understand has been identified as S. 1300. This bill would help us meet the requirements not only of today, but would give us the flexibility to take advantage of the new technologies needed to solve the problems of tomorrow. The bill differs from the existing law in a number of ways.

Briefly, it would—

Apply to all underground and surface coal mines;

Require at least three times a year the inspection of every portion of every underground coal mine;

Extend the Secretary's authority over all types of accidents, not just the disaster type such as fire, explosions, floods, et cetera;

Authorize the Secretary to propose mandatory health and safety standards for all coal mines;

Provide for review of the standards by an expanded Coal Mine Health and Safety Board;

Provide civil penalties for the violation of a mandatory standard;

Require immediate evacuation of all persons in the case of an imminent danger;

Provide for the immediate withdrawal of persons, after notice, in cases of an unwarrantable failure to comply with a mandatory health or safety standard;

Provide that all withdrawal orders remain in effect until modified or terminated by the inspector; and

Expand our research activities and capabilities.

The need for this type of legislation is unmistakable. The present law is aimed exclusively at those types of accidents in which five or more persons might die as a result of fire, explosion, flood, or other such major disaster. Statistics show that, since this law was enacted, the fatality rate for major disasters has been cut by about 50 percent. However, the statistics indicate that there has been no change in the fatality or injury rates from the day-to-day type of accidents over the past 20 years.

In 1968, 221 of the 309 fatalities were recorded as being caused by accidents not covered by the provisions of existing law. The proposal that this administration is urging Congress to approve is designed to reduce all accidents that may cause injury or death in our coal mines.

If the day-to-day accidents that cause most of our coal mine injuries and deaths are to be reduced or eliminated, we need a law that gives broad enforcement powers to the inspectors and provides stronger incentives for management and labor to improve the working conditions of the mine. We must concern ourselves with the conditions that may cause an accident that injures or kills a single miner, as well as the accidents which are classified as "disastrous" because they kill five or more.

In our opinion, the single most important feature of this bill is the provision that would require the Secretary of the Interior to develop mandatory health and safety standards for all coal mines.

The precedents for such authority are many,



and I know of no instance in which the granting of it by Congress to a regulatory agency has proved anything but highly successful. In my view, the great speed with which technology advances makes it essential that in administering coal mine health and safety laws the Secretary of the Interior has a flexibility of response to rapidly changing conditions.

Many of the interim standards contained in the administration's proposal have long been recognized as needed and desirable by industry, labor, and the Department of the Interior. They have not, however, been implemented because the authority for setting standards rests with Congress and has not been delegated to the Secretary of the Interior.

It is not practicable to expect Congress to enact specific and detailed health and safety standards. The fact that Congress has changed the coal mine safety standards only three times in the last 30 years demonstrates the inadequacy of the legislative route for establishing mine health and safety standards. We must allow sufficient flexibility in the setting of standards so that new technology can be utilized for the benefit of the miners.

There is another important safety provision in the bill which we want to call to your attention. This bill, if enacted, would eliminate the "nongassy" classification for underground mines. Since the early 1920's, stricter safety precautions have been required for the so-called gassy mines. However, experience has shown on numerous occasions the so-called nongassy mines have had gas ignitions killing and injuring miners. We believe that the distinction should be eliminated.

The elimination of this classification will, however, have far-reaching economic implications. Approximately 85 percent of the Nation's underground mines are now classified "nongassy." These mines produce about 39 percent of underground production. Their reclassification would require that they obtain certain equipment and maintain it in a satisfactory manner. This will require large new investments and increased operating costs.

In addition to the safety features, S. 1300, in an effort to combat the "black lung" disease, would establish a dust standard for underground coal mines. The legislation would, within 6 months after enactment, require that each operator maintain the atmosphere in each active working place in the mine at or below 4.5 milligrams of dust per cubic meter of air.

In those individual mines where engineering

technology requires more time, the Secretary could grant, under our proposal an extension—not to exceed 6 months. We want to point out three things with regard to the short-term dust standard of 4.5 milligrams per cubic meter of air:

First, we are advised by the Bureau of Mines that the state of the art of dust control technology is not sufficiently advanced to permit an immediate industrywide imposition of a 3 standard.

Second, none of the other bills pending in the Congress establish a date certain for effectuating any dust standard.

Third, known technology can now be applied so as to achieve the recommended 4.5 standard in a relatively short period of time. The subcommittee should note, based on preliminary tests in about 20 mines, that moving to a 4.5 standard will require a reduction in dust counts in approximately 50 percent of the mines in the United States.

We have also directed the Bureau of Mines to start immediately to work with industry, equipment manufacturers, and labor to accelerate and expand research for the purpose of developing new technology which would permit a standard of 3 or lower. I am sure that the industry, the equipment manufacturers, and labor will cooperate fully with the Bureau in this endeavor.

\* \* \*

Let me at this point make this administration's position very clear on the subject of a single health and safety bill versus two bills—one on health and one on safety. We recommend and strongly urge one bill covering both subjects. The health and safety of the coal miner are so closely interwoven that it is inappropriate to even contemplate their consideration as separate issues.

SENATOR WILLIAMS: . . . I have one further observation. In your statement you state an objective in making the mining industry the safest, the most healthful, and the most productive in the world. I wonder if the Department has engaged in any comparative studies in terms of safety of the mining industry in our country and the coal mining industry, say, in Great Britain, Germany, Poland, and other countries that are known to be major coal producers. And, of course, when I say safety, I mean, of course, in terms of physical safety, both the structure and in the dust and health aspects.

MR. O'LEARY [Director, Bureau of Mines, accompanying Secretary Hickel]: Mr. Chairman,

we are aware of the data with regard to all activity and with regard to health and safety experience in foreign mines. I think we find two things that are significant. The productivity is perhaps a third to a fourth of that of the United States on the average in Europe and Great Britain, for example.

At the same time the safety records are significantly better than those in the United States.

\* \* \*

I think there are two factors there. They are not mechanized, and they do not have thereby the inherent hazards that highly mechanized, very rapidly moving mining equipment has.

SENATOR RANDOLPH: That is exactly right.

MR. O'LEARY: At the same time, I think it is also fair to say when we were at that stage in development, that is, when we were on conventional mining techniques, that our accident rates were significantly higher than the current European experience. I think it is undeniable that there has been more attention to health and to safety in Europe.

We are finding now, however, a trend which is just beginning to appear as Europe mechanizes; the safety records are beginning to deteriorate a bit and perhaps the health records as well. This is very, very preliminary.

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h.

**Statement of John Corcoran, President,  
Consolidation Coal Co. and Chairman of the  
Board of the National Coal Association—  
March 12, 1969**

. . . When I appeared before the Secretary of the Interior's Conference on Coal Mine Safety on December 12, 1968, I stated:

There can be no question that the health and safety of employees in the coal mining industry must be given first priority. On humanitarian grounds alone this should be self-evident.

If this is not enough, enlightened self-interest will lead us to the same inescapable conclusion.

Our trained and experienced employees constitute our most valuable resource and the protection of their health and safety is an absolute prerequisite to the successful and continued operation of our business. . . . In the regulatory area, we favor and will support any meaningful and constructive changes in laws and regulations that will improve coal mine safety.

\* \* \*

Perhaps, because the demands of our utility customers are greater today than ever before, we are faced, time and again, with the charge that the coal industry places production and profits ahead of safety.

Even if we are as calloused or as unconcerned about safety as this charge would imply, the plain fact is that production, profits, and safety are so closely interrelated that one is impossible without the other.

\* \* \*

While the coal industry's safety record has improved in recent years, there is no question that further improvement is needed.

In 1967, the accident frequency rate—that is, accidents per million man-hours of exposure—for the entire bituminous coal industry was 41.7 as contrasted with a frequency rate of 48.6 in 1950.

For those companies represented by the Bituminous Coal Operators Association, the accident frequency rate was 31.7 in 1967.

For comparative purposes I am advised that the preliminary 1967 frequency rate figure for all manufacturing is approximately 14.0.

\* \* \*

[I]n considering specific changes, we should be certain that they are meaningful and constructive and that they truly will improve safety for it is sometimes quite easy to allow the emotion of a Farmington disaster to cloud our judgment and cause us to accept proposals that might not achieve the results all of us are seeking.

It is with this thought in mind that I would like to direct my remarks to certain general principles that are common to most of the proposed legislation, although variously expressed in the different bills.

Most of the bills before this committee provide for interim safety standards, with the right in the Secretary of the Interior to promulgate additional standards and to revise them from time to time.

I believe it would be preferable that the standards set by Congress should be mandatory, not interim, standards, and that they should continue in effect until amended by the Congress. This would insure a separation of the legislative and enforcement functions and, in the area of safety regulation, I believe this separation is important.

However, if the Congress should choose to delegate its legislative functions, I would cer-

tainly question the desirability of delegating such sweeping powers to any one individual without adequate review provisions.

In some of the proposed legislation there is not even any appeal from a standard set by the Secretary, however unreasonable or unworkable it may appear to be.

It is true that a coal operator could refuse to comply with a standard set by the Secretary under this provision, thus subjecting himself to an appealable order and having the standard reviewed in such a proceeding.

But if our objective is to see that reasonable and workable standards are set, it seems highly inappropriate that a coal operator must deliberately violate the standard in order to have its reasonableness and lawfulness properly tested.

There is now in existence a Coal Mine Safety Board of Review, a group composed of knowledgeable coal mining experts, representing the public, the men working in the mines, and the operators. This Board is already performing valuable functions under the existing law.

I would strongly recommend that it be given authority to review any proposed standards set by the Secretary before they become effective. In this way the desirability for quick action can be maintained and at the same time the propriety of proposed standards could be subjected to the scrutiny of an expert tribunal.

In addition any standards, however promulgated, should be subject to judicial review in the event their propriety is questioned.

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i.  
**Statement of James R. Garvey, Vice President  
of National Coal Association and President  
of Bituminous Coal Research, Inc.—  
March 12, 1970**

\* \* \*

[T]he coal industry is not opposed to dust standards for the protection of the health of coal miners; we agree they are needed. But we do believe that a constructive and planned program should be the basis for development of such standards rather than a hasty, arbitrary selection of numbers. A planned approach can lead to the development of effective dust standards needed to protect the health of miners, while at the same time enabling industry and government to cooperate in the work necessary, not only in the resolution of such standards but also in the development of methods for control and measurement of airborne dust.

On the other hand, the arbitrary selection of standards, unsupported by substantive evidence and unaccompanied by a definitive description of sampling and measurement techniques, can only lead to confusion in the attainment of the desired health objectives.

\* \* \*

High concentrations of respirable dust resulting from coal mining may very well be a health hazard, as the available evidence apparently indicates. But the level, above which adverse health effects occur, has not been determined.

It is urged that a dust standard be established in accord with the latest medical or other evidence related to dust exposure and the availability of reliable instrumentation for that dust measurement. Further, it is recommended that current technology for control of the dust generation during mining and for minimizing airborne dust distribution in possibly health-hazardous concentrations to other working places in the coal mine, plus the availability of personal protective devices for prevention of inhalation of potentially hazardous concentrations by the miner, be considered. In other words, we request that any law written take into account all the factors involved in achieving the objective, namely, health protection.

\* \* \*

In most health literature relating to dust and health effects, the medical term "pneumoconiosis" appears. The word is generic, meaning dust in the lungs. Medical evidence indicates that accumulation of excessive amounts of some dusts in the lungs over a period of years may result in development of a disease which is related to the type of dust inhaled.

\* \* \*

Until recently, medical authorities in this country did not recognize that the inhalation of respirable coal dust could result in a pneumoconiosis disease. It was believed that those miners who did develop disease symptoms acquired them as a result of inhalation of silica particles sometimes associated with coal dust. Since, in general, the silica content of coal dust in U.S. bituminous mines is 2 percent or less, considered to be below the value for the development of silicosis, the health problem of most bituminous coal dusts was not considered significant.

However, in the late 1930's, British medi-

cal researchers noted the prevalence of pulmonary disabilities in coal miners which was not related to the silica content of coal dust. This resulted in the instigation of a comprehensive study of the subject beginning in 1953 and still continuing.

Although the U.S. Public Health Service suspected health effects from coal dust inhalation as early as 1952, it was not until a prevalence study of coal workers' pneumoconiosis in the miners of the Appalachian region, started in 1963 and completed 2½ years later, that substantive evidence of possible health effects became available. The results of that study covering 4,000 coal miners indicated that for all working miners, 9.5 percent had X-ray evidence of pneumoconiosis, of which 6.5 percent were classified as the simple variety and 3 percent the complicated variety.

\* \* \*

Further, the evidence of coal workers' pneumoconiosis revealed by the Public Health survey related the prevalence to years of employment in the mine, but it could not be related to coal dust concentration, because data on dust were not available. In other words, we do not know what amount of coal dust exposure causes disease. We do know that if you are exposed by working a number of years in the mine, you do have an effect.

\* \* \*

The conclusion that the transition to continuous mining machines has caused an increase in exposure to respirable dust in coal mining operations has not been substantiated by the limited data which are available and, quite the contrary, appears to be true.

In the early 1950's the continuous mining machine was introduced in coal mining, and its use has increased ever since. There are several reasons for our belief and the belief by many other experts that such machines actually reduced the respirable dust exposure.

First of all, as has already been mentioned here this morning, high-pressure water sprays are standard equipment on almost all continuous miners and are used to allay the dust as the coal is broken from the face.

In addition, with the continuous mining machine it is usually necessary to have greater quantities of air at the face than with past mining practices. This is to dilute and carry away the methane which is liberated in increasing

quantities by the more rapid rate of penetration of these machines.

Increasing air flow to control methane also reduces dust concentration by carrying it away from the face and where the men are working. We certainly do not believe that return to former mine practices is justified.

A suggestion made in one of the bills before this Congress "that all coal to be mined should be first under cut, center cut, top cut or sheared" is contrary to what evidence we have on respirable dust exposure.

Gentlemen, I believe it is of interest to note that since it apparently requires, again referring back to my medical reading, 15 to 20 years for medical evidence of pneumoconiosis to appear, at least X-ray evidence, the health data obtained by the Public Health survey in 1963 probably relate to dust concentrations associated with methods of mining prior to the introduction of the continuous miner.

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#### NOTE

BITUMINOUS COAL OPERATORS' ASSOCIATION  
POSITION AS TO SUFFICIENCY OF THE FEDERAL  
COAL MINE SAFETY ACT—JULY 31, 1967

The considered view of the coal mine industry, as represented by B.C.O.A. and confidently that of large numbers of operators not represented by B.C.O.A., is that the 1966 Federal Coal Safety Act does not materially lack sufficiency and that present considerations for further amendments should be few, if any.

This view is in no sense negative, but rather one concluded from objective analysis of the total problem and the 20 proposals made by the Bureau.

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j.

**Testimony of Dr. I. E. Buff, Chairman,  
Committee of Physicians for Miners' Health  
and Safety, Charleston, W.Va.—  
March 13, 1969**

DR. BUFF: . . . Now I want the lights out, please, and I want to show you what we are talking about.

SENATOR WILLIAMS: Is this film or slides?

DR. BUFF: Slides . . . [T]his is what you see in a coal miner, black lung. It is dead, half dead. The carbon dioxide cannot come out. There it comes in eddies. You do not get a transmission

of oxygen to the carbon dioxide. So what happens. You get an individual whose brain is damaged, whose lungs are damaged, whose kidneys are damaged, whose heart is damaged, whose bones are damaged.

There is not an organ in the body that is not affected by high CO<sub>2</sub>. What it means basically is this: You are not getting the feed line, the oxygen, to the muscle, to the tissue, and the tissue works halfway.

Well, a halfway-working liver makes you look half dead. This is the reason why people with black lung look like they have got cancer.

If I had my choice of cancer of the lung or black lung I would take cancer of the lung because you live 4 or 5 years and you die.

With this you live 15 and 20 years and you choke to death as if one takes a string and ties it around your neck, day by day, tighter and tighter, begging for a little air, just a little air.

This is a disgrace; this is terrible. It should never have existed and it should be eliminated from our people in this country.

\* \* \*

[The coal miner] has nowhere to go. He has no paved streets. They are made of dirt and coal dust. His hair is terrible. His water has iron and sulfates in it and acids. And the sewerage system is something of the 17th century.

You just dump it in a creek. But there is no sewage system in very many coal mining areas in this country. They throw it out of the back yard or in the creek.

They can't get life insurance of over \$1,000 or \$2,000. Who would want to give it to them? Because they live 10 years less than the older population. It is risky.

Then in West Virginia last year, 1 in 300 miners was killed. This is not a good risk for an insurance company. Who would give a coal miner accident insurance?

SENATOR SAXBE: You mean on the average, 1 in 300 miners was killed?

DR. BUFF: Last year. As far as accident insurance, as of January 1969, in 1 month the West Virginia State Compensation Department reported 1,140 accidents in 1 month. Multiply this by 12 and divided into 43,000 miners and you come up with a 30 percent accident rate.

What industry in this country can even touch it? Who would give him accident insurance? That means if a man works 3 years he is bound to get hurt. And the worse thing, and I think this is what has always bothered me so

much and I know it is going to bother you, the suppression of brains in the coal mining community.

The schools are bad, the teachers are the worst that you can get, and they really do not want these people to get a high school education and go to college and become doctors, lawyers, scientists, and other things—the professions.

They want to add fodder to that machine and the women are kept ignorant. The men won't stay there without women.

At West Virginia University, two professors enlisted 25 graduate students to go into Monongalia County to help with preventing dropouts.

Do you know what happened? They asked for \$3,000. The service club refused to give it to them. They went to Charleston, to a very benevolent individual; he refused.

The basic reason was that this is a bad thing to start—education at this level among the coal miners' children. But they put the money out of their own pockets for transportation and I can report to you now that students at West Virginia University are doing a great job in trying to keep these children from becoming dropouts and giving them incentives to learn.

Let us talk about the economic pressure of the coal company. If the man does not behave he gets blacklisted. If blacklisted, he can't get any job in another coal company. These are not idle remarks. They are true, it happens every day.

In some coal communities they have police departments; they remind you of the Gestapo of Germany. Let us take the medical care in a coal mining community. The medical care is second or third rate.

I want to report to you that we have had 50 meetings of miners in the state of West Virginia to educate them in what dust disease was, what it does, and how it will affect them and what they should do about it.

In the first place, we have the best educated coal miners in the United States on dust disease. They really know their stuff.

In the last fight with the legislature in order to get a compensation law, and I would like to hesitate here a minute and show you what happens in Charleston [putting on white hat].

The legislators wore the white hats when they talked to the miners and they said, "We are for you."

They went into committee and [witness puts on black hat] they put the black hat on of the coal operator. That is exactly how they voted. They tore this law to pieces.

When they came out [witness puts on white hat again] they said, "I tried but I couldn't do anything."

At these 50 meetings that I have had I would shake hands with all the coal miners. Some would have one finger missing, some two fingers, some three fingers, some could not raise their arm, some limped, some, you look on their face and you see the scars of coal mining accidents.

I would say to a man, "How old are you?" He would say, "30." I would say, "You look like 50." Another one would say he is 40. He looked like 60. They actually deteriorate earlier than other people.

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[G]entlemen, I am disturbed by the fact that the Federal Government gave \$63 million for coal research for new markets in 1969 and not even a million dollars for coal dust control.

Why do you give so much for the production and nothing for the man? They have also an education program because the coal miners are getting disabled at a fast rate. So they go to the high schools and they enlist these students with Federal funds for training.

I asked many high schools to give the other side of the story as far as the health of the coal miner, the fact that if he does not die in an accident he may die of black lung and that he will be sick the last 10 or 15 years of his life.

But only until recently did anyone allow me to speak and that was in Wyoming County, with three high schools.

Yet the Federal Government gave \$5 million for this program.

Mr. Chairman, the Department of Education of this government wrote me and said they are interested in education, they are not interested in health. The Department of Mines wrote me and said these people will be strip miners who are trained so they do not get dust disease. This is an exaggeration.

Strip miners get dust disease the same as anyone else. . . .

Now let us get to the crux of the problem, the black lung business where there is so much argument as to how many cases are there.

I know because in previous testimony there have been some who say it is a rare disease, some say it is like phlegm in the nose and you flop it out.

But you can't do that. Because once it gets in the lungs it is progressive; 1,000 autopsies were done in the Beckley hospital. Eighty-five percent showed coal dust disease. We figure that

about 50 percent of the miners eventually become disabled from this disease. It is a progressive disease.

If a man leaves a mine, 10 years later it affects him. He gets short of breath, he develops the emphysema, he develops the right heart failure and out he goes.

Of course, it is easy enough to say that mining did not have anything to do with it. But it does.

Those who do not have coal dust disease do not get right side of the heart failure. What are the symptoms? What does a coal miner look like? What does he say? He says, "I am so short of breath, I can't work like I used to. When I work real hard, why, I get dizzy."

He says, "I start coughing, I am spitting up black stuff."

He uses about 30 to 40 percent of the energy of his body in breathing while in a normal person it is 10 percent. . . .

\* \* \*

And, there, dust. You talk about dust standards. You know it makes me smile sometimes the way you talk about 3 milligrams and about 4½ milligrams. My lord, in some of those mines it is 300 milligrams. You can't see a foot ahead of you.

So, the dust on the ground these men have told me is such they have worked in 18 inches and 20 inches of dust which should have been removed because it causes explosions and it makes people sick, humans sick, not animals, but humans.

Now the most dastardly practice in this book is working "ahead of air." That means ahead of ventilation. I should say about 90 percent of the miners have done this in time.

The reason it is bad is because they have bad lungs anyhow, they can't help it, and if you cut the oxygen down from 19½ to 18 percent the man becomes unconscious. So they are hauling him out of the mines, "heart attack, heart attack," to the hospital.

He doesn't have a heart attack, he has respiratory failure. Heart attack is not compensable, respiratory failure is. You have to be a pretty strong doctor to make a diagnosis of respiratory failure if you work for a coal company. I do not think you would be around very long.

\* \* \*

Now I think a lot has been said about masks in the prevention of dust. I saw, Mr. Chairman, a picture of you with a mask on you yesterday.

SENATOR WILLIAMS: It was upside down.

DR. BUFF: I know that. Basically, I want to tell you that mask wouldn't help you. It is a waste of time. The particles of dust that cause pneumoconiosis, the filter in that mask will not take out because if it took that particle of dust out you could not breathe. They are the same size as air particles.

\* \* \*

... If you cut the dust out you cut the air out. That is why these fellows start working in it and they throw it off and they say, "I can't work in this thing." It suffocates you.

They are not sick in the head. They know what they are talking about.

And then there is the other problem. Most of them have lung disease. If you cut down the oxygen supply through that mask he is going to get silly, he is going to see double. So he throws it off.

\* \* \*

The people in England and the people in Czechoslovakia have a good system. We say they don't produce coal in the proportion that we produce it and that is why it wouldn't work here. No one knows. No one knows.

SENATOR EAGLETON: Are there statistics available on the incidence of black lung in Wales and in Czechoslovakia?

DR. BUFF: Yes, sir, in Wales it has been reduced from 60 to 20 percent.

SENATOR EAGLETON: What about Czechoslovakia?

DR. BUFF: It is supposed to be somewhere around 25 percent now. I do not know what it was recently but it is going down. They are very strict on this dust control.

SENATOR EAGLETON: In your opinion, what are the key things they are doing that we are not doing?

DR. BUFF: They have a system where they suck the air, when they cut the coal they cut large pieces instead of small pieces.

They have like a cyclone over the machine. It sucks back the air. It goes through a wool-cotton filter and then an electrostatic precipitator and it comes out clean.

It is a little expensive but, my lord, we are talking about human lives. We are not talking about other things.

Now if the industry itself says that they cannot reduce dust, it is not economically feasible, it will put men out of work, it will put their wives and children on the want list, I give

you one answer: What is the price of a husband that is killed in an accident?

What is the price of a coal miner who chokes to death the last 10 years of his life and leaves his family and his boys to support him?

Is this fair? Let us consider another thing about economic feasibility.

The Czechoslovakian Government and the British Government compete with us a little bit in coal. We want private industry to do this but if they cannot, if they say they will not, then the government of these United States will have to do it for them.

As far as West Virginia is concerned, I am worried about the fact that when a coal miner goes into the mines you know he has no toilet. He does his business on site or up the hollow.

He breathes this stuff after it dries and he gets fungus infection in his lungs from this. I asked a couple of coal operators would they put toilets in the mines, chemical toilets?

The answer I got was "Do you know I have 300 miners? They put out 2 pounds of waste a day. That would cost me 25 cents a man or \$75 a day for toilets. Do you think I'm nuts?"

\* \* \*

The chief of mines of Kentucky is a coal operator. The chief of mines of West Virginia used to work for Consolidation Coal. No vested interest, just a tinge.

What we need is a rigid health and safety act with 100 percent enforcement.

We need this enforced like we do with the other agencies of the government.

We do not want any hanky-panky. We do not want an inspector coming around and letting them know 3 weeks in advance they are coming.

We do not want an inspector coming around and the superintendent meets him and says, "Don't go to mine No. 1, go to No. 9. No. 9 is cleaned up. The other is filthy."

This happened to Armco. The inspector went to one and closed the mine. I think he might have regretted that because that is an awful lot of pressure on one company. They did clean it up and, thank God, they might have saved some lives.

But we feel that the coal industry—the coal owners, because I do not think we should talk about operators or producers—I think we ought to get to the gist of it, who owns this coal.

I think the Federal Government should find out who owns the coal mines. You are jumping on the wrong guy when you jump on the operator. He leases; he takes orders. He is just a boy.



The coal owner is the boss. You know as well as I do that most big corporations do not have too much respect for state law but they do respect Federal laws.

Gentlemen, one corporation tried to play a little game with the Federal Government and the officers ended up in jail. You do not play with the Federal Government.

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k. **Testimony of Donald L. Rasmussen, Chief, Pulmonary Section, Appalachian Regional Hospital, Beckley, W.Va.—March 13, 1969**

During the past 6 years, I have been engaged in the evaluation of pulmonary function and work capacity in coal miners from southern West Virginia, southwestern Virginia and eastern Kentucky. I have become acquainted with the lung diseases afflicting miners and have demonstrated physical impairment in many. I have also become acquainted with some of the socio-economic consequences of the lung diseases of coal miners. The latter are responsible for suffering not only of the miner but also his family.

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Among the 3,000 miners evaluated in our laboratory, approximately 50 percent have been found to have significant impairment of pulmonary capacity. An additional 40 percent show varying degrees of impairment, while 10 percent show little or no abnormality. Of interest is the fact that the x-ray findings bear little relationship to functional capacity. Thus, men whose x-ray reveals only a widespread increase in linear markings may have impairment as great as one with advanced pneumoconiosis (complicated pneumoconiosis or so-called "progressive massive fibrosis"). We are aware of men in the 70 and 80 year group with advanced pneumoconiosis, who are apparently as healthy as age permits.

Our observations in Beckley have been challenged by the coal corporations and are not recognized by industry-oriented physicians nor even a number of experts in various medical schools as well as within the U.S. Public Health Service. These critics base their opinions on published medical evidence, largely that from Britain. None of the criticism is based on actual test results.

Much emphasis has recently been placed on cigarette smoking and this has been labeled as the cause of disabling lung disease in coal miners.

The effects of cigarette smoking are obvious in our miners and cigarette smoking can produce measurable abnormalities, even in our subjects who have no overt bronchitis. On the other hand, the effect of working at the face in mechanized operations shows a more marked effect than any other known factor, including cigarette smoking. Workers at the face in mechanized operations appear to develop pulmonary impairment on the average 5 years earlier than all other workers within the mine. This is even more startling since certain miners in other areas are also exposed to relatively high dust concentrations.

Suggestions by the coal industry that "additional research" is warranted before dust suppression is imposed should be regarded as evidence of a total lack of regard for human life and health. We are disturbed by the increasing frequency with which we encounter men less than 40 years of age who have been employed for under 20 years who are significantly impaired or exhibit clear-cut abnormalities of pulmonary function. Almost always, these men have been employed at the working face. The majority of such men have worked on or near continuous mining machines for 3 to 10 years only, having spent the earlier years operating more conventional machines. It is our opinion, that unless immediate and rigid dust control measures are instigated, we will see a shocking increase in pulmonary disability within 5 to 10 years.

Much more research is indeed necessary in order to more clearly define the dust diseases of coal workers, and to find additional methods of safeguarding the health of miners. Methods of early detection of impaired health are required to identify those men who should be moved to non-dusty areas. Uniform workman's compensation based on objective physiologic methods should be sought, and much more effort is required to provide employment for the presently affected miners who, under present practices, are doomed to an early disability and impoverishment.

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**Testimony of Dr. Harvey A. Wells, Coordinator, Conemangh Valley Memorial Hospital, Johnstown, Pa.—March 13, 1969**

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My experience in West Virginia and Pennsylvania coalfields leads me to the inescapable conclusion that the main factor that has led to the



deplorable conditions in and around the mines is the fact that employers are effectively immune from suit under most state compensation acts and they are not liable for even the most callous disregard for the health and safety practices.

For example, in West Virginia, the injury from death and disease caused by gross negligence of an employer cannot be compensated by a jury unless the essential elements of first-degree murder can be demonstrated.

Relatively minor additions to the proposed Federal legislation could result in major benefit in the workplace and make it much cheaper to run a safe shop than a dangerous and unhealthy one.

What I am saying, gentlemen, is that we must give these men back the right to a jury trial. This denial has led to the deplorable conditions and if we can give them the right to go to a jury in cases where there is negligence and bad health and safety practices, we will go a long way toward the elimination of these bad practices because the man who does the wrong will have to pay the bill.

This is the American way, the way it should be done. Because of the inequities in the various states' compensation acts, the employers do not have to answer for their wrongdoings.

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SENATOR EAGLETON: If a 3-milligram limit were adopted and rigorously enforced, would the incidence of black lung be reduced or would the disabling effects of black lung simply be prolonged?

What I am getting at is this, would a 3-milligram limit merely prolong the work and life expectancy of the worker but sooner or later black lung will appear in many coal miners even with the 3-milligram limit for coal miners?

DR. RASMUSSEN: I do not think anyone can answer your question specifically related to a 3-milligram level, whether continued exposure at this level would be, let us say, completely well tolerated and handled by every miner in the mine for any length of time.

We do not know the answer to this question. I think that the British experience would certainly indicate that a lower dust level than we now have would result in a much lower incidence of the disease.

I suspect that were men to remain in the mines for, say, a greater length of time that some of them would again begin to show change.

I think it is a time and dose related matter.

DR. BUFF: You asked how much money was spent by Britain. In 1967, \$10 million. We spent \$4½ million in 5 years. Great Britain in 1 year, \$10 million. A smaller country.

SENATOR WILLIAMS: Thank you, gentlemen. We have a vote. I would like to say while we have had this remarkable testimony, Apollo 9 reentered the atmosphere and is down safe.

SENATOR RANDOLPH: I want the record to show that sometimes we do better above earth and on earth than we do under the earth.

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SENATOR BELLMON: . . . As I listened to . . . [your] statement, I was impressed by the fact that Utah and Oklahoma have a great deal in common in that both of us have immense reserves of coal which are not presently being mined as much as we hope they will be in the future, and the question that comes to my mind now is the one that has crossed my mind when other witnesses have appeared, and that is this: Whether or not there is danger that in drafting safety legislation we will be so restrictive that we may limit the future development of our coal reserves and cause the cost of coal mining to go up to the point where our industries and utilities will turn increasingly to petroleum and perhaps nuclear power as a sort of energy.

As a Senator from Utah, are you concerned we need to be careful we don't go too far in this field, or do you feel there is a danger here?

SENATOR MOSS: Well, of course, it is to be hoped that the mining of coal will be economic and profitable and, therefore, this resource will be used. But I don't think that we can sacrifice any safety measure for the men who must mine the coal, in order to keep the cost down.

I think even if the resource has to stand unused, that we must not bring it out of the mines by endangering their health or periling their lives, by proceeding with less than the maximum amount of safety that we can possibly devise for the mines.

I really don't worry too much. I think the use of coal is on the increase now, and I think our energy demands are going up at such an astronomical rate that surely the coal fields will be called upon to furnish a good part of that energy. As we go forward in developing coal, I want the safety factor kept paramount. I wouldn't restrain any effort in the safety field just to keep the cost down.

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**Testimony of Dr. William H. Stewart,  
Surgeon General, Public Health Service—  
March 18, 1969**

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For over 30 years, the Public Health Service has undertaken cooperative studies with the Bureau of Mines on coal miners' health problems. Not until 1963, however, did the Department first receive funds for the specific support of operations in this area. Our first major project was a prevalence study of pneumoconiosis in soft coal miners in Appalachia and other coal mining areas.

This study established pneumoconiosis among soft coal workers in the United States as an occupational respiratory disease of serious and previously unrecognized magnitude. Our research showed that one in 10 men in the mines and one in five of the former miners in Appalachia showed X-ray evidence of this chronic respiratory disease. Data from postmortem examinations would indicate an even higher prevalence of this disease.

For work periods less than 15 years underground, the occurrence of pneumoconiosis among miners appeared to be spotty and showed no particular trend. For work periods greater than 15 years underground, there was a linear increase in the prevalence of the disease with years spent underground.

The major shortcoming of this study was the lack of environmental data, especially on dust concentrations, particle size and chemical composition. For information on the relationship between environmental factors and the disease, we consulted with the British National Coal Board, which provided us with technical information indicating a straight-line relationship between the amount of dust breathed and the progression of pneumoconiosis in miners.

\* \* \*

The United States is the only major coal-producing nation in the world which does not have an official Government standard for coal mine dust. Since Great Britain began requiring dust control efforts in the coal mines—which resulted in reduced concentration levels—there has been a substantial reduction there in the prevalence of coal workers' pneumoconiosis.

Thus, the incidence of new cases in miners has decreased from 8.1 new cases per 1,000 miners in 1955 to 1.9 new cases per 1,000 miners in 1967; the age specific prevalence of simple coal

miners pneumoconiosis has also decreased as has the overall prevalence (12.5 percent in 1959–62 as compared to 10.9 in 1964–67).

An official respirable dust standard for coal mines could, in our opinion, if properly enforced, make a significant reduction in new cases of pneumoconiosis and decrease the rate of progression of old cases. Last year we concluded that sufficient data were available to recommend the adoption of an interim coal dust exposure standard for miners, pending further refinement of technical knowledge. After careful analysis of the British and Pennsylvania experiences, and after consultation with many authorities, we concluded that:

An interim standard should represent no more than a reasonable degree of risk to our miners, given our present technology, and be one that would significantly reduce the rate at which new cases of pneumoconiosis would develop in the future and old cases would progress.

On the basis of those conclusions, last December, the Secretary of Health, Education, and Welfare recommended to the Department of the Interior a Federal standard which could be used to lower respirable dust levels in coal mines. This standard called for a respirable dust level not to exceed 3.0 milligrams per cubic meter as measured by the Mining Research Establishment (MRE) horizontal elutriator instrument.

We recommended this standard in the conviction that it could, if adopted and properly enforced throughout the coal mining industry, make a significant reduction in coal miners' pneumoconiosis. This standard, if adopted and enforced, would place the United States along with other major coal-producing nations which have set health standards for dust exposures in the coal mining industry.

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SENATOR RANDOLPH: Dr. Stewart, we have heard conflicting testimony as to the number of cases of coal workers with pneumoconiosis—that is a word I am having trouble with this morning. How much black lung disease is prevalent in the United States?

I recall one source made an estimation of 100,000, of 135,000 workers.

Now, does the U.S. Public Health Service have reliable statistics—I shouldn't perhaps use the word reliable—but statistics that can be presented to the subcommittee?

DR. STEWART: Yes; I have used the figure 100,000 as an estimate of pneumoconiosis that was based on the prevalence rates we found in

our study of both active miners and of inactive miners. I think the difficulty arises, Senator Randolph, when people try to apply that to 135,000 active miners. We included also the inactive miners in the estimate. One out of 10 active miners, one out of five inactive miners.

The difficulty we have had is in determining how many inactive miners there are. We used a figure of 400,000 inactive miners, derived from a variety of data, in making our estimate of the cases of pneumoconiosis. In addition we know from other data, for example—the known number of disabled miners in Pennsylvania and the total man-years of mining experience between 1940 and 1955, that you can come up with a figure close to 100,000 for the total of coal miners' pneumoconiosis cases.

We also know from the disability insurance statistics of the Social Security Administration that the disability among miners from respiratory diseases is much greater than the average that you would expect.

As far as what is needed for making a decision that you have a health problem, that we need to take some preventive measures, that estimate of 100,000 cases is a perfectly good statement.

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SENATOR EAGLETON: Doctor, do I take your testimony to mean that clearly from a medical point of view, due to research that has been made available to you, part of which the Public Health Service has participated in, that 3.0 is clearly a preferable permissible level than anything higher thereto, 4.5, or anything else you might pick out of the air?

DR. STEWART: From a medical standpoint, that is correct.

SENATOR EAGLETON: So, in terms of you as a professional physician, you would recommend, would you not, 3.0 from a medical point of view, not being a construction expert or design engineer or coal mining?

DR. STEWART: Ideally, I recommend no dust. But knowing the practicalities of the situation, you can never reach that. And this is so true of many things we do. We are always measuring the benefit and risk on so many things. The determination of that level depends on the technology of mining and the determination to reach some level.

SENATOR EAGLETON: But the health of the coal miner is going to be better protected by a 3.0 level than by a 4.5 level?

DR. STEWART: This is correct.

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# **Statement of Representative Ken Hechler, West Virginia—March 20, 1969**

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Whenever legislation of this nature is discussed, a lot of attention is put on the economics of the industry and what it will cost the industry to clean up the mines and take strict safety precautions. The phrase "technologically feasible" is used to describe limits beyond which some people claim it is impossible to go. There is a tendency to look at this problem in terms of certain entrenched forces and practices which some people claim are immutable. Therefore, some people claim we had better not do anything unless there is full agreement throughout the industry.

I submit that we ought to start with an entirely different premise—the individual human being. We ought to find out what is necessary to protect the life, the health, and the safety of these 144,000 human beings who work in the coal mines. And *then* we ought to go ahead boldly and take the measures necessary to protect these human beings.

I believe we can not only achieve a high degree of health and safety for the individual coal miner but also keep the industry prosperous. I am encouraged in this belief by the progressive coal operators, the new breed of young and imaginative coal operators who are convinced that this approach is not only possible but essential for the survival of the industry.

Today, more than ever before, the people of this nation are concerned with their environment—how they live, the air they breathe, and the water they drink. That is why in recent weeks over 40,000 coal miners in West Virginia, against the opposition of their own union, rose up and said: "We want something done about these conditions, and the disease of black lung from which coal miners suffer." Some observers marvelled that the miners weren't satisfied just with high wages—they wanted freedom to breathe fresh air, and they were willing to give up their pay to fight for fresh air. The old rules of economics and politics are being swept away in the surging drive for a better life, for the dignity of the individual, for the quality of life which a human being lives here on earth. The coal miner has been watching the grandeur of science and technology bring a new life to millions of Americans, and gouge out more coal per minute, while doing nothing to improve his health and safety.

In fact, science and technology has brought greater threats to the health and safety of the coal miner.

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The main point I want to make, Mr. Chairman, is this: year after year after year, we have compromised this issue at the expense of the human beings who suffer the consequences of injury and death in the coal mines. We have allowed loopholes to be driven into the legislation which have made a mockery of effective mine safety regulations. Down through the years, we have tolerated a Bureau of Mines which has been subservient to every special interest except the man who mines the coal. The Bureau of Mines has been moribund, a colossal failure when it came to protecting the men in the mines, production-oriented, filled with dead wood, backward and bureaucratic, stifling the initiative of any imaginative official, leaderless, having the backbone of a chocolate éclair, and rarely challenged either to do its job or seek more effective tools to do a better job.

Each of us remembers what we were doing on a red-letter day. I recall talking with two or three officials on the day of the Farmington disaster. Director O'Leary was easy to reach, and he ticked off clearly and crisply what had to be done to strengthen the enforcement of existing regulations, and what needed to be done in the future. To a friend of mine, all one official could say was: "This was a great mine, it produced 9,600 tons a day." High up in the Department of the Interior, another official warned me on November 20: "Don't let an accident like this excite you. After all, nobody did this on purpose. And please, Mr. Congressman, don't indulge in any recriminations against anybody on account of this."

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O. **Testimony of Dr. Ian Higgins, School of Public Health, University of Michigan—March 26, 1969**

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SENATOR WILLIAMS: I wonder if our feeling, that we should make mandatory an annual physical examination, fits in here at all?

DR. HIGGINS: This is more frequent than I think is necessary. In fact, in the British mines I think the general view is about once every 5 years is probably sufficient.

The development of pneumoconiosis is so slow, given reasonably safe dust levels, that annual X-rays are unnecessary and probably not worth the cost. On the other hand, I think if one is dealing with an area where the dust levels may

be less certain, which may be the case in this country, then something more frequent than 5 years might be desirable and possibly 3 years would be a safer time.

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SENATOR WILLIAMS: Is it possible, do you think, to get what I call an early warning, to catch a man in time so that he could be withdrawn from employment at the face or maybe out of the mine altogether?

DR. HIGGINS: I think there is no doubt detecting the early X-ray changes; the early warning as you call it, is sound preventive medicine and effective.

I strongly believe, however, that compulsion should not be instituted. It may be in the man's best interests to leave the mines. If he does not want to do so, he should not be compelled to do so. I think Britain is unique in Europe in that no man is made to follow any advice he is given. The only justification for suspending him is active tuberculosis which would make him a risk to his fellows. I believe that only by stressing advice and avoiding compulsion will one get full cooperation from the miners for periodic X-rays.

SENATOR WILLIAMS: He makes the judgment, but he makes it based on information.

DR. HIGGINS: He makes it on the best evidence that can be given to him.

SENATOR WILLIAMS: We are doing this all the time, those of us who smoke. We are making a judgment.

DR. HIGGINS: That is right.

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P. **Testimony of Cloyd D. McDowell, President, Harlan County Coal Operators Association, Harlan, Ky.—March 26, 1969**

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. . . Regarding mandatory health standards for controlling dust in underground mines, we agree that some control is needed. However, there is a greater need for research and study of this problem in order to establish all the facts pertaining to it rather than setting an arbitrary limit of 4.5 milligrams of dust per cubic meter of air, which may or may not be the proper limit.

We do know that one cigarette contains over 35 milligrams of tar and nicotine which we presume would be in the form of respirable dust if it is inhaled as smoke. Are we then expected to keep mine air purer than a room in which a cigarette

has been smoked? Medical witnesses have testified that they are not sure that cigarette smoking is harmful to health. Others say it is and yet not a law has been passed to prevent the sale or use of cigarettes.

A lung specialist from Great Britain visited all of the United Mine Workers' hospitals in 1958 to instruct these doctors in the diagnosis of lung diseases, including pneumoconiosis. I attended the meeting held in Harlan and heard him say that breathing coal dust would not cause coal miners' pneumoconiosis unless there was another substance present.

Dr. William H. Anderson, of Louisville, was present at this meeting and since has made a detailed study of coal miner's pneumoconiosis. He has testified in case after case that coal dust by itself would not cause miners' pneumoconiosis.

Coal dust per se may or may not be harmful to the health of miners, but it could be other factors such as the amount of sulphur in the dust, or the silica content or some other substance. We are told that anthracite coal dust is more abrasive than bituminous and it may be for this reason that a larger number of miners are affected by dust in Pennsylvania than in other states.

We believe that the dust problem should be attacked from the standpoint of preventing dust by requiring the manufacturers of mine machinery to build dust-suppressing attachments on all mine equipment rather than establishing a level of dust concentration. We have had a great deal of success in preventing dust by using water sprays on coal-cutting equipment. The use of dust collectors and respirators should be required where necessary and many other means can be developed through research if funds were available.

We realize that safety problems multiply in geometric ratio to the amount of coal produced in a section of a mine. The faster the coal is removed from the face, the greater will be the amount of dust produced. Also, the amount of methane liberated will be increased proportionately.

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A mine is as safe as the people that work in the mine. No law should be passed that sets a penalty for one group of people while other people are exempted from the penalty even though they may violate the law endangering themselves and others. From my own experience I can testify that over 95 percent of the safety violations are committed by workmen and not by the operator

in our mines. However, should an official or an employee knowingly violate any law that will endanger the health and safety of the people in the mine, he should be penalized.

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As far as we can determine, there is no basis for setting a limit of 4.5 milligrams of allowable float dust in the mine atmosphere. We are aware of the clamor of many people who have never been in a coal mine to have something done about this problem. Long before the "black lung" issue was brought up, many of our mines had installed water on their continuous miners and were using a wetting agent such as calcium chloride and others to allay dust on their haulage roads, to improve vision and prevent a possible dust explosion.

Now I would like to state here that in our areas I know of no continuous mine machine that is being operated without water.

I do not believe that coal dust is entirely responsible for all the respiratory diseases that occur in coal miners. I can say from my own experience that smoking cigarettes is one of the greatest contributors to lung disease. I was reared in a coal mining town. My father was a cutting machine operator in the mines from 1913 until his retirement in 1953. He lived to be 80 years of age and was never troubled with "black lung" or any respiratory illness. He never smoked a cigarette in his life, which may account for his good health and longevity.

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9. **Testimony of John F. O'Leary, Director,  
U.S. Bureau of Mines—May 2, 1969**

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[T]he Bureau of Mines has a research budget of around \$2 million a year and most of that, I am sorry to say, is spent in testing. When I made inquiries as to the exact distribution of the budget I found perhaps \$300,000 a year has been spent on the entire field of health up until now. That is why before Mannington immediately upon assuming this office I asked for an additional million dollars. About a like amount is spent on safety. Mr. Mullins pointed out that it took us some 8 years to develop the monitor, and I want to put as a corollary to that at \$50,000 a year. Now I don't know that you could have done it at \$100,000 a year in 4 years but I know you could have appreciably shortened that 8-year period of

time if the system had been willing to put the money to it.

I think that really brings me to a kind of sad commentary on the system that develops the technology here. In fact, the coal industry does not develop its own technology, it is developed largely externally outside the coal industry.

## NOTES

### NOTE 1.

LETTER FROM RUSSELL E. TRAIN, UNDER-SECRETARY OF THE INTERIOR, TO SENATOR HARRISON A. WILLIAMS, JR.—MAY 16, 1969

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Industrial health standards cannot be based upon personal health criteria alone. Consideration must also be given to engineering feasibility.

The proposed interim standard for respirable coal mine dust is a case in point. Because of technological limitations, it is infeasible to establish a standard immediately at the level indicated to be desirable from a personal health standpoint alone. New technology is needed to achieve a better standard, and the Bureau of Mines is better equipped than any other Federal agency to move the industry in the direction of greater health as well as increased safety. . . .

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### NOTE 2.

LETTER FROM RALPH NADER TO STEWART L. UDALL, SECRETARY OF THE INTERIOR—MARCH 23, 1968

. . . I am writing in the hope that you will bring your immediate, personal attention to bear upon the tragic plight of coal miners resulting from unsafe coal mine practices. The Bureau of Mines in your Department has, under the Federal Coal Mine Safety Act of 1952, the responsibility for promoting coal mine safety and informing Congress of the need for additional legislation. As the second most hazardous occupational category in terms of disabling injuries and the first in terms of pneumoconiosis, coal mining presents a challenge to the Bureau of Mines that is readily apparent, except apparently to the Bureau of Mines.

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The fundamental explanation for the Bureau's lassitude toward the demands of mine health and safety conditions is that it is the captive of the coal mine operators which include

large steel companies who own mines. The Bureau follows the undesirable practice of holding regular private meetings with the Bituminous Coal Operators' Association (BCOA) and the United Mine Workers (UMW) and accepting the recommendations which they agree upon to transmit to Congress.

The UMW leadership has shown a consistent bias in favor of the coal operators' viewpoints toward preventive safety policy which has produced an enduring indifference towards preventive safety and health measures for their own membership and especially towards coal miners that do not happen to be unionized. Alas for the brotherhood of workers! Its rhetoric aside, the UMW leadership has been persuaded by coal management into choosing the alleged health of the industry over the health of its workers. The specious choice of jobs over more safety is drummed into UMW officers by management and the choice has been to ignore needed safety improvements and especially preventive dust control. The UMW *Journal* devotes endless space to the threat of other energy sources to coal and virtually nothing to the crucial matter of coal dust hazards. The union has built hospitals to receive the human debris from the mines but very little to push for preventive dust control and needed, but neglected, safety practices whose furtherance the rank and file entrust to their leaders.

\* \* \*

### NOTE 3.

LETTER FROM STEWART L. UDALL TO RALPH NADER—JUNE 12, 1968

\* \* \*

. . . I am frank to confirm your general conclusion: although pneumoconiosis has been recognized for many years as a serious health problem in the coal mining industry, we have moved very slowly toward corrective measures. . . .

[O]ne of the more difficult technical handicaps in coming to grips with pneumoconiosis is that of securing accurate measurement and analysis of coal mine dust. The Bureau has not been idle in tackling this fundamental issue, having devoted extensive time and effort toward the development and testing of the required instrumentation. With that work completed, it has now begun the major survey of dust float in coal mines—a prerequisite to establishing effective standards. Even before precise scientific information is available, however, it may be possible to formulate interim guidelines and to take other steps to mini-

mize health hazards, pending more definitive standards to be prescribed and enforced.

To be equally candid about the seriousness of pneumoconiosis, it is clear that the demonstrated incidence of this insidious and irreversible malady warrants most serious concern. A statistically reliable sample survey conducted by the Public Health Service over the years 1963-64 revealed that 9.5 percent of currently employed miners and 18.6 percent of formerly employed miners had contracted the disease. The survey also indicated that its incidence increases with years of work underground. For example, of the currently employed population in the sample, 4 percent of those with 10-19 years of underground work had the disease while 8.6 percent of those with 20-29 years' exposure were afflicted. The higher percentage among the formerly employed could support an inference that exposure has led to changes in occupation, if not to disabling illness. Only a detailed case study can reveal the true facts, however, since it is also possible to surmise that improved ventilation and other preventive measures in relatively recent years may have reduced exposure in the present mine workforce.

\* \* \*

The role of the Bureau with respect to problems of occupational health in coal mines has remained investigative and advisory under the original Title I, and in this area the Bureau has not assumed the kind of leadership it has displayed in the field of safety. This situation is in part attributable to the existence of a division of responsibility in the area. Out of a laudable desire to avoid wasteful duplication of effort, the Bureau has operated under a long-standing agreement with the Public Health Service under which the latter agency carries on the basic research relating to occupational diseases affecting the mining industry. While we have no present intention of abandoning that arrangement, it is clear from our analysis to date that more efficient coordination and cooperative mechanisms are needed.

\* \* \*

#### NOTE 4.

LETTER FROM RALPH NADER TO  
STEWART L. UDALL—JUNE 27, 1968

\* \* \*

1. Medical opinion by physicians working in the coal mine health area believe that the incidence of pneumoconiosis is considerably higher

than the 1963-64 figures in your letter. Their reasoning is based on the fact that the present years are beginning to reflect the toll of the mechanization which began in the Fifties and which produces more and finer coal dust. Another reason is given by Dr. Walter A. Laquer of the Beckley (W.Va.) Appalachian Regional Hospital; namely, that many cases of pneumoconiosis are not detected by x-rays.

2. Exactly what corrective measures you have in mind were not made clear. From expressions within the Department and within the Department of Health, Education and Welfare, it is all too likely that refuge will be taken in undertaking a long range study to measure more mines for dust content and testing the required instrumentation. Fortunately, from the long experience in England and West Germany, which are far ahead of the United States in coal dust control, instrumentation and standards are available for use in this country. Action can and must be taken now, even though further research and testing be fostered. Experience in the Ontario metal mines can be utilized as well. Although the fact that some other nations have put into practice a more humane health policy than that prevailing in the United States (where apparently miners' lives are still considered cheaper than coal dust control) should be a source of shame, it can also be a spur for jettisoning any further neglect here. It is deeply dismaying to recall that the 1936 Pneumoconiosis Conference in this country concluded with recommendations which were utilized by the British and in Ontario for many years but have been ignored in this country. In some states, such as West Virginia and Kentucky, public authority has been in the grip of the coal-steel barons for so long in this area that even the pitiful human debris that mark many retired coal miners receives a disgraceful pittance of workman's compensation and that only for certain, not all, worker-related diseases. West Virginia does not recognize pneumoconiosis as a compensable disease. Can there be any conceivable excuse for permitting mining conditions that lead to the deposit of human excreta randomly to further more disease or allowing misdiagnosis of miners who collapse at work to avoid responsibility for methane and other gaseous concentrations?

The division of responsibility which you note between the Bureau of Mines and the Public Health Service does not extend to a division of responsibility over control. It is a division, if any, between control and research. Control remains with the Bureau and there is no exculpation de-



rivable from any relationship with the Public Health Service which has its own immobilities and indecisions. Nothing can even partially excuse the fact that the Bureau has done nothing about controlling dust in the mines, has never once closed down a mine for dust—not gas hazard-related but lung hazard-related. The Bureau does not even have a ranking of mines in terms of dust density.

3. [Y]our reference to the Department's advising the Labor Committee of both houses of Congress, neglects to mention the insupportable delays first in sending your promised recommendations to Congress by April, not yet delivered, and second, in view of the lateness of the session and early adjournment, the Department has effectively terminated any prospect of legislation this year.

4. If these hundreds of thousands of violations of the safety code in the past decade were not deterred because the code is advisory only, what has kept the Department from working openly and vigorously to have these codes enacted into law with penalties for violation? This lassitude, to use a kind word, has turned the Department's inspection process into a tragic farce. The Department has been restricting itself to imminent disaster situations and major disasters, defined as exceeding five fatalities, and overlooking the smaller hazards that can take single lives. This same attitude has permitted the continuance of a shocking gap in the 1952 law which exempted, among others, the working face of the mine which is the area of greatest accident-injury frequency.

\* \* \*

5. On page 4 of your letter discussing the 1963 Task Force on Coal Mine Safety and current findings and recommendations, some clarification is needed. It is simply not accurate to imply that "elimination of the reasonable time provisions" was a reform because replacing them was the "unwarrantable" violation proviso. This places an onerous burden on mine inspectors, hampering enforcement by requiring mine inspectors to show that all violations must be unwarrantable before penalties or a notice to abate such conditions can be imposed. This means that a withdrawal and debarment order will not be issued if the repeated violation did not arise as a consequence of a lack of due diligence on the part of the operator.

6. Last year's report, noted in your letter, capitulated to the demands of the coal mining in-

terests and became explicitly inconsistent with the recommendation in the 1963 Task Force report pertaining to nonpermissible electrical equipment. The Task Force recommended their elimination within a one year period; the 1967 report suggested a five year period to terminate—4 years later. Moreover, over the strong objections of some mining inspectors, the Bureau of Mines has allowed mine owners to move this unsafe electrical equipment into entirely different mines—often many miles away and sometimes across state lines. This is an example of the Bureau of Mines' discretion and another reason why your office should exercise a thorough supervision.

r.

**Letter from Walter J. Hickel, Secretary of the Interior, to Senator Ralph Yarborough, Chairman, Committee on Labor and Public Welfare—July 17, 1969**

Your committee, yesterday, requested this department's comments on the provisions of the July 9, 1969, committee print of the coal mine health and safety legislation which give us concern.

\* \* \*

#### *The Dust Standard*

The panel proposed in the print should be allowed to waive the dust standard on a mine-by-mine basis for six months after the effective date of the dust standard where it determines that the procurement, installation, and associated construction of equipment and facilities necessary to attain the dust standard cannot be accomplished in the first six months after enactment.

We continue to favor the provision of S. 2405 establishing no exact date for a 2.0 standard to be reached but requiring that the Secretary establish this standard as soon as possible. With the present state of technology, it is possible to predict with the research already underway that a 3.0 standard could be attainable in three years. The attainment of a 2.0 standard, however, may depend on the outcome of research that has yet to be started and the results of which are unknown and unpredictable. If the research is extremely favorable, the time required to meet the standard could be less than 6 years. If the research fails, it may require more than the 6 years provided in the print. The print itself recognizes that it may not be possible to attain a 2.0 standard within 6 years and authorizes the Secretary to establish a new schedule specifying a later time



for compliance with the 2.0 standard and requiring him to notify the Congress of such new schedule. If either House does not object by resolution within 60 legislative days, the new schedule will become effective. If the Committee insists on a 6 year schedule for the 2.0 standard, this additional authority is essential in view of the present state of technology.

\* \* \*

#### *Time Period for Making Equipment Permissible*

The print provides that nonpermissible electric face equipment over 25 horsepower at nongassy mines be made permissible within 16 months after enactment, with provision for the panel to issue permits of noncompliance to use such nonpermissible equipment for up to an additional 44 months, or a total of 60 months after enactment. We understand that the Committee is considering lowering this period to a total of 48 months.

At the request of your Committee, a statistical sample was taken by our field personnel on the number and condition of existing nonpermissible equipment in use and the cost of their conversion. This sampling which was conducted in a very short time and represented, for example, in the case of nongassy small mines, only 90 out of over 2,700 such mines might suggest that the conversion of all this type equipment would be possible within 3 to 4 years. We believe that the data, because they are so limited, do not justify the selection of a specific time period. In our opinion, the time period should be left open. If the panel finds that the equipment and parts are available for the conversion in a shorter period, then we are certain the panel will not grant extensions beyond that period.

#### *Penalties*

The print establishes a criminal penalty against the miner and operator for violating knowingly a health or safety standard.

In regard to the miner, there is only one standard involving smoking or carrying matches underground which places any obligation on him. In all others, the operator is solely responsible for compliance. We believe that a criminal penalty against the miner for violating the standard is not appropriate, but we would not object to a civil penalty against the miner for violating the smoking standard since this is of serious consequences to other persons underground.

In regard to the operator, we believe that the

civil penalty is sufficient, particularly when we consider the difficulty in proving a criminal violation.

\* \* \*

#### *Compensation*

The print provides that, in cases where a mine or portion thereof is idled, by reason of an unwarrantable failure closing order, the miners shall be fully compensated by the operator. Similarly, under Title I of the print, the miner who is moved to another portion of the mine because of developing pneumoconiosis is guaranteed his regular rate of pay. In our opinion, both of these matters should be left for negotiation between management and labor.

#### *Miscellaneous*

\* \* \*

Section 301(a) provides that a minimum of 4 inspections annually must be made of the entire underground coal mine. S. 2405 established the minimum at 3 inspections annually. We fail to see any real advantage in this requirement, particularly when it is so difficult to obtain qualified inspectors. Experience shows that at many large mines an inspection of the entire mine may run as long as a month. In our opinion, more has been gained since Farmington through the increased frequency of spot inspections, which do not, usually, cover the entire mine. Spot inspections, plus civil penalties, will be our most effective tools. On the other hand, if we find that increased full scale inspections are necessary for safety purposes, the bill provides that additional inspections can be required.

\* \* \*

#### NOTES

##### NOTE 1.

LETTER FROM RUSSELL E. TRAIN, UNDER-  
SECRETARY OF THE INTERIOR, TO  
SENATOR RALPH YARBOROUGH—MAY 22, 1969

\* \* \*

The Department, through the Bureau of Mines, has been engaged in a program of research and development in connection with this industry since 1910. Many of the health and safety innovations in this industry were developed by the Bureau, such as the introduction of the widespread use of roof bolts. Until recently our annual research and development budget for health and safety has been about \$2 million an-

nally, a good portion of which has been devoted to "testing" rather than research. In fiscal year 1970, however, we have increased this budget substantially to \$3.3 million. We recognize that there is a need for more health and safety research for this industry, but we doubt that a system which taxes the production of coal and earmarks the revenues for health and safety research is the most appropriate method to supply this need.

\* \* \*

[O]ther dangerous industries, such as the nuclear industry which is a competitor of the coal industry, are not required to pay directly for health and safety research. The proposal might tend to confine the entire research and development effort to the government. The future research and development effort should, in our opinion, not be carried out by the government alone. Industry should be encouraged to assume, on an industry-wide basis, a greater role in this area.

Further, we believe that tax on the coal industry for health and safety research and development purposes should not be imposed without thorough study of its impact on the industry and the consideration of alternative approaches. To our knowledge, this has not been done to date.

\* \* \*

#### NOTE 2.

##### BEN A. FRANKLIN U.S. LAGS IN EFFORT TO IMPLEMENT MINE SAFETY LAW\*

The frequently troubled coal regions are being placed in a further state of turmoil these days as a result of lagging efforts to implement a new coal mine safety law.

The law involved is the Federal Coal Mine Health and Safety Act of 1969 . . .

\* \* \*

The law has its partisans and detractors here, and few officials claim to be objective about it. But even many of those who make that claim say it may be trying to accomplish too much reform all at once.

The wide scope of the new law is generally welcomed by government safety men. But it is causing concern and leading to forecasts of dislocations that will cause a decline in support when coal reserves are critically short. Accordingly, the Bureau of Mines expects to begin with "token enforcement."

The coal mining industry is howling, predicting crippling reductions in fuel supplies for electric utilities and trying to negotiate last-minute relief as the new law heads toward a series of effective dates beginning April 1. The coal industry says it is working in the dark, without knowing what is expected of it.

There is disarray at the Bureau of Mines, stemming in part from a frantic bureaucratic scramble to obey the timing imposed by Congress. Some provisions of the law, which industry leaders contend will "just explode normal coal production," have a very short fuse.

The worst crisis is being caused by the fact that the most costly and controversial section of the law—language establishing for the first time strict limits on the amount of lung-damaging microscopic coal dust that will be allowed in the air that the miners breathe if the operator expects to stay in production—were required by law to be translated by government health and safety experts into precise regulations for the mine owners by Feb. 28.

They were not. A complete set of proposed dust regulations was drafted nearly on time by John F. O'Leary, who was director of the Bureau of the Bureau of Mines until he was forced out by the Nixon administration reportedly at the coal industry's insistence.

But the delay in publishing the regulations since then reflects the fact that the leaders of the mining industry have been holding closed-door conferences with government officials on the O'Leary draft.

In a letter Friday to Walter J. Hickel, the Secretary of the Interior, who has jurisdiction over the Bureau of Mines, W. A. Boyle, president of the United Mine Workers said that "we assume" the delay "is a further indication of the callous disregard for the health and safety of the coal miner."

\* \* \*

The dust regulation deadline now is three weeks past. The O'Leary regulations, which officials in the Bureau of Mines insist "have not been substantively weakened," are still not ready

\* *The New York Times* 15, cols. 1-2 (March 23, 1970). © 1970 by The New York Times Company. Reprinted by permission.

for promulgation in the Federal Register. As a result, other deadlines are piling up.

\* \* \*

#### NOTE 3.

##### COAL MINERS TELL SENATE INQUIRY SAFETY RULES ARE BEING BROKEN\*

Miners told Senator Harrison A. Williams, Jr. today that safety regulations were "constantly being broken" at a gaseous United States Steel Corporation mine near this southwestern Pennsylvania town.

Mr. Williams, Democrat of New Jersey, took members of his Senate subcommittee into the coal fields to hear the complaints of dissident miners. He talked to men preparing to enter United States Steel's Cherokee shaft in nearby Bentleyville, Pa. on the morning shift. It was the miners' first full day back on the job since a three-day strike over safety issues.

"I can see already that they're not approaching what the law requires," Mr. Williams, author of the new Coal Mine Health and Safety Act, said of Federal officials.

Mickey Britvich, of Denbo, Pa., vice president of Local 1248 of the United Mine Workers union told Mr. Williams that the government had not sent any investigators to the mine to test for gaseous conditions although the law required them to inspect one every five days.

"The safety regulations are constantly being broken," Mr. Britvich said. "We have to keep pressure on them all the time."

\* \* \*

#### NOTE 4.

##### BEN A. FRANKLIN CHAIRMAN OF TWO SENATE COMMITTEES URGE A FEDERAL INQUIRY INTO FATAL COAL MINE EXPLOSION IN KENTUCKY†

Demands for investigations to fix the cause and responsibility for the eastern Kentucky coal mine explosion that killed 38 men last Wednesday began to mount here today.

The chairmen of two Senate committees strongly urged the Interior Department to convene immediately a public hearing in eastern

Kentucky in which the Bureau of Mines would take the testimony of miners, their widows [sic] and Federal mine safety officials. They asked that the hearing look into the enforcement of the new Federal Coal Mine Health and Safety Act of 1969.

The request was made in a letter to Acting Secretary of the Interior Fred J. Russell from Senators Henry M. Jackson of Washington, chairman of the Committee on Interior and Insular Affairs, and Harrison A. Williams, Jr. of New Jersey, chairman of the Committee on Labor and Public Welfare. Both Senators are Democrats.

In drafting the mine safety act, their letter said, "Congress took great care to provide you with all the necessary tools to assure safe working conditions" in the mines. It said "the public has a right to know" what caused the Kentucky disaster exactly one year after passage of the law.

\* \* \*

In letters to Senator Williams and Representative Perkins, Ralph Nader, the safety advocate, charged that the Kentucky disaster showed "the Administration's continued and flagrant disregard of the coal mine health and safety law." He said that a Congressional committee should determine who was responsible for the Bureau of Mines, decision not to inspect the mine on Dec. 22, deadline for abatement of a number of previously reported safety violations.

Representative Ken Hechler, Democrat of West Virginia, speaking on the House floor, said that the White House had "lobbied behind the scenes to weaken the mine safety bill" and that the President threatened to veto it last December.

"The President is in a position now to insist that the law be enforced," Mr. Hechler declared. "Let him send out the word that if he is really interested in law and order, here is a good place to start."

## 2.

### The Health and Safety of Astronauts

#### Derek J. de Solla Price The Case of the Kamikaze Astronauts\*

Like the interest displayed by Sherlock Holmes in the barking of the dog during the

\* *The New York Times* 29, col. 1 (June 27, 1970). © 1970 by The New York Times Company. Reprinted by permission.

† *The New York Times* 34, col. 1 (January 3, 1971). © 1971 by the New York Times Company. Reprinted by permission.

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night, we must see some significance in the Kamikaze astronauts that did *not* participate on either side of the race for the Moon. Early in the history of the Soviet-American competition following Sputnik it must have become quite clear to both sides that one could "cheat" in the race by designing a non-return soft landing on the Moon. The economy of not carrying the fuel to blast off from the lunar surface and make the return to a soft landing on the Earth is so very great that a couple of generations of rocketry, several years of work and very many billions of dollars separate the two objectives. There seems little doubt that either side could have won by mounting a mission that would have set a live man on the surface of the Moon but left him to a certain death there sooner or later after hours or days or even weeks of useful research work as oxygen and water ran out.

Let it be said immediately the life would not have been lost in any cause so trivial as a simple winning of a race between two superpowers. An early knowledge of the nature of the lunar surface might have made unnecessary much of the subsequent effort and money, and it could even have saved the lives that were lost in working towards the objective of landing men on that hostile surface with only a good chance, but still no presumed certainty of safe return. We might even have learned enough that the decision would have been to abandon the lunar target and plan more slowly for an eventual planetary exploration of Venus or Mars a decade or so later.

Let it also be said that there must have been no dearth of ready volunteers on both sides for such a certain suicide mission. Given that it was seen as part of the glorious destiny of mankind and of high national purpose, there would have been as many ready to lay down their lives without hope. Such Kamikaze suicide volunteers have always been forthcoming in wartime and insurrection, but also for geographical exploration as well as for scientific research. The very glamour of space for a generation brought up on science fiction and the knowledge that the sacrifice would be more visible and more assured of immortal fame than almost any preceding case would clearly have produced candidates galore, including several already vetoed by the space agencies of the two nations concerned and (for what it is worth) the present writer, too.

In spite of all this it is quite clear that not only was such a mission rejected, but it seems highly probable that the possibility was rejected

out-of-hand and without even fleetingly serious attention by either side. After all the fuss of leaving a dog to die in orbit, any nation that left the first man on the Moon to die would have been exhibiting a gross and utter callousness in full view of the world and in such a way that the mere scientific and moral victories over space would have become a sour prize. In retrospect however, if it is true that both nations seriously overspent at this stage and risked a considerable and lasting world reaction against science and space in this process, it might have been much wiser to permit the sacrifice of a single life in a blaze of glory instead.

## NOTE

### AN INTERVIEW WITH RALPH E. LAPP THE COMING TRIP AROUND THE MOON\*

\* \* \*

*Do you think, then, that we are taking unnecessary risks in our race to the moon?*

We are pushing our luck, gambling that everything will work perfectly. NASA experts will assure you that they have thought through the risks and have planned for them. Well, they didn't in Apollo-204. They maintain they have backup systems in case there are systems malfunctions. They also contend that risk evaluation can take place at critical points in Apollo's flight path. For example, the decision to accelerate from an Earth orbit to escape velocity can be made in orbit. Likewise the decision to descend into a lunar orbit can be made when all systems have been checked out just prior to injecting into that orbit.

\* \* \*

*If Apollo's main engine fails to fire in a lunar orbit, what happens?*

Our astronauts will be stranded in orbit.

*You mean they are condemned to death?*

Well, they can't get out and fix the engine. They are completely dependent on that engine firing. There is no backup there.

\* \* \*

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\* *The New Republic* 16-17 (December 14, 1968). Reprinted by permission of THE NEW REPUBLIC. © 1968, Harrison-Blaine of New Jersey, Inc.

*Has NASA studied this problem of rescue?*

Yes, they did several years ago. However the study showed that it would be costly to provide for a rescue capability, and it would slow down the space program. It was abandoned. . . .

*So the United States has no backup capability to rescue or relieve the Apollo crew?*

No. Apollo-9 won't be ready to fly until one or two months after Apollo-8. It's not slated for a lunar goal but rather to run a 10-day earth-orbital check-out of the lunar module.

We could delay the program so that Apollo-9, configured to have a relief-capability, would be on the pad ready for launch if Apollo-8 runs into trouble.

If we can relieve the crew, either through exchange or by provisions, I'm sure that NASA would undertake heroic measures to rescue the astronauts. It would be a fantastic rescue mission and might have more impact than the original flight. At the very least we ought to be prepared to attempt a rescue.

\* \* \*

*How much does an Apollo cost?*

The unit cost depends on how you distribute charges for research, development, production facilities and launch complexes. NASA gives out a figure of \$349 million based on a production rate of two a year. But if you take total expenditures for manned space flight and distribute them over 11 Apollos the unit cost would be about \$2 billion. Somewhere between these two values is a reasonable estimate.

\* \* \*

*If the astronauts were marooned on the moon couldn't we ferry supplies to them?*

NASA has not planned to do so. I would suggest it would be better to break the trail to the moon by landing supplies in advance. Then have the LEM (lunar excursion module) descend from orbit to this site. However it would change our lunar timetable; we might not be the first on the moon.

*Is advance landing of supplies practical?*

NASA successfully landed five Surveyor devices on the lunar surface and many television pictures were sent back to earth. Landing a cargo would be a much simpler operation. This pathfinder payload could be fitted with a radio beacon to guide in the LEM.

\* \* \*

### 3.

#### Risking Lives—At What Cost?

a.

#### Guido Calabresi The Costs of Accidents A Legal and Economic Analysis\*

\* \* \*

Our society is not committed to preserving life at any cost. In its broadest sense, the rather unpleasant notion that we are willing to destroy lives should be obvious. Wars are fought. The University of Mississippi is integrated at the risk of losing lives. But what is more pertinent to the study of accident law, though perhaps equally obvious, is that lives are spent not only when the *quid pro quo* is some great moral principle, but also when it is a matter of convenience. Ventures are undertaken that, statistically at least, are certain to cost lives. Thus we build a tunnel under Mont Blanc because it is essential to the Common Market and cuts down the traveling time from Rome to Paris, though we know that about one man per kilometer of tunnel will die. We take planes and cars rather than safer, slower means of travel. And perhaps most telling, we use relatively safe equipment rather than the safest imaginable because—and it is not a bad reason—the safest costs too much. It should be apparent that while some of these accident-causing activities also result in diminution of accidents—the Mont Blanc tunnel may well save more lives by diminishing traffic fatalities than it took to build it—this explanation does not come close to justifying most accident-causing activities. Railroad grade crossings are used because they are cheap, not because they save more lives than they take.

Since we are not committed to preserving life at any cost, the question is the more complex one of how far we want to go to save lives and reduce accident costs. This leads us to the second myth: that economic theory can answer the question. Just as economic theory cannot decide for us whether we want to save the life of a trapped miner, so it cannot tell us how far we want to go to save lives and reduce accident costs. Economic theory can suggest one approach—the market—for making the decision. But decisions balancing lives against money or convenience cannot be purely monetary ones, so the market method is never the only one used.

\* New Haven: Yale University Press 17-20, 23-31 (1970). Reprinted by permission.

The decision to build the Mont Blanc tunnel is not based solely on whether the revenue received from tolls will pay for the construction costs, including compensation of the killed and maimed. Neither is the decision to permit prostitution based solely on whether it can pay its way. Such pure free enterprise decisions have never been acceptable and have been, in fact, rejected by even the most orthodox of classical economists, who did, however, feel it necessary to explain the rejection through the use of such terms as external social costs and benefits, concepts which are not self-defining and are in fact as narrow or as broad as any society cares to make them.

The issue, whether or not expressed in terms of hidden social costs or hidden social savings theories, is how often a decision for or against an activity should be made outside the market. Such decisions operate on the one hand to create subsidies for some activities that could not survive in the marketplace, and on the other to bar some activities that could more than pay their way. The frequency with which decisions to ignore the market are made tells something about the nature of a society—welfare, laissez faire, or mixed. It is clear, however, that in virtually all societies such decisions to overrule the market are made, but are made only sometimes.

In accident law too, the decision to take lives in exchange for money or convenience is sometimes made politically or collectively without a balancing of the money value of the lives taken against the money price of the convenience, and sometimes made through the market on the basis of such a value. The reasons for choosing one way rather than the other are not entirely reasons of principle. Great moral issues lend themselves to political determination and must be decided in whatever political way a society chooses. But whether to use rotary mowers instead of reel mowers and what method to use for making steel are questions not easily answered collectively. For one thing, they occur too frequently. Every choice of product and use involves, tacitly or otherwise, a decision regarding safety and expense. The dramatic cases can be resolved politically. We ban the general sale of fireworks regardless of the ability or willingness of the manufacturer to pay for all of the injuries resulting from their use. But we cannot deal with every issue involved in every activity through the political process. In most cases, the marketplace serves as the rough testing ground. A manufacturer is usually free to employ a

process that occasionally kills or maims if he is able to show that consumers want his product badly enough to enable him to compensate the injured. Economists would say that, except in some areas where collective decisions are needed, this is the best method for deciding whether the activity is worth having. But the tautologous nature of this statement makes it clear that ultimately, we collectively, and not economics, are the boss.

In other words, although the market can help us to decide how far we wish to go to avoid accidents, it cannot solve the whole problem for us. And when we overrule the market and ban an accident-causing activity that can pay its way or subsidize an activity that cannot, we are not violating absolute laws. We are making the same type of choice between accidents and accident-causing activities that the market makes, but we are choosing, for perfectly valid reasons, to make it in a different way. We are preferring a collective approach or method (e.g. because it enables consideration of nonmoney costs which the market cannot deal with, or because in the particular instance it is cheaper) to a market approach, even though the market might allow for individual differences in tastes and desires that the collective decision might tend to ignore.

\* \* \*

One final . . . word of caution may be useful. When we deal with accidents we are dealing with costs, for that is what accidents involve. We are, to be sure, also dealing with emotional and moral attitudes, but we are always dealing with these in relation to costs. If we were not, we would wish to avoid accidents "at all costs." It follows that in examining any approach to accidents we must always keep in mind the cost of establishing and effectuating the approach, as well as the benefits the approach is expected to bring about. These costs and benefits, moreover, must be compared with the costs and benefits of alternative approaches. We must, in short, always ask whether the game is worth the candle, not only in terms of the cost of the candle but also in terms of other games we might be playing.

\* \* \*

What, then, are the principal goals of any system of accident law? First, it must be just or fair; second, it must reduce the costs of accidents.

#### *Justice*

Justice, though often talked about, is by far the harder of the two goals to analyze. It is often

said that a particular system of accident law, be it fault, social insurance, or enterprise liability, is supported by one's sense of fairness or justice. But such statements are rarely backed up by any clear definition of what such support means, let alone by any empirical research into what is considered fair.

In fact, it is doubtful that such empirical research would tell us very much anyway. As one scholar has observed, it is much easier to describe instances of *injustice* than examples of justice. We are much surer that particular processes or results are unfair than that particular arrangements are just in some positive sense. We can readily document specific injustices that occur in existing systems, such as the fault system or workmen's compensation. But the requirements of fairness that those systems may meet are difficult to define and therefore are usually stated in generalities, in hope of striking a responsive chord. This responsive chord, however, may be an inadequate guide to what our reaction would be if the system were changed. Conversely, while it is fairly easy to argue that particular untried systems will cure current injustices, it is much harder to foresee the injustices they may create.

More important, claims that particular systems are just, like those that justice is in some sense a goal concurrent with accident cost reduction, fail to ring true. They seem to suggest that a "rather unjust" system may be worthwhile because it diminishes accident costs effectively; or, conversely, that there is one system that can be termed just to the exclusion of all others, i.e. that is supported by justice in the same sense that economic efficiency may prefer one system to all others. But the words just and unjust do not sound right to me in either of the statements. They ring true in rather different contexts, as when we say that we reject a particular system or parts of it as unjust, or that a system taken as a whole does not violate our sense of justice. This suggests that justice is a totally different order of goal from accident cost reduction. Indeed, it suggests that it is not a goal but rather a constraint that can impose a veto on systems or on the use of particular devices or structures within a given system (e.g. administrative tribunals under the fault system) even though those same structures might not be unjust in another system (e.g. administrative tribunals under workmen's compensation).

All this discussion may make the concept of justice seem both negative and elusive. But it

affords no excuse for ignoring justice in discussing accident law. Our reaction to accidents is not a strict dollars-and-cents one. If it were, I doubt that we would accept railroad crossing accidents because it costs too much to eliminate grade crossings and yet spend "whatever it takes" to save a known individual trapped in a coal mine. An economically optimal system of reducing accident costs—whether decisions are made collectively, through the market, or through a combination of both—might be totally or partially unacceptable because it strikes us as unfair, and no amount of discussion of the efficiency of the system would do much to save it. Justice must ultimately have its due.

But if the elusiveness of justice cannot justify ignoring the concept, it at least justifies delaying discussion of it. The fact that what is unfair is easier to define than what is fair, like the fact that what is fair in one system may be unfair in another, indicates that it would be better to examine the requirements of accident cost reduction first and then to see how various untried methods and systems suggested by that goal compare in terms of fairness with the systems we use today—how, in other words, they comply with our general sense of fairness and whether they are more or less likely to create specific instances of injustice than the current systems. Such an approach may not lead us to the fairest systems possible but it may well indicate whether change is desirable.

#### *Reduction of Accident Costs*

Apart from the requirements of justice, I take it as axiomatic that the principal function of accident law is to reduce the sum of the costs of accidents and the costs of avoiding accidents. (Such incidental benefits as providing a respectable livelihood for a large number of judges, lawyers, and insurance agents are at best beneficent side effects.) This cost, or loss, reduction goal can be divided into three subgoals.

The first is reduction of the number and severity of accidents. This "primary" reduction of accident costs can be attempted in two basic ways. We can seek to forbid specific acts or activities thought to cause accidents, or we can make activities more expensive and thereby less attractive to the extent of the accident costs they cause. These two methods of primary reduction of accident costs are not clearly separable. . . .

The second cost reduction subgoal is concerned with reducing neither the number of accidents nor their degree of severity. It concentrates



instead on reducing the societal costs resulting from accidents. I shall attempt to show that the notion that one of the principal functions of accident law is the compensation of victims is really a rather misleading, though occasionally useful, way of stating this "secondary" accident cost reduction goal. The fact that I have termed this compensation notion secondary should in no way be taken as belittling its importance. There is no doubt that the way we provide for accident victims *after* the accident is crucially important and that the real societal costs of accidents can be reduced as significantly here as by taking measures to avoid accidents in the first place. This cost reduction subgoal is secondary only in the sense that it does not come into play until after earlier primary measures to reduce accident costs have failed.

\* \* \*

The third subgoal of accident cost reduction is rather Pickwickian but very important nonetheless. It involves reducing the costs of administering our treatment of accidents. It may be termed "tertiary" because its aim is to reduce the costs of achieving primary and secondary cost reduction. But in a very real sense this "efficiency" goal comes first. It tells us to question constantly whether an attempt to reduce accident costs, either by reducing accidents themselves or by reducing their secondary effects, costs more than it saves. By forcing us to ask this, it serves as a kind of general balance wheel to the cost reduction goal.

\* \* \*

It should be noted . . . that these subgoals are not fully consistent with each other. For instance, a perfect system of secondary cost reduction is . . . inconsistent with the goals of reducing primary accident costs. We cannot have more than a certain amount of reduction in one category without forgoing some of the reduction in the other, just as we cannot reduce all accident costs beyond a certain point without incurring costs in *achieving* the reduction that are greater than the reduction is worth. . . .

In this sense, it may seem unwise to divide accident cost reduction into three subgoals at all. It might seem better to lump all accident costs together and concentrate on finding that point at which further accident cost reduction is not worth its costs, especially since the division of accident cost reduction into subgoals is ultimately an arbitrary one. . . .

\* \* \*

One could, of course, consider the goals of accident law more broadly and ask what accident law may do to cure evils in our society that are not a result of accidents. The list of possibilities would be endless, and any analysis of accident law would be virtually impossible. Nevertheless, we should be aware that accident law, like any other branch of law, can be used to accomplish an enormous variety of goals. . . .

\* \* \*

## NOTE

GUIDO CALABRESI

### REFLECTIONS ON MEDICAL EXPERIMENTATION IN HUMANS\*

The problem of experimentation on humans necessarily looks rather different to one who has concentrated on accident law than it does to the doctor or even to the jurist. The torts professor sees the possibility of a choice between the life, well-being, or comfort of a given patient and the lives or well-being of unknown future patients. He is immediately struck that the issue in medical experimentation is the risking of lives to save other lives while in accident law, almost always, the issue is the taking of lives simply because saving them costs too much.

In torts law, we have become accustomed to the fact that many activities are permitted, even though *statistically* we know they will cost lives, since it costs too much to engage in these activities more safely or to abstain from them altogether. We have grade crossings, even though we know that with grade crossings a certain number of people will be killed each year and even though grade crossings could be eliminated relatively easily. We use automobiles—knowing that they cost us fifty thousand lives each year—because to use safer, slower means of transport would be far too costly in terms of pleasures and profits forgone. Worse even than that, we use automobiles with relatively cheap (but relatively dangerous) tires, airports with relatively cheap (but relatively dangerous) control systems, and so on *ad infinitum*. And we do this because we deem the lives taken to be cheaper than the costs of avoiding the accidents in which they are taken.

From the perverse standpoint of accident

\* 98 *Daedalus* 387-393 (1969). Reprinted by permission of *Daedalus*, Journal of the American Academy of Arts and Sciences, Boston, Massachusetts.



law, then, the whole fury about medical experimentation would seem to be a tempest in a teapot. Surely it is more justifiable to take some lives in order to save more lives than it is to take lives simply to save money, as we do in the accident field. But this view, I fear, is far too superficial. Even in the accident field, there are many occasions when we do treat life as a pearl beyond price. When a known individual is trapped in a coal mine, we try to rescue him at enormous money cost and even at the risk of many other lives. Yet if we always gave human life the value we give to the life of the man in the coal mine, we would surely abolish grade crossings, make cars and airports much safer, and perhaps even forbid "non-essential" driving completely. What is the meaning of this apparent paradox? And what does it tell us about medical experimentation?

The first possible explanation has to do with statistics. Somehow a man is less a man to us when he is simply a number. We know the man trapped in the coal mine, just as we often know the patient subjected to experimentation. The statistical accident victim we do not know, and so we can ignore him. But that is not in itself an adequate explanation. The statistical victim is just as real as the man in the coal mine. If we want to be fully rational, we must admit to ourselves that he has as much of a family as a known victim, that he and they suffer as much when he is killed, and that only a willful ignoring of reality enables us to treat him as less real than the man trapped in the coal mine.

But perhaps this willful ignoring of statistical victims is less foolish, though no more "rational," than it might seem at first glance. We are committed to "humanism," to the dignity of the individual, and to human life. Much of the fabric of our society depends on our belief in this commitment, as do most of our traditional and "cherished" liberties. Accident law indicates that our commitment to human life is not, in fact, so great as we say it is; that our commitment to life-destroying material progress and comfort is greater. But this fact merely accentuates our need to make a bow in the direction of our commitment to the sanctity of human life (whenever we can do so at a reasonable total cost). It also accentuates our need to reject any societal decisions that too blatantly contradict this commitment. Like "free will," it may be less important that this commitment be total than that we believe it to be there.

Perhaps it is for these reasons that we save

the man trapped in the coal mine. After all, the event is dramatic; the cost, though great, is unusual; and the effect in reaffirming our belief in the sanctity of human lives is enormous. The effect of such an act in maintaining the many societal values that depend on the dignity of the individual is worth the cost. Abolishing grade crossings might save more lives and at a substantially smaller cost per life saved, but the total cost to society would be far greater and the dramatic effect far less. I fear that if men got caught in coal mines with the perverse frequency with which cars run into trains at grade crossings, we would be loath to rescue them; it would, in the aggregate, cost too much. Lest this remark seem unduly cynical, we might consider our past unwillingness to keep all but a few victims of renal failure alive by use of artificial kidneys. Until now, artificial kidneys have cost too much, and people perversely have suffered kidney failure too frequently, so even though the victim was as clearly known to those who had to decide whether to save him as is the man in the mine, the answer quite frequently was no.

It should be clear that the foregoing does not mean that individual human life is not valued highly. Nor, certainly, does it suggest that we are indifferent to when and how society should choose to sacrifice lives. Quite the contrary; it indicates that there is a deep conflict between our fundamental need constantly to reaffirm our belief in the sanctity of life and our practical placing of some values (including future lives) above an individual life. That conflict suggests, at the very least, the need for a quite complex structuring to enable us *sometimes* to sacrifice lives, but hardly ever to do it blatantly and as a society, and above all to allow this sacrifice only under quite rigorous controls. (This last desire to control individual takings and yet to keep society from being the blatant taker itself reflects a conflict of desires.) I suggest that the problem with human experimentation lies in the fact that, unlike accidents, it has seemed to be quite unamenable to most of the complex "indirect" controls over takings of lives we have so far developed in our society.

In the field of accidents, much of the control over the taking of human lives is accomplished by what economists call the market. Limbs and lives are given a money value; the activities that take lives or limbs in accidents pay the victims; and people quite coldly decide whether it is cheaper to install a safety device or to pay for the accidents that occur because the safety device is

missing. Despite the enormous oversimplification of the foregoing example (the effect of "fault" in determining accident payments, for instance, is ignored), it indicates how "accidents" are controlled in an indirect fashion which, nonetheless, takes into account both the values of lives taken and the cost of saving them.

The beauty of the market device is that no one seems to be making the decisions to take lives and, therefore, no blatant infringement of the commitment to human life as sacred occurs. Moreover, when society *does* enter into the accident field directly, it is usually to impose more stringent prohibitions, regulations, or safety standards than the market would bring about. We do not allow drunken driving—any more than we allow murder—even though the drunk may be perfectly willing to compensate his victim. The consequence is that collective societal action seems always to be directed toward preserving the individual life rather than taking it, and our commitment is further strengthened. (Only a few professors worry that failure to go beyond the market in areas where the individual choice to take lives is less obvious than in drunken driving or murder is also a societal decision, but one which lets lives be taken. Such ratiocinations of professors happily do not destroy the picture of a self-contained system in which almost all collective decisions are life-saving ones.)

Other elements of accident law serve to reduce still further the blatantness of the taking. In many situations, the victim can be said to have, to some extent at least, consented to the risk. Consent is often actually very dubious. Are we, in fact, free to avoid driving cars? Is a tunnel-digger free to engage in a safer occupation? And is there any consent at all when a pedestrian is run down by a car? But these questions are neither here nor there. They would be crucial were free consent the keystone of the system (as it may have to be in medical experiments). Where, however, consent serves merely to lessen further the directness of a taking that is already controlled by a seemingly impersonal system, even semi-free consent suffices to support the belief that our society prizes individual lives above all.

The same is true about the introduction of moral elements like fault into a system of accident law. The search for a faulty party on whom damages must rest can seriously undermine the market control system I have described. For this

and other reasons, fault may well be on its way out. This is especially true since too many people have come to realize that frequently the search for a faulty party in an accident is either a sham or a fraud. When people still believed otherwise, however, the semblance of a search for a faulty party served, like consent, to reinforce the belief that the level of accidents was a matter of individual choice and not something society determined.

Finally, the temporal juxtaposition of decisions to avoid accidents and lives taken serves to make "accidental" takings of lives seem less blatant. At the time a decision to adopt a safety device is to be made, the cost of the device is both present and real; the accident costs to be saved may also be statistically known; but the lives themselves are in the future and seem conjectural. Once again, if the decision is made against the device, even the individual making the decision—let alone society—does not seem to be choosing "certainly" to destroy lives.

In medical experiments, much of this process seems reversed. It is the lives to be saved by the experiment that seem future and conjectural, while the life to be risked or taken is both present and real. Most of the elements of fault are absent—the victim usually is sick through no choice or fault of his own. As a result, only the possible presence of consent seems left to lessen the blatantness of the choice to risk a life. But consent can no more do the whole job here than it can in accidents. Totally free consent is simply too rare an animal. The usual semi-free consent serves in accident situations because it reinforces an adequate system of control governing more generally when lives are to be taken, but without seeming to infringe on our basic commitment to human life. Just such a system of control is needed in medical experimentation.

Perhaps, however, we have accepted the analogy between medical experiments and accidents too quickly. It may be that the taking of lives that happens in accident situations is different in substance from the taking that occurs in medical experiments, or would occur were we to let the man trapped in a coal mine die. If there is a difference that goes beyond the matter of appearances, beyond the existence in accidents of a complex self-operating system of control, then the problem of medical experiments cannot be solved simply by devising complex, indirect control mechanisms to balance society's interest in present as against future lives.

The nub of the argument is this: The notion is incorrect that we in some sense choose the number of people who will be killed in automobile accidents by choosing a market system that will determine how much safety is worth. The notion is only made plausible by a verbal trick—by using the words “we choose” to describe both the effects of the social system in which we live and which we tolerate, but which we cannot in fact be said to choose, and events as to which we can be said to exercise purposive choice. “We” do not choose automobile accidents, the argument runs, any more than “we” choose a world in which hundreds of thousands of Indians die young of disease and lack of food. We do not choose this because the alternative is never presented in a realistic enough fashion; it is never presented so that the costs of saving the Indians are clear. The costs of saving the Indians, like the costs of avoiding automobile accidents, are the costs of moving from an existing social system to a new one. As such, they are unknown and involve a substantial risk that, whatever pattern of life the new system brings, *more* lives will be taken than in the old system. How different, the argument concludes, is this passive tolerance of the world as it is from the active choice to let someone caught in a coal mine die, or from the decision to risk an individual human life in a medical experiment.

I do not believe that this argument destroys the usefulness of the analogy between medical experiments and automobile accidents. In a way, it is no more than a mixture of two quite different points. First, a choice to save a life at the price of paying readily ascertainable costs is very different from a choice to save lives when the saving would be accomplished only by a radical restructuring of society entailing unknown costs. This point is certainly true, but does not distinguish many accident situations from medical experiments. Second, there is a genuine difference between a positive choice to subject someone to a risk or to take his life and a passive acquiescence in a system that results in lives being taken when they could be saved at ascertainable costs. This second is a distinction which, I claim, has only psychological significance. Because the choice to take lives is less obvious, it is less destructive of the essential myth that human life is a pearl beyond price.

There are, to be sure, accident situations where lives could be saved only by restructuring our whole social system. Giving up the automo-

bile altogether might be an example. It is hard to know what the full costs of that decision would be, or whether in the end the change would save or cost more lives. As such, it is fair to say that we do not choose to take lives by having automobiles in the same sense as we choose to let the man in the mine die if we fail to rescue him. But there are other situations where lives can be saved without such a radical change. Abolition of grade crossings, differently made automobiles, and more safely constructed highways—all would save lives in exchange for readily determinable costs. We can (but do not) require these. We allow their establishment to be controlled by the market. What is more, we readily observe the results of market control. We then discuss in Congress whether intervention is justified, and we often decide not to intervene. In our passivity, we are choosing to let the indirect market control method make the choice between lives and costs, and no amount of talk about merely tolerating an existing social system can change that.

But this second point may be more subtle. It may center on the fact that there is no one who can clearly be identified with the “we” in the last paragraph. No one has purposefully chosen the market method of controlling accidents, and no one, in our society, has the clear responsibility for making radical changes in the method. These facts happily leave us with the feeling that no one is directly responsible for any specific life taken and that neither as individuals nor as a society do we choose against lives in order to save money. Yet it remains true that we are unlikely to want to scrap the system of control that luckily has come into being. And to say this is precisely to say that a method which gives *satisfactory* control of the choice between lives and cost is operating without anyone bearing the onus of having purposefully chosen the method, let alone the onus of seeming to destroy individual lives for the sake of money. Since no adequate control system over medical experiments has arisen by itself, we cannot avoid the onus of working purposefully toward establishing a control system. This indicates that we will not end with so psychologically satisfactory a result as we have in the field of accidents. But, if anything, this fact heightens the need for establishing a system in which the actual choice over the taking of lives is as diffuse as possible.

Thus, the question remains as to whether or not we can find a control system in the medi-

cal experiment field that affords an adequate balancing of present against future lives and is still sufficiently indirect and self-enforcing as to avoid clear and purposive choices to kill individuals for the collective good.

b.

**Edmond Cahn**

### **Drug Experiments and the Public Conscience\***

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The thalidomide tragedy of 1962 showed that it was not only the manufacturers and dispensers of drugs who needed to reassess their moral responsibilities. It also revealed a certain disease of hypocrisy affecting large portions of the American people. The hypocrisy manifests itself in two different but related syndromes, which for purposes of convenience we can call the "Pharaoh syndrome" and the "Pompey syndrome."

When ancient Pharaoh built a pyramid, it is possible that his more methodical overseers might have reported that the cost of construction had included some thousands or hundreds of thousands of human lives. Proper accounting would have computed these lives as part of the over-all expense to the Egyptian throne; someone may even have kept comparative figures of mortalities from construction job to construction job. Be that as it may, Egyptian records do not say that anyone hesitated to expend a few, a few thousand, or a few hundred thousand workers.

Nowadays many Americans are satisfied to refer to the thalidomide episode and its consequences with some mildly regretful remarks about "the social cost of progress." To some of us, such remarks do not seem quite adequate. We suspect that it requires an authentic "Pharaoh syndrome" to convert misshapen babies with flippers for arms into mere items of "social cost." We grant, of course, that there is something conveniently impersonal about the phrase "social cost."

\* \* \*

The second or "Pompey syndrome" may be even more popular in this country. I have taken the name from young Sextus Pompey, who ap-

pears in Shakespeare's *Antony and Cleopatra* in an incident drawn directly from Plutarch; Pompey, whose navy has won control of the seas around Italy, comes to negotiate peace with the Roman triumvirs Mark Antony, Octavius Caesar, and Lepidus, and they meet in a roistering party on Pompey's ship. As they carouse, one of Pompey's lieutenants draws him aside and whispers that he can become lord of all the world if he will only grant the lieutenant leave to cut first the mooring cable and then the throats of the triumvirs. Pompey pauses, then replies in these words:

Ah, this thou shouldst have done,  
And not have spoke on't! In me 'tis villainy;  
In thee't had been good service. Thou must know  
'Tis not my profit that does lead mine honour;  
Mine honour, it. Repent that e'er thy tongue  
Hath so betrayed thine act; being done unknown  
I should have found it afterwards well done,  
But must condemn it now. Desist, and drink.

Here we have the most pervasive of moral syndromes, the one most characteristic of so-called respectable men in a civilized society. To possess the end and yet not be responsible for the means, to grasp the fruit while disavowing the tree, to escape being told the cost until someone else has paid it irrevocably; this is the Pompey syndrome and the chief hypocrisy of our time. In the days of the outcry against thalidomide, how much of popular indignation might be attributed to this same syndrome; how many were furious because their own lack of scruple had been exposed! So many did not really care, did not even want to know what the new drugs might cost in terms of human injuries and fatalities. The dispensers of thalidomide had outraged the public by breaking an unwritten law—the law against interrupting the public's enjoyment of fruits with disagreeable revelations about the tree and the soil where the fruits have grown.

. . . Posterity will not fail to recognize the Pharaoh and Pompey syndromes in the behavior of our contemporary public. As we in our day can smile condescendingly at the primitives and ancients who practiced human sacrifice for what they considered to be the general good of the tribe or nation, future generations may ask how we could make human sacrifice more acceptable in our day by calling it "social cost." . . .

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\* Paul Talalay, ed.: *Drugs in Our Society*. Baltimore: The Johns Hopkins Press 255, 258-261 (1964). Reprinted by permission.

## C.

## Man's Readiness to Delegate Authority to Experts

Technologically advanced societies, with their complex division of labor, accord a unique position to persons whose training qualifies them to apply specialized knowledge in the resolution of individual problems. As "professionals" they are given authoritative status to define the qualifications and rules of their discipline and to work with relative autonomy, unfettered by many forms of social control. Since most experimenters are identified with one of the traditional professions, their relationship both to society and to individual subjects is colored by prevailing attitudes toward professionals in general.

Usually, the professional's services are sought by the client. Once the client has "placed himself in the expert's hands," it is generally the expert who selects the goals and means of action and who decides how his skills can best be employed to satisfy the client's needs. Such interactions are supported by an actual willingness of the patient/client to trust his expert or by an expectation that he do so. While there are acknowledged and unacknowledged constraints on the professional, many of them seem to increase, rather than to limit, his professional autonomy. For example, licensing statutes, designed to screen out incompetent practitioners, also safeguard the profession's monopolistic power.

The materials on the authority of the professionals in nonexperimental settings are of interest to us primarily for the light they shed on experimental interventions. The differences between these two settings often become blurred. This influences not only subjects, who unlike patients have usually yielded the role of initiator to the professional, but also investigators who typically conduct themselves according to standards and habits developed in clinical settings. Thus, in seeking answers to the following questions, consider whether the characteristics of investigators differ from those of other professionals so as to alter the authority which investigators should exercise:

1. What are the distinctive qualities of professional expertise?
2. In what ways do these qualities affect the authority of the expert?
3. To what extent do the values promoted by reliance on professional expertise conflict with other values in our society?
4. How and to what extent should the authority of professionals be extended or limited?
5. To what extent are either the individual's or society's interests well served by leaving the client's protection to the professional's judgment of what is in the client's "best interests"?
6. To what extent should nonprofessionals control the actions of professionals with respect to individual problems or issues of social policy?

## 1.

**The Professional in Society—Power and Competition**

a.

**Howard S. Becker  
The Nature of a Profession\***

\* \* \*

Professions, as commonly conceived, are occupations which possess a monopoly of some esoteric and difficult body of knowledge. Further, this knowledge is considered to be necessary for the continuing functioning of the society. What the members of the profession know and can do is tremendously important, but no one else knows or can do these things. The archetype in this respect is medicine, which is supposed to have an absolute monopoly of the knowledge necessary to heal the sick. Healing the sick and maintaining the health of the society is seen as one of the important functions which must be performed if the society is to maintain its equilibrium.

The body of knowledge over which the profession holds a monopoly consists not of technical skills and the fruits of practical experience but, rather, of abstract principles arrived at by scientific research and logical analysis. This knowledge cannot be applied routinely but must be applied wisely and judiciously to each case. This has several consequences.

In the first place, it is supposed that only the most able people will have the mental ability and the proper temperament to absorb and use such knowledge. Therefore, recruitment must be strictly controlled, to ensure that those who are not qualified do not become members of the profession. Recruitment is controlled, first, by careful weeding out of prospective candidates, and, then, by a lengthy and difficult educational process which eliminates those who were mistakenly selected. Lengthy training is considered necessary anyway, because the body of knowledge is supposed to be so complex that it cannot be acquired in any shorter time.

Secondly, it is felt that entrance into professional practice must be strictly controlled, and that this control must ultimately lie in the hands of members of the profession itself. Difficult ob-

stacles, in the form of examinations of all kinds, must be surmounted by candidates for practice, and no one must be allowed to practice who has not so demonstrated his competence. This means that the police power of the state must be utilized, through the device of licensure procedures, to control entrance into practice. But if the knowledge monopolized by the profession is so difficult to acquire, it follows that no layman can fully acquire it and, therefore, that the governmental bodies which grant licenses must be controlled by members of the profession itself. Similarly, the approval and accreditation of educational institutions and procedures must also be done by members of the profession. In short, the professional, by virtue of the esoteric character of his professional knowledge, is free of lay control.

Finally, since recruitment, training, and entrance into practice are all carefully controlled, any member of the professional group can be thought of as fully competent to supply the professional service.

Any profession which so monopolizes some socially important body of knowledge is likely to be considered potentially dangerous. It might use its monopoly to enrich itself or enlarge its power rather than in the best interests of its clients. The symbol of the profession, however, portrays a group whose members have altruistic motivations and whose professional activities are governed by a code of ethics which heavily emphasizes devotion to service and the good of the client and condemns misuse of professional skills for selfish purposes. This code of ethics, furthermore, is sternly enforced by appropriate disciplinary bodies. Professional associations have as their major purpose the enforcement of such ethical codes.

The client, therefore, is supposed to be able to count on the professional whose services he retains to have his best interests at heart. He rests comfortable in the knowledge that this is one relationship in which the rule of the market place does not apply. He need not beware but can give his full trust and confidence to the professional who is handling his problems; the service given him will be competent and unselfish. This is conceived as necessary if the professional is to perform his work successfully. If the patient cannot trust the physician completely, he will withhold facts that might be vital in successful treatment; the lawyer cannot protect his client's interests without full knowledge of his client's affairs, and this might be withheld if the client could not trust him.

\* Nelson B. Henry, ed.: *The Sixty-First Yearbook of the National Society for the Study of Education*. Chicago, Ill.: The National Society for the Study of Education 27-46 (1962). Reprinted by permission.

If the client is to trust the professional completely he must feel that there are no other interests which will be put before his in the performance of the professional activity. Among the other interests which might intrude are the interests related to institutions within which the professional makes his career. Thus, the ideal professional is a *private* practitioner, in business for himself, so to speak. He has no ties to a superior officer or bureaucratic system of rules; he receives his income directly from fees paid by the client, not from any third party.

A final element in the symbol of the profession is not so often mentioned in the attempts to define the term. This is the image of the profession and the professional as occupying an esteemed position in the society. Members of the professions are usually thought of, as they often in fact are, as people of sizable income and high community prestige. They are considered to be entitled to an important voice in community affairs. Professional associations are thought of as important public institutions, similarly entitled to a voice in public affairs, particularly (although not exclusively) with respect to those issues which touch on their professional concerns or competence.

The above features are the essential components of the symbol "profession." To risk repetition, this symbol does not describe any actual occupation. Rather, it is a symbol that people in our society use in thinking about occupations, a standard to which they compare occupations in deciding their moral worth. It represents consensus in the society about what certain kinds of work groups *ought* to be like, though it is not an accurate picture of any reality.

What role does this symbol play in the operation of our society and in the functioning of work groups? In the first place, the symbol can be seen as containing an ideology which provides a justification and rationale for one very important aspect of the work situation of those groups possessing the title. Professionals, in contrast to members of other occupations, claim and are often accorded complete autonomy in their work. Since they are presumed to be the only judges of how good their work is, no layman or other outsider can make any judgment of what they do. If their activities are unsuccessful, only another professional can say whether this was due to incompetence or to the inevitable workings of nature or society by which even the most competent practitioner would have been stymied. This image of the professional justifies his de-

mand for complete autonomy and his demand that the client give up his own judgment and responsibility, leaving everything in the hands of the professional.

This analysis may lead some people to conclude that I am saying that the symbol of the profession is used simply as a device by which the self-interest of the work group can be furthered. This would be incorrect. Professional autonomy may be used strictly in the interests of the client; in fact, it is likely that without some measure of autonomy the client's interests cannot be well served. If a doctor is not free to make the diagnosis he thinks correct and prescribe the course of treatment he thinks most efficacious rather than the one the patient finds most palatable, the patient's health may indeed suffer. In short, the symbol of the profession is not merely selfish propaganda; many of the propositions contained in it are in large part true. Nevertheless, we must not forget that it is a symbol, rather than an exact description of reality, and that it may be used for political purposes.

\* \* \*

If deviations from the symbol of the profession were simply the result of natural human orneriness—of Original Sin, so to speak—the sociologist would indeed be nothing but a muckraker in drawing attention to them. But the fact is that deviations from the ideal are neither random nor idiosyncratic. They do not occur because a few professionals are bad men or weak men. They occur systematically and are created by the operation of social forces. In other words, they are integral parts of the social structure of occupational life and are regarded as deviations because they are considered morally unworthy. (Not all the deviations I will speak of are so heavily morally toned, although some are.) I confine myself to a consideration of medicine and law, because these occupations most closely approach the symbol. Also, medicine and law have been much studied by sociologists, so that we have some accurate descriptions with which to compare the symbol.

Medicine and law fail to match the symbol in that neither actually holds a monopoly over its esoteric knowledge or functions. Lawyers do a great many things, but hardly any of them are not done on occasion, or, often, as full-time work, by people who are not lawyers. In fact, many lawyers suffer greatly because of the competition of nonlawyers in many fields of legal work. Lawyers do tax work and draw up wills;



but accountants also do tax work, and officers of banks are quite willing and able to draw up wills. The lawyer does maintain a monopoly over one area: appearing in court to defend clients; but this represents a small part of the lawyer's work. Similarly, doctors perform the function of healing the sick. But they share this function with members of many other occupations: osteopaths, chiropractors, chiroprudists, faith healers, and so on. Nor is their knowledge restricted to the circle of those who are fully trained and licensed physicians, for much of it is created by and known by nonphysicians who are scientists.

The reality differs from the symbol in another respect. All members of a profession are not equally competent to supply the *core service*—"the most characteristic professional act." This is true because of the great internal differentiation and specialization which characterize present-day professions. It is not only that there is a technical division of labor among the various specialties, although this is certainly so much the case that the services provided can only be provided by a member of a particular specialty. But beyond this we find that the specialties differ so in ideology, sense of mission, work activities, and work situation that they can most profitably be thought of as distinct occupations rather than as specialized aspects of one occupation. . . .

The relations of clients and professionals, in fact, are quite different from those specified in the symbol. Ideally, the client puts his full faith and trust in the professional whose services he uses. But this is not the way clients behave. They continually make judgments about the work and capabilities of the professionals they use. Medical patients often change doctors, and they do this because they have decided for themselves, on the basis of their own knowledge and experience or, frequently, on the advice of friends, relatives, and neighbors, that another doctor will do a better job for them. Research has shown that patients distinguish between diseases which are ordinary and everyday and therefore can be treated by any doctor (so long as he is convenient and inexpensive) and those diseases which are dramatically out of the ordinary and require the services of a doctor who can convince them, in one way or another, that he is specially good. The clients of professionals, in short, characteristically reserve a right of judgment denied to them by the symbol.

Similarly, the symbol of a work group bound by a code of ethics designed to protect

the client is in some ways not a realistic description. Every profession contains unethical practitioners. This would be of small importance, as I have said, if it were simply a matter of human frailty, of weak men succumbing to temptation. But it appears that this is a chronic feature of the social structure of prestigious occupations, another aspect of their differentiation and specialization (although in this case the differentiation takes place with respect to ethicality rather than technique). Hughes points out that lawyers deal with human quarrels:

A lawyer may be asked whether he and his client come into court with clean hands; when he answers, "yes," it may mean that someone else's hands are of necessity a bit grubby. For not only are some quarrels more respectable, more clean, than others; but also some kinds of work involved in the whole system (gathering evidence, getting clients, bringing people to court, enforcing judgments, making the compromises that keep cases out of court) are more respected and removed from temptation and suspicion than others. In fact, the division of labor among lawyers is as much one of respectability (hence of self concept and role) as of specialized knowledge and skills. One might even call it a moral division of labor, if one keeps in mind that the term means not simply that some lawyers are more moral than others; but that the very demand for highly scrupulous and respectable lawyers depends in various ways upon the availability of less scrupulous people to attend to the less respectable legal problems of even the best people.\*

Very little is known about the social systems in which unethical practice is embedded; this is one of the most neglected areas in the study of professions, partly because of the practical difficulties involved and partly, I fear, because to study such problems calls attention to the disparity between symbol and reality.

Finally, professional practitioners are typically not as autonomous as the symbol would have us believe. The constraints which belie the symbol have several different sources. In the traditional pattern of private practice for a fee, professionals may be constrained by the wishes of their clients. In so far as a professional depends on his reputation among laymen for his practice, there is a continuing pressure for him to give the kind of service that, in the layman's eyes, is satisfactory. Freidson has noted that the general medical practitioner, of all physicians, is most in this position and is most likely to defer to his patients' wishes with respect to methods

\* E. C. Hughes, *Men and Their Work*. Glencoe, Ill.: The Free Press 71 (1958).



of treatment. Depending essentially on other laymen for his referrals (standing, as Freidson says, at the apex of the "lay referral structure" which also includes friends, neighbors, relatives, and the corner druggist), the general practitioner tends to be sensitive to the demands of clients for new drugs, for instance, and to avoid procedures which may seem unnecessary and unpleasant to his patients.

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## NOTES

### NOTE 1.

EVERETT C. HUGHES  
MEN AND THEIR WORK\*

\* \* \*

An occupation consists, in part, of a successful claim of some people to *licence* to carry out certain activities which others may not, and to do so in exchange for money, goods or services. Those who have such licence will, if they have any sense of self-consciousness and solidarity, also claim a *mandate* to define what is proper conduct of others toward the matters concerned with their work. The licence may be nothing more than permission to carry on certain narrowly technical activities, such as installing electrical equipment, which it is thought dangerous to allow laymen to do. It may, however, include the right to live one's life in a style somewhat different from that of most people. The mandate may go no further than successful insistence that other people stand back and give the workers a bit of elbow room while they do their work. It may, as in the case of the modern physician, include a successful claim to supervise and determine the conditions of work of many kinds of people; in this case, nurses, technicians and the many others involved in maintaining the modern medical establishment. In the extreme case it may, as in the priesthood in strongly Catholic countries, include the right to control the thoughts and beliefs of whole populations with respect to nearly all the major concerns of life.

Licence, as an attribute of an occupation, is ordinarily thought of as legal permission to carry on a kind of work. There is a great body of jurisprudence having to do with the matter of licence, both in principle and as it occurs in various occupations. I have in mind something both

broader and deeper, something that is sometimes implicit and of undefined boundaries. For it is very difficult to define the boundaries of the licence to carry on a certain kind of activity. What I am talking of is a basic attribute of society. Occupations here offer us an extreme and highly lighted instance of a general aspect of all human societies. For society, by its very nature, consists of both allowing and expecting some people to do things which other people are not allowed or expected to do. All occupations—most of all those considered professions and perhaps those of the underworld—include as part of their very being a licence to deviate in some measure from common modes of behavior. Professions also, perhaps more than other kinds of occupations, claim a legal, moral and intellectual mandate. Not merely do the practitioners, by virtue of gaining admission to the charmed circle of colleagues, individually exercise the licence to do things others do not do, but collectively they presume to tell society what is good and right for the individual and for society at large in some aspect of life. Indeed, they set the very terms in which people may think about this aspect of life. The medical profession, for instance, is not content merely to define the terms of medical practice. It also tries to define for all of us the very nature of health and disease. When the presumption of a group to a broad mandate of this kind is explicitly or implicitly granted as legitimate, a profession has come into being.

The understanding of the nature and extent of both licence and mandate, of their relations to each other and of the circumstances in which they expand or contract is a crucial area of study not merely of occupations, but of society itself. In such licences and mandates we have the prime manifestation of the *moral division of labor*; that is, of the processes by which differing moral functions are distributed among the members of society, both as individuals and as kinds or categories of individuals. Moral functions differ from each other both in kind and in measure. Some people seek and get special responsibility for defining the values and for establishing and enforcing social sanctions over some aspect of life. The differentiation of moral and social functions involves both the setting of the boundaries of realms of social behavior and the allocation of responsibility and power over them. One may indeed speak of jurisdictional disputes concerning the rights and the responsibilities of various occupations and categories of people in defining

\* Glencoe, Ill.: The Free Press 78-80 (1958). Reprinted by permission of the author who retains all rights.

and maintaining the rules of conduct concerning various aspects of personal and social life.

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#### NOTE 2.

EVERETT C. HUGHES  
PROFESSIONS\*

Professions are more numerous than ever before. Professional people are a larger proportion of the labor force. The professional attitude, or mood, is likewise more widespread; professional status, more sought after. These are components of the professional trend, a phenomenon of all the highly industrial and urban societies; a trend that apparently accompanies industrialization and urbanization irrespective of political ideologies and systems. The professional trend is closely associated with the bureaucratic, although the queen of the professions, medicine, is the avowed enemy of bureaucracy, at least of bureaucracy in medicine when others than physicians have a hand in it.

A profession delivers esoteric services—advice or action or both—to individuals, organizations or government; to whole classes or groups of people or to the public at large. . . . [T]he action—it is assumed or claimed—is determined by esoteric knowledge systematically formulated and applied to problems of a client. The services include advice. The person for or upon whom the esoteric service is performed, or the one who is thought to have the right or duty to act for him, is advised that the professional's action is necessary. Indeed, the professional in some cases refuses to act unless the client—individual or corporate—agrees to follow the advice given.

The nature of the knowledge, substantive or theoretical, on which advice and action are based is not always clear; it is often a mixture of several kinds of practical and theoretical knowledge. But it is part of the professional complex, and of the professional claim, that the practice should rest upon some branch of knowledge to which the professionals are privy by virtue of long study and by initiation and apprenticeship under masters already members of the profession.

\* \* \*

Professionals *profess*. They profess to know

better than others the nature of certain matters, and to know better than their clients what ails them or their affairs. This is the essence of the professional idea and the professional claim. From it flow many consequences. The professionals claim the exclusive right to practice, as a vocation, the arts which they profess to know, and to give the kind of advice derived from their special line of knowledge. This is the basis of the license, both in the narrow sense of legal permission and in the broader sense that the public allows those in a profession a certain leeway in their practice and perhaps in their very way of living and thinking. The professional is expected to think objectively and inquiringly about matters which may be, for laymen, subject to orthodoxy and sentiment which limit intellectual exploration. . . .

Since the professional does profess, he asks that he be trusted. The client is not a true judge of the value of the service he receives; furthermore, the problems and affairs of men are such that the best of professional advice and action will not always solve them. A central feature, then, of all professions, is the motto—not used in this form, so far as I know—*credat emptor*. Thus is the professional relation distinguished from that of those markets in which the rule is *caveat emptor*, although the latter is far from a universal rule even in exchange of goods. The client is to trust the professional; he must tell him all secrets which bear upon the affairs in hand. He must trust his judgment and skill. In return, the professional asks protection from any unfortunate consequences of his professional actions; he and his fellows make it very difficult for anyone outside—even civil courts—to pass judgment upon one of their number. Only the professional can say when his colleague makes a mistake.

The mandate also flows from the claim to esoteric knowledge and high skill. Lawyers not only give advice to clients and plead their cases for them; they also develop a philosophy of law—of its nature and its functions, and of the proper way in which to administer justice. Physicians consider it their prerogative to define the nature of disease and of health, and to determine how medical services ought to be distributed and paid for. Social workers are not content to develop a technique of case work; they concern themselves with social legislation. Every profession considers itself the proper body to set the terms in which some aspect of society, life or nature is to be thought of, and to define the gen-

\* 92 *Daedalus* 655-657 (1963). Reprinted by permission of *Daedalus*, Journal of the American Academy of Arts and Sciences, Boston, Massachusetts.

eral lines, or even the details, of public policy concerning it. The mandate to do so is granted more fully to some professions than to others; in time of crises it may be questioned even with regard to the most respected and powerful professions.

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b.

**Mancur Olson**  
**The Logic of Collective Action\***

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Many of those who criticize organized labor because of the coercion entailed in labor unions are themselves members of professional organizations that depend upon compulsion as much as unions do. Many organizations representing prosperous and prestigious professions like the law and medicine have also reached for the forbidden fruits of compulsory membership. There is in fact a pervasive tendency towards compulsion in professional associations generally. "The trend," writes Frances Delancey, "is toward the professional guild." This is what many other scholars have also observed. "A characteristic of the politics of the professional association," according to V. O. Key, "is their tendency to seek the reality, if not invariably the form, of a guild system." J. A. C. Grant argues that the guild "has returned. Its purposes are the same as in the Middle Ages." The guild form of organization is often adopted not only by the ancient and learned professions, but also by undertakers, barbers, "beauticians," "cosmeticians," plumbers, opticians, and other groups interested in professional status. This adoption of the guild form of organization is evidence for the by-product theory of large pressure groups, for compulsory membership has always been, Grant points out, "the first rule" of the guild system.

The self-regulating guild with compulsory membership has reached its furthest degree of development in many state bar associations. Many state legislatures have been induced to require by law that every practicing lawyer must be a member of the state bar association. These bar associations have closed shops enforced by government, and thus should be the envy of every labor union.

The modern professional associations or guilds are moreover coming to resemble "minia-

ture governments." They have "all the types of power normally exercised by government." State governments often give the professional groups authority to govern themselves (and to a degree their clients) and to discipline any members of the profession that do not maintain the "ethical" standards the profession finds it expedient or appropriate to maintain. It follows that, even when membership in these associations is not a legal requirement, the individual in professional practice knows that he has an interest in maintaining membership in good standing with the professional association.

The advantages of maintaining membership and good relationships with a professional association may be illustrated by the fact that it was not found expedient to release the name of a doctor who had written to a congressional committee to argue that "the central organization of the AMA in Chicago has no idea what the average physician wants his patients to have." Oliver Garceau, author of the classic work on the American Medical Association, has argued that the recalcitrant doctor in trouble with organized medicine may face "a genuine economic threat." When the American Medical Association blocked the Denver city council's program for Denver General Hospital in 1945, a Denver councilman, according to *Time Magazine*, was driven to exclaim: "Nobody can touch the American Medical Association. . . . Talk about the closed shop of the AFL and the CIO—they are a bunch of pikers."

The role of coercion, even in its subtler forms, in the American Medical Association is, however, probably less important as a source of membership than the noncollective benefits the organization provides its membership. According to Garceau, there is "one formal service of the society with which the doctor can scarcely dispense. Malpractice defense has become a prime requisite to private practice." One doctor who had founded a cooperative hospital, and lost his membership in his medical society, discovered that not only had he lost his chance to have other doctors testify in his behalf during malpractice suits, but that he had lost his insurance as well. The many technical publications of the American Medical Association, and the state and local medical societies, also give the doctor a considerable incentive to affiliate with organized medicine. The American Medical Association publishes not only its celebrated *Journal*, but also many other technical periodicals on various medical specialties. Since the nineteenth century

\* Cambridge, Mass.: The Harvard University Press 137-140 (1965). Reprinted by permission.

the *Journal* alone has provided a "tangible attraction for doctors." The importance of this attraction is perhaps indicated by a survey conducted in Michigan, which showed that 89 per cent of the doctors received the *Journal of the American Medical Association*, and 70 per cent read a state society journal, but less than 30 per cent read any other type of medical literature. The *Journal* has been, moreover, the "prime money maker of the organization." Much of the organization's revenue, according to Garceau, comes from drug companies' advertisements—advertisements which Garceau believes helped companies obtain the AMA seal of approval for their products. The conventions of the American Medical Association and many of its constituent organizations also provide technical information needed by doctors, and thus give the member a "direct return in education" for the investment in dues, just as the medical journals do.

In short, by providing a helpful defense against malpractice suits, by publishing medical journals needed by its membership, and by making its conventions educational as well as political, the American Medical Association has offered its members and potential members a number of selective or noncollective benefits. It has offered its members benefits which, in contrast with the political achievements of the organization, can be withheld from nonmembers, and which accordingly provide an incentive for joining the organization.

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#### NOTE

WILLIAM J. GOODE  
THE PROTECTION OF THE INEPT\*

The dissident have throughout history voiced a suspicion that the highly placed have not earned their mace, orb, and scepter. . . . Against the grandiloquent assertion of kings that they were divinely appointed, both court jesters and the masses have sometimes laughed, and asked, where were their virtue and wisdom? . . .

. . . In our less heroic epoch, we are assured that we live in an achievement-oriented society, and the norm is to place individuals in their occupations by merit. . . .

. . . However the privileged (at all levels of privilege) do try systematically to prevent the talent of the less privileged from being rec-

ognized or developed. And though analysts of stratification assume that social mobility is an index of open competition, ample if unsystematic evidence suggests that both the able and the inept may move into high position.

These comments . . . describe arrangements which every social system exhibits, and which cope with a universal system problem: How to utilize the services of the less able?

The social responses to this problem are the resultant of two sets of factors in tension: protection of the inept; and protection of the group from the inept. In almost all collectivities . . . the arrangements for protecting the less able seem to be more pervasive, common, and effective than those for protecting the group from ineptitude. . . .

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Let us . . . consider briefly some of the wide array of evidence that groups do not typically expose or expel their members for lesser achievement or talent. . . .

Almost every inquiry into the productivity of workers has shown that the informal work group protects its members by setting a standard which everyone can meet, and they develop techniques for preventing a supervisor from measuring accurately the output of each man. Higher level management has for the most part evaded such scrutiny, but industrial sociologists have reported comparable behavior there, too. The protection of one another by lower-level workers might be due to less commitment; the fact that higher-level men do the same suggests the need for a more general explanation.

All professions, while claiming to be the sole competent judges of their members' skills, and the guardians of their clients' welfare, refuse to divulge information about how competent any of them are, and under most circumstances their rules assert it is unethical to criticize the work of fellow members to laymen. Wall Street law firms try to find good positions in other firms for those employees they decide are not partnership material. When a new profession is organized, grandfather clauses permit older practitioners with less training to continue in practice without being tested. When hospitals begin to demand a higher performance standard from those who enjoy staff privileges, inevitably rejecting some, both patients and physicians object. One study of a group of physicians showed that there was little relationship between an M.D.'s income and the quality of medical care he gave to his patients.

\* 32 *American Sociological Review* 5-8 (1967). Reprinted by permission.

Wherever unions are strong, foremen know that promotion by merit rather than by seniority is unwise, and in any event unusual. Many corporations do not fire their managers; they find or create other posts for them. Employees are close students of promotion behavior, and are "notoriously suspicious and cynical" about management claims that promotions are through merit. Many are not convinced the best men are at the top. More generally, members of what Goffman calls "teams" (army officers, parents, policemen, managers, nurses, and so forth) protect each other from any exposure of their errors.

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Few are fired for incompetence, especially if they last long enough to become members of their work group. One consequence is that, in craft or white collar jobs, higher standards are set for obtaining a job than for performance. The result is that a high level of formal education is often necessary for jobs that any average eighth-grader could learn to perform rather quickly. Once the person enters his work group, however, the social arrangements do not permit much overt discrimination between the less able and the rest. . . .

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c.

### **Murray T. Bloom The Trouble with Lawyers\***

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It started with Ford Hoffman, a Phoenix real estate broker—a nonfirebrand and nonrevolutionary. In fact Hoffman still isn't mad at lawyers. He is still a bit incredulous that his little \$4,000 routine real estate deal started the great battle.

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The revolution began early in July, 1952, when Hoffman as a licensed broker drew up an agreement of sale, warranty deed, quitclaim deed and bill of sale on a property. They sound awfully impressive, but in fact they are just printed forms and filling them in took Hoffman less than fifteen minutes. He made no charge for the work, but he did get a broker's commission on the sale. It didn't even occur to Hoffman that he

was guilty of one of the dirtiest phrases in the legal lexicon—"unauthorized practice of law." (UPL spewed out as initials at bar association meetings makes a sneering epithet.)

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Among lawyers themselves there is considerable disagreement about . . . UPL committees. Former Supreme Court Justice Wiley Rutledge, a conservative, said in 1941:

I do not like what I fear is becoming the bar's trade-union approach to the problem. . . . I doubt whether . . . it is necessary or desirable for bar associations to become closed shops, not only as to the business lawyers are now performing but as to a great deal of business which has always been done by other men, although lawyers may have been doing it for a long time.

The more orthodox view was expressed in 1951 by Edwin M. Otterbourg, a New York lawyer who was one of the pillars of the UPL committee:

Actually, unauthorized practice of law is a swindle upon the public. Whenever it takes place, some person receives either incompetent or unqualified advice, or advice which cannot be honestly disinterested. . . . Reliance upon such advice may result in irreparable injury and loss. . . .

For several months I tried to find serious instances of such injury and loss suffered by the public. I couldn't. I finally stopped trying when Professor Quintin Johnstone, of the Yale University Law School, wrote in a 1967 book: "Nor are there many instances of persons being harmed from having laymen do their legal work." Further, he went on, there is a "public benefit from much of the lay competition that now exists. Lay legal services often are performed at lower cost than if the work were done by lawyers."

In fact Ford Hoffman was simply filling out forms without extra charge—which every real estate man in the state had been doing long before Arizona became a state.

It started friendly. Hoffman was told that it was going to be a test case when the State Bar of Arizona on October 30, 1953, filed its suit against him claiming that he had practiced law without a license. At the same time a similar action was brought against five title insurance companies, since they, too, often filled out these forms in cases where real estate purchasers bought title insurance.

Superior Court Judge Henry S. Stevens ruled in favor of Hoffman and the title companies. He said real estate brokers and title com-

\* New York: Simon and Schuster 110-124 (1968). Copyright © 1968, by Murray Teigh Bloom. Reprinted by permission of Simon and Schuster.

panies could continue to prepare certain legal documents and perform some other services incidental to their businesses without being guilty of unauthorized practice of law.

The State Bar now carried the case to the State Supreme Court, but Hoffman, who felt he had already spent too much in defending himself, dropped out of the case. On November 1, 1961, the Arizona Supreme Court unanimously overturned the lower court ruling. Neither real estate brokers nor title companies could prepare any of the documents, including even the simple printed preliminary purchase agreements.

The fact that the brokers didn't get paid for the work of filling out these printed forms didn't matter, said the court. That they had been doing it unhindered for many years also didn't count. But, the court admitted, "the record does not disclose any testimony regarding specific injury to the public from [these] practices."

The court was also out to close all loopholes, such as the possibility of the brokers and title companies getting legislation giving them the right to do what they had been doing all along: "... although the legislature may impose additional restrictions which affect the licensing of attorneys, it cannot infringe on the ultimate power of the court to determine who may practice law."

Now what had started out as "a friendly little test case" lost all cozy amicability. The Arizona Bar, which had been supported by the American Bar Association, rejoiced at a great victory. The Arizona Association of Realtors, of which Ford Hoffman was not a member, got into the battle. This was *serious*.

Why should real estate men think their very livelihood was affected by the Supreme Court ruling?

Robert E. Riggs, now a professor of political science at the University of Minnesota, but then a young lawyer in Arizona, analyzed the realtors' problem cogently:

Although loss of the right to prepare other documents might be an inconvenience to the real estate agent, the preliminary sales agreement was vital. By it the agent obtained the signatures of both parties, binding them to the sale. Sales could be lost if a prospective buyer or seller changed his mind while waiting for a lawyer to draft the sales contract. Cost was also a consideration, since the agent might have to pay the lawyer for filling the blanks on many sales agreement forms for sales that were never consummated.

Now, too, the newspapers of Arizona began to see some of the sweeping implications of the

Supreme Court decision. The Tucson *Daily Citizen*, for example, saw it as

likely to hike the cost of the average real estate deal. What is more, the ruling will put more money in the pockets of the lawyers . . . the customers are now forced to retain attorneys to represent them. . . .

But for the Arizona *Daily Star* there was an even more important issue involved: The Supreme Court, in denying the Legislature the right to determine what constitutes the practice of law, had

arrogated to itself and to members of the State Bar of Arizona a legalized special privilege that is denied all other professions. . . . Thus we see the court composed of lawyers settling it in favor of lawyers. . . . It would be just as sensible and justified if the medical doctors would demand that such specialized services as blood examinations, and microbiology, be done by licensed M.D.'s. It is now done by technicians. Nurses would be denied the right to take cardiograms, and so on. . . . It is adverse to the public interest in the additional cost it imposes on the average citizen.

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In Arizona, as in twelve other states, the people can institute a constitutional amendment. In order to place the amendment on the regular ballot, at least 15 percent of the registered voters have to sign the initiative petition. Some 60,000 signatures would be required for such an amendment to be voted on in the November, 1962, general election. And the petitions with the names would have to be filed not later than July 6. They had four months.

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On July 6, 1962, the real estate men filed some 107,420 signatures on their petition with the Secretary of State of Arizona. This was nearly twice as much as the needed 60,000 and represented 28 percent of the electorate. It was also the largest number of signatures ever filed on an initiative measure in the state's history. The Secretary of State assigned it a number, and now the conflict was to become the Battle of Proposition 103.

That day the *Arizona News* of Phoenix declared editorially for the real estate men.

The cost of an attorney's fee has been added to the rather heavy closing costs when a home or other piece of property changes hands. . . .

The real estate men and title companies were expected to be content with this ruling—just pass it on to the customer as manufacturers do when the

cost of a product goes up with wage or other production increases.

In taking their case to the people, the realtors are warning the public that the legal fraternity is known to be eyeing other professions from which they could collect a legal fee in every individual transaction. . . .

. . . this appears to be a case in which, unless the people speak, their rights will be submerged by the selfish demands of a profession which has demonstrated throughout the contention on this issue, that it has regard for no right but its own.

Now for the first time the Arizona Bar became worried. Promises of help were sought at the American Bar Association convention in August. There the National Conference of Bar Presidents urged the ABA to lend all possible assistance to the State Bar of Arizona in its fight against the proposed amendment. In Arizona two public relations firms and an advertising agency were taken on for the campaign.

The campaign slogans emerged quickly. The realtors chose "Protect Your Pocketbook" and "Protect Your Right to Choose," and the bar's was "Save Our Constitution." . . . The realtors got an unexpected assist from a local law journal, the *Arizona Weekly Gazette*, which carries local court announcements and news of pleadings. The *Gazette* had a long news item from Salt Lake City where an unauthorized practice of law group had cited the many professions and occupations that were currently infringing on law practice. Included were architects, "who quite generally draw construction contracts and notices of completion"; banks and trust companies, "when they go too far in estate planning"; claims adjusters, when they intervene between attorney and client; CPAs, "when they give tax advice without limitation"; life insurance brokers and salesmen, "when they give estate tax and estate planning advice"; and notaries public, who "prepare legal documents"; and, of course, real estate brokers.

The news story, widely reprinted in realtor ads, surely helped convince some of these groups that the lawyers would turn on them, too, when they had finished with the realtors.

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By November 2, Election Day, there was little doubt that the realtors would win. The only real question was by how much. Even the realtors were surprised by their majority; some 236,856 voters supported them, and only 64,507 were against the amendment, a majority of nearly 4 to 1.

The postmortems for the lawyers were so-

bering. They knew their profession had acquired a more tarnished image. In a letter to the *Journal of the American Bar Association* in May, 1963, an Oklahoma attorney spoke for many when he wrote: "In the Arizona case it was apparent that the bar was more concerned about its loss of business than it was about public welfare. . . ."

An Arizona lawyer, Robert E. Riggs, a Mormon who had practiced in his father's law office in Tempe for a year before deciding that he would be happier teaching political science, did an analysis of the battle for the *Arizona Law Review*. "But," he told me recently, at the University of Minnesota where he is teaching, "they wouldn't print it. They felt I was too hard on the lawyers, too critical of the bar." The study, which appeared in the *Southern California Law Review*, actually was quite mild in its conclusions:

This may be a time for reappraisal, as the *American Bar Association Journal* has suggested. If such it be, this essay is a plea that the reappraisal look beyond tactics and methods to the basic premises and concepts defining the role of the legal profession in society.

In looking back on the case, Dr. Riggs still feels that "the bar is very privileged and very protective of itself." He still finds it incredible that the Arizona Supreme Court had ruled as it did in outlawing the preparation of any contracts by realtors. "How can judges be impartial vis-à-vis lawyers in a matter such as this?"

\* \* \*

In Illinois the brokers and the lawyers were embroiled in a ten-year court battle that was finally resolved in October, 1966. Here the bar won clearly: only lawyers could handle the completion of all real estate transactions, regardless of size. Both the buyers and sellers had to have attorneys, and the lawyers, of course, had to be paid.

For a time the Illinois brokers thought of getting a constitutional amendment along the Arizona lines, but in Illinois a two-thirds approval of each house of the legislature is required before the proposed amendment can go on the ballot. Presumably when the brokers took a head-count of the many lawyers in the legislature, they decided the idea wasn't too practical.

The Chicago *Daily News* interpreted the lawyers' victory this way on October 27, 1966:

Don't be surprised if the final cost of buying that new house you are buying is \$200 more than you figured. . . . Sources within the legal profession told



the *Daily News* the agreement would primarily affect the average home buyer, who previously did not hire an attorney. He could now pay as much as \$325 extra on the purchase of a \$30,000 home. . . .

\* \* \*

There are about a million new homes built in the United States each year and an estimated two to two and a half million existing homes sold—or a total of about three and a half million home sales. The increase in legal fees, if the Illinois verdict spreads, could be hefty. At a modest set of legal fees totaling only \$200 per house, this would come to \$700,000,000 a year added to the cost of living in a house of your own.

Fortunately, several State Supreme Courts have not seen fit to go along with the zealous unauthorized practice committees. In Michigan, Missouri, Wisconsin and Minnesota, real estate brokers are still permitted to fill in forms without the help of lawyers. These states, in effect, are following the reasoning set forth by the Minnesota Supreme Court in 1940:

It is the duty of this court so to regulate the practice of law and restrain such practice by laymen in a common sense way in order to protect primarily the interest of the public and not to hamper and burden such interest with impractical technical restraints. . . .

The rare instances of defective conveyances in such transactions are insufficient to outweigh the great public inconvenience which would follow if it were necessary to call in a lawyer to draft these simple documents.

\* \* \*

d.

**In the Matter of Community Action for  
Legal Services, Inc.  
26 App. Div.2d 354, 274 N.Y. S.2d 779  
(Sup. Ct. 1966)**

\* \* \*

BREITEL, J. Three applications on behalf of proposed corporations wishing to practice law under the provisions of section 280 of the Penal Law are pending before the court. The proposed corporations would be Community Action for Legal Services, Inc., New York Legal Assistance Corporation, and Harlem Assertion of Rights, Inc. . . . The plans are to establish neighborhood law offices and provide representation for disadvantaged members of the community, disadvantaged because of poverty and just as often because of disfavored minority status. The financial support for the programs is expected to come largely, if not exclusively, from Federal

funds under the auspices and control of the Federal Office of Economic Opportunity.

Regrettably, the present applications may not be approved by the court. In rejecting them, however, the court invites the resubmission of proposals free from the infirmities in the present applications, with the suggestion that any new applications be submitted promptly, in final form, and in succinct integrated documents.

\* \* \*

Section 280 of the Penal Law is the governing statute making it a crime for corporations to practice law. It contains, however, an exception for charitable or other corporations which have first obtained approval by the applicable Appellate Division. It is immediately evident that the allowable practice of law by corporations is highly exceptional, permissible only in carefully circumscribed conditions consonant with the policy of limiting the practice of law to licensed professionals. This is a general principle to be observed.

Basic to the principle is that the restriction of legal practice to lawyers and the maintenance of professional standards are for the benefit of the public and not for the economic preservation or professional enhancement of the Bar. The professional standards and Canons of Professional Ethics are justifiable only as protective of the public. Inherent to the legal professional system is the direct and often summary control and discipline of lawyers by the courts. . . . No similarly direct, and rarely any summary, control over laymen exists in the courts. The direct and summary control over the members of the profession by any agency, but especially by the courts, is a unique difference distinguishing the legal profession from other professions.

The most embracing of the applications is that on behalf of Community Action for Legal Services, Inc. (CALS). CALS itself would not practice law but it would finance and control subcontractors or delegate agencies which would. . . . CALS would have a board of directors of 32 members, at least 12 of whom are to be chosen from or recommended by the New York City Council Against Poverty from "nominations made by community committees in designated poverty areas in the City." It might have as many as 35 directors who shall "in the main" be lawyers. It would have power to establish "guidelines" for the operation of legal services programs, that is, neighborhood law offices, and render advice and assistance to delegate agen-



cies. . . . CALS would have an advisory committee on legal services with 18 members (only 6 of whom will definitely be lawyers) who would be chosen by law school deans and delegate agencies not represented on the CALS board.

\* \* \*

[The court's] official concern is with the feasibility of maintaining minimal standards. The interposition of supervising licensed corporations and the unlimited power of contracting out of legal services to delegate, subdelegate, and sub-subdelegate agencies is a thicket through which none could penetrate, even if the non-lawyer controls were eliminated. Hence, recognizing the novelty and difficulty of the programs contemplated and the anticipated use of laymen by the lawyers to a degree never before countenanced in the profession, the court should not licence more than one legal assistance corporation, using Federal economic antipoverty funds, in any one area, as large as a county or at least half a county. One set of neighborhood law offices operated by one entity in such an area is both enough and all that could be responsibly supervised by any one directorate and by this court insofar as gross breaches of ethics may ever become involved. The present proposals are just unworkable because of the clumsy overlapping, excessive layers of organizations, and the built-in incentives to competitive antipoverty law offices operating in the same area.

On the basis of these remarks, the pending applications are deficient. No matter how many interpositions of corporations and boards are provided, with respect to each proposed corporation the lawyer operations would be subject ultimately to lay control. This is not permissible if the public is to be protected from abuses and if this court is to carry out its responsibility to enforce minimum standards on those over whom it has direct control. Nor does the statute permit it.

In this connection the court would have no concern, as it could not, with any council or advisory group, consisting exclusively or largely of laymen, in any legal assistance corporation, reviewing and addressing itself to broad questions of policy. But the court is concerned that such a corporation be directly controlled and supervised by lawyers summarily responsible to the court for the maintenance of professional standards. . . .

. . . Recognizing the need for diverse representation and the requirements of the Federal

agency for community involvement, it is suggested that those needs be met in a council or other policy group as mentioned above and not in the board of directors or managers of law offices. This should involve no serious difficulty. The management of law offices is not something in which even involved members of the community may be deemed to be competent. No one would assume that patients are competent to tell physicians how to practice their profession. On the other hand, the competence of lay members of the community to speak for themselves on broad questions of policy affecting the community cannot be gainsaid.

At this point, it may be helpful to note that those who sponsor a legal assistance corporation need not be lawyers. But once a licensed corporation is created, it could and should be cut loose from the sponsors, if they are not lawyers or cannot meet the standards required for operation of a legal assistance corporation. This separation should apply to hiring and fiscal functions, for the power to hire and that of the purse are overwhelming. . . . In making these comments the court is not officially concerned with the efficiency, economy, or esthetics of any planned program, none of which is its proper concern. Rather it is concerned with insuring that the public will receive the best available legal services in the same way as those who retain their own private lawyers, with effective recourse to the court for gross professional failure.

\* \* \*

The court, in conclusion, affirms its recognition of the importance of the programs involved. It accepts, indeed is hospitable to the view, that new institutions must be fashioned to function alongside traditional legal aid societies. It believes that such a development will involve change and enlargement in traditional concepts of professional standards, ethics, and office organization. Nevertheless, it still must require that protection of the public be the paramount and final determinant of the form and content of any institutional development of these new programs over which it has responsibilities. Certainly, factional, political, and narrow group or professional interests must be either subordinated or ignored entirely in the fashioning of these new institutions, if the service of such interests would impede the protection of the public.

\* \* \*

## 2.

**The Professional in Practice—  
Traditional Concepts of Power and Authority**

a.

**Alexander Solzhenitsyn  
Cancer Ward\***

First, Ludmila Afanasyevna took Kostoglotov into the treatment room. A female patient had just emerged after her session. The huge 180,000-volt X-ray tube, hanging by wires from the ceiling, had been in operation almost nonstop since 8 A.M. There was no ventilation and the air was full of that sweetish, slightly repellent X-ray warmth.

\* \* \*

She was in a hurry, not only because she wanted to get out quickly, but also because the X-ray program could not be delayed even for a few minutes. She motioned Kostoglotov to lie on the hard couch under the X-ray tube and to uncover his stomach. Then she went over his skin with some sort of cool, tickly brush. She outlined something and seemed to be painting figures on it.

After this she told the nurse about the "quadrant scheme" and how she was to apply the tube to each quadrant. She then ordered the patient to turn over onto his stomach and she brushed some more lines on his back. "Come and see me after the session," she said.

\* \* \*

He went to see Dontsova. She was sitting in the short-focus apparatus room. Through her square glasses, rounded at the four corners, she was examining some large X-ray films against the light. Both machines were switched off, both windows were open and there was no one else in the room.

"Sit down," said Dontsova drily.

He sat down. She went on comparing the X-rays.

Although Kostoglotov argued with her, he did it only as a defense against the excesses of medicine, as laid out in a mass of instructions. As for Ludmila Afanasyevna herself, she inspired only confidence, not just by her masculine decisiveness, but by the precise orders she gave as

she watched the screen in the darkness, by her age and her indisputable dedication to work and work alone, but also, above all, by the confident way in which, right from the very first day, she had felt for the outline of his tumor and traced its circumference so precisely. The tumor itself proclaimed the accuracy of her touch, for it had felt something too. Only a patient can judge whether the doctor understands a tumor correctly with his fingers. Dontsova had felt out his tumor so well that she didn't need an X-ray photograph.

She laid aside the X-ray photographs, took off her glasses and said, "Kostoglotov, there is too big a gap in your case history. We must be absolutely certain of the nature of your primary tumor."

When Dontsova started talking like a doctor, she always spoke much more quickly. In one breath she would leap through long sentences and difficult terms. "What you tell us of your operation the year before last and the position of the present secondaries, is in agreement with our diagnosis. However, there are other possibilities which can't be excluded, and this complicates your treatment for us. You'll understand it's impossible now to take a sample of your secondary."

"Thank God! I wouldn't have let you take one."

"I still don't understand why we can't get hold of the slides with the sections of your primary. Are you absolutely sure there was a histological analysis?"

"Yes, I'm sure."

"In that case why were you not told the result?"

She rattled on in the rapid style of a busy person. Some of her words slipped by and had to be guessed at.

Kostoglotov, however, had got out of the habit of hurrying. "The result? There were such stormy goings-on where we were, Ludmila Afanasyevna, such an extraordinary situation that I give you my word of honor. . . . I'd have been ashamed to ask about a little thing like my biopsy. Heads were rolling. And I didn't even understand what a biopsy was for." Kostoglotov liked to use medical terms when he was talking to doctors.

"Of course you didn't understand. But those doctors must have understood. These things can't be played about with."

\* \* \*

"[B]ut I want to know when I can go back [home]". . . Kostoglotov looked at her somberly.

\* New York: Farrar, Straus & Giroux, Inc., Bantam Books 65-80, 85-88 (1969). Reprinted by permission of Farrar, Straus & Giroux, Inc. © Alexander Solzhenitsyn, English translation © The Bodley Head, Ltd., 1968.

"You will go home," Dontsova weighed her words one by one with great emphasis, "when I consider it necessary to interrupt your treatment. And then you will only go temporarily."

Kostoglotov had been waiting for this moment in the conversation. He couldn't let it go by without a fight.

"Ludmila Afanasyevna! Can't we get away from this tone of voice? You sound like a grown-up talking to a child. Why not talk as an adult to an adult? Seriously, when you were on your rounds this morning I . . ."

"Yes, on my rounds this morning"—Dontsova's big face looked quite threatening—"you made a disgraceful scene. What are you trying to do? Upset the patients? What are you putting into their heads?"

"What was I trying to do?" He spoke without heat but emphatically, as Dontsova had. He sat up, his back firm against the back of the chair. "I simply wanted to remind you of my right to dispose of my own life. A man can dispose of his own life, can't he? You agree I have that right?"

Dontsova looked down at his colorless, winding scar and was silent. Kostoglotov developed his point:

"You see, you start from a completely false position. No sooner does a patient come to you than you begin to do all his thinking for him. After that, the thinking's done by your standing orders, your five-minute conferences, your program, your plan and the honor of your medical department. And once again I become a grain of sand, just as I was in the camp. Once again nothing *depends* on me."

"The clinic obtains written consent from every patient before every operation," Dontsova reminded him.

(Why had she mentioned an operation? He'd never let himself be operated on, not for anything!)

"Thank you! Thank you for that anyway! Even though it's only for its own protection, the clinic at least does that. Unless there's an operation you simply don't ask the patient anything. And you never explain anything! But surely X-rays have some effect too?"

"Where did you get all these rumors about X-rays?" Dontsova made a guess. "Was it from Rabinovich?"

"I don't know any Rabinovich!" Kostoglotov shook his head firmly. "I'm talking about the principle of the thing."

(It was in fact from Rabinovich that he'd

heard these gloomy stories about the aftereffects of X-rays, but he'd promised not to give him away. Rabinovich was an outpatient who had already had more than two hundred sessions. He'd made very heavy weather of them and with every dozen he'd felt closer to death than recovery. Where he lived no one understood him, not a soul in his apartment or his building or his block. They were healthy people who ran about from noon till night thinking of successes or failures—things that seemed terribly important to them. Even his own family had got tired of him. It was only here, on the steps of the cancer clinic, that the patients listened to him for hours and sympathized. They understood what it means to a man when a small triangular area grows bone-hard and the irradiation scars lie thick on the skin where the X-rays have penetrated.)

Honestly, there he was talking about "the principle of the thing!" Wasn't that just what Dontsova and her assistants needed—to spend days talking to patients about the principles on which they were being treated! Where would they find the time for the treatment then?

Every now and again some stubborn, meticulous lover of knowledge, like this man or Rabinovich, would crop up out of a batch of fifty patients and run her into the ground, prizing explanations out of her about the course of his disease. When this happened, one couldn't avoid the hard task of offering the occasional explanation. And Kostoglotov's case was a special one even from the medical point of view by virtue of the extraordinary negligence with which it had been handled. Up to the time of her arrival on the scene, when he had finally been allowed out to receive treatment, it was as if there had been a malicious conspiracy to drive him to the very borderline of death. His case was a special one too because of the exceptionally rapid revival which had begun under X-ray treatment.

"Kostoglotov! Twelve sessions of X-rays have turned you from a corpse into a living human being. How dare you attack your treatment? You complain that they gave you no treatment in the camp or in exile, that they neglected you, and in the same breath you grumble because people *are* treating you and taking trouble over you. Where's the logic in that?"

"Obviously there's no logic," Kostoglotov shook his shaggy black mane. "But maybe there needn't be any, Ludmila Afanasyevna. After all, man is a complicated being, why should he be explainable by logic? Or for that matter by economics? Or physiology? Yes, I did come to

you as a corpse, and I begged you take me in, and I lay on the floor by the staircase. And therefore you make the logical deduction that I came to you to be saved *at any price!* But I don't want to be saved at any price! There isn't anything in the world for which I'd agree to pay *any price!*" He began to speak more quickly. It was something he never liked doing, but Dontsova was making an attempt to interrupt and he still had a great deal more to say on the subject. "I came to you to *relieve my suffering!* I said, 'I'm in terrible pain, help me!' And you did. And now I'm not in pain. Thank you! Thank you! I'm grateful and I'm in your debt. Only now let me go. Just let me crawl away like a dog to my kennel, so I can lick my wounds and rest till I'm better."

"And when the disease catches up with you, you'll come crawling back to us?"

"Perhaps. Perhaps I'll come crawling back to you."

"And we shall have to take you?"

"Yes! And that's where I see your mercy. What are you worried about? Your recovery percentages? Your records? How you'll be able to explain letting me go after fifteen sessions when the Academy of Medical Science recommends not less than sixty?"

Never in her life had she heard such incoherent rubbish. As a matter of fact, from the records' point of view it would be to her advantage to discharge him and make a note of "Marked improvement." This would never apply after fifty sessions.

But he kept hammering away at his point.

"As far as I'm concerned, it's enough that you've driven back the tumor and stopped it. It's on the defensive. I'm on the defensive too. Fine. A soldier has a much better life in defense. And whatever happens you'll never be able to cure me completely. There's no such thing as a complete cure in cancer. All processes of nature are characterized by the law of diminishing returns, they reach a point where big efforts yield small results. In the beginning my tumor was breaking up quickly. Now it'll go slowly. So let me go with what's left of my blood."

"Where did you pick up all this information, I'd like to know?" Dontsova frowned.

"Ever since I was a child I've loved browsing through medical books."

"But what *exactly* are you afraid of in our treatment?"

"Ludmila Afanasyevna, I don't know what to be afraid of. I'm not a doctor. Perhaps you know but don't want to tell me. For example,

Vera Kornilyevna wants to put me on a course of glucose injections. . . ."

"Absolutely essential."

"But I don't want it."

"Why on earth not?"

"In the first place, it's unnatural. If I need grape sugar, give it to me through the mouth! Why this twentieth-century gimmick? Why should every medicine be given by injection? You don't see anything similar in nature or among animals, do you? In a hundred years' time they'll laugh at us and call us savages. And then, the way they give injections! One nurse gets it right first time, another punctures your . . . your ulnar flexion to bits. I just don't want it. And now I see you're getting ready to give me blood transfusions. . . ."

"You ought to be delighted! Somebody's willing to give their blood for you. That means health, life!"

"But I don't want it! They gave a Chechen here a transfusion in front of me once. Afterwards he was in convulsions on his bed for three hours. They said, 'Incompatible compatibility!' Then they gave someone else blood and missed the vein. A great lump came up on his arm. Now it's compresses and vapor baths for a whole month. I don't want it."

"But substantial X-ray treatment is impossible without transfusion!"

"Then don't give it! Why do you assume you have the right to decide for someone else? Don't you agree it's a terrifying right, one that rarely leads to good? You should be careful. No one's entitled to it, not even doctors."

"But doctors *are* entitled to that right—doctors above all," exclaimed Dontsova with deep conviction. By now she was really angry. "Without that right there'd be no such thing as medicine!"

"And look what it leads to. You're going to deliver a lecture on radiation sickness soon, aren't you?"

"How do you know that?" Ludmila Afanasyevna was quite astonished.

"Well, it wasn't very difficult. I assumed. . . ."

(It was quite simple. He had seen a thick folder of typescript lying on her table. Although the title was upside down, he had managed to read it during the conversation and had understood its meaning.)

". . . Or rather I guessed. There is a new name, radiation sickness, which means there must be lectures about it. But you see, twenty years ago you irradiated some old Kostoglov in spite of his protests that he was afraid of the

treatment, and you reassured him that everything was all right, because you didn't know then that radiation sickness existed. It's the same with me today. I don't know yet what I'm supposed to be afraid of. I just want you to let me go. I want to recover under my own resources. Then maybe I'll just get better. Isn't that right?"

Doctors have one sacred principle: the patient must never be frightened, he must be encouraged. But with a patient as importunate as Kostoglotov exactly the reverse tactics were required—shock.

"Better? No, *you won't get better!* Let me assure you"—her four fingers slammed the table like a whisk swatting a fly—"that you won't. You are going—" she paused to measure the blow—"to die!"

She looked at him to see him flinch. But he merely fell silent.

"You'll be exactly like Azovkin—and you've seen the condition he's in. Well, you've got the same disease as him in an almost identical state of neglect. We're saving Ahmadjan because we began to give him radiotherapy immediately after his operation. But with you we've lost two years, can you imagine it? There should have been another operation straight away on the lymph node, next to the one they operated on, but they let it go, do you see, and the secondaries just flowed on! Your tumor is one of the most dangerous kinds of cancer. It is very rapid to develop and acutely malignant, which means secondaries appear very quickly too. Not long ago its mortality rate was reckoned at 95 per cent. Does that satisfy you? Look, I'll show you..."

She dragged a folder out of a pile and began to rummage through it.

Kostoglotov was silent. Then he spoke up, but quietly, without any of the self-confidence he had shown a few minutes earlier.

"To be frank, I'm not much of a clinger to life. It's not only that there's none ahead of me, there's none behind me either. If I had a chance of six months of life, I'd want to live them to the full. But I can't make plans for ten or twenty years ahead. Extra treatment means extra torment. There'll be radiation sickness, vomiting... what's the point?"

"Ah yes, I've found it! Here are our statistics." And she turned toward him a double page taken from an exercise book. Right across the top of the sheet was written the name of his type of tumor. Then on the left-hand side was a heading, "Already dead," and on the right, "Still alive." There were three columns of names, writ-

ten in at different times, some in pencil, some in ink. On the left there were no corrections, but on the right, crossings out, crossings out, crossings out. . . . "This is what we do. When a patient's discharged, we write his name in the right-hand list and then transfer him to the left-hand one. . . . Still, there are some lucky ones who've stayed in the right-hand one. Do you see?"

She gave him another moment to look at the list and to think about it.

"You *think* you're cured." She returned to the attack with vigor. "You're as ill as you ever were. You're no different than when you were admitted. The only thing that's been made clear is that your tumor *can* be fought, that all is not lost yet. And this is the moment you choose to announce you're leaving! All right, go! Get your discharge today! I'll arrange it for you now. And then I'll put your name down on the list—'Still alive.'"

He was silent.

"Come on, make up your mind!"

"Ludmila Afanasyevna"—Kostoglotov was ready for a compromise—"Look, if what's needed is a reasonable number of sessions, say, five or ten..."

"Not five or ten! Either no sessions at all or else as many as are necessary! That means, from today, two sessions daily instead of one, and all the requisite treatment. And no smoking! And one more essential condition: you must accept your treatment not just with faith but with *joy!* That's the only way you'll ever recover!"

He lowered his head. Part of today's bargaining with the doctors had been in anticipation. He had been dreading that they were going to propose another operation, but they hadn't. X-ray treatment was tolerable, it wasn't too bad.

Kostoglotov had something in reserve—a secret medicine, a mandrake root from Issyk Kul. There was a motive behind his wish to go back to his place in the woodlands—he wanted to treat himself with the root. Because he had the root, he'd really only come to the cancer clinic to see what it was like.

Dr. Dontsova saw she had won the battle and could afford to be magnanimous.

"All right then, I won't give you glucose. You can have another injection instead, an intramuscular one."

Kostoglotov smiled. "I see I'm going to have to give way."

"And please, see if you can hurry up that letter from Omsk."

As he left the room it seemed to him that

he was walking between two eternities, on one side a list of the living, with its inevitable crossings out, on the other—*eternal* exile. Eternal as the stars, as the galaxies.

The strange thing is that if Kostoglotov had persevered with his questions—What sort of injection was it? What was its purpose? Was it really necessary and morally justified?—if he had forced Ludmila Afanasyevna to explain the workings and the possible consequences of the new treatment, then very possibly he would have rebelled once and for all.

But precisely at this point, having exhausted all his brilliant arguments, he had capitulated.

She had been deliberately cunning, she had mentioned the injection as something quite insignificant because she was tired of all this explaining. Also, she knew for sure that this was the moment, after the action of the X-rays in their pure state had been tested on the patient, to deal the tumor yet another crucial blow. It was a treatment highly recommended for this particular type of cancer by the most up-to-date authorities. Now that she anticipated the amazing success that attended Kostoglotov's treatment, she could not possibly weaken before his obstinacy or neglect to attack him with all the weapons she believed in. True, there were no slides available with sections of his primary, but all her intuition, her powers of observation and her memory suggested to her that the tumor was the kind she suspected—not a teratoma, but a sarcoma. . . .

It was on this very type of tumor with precisely these secondaries that Dr. Dontsova was writing her doctoral thesis.

\* \* \*

Today, however much she bustled about the clinic, there was something gnawing at her self-confidence, and at her sense of responsibility and authority. Was it the pain she could clearly feel in her stomach? Some days she didn't feel it at all, other days it was weaker, but today it was stronger. If she wasn't an oncologist she'd have dismissed it or else had it investigated without fear. But she knew the road too well to take the first step along it: to tell her relatives, to tell her colleagues. When it came to dealing with herself she kept herself going with typical Russian temporizing: Maybe it'll go away. Maybe it's only my nerves.

But it wasn't just that, it was something else that had been gnawing at her all day, like a splinter in the hand, faintly but persistently.

Now that she was back in her own little den, sitting at her own table and reaching out for the file on "Radiation Sickness" which the observant Kostoglotov had noticed, she realized that all day she had been more than upset, really wounded by that argument with him about the right to treat.

She could still hear his words: "Twenty years ago you gave radiation treatment to some old Kostoglotov who begged you not to do it. You didn't know about radiation sickness then!"

And in fact she was due shortly to give a lecture to the society of X-ray specialists on "The Late Aftereffects of Radiotherapy." It was almost exactly what Kostoglotov had reproached her with.

It was only recently, a year or two ago, that she and other X-ray specialists here and in Moscow and in Baku had begun to observe certain cases that could not immediately be understood.

A suspicion arose. Then it became a guess. They began to write letters to each other and to speak about it, not in lectures at this stage but in the intervals between lectures. Then somebody read a paper in an American journal, and somebody else read another. The Americans had something similar brewing. The cases multiplied, more and more patients came in with complaints, until suddenly it was all given a name: "The late aftereffects of radiotherapy." The time had come to speak of them from the rostrum and to reach a decision.

The gist of it was that X-ray cures, which had been safely, successfully, even brilliantly accomplished ten or fifteen years ago through heavy doses of radiation, were now resulting in unexpected damage or mutilation of the irradiated parts.

It was not so bad, or at any rate it was justifiable, in the case of former patients who had been suffering from malignant tumors. Even today there would have been no other solution. They had saved the patient from certain death in the only way possible; they had given large doses because small doses would not help. And if the patient reappeared today with some sort of mutilation, he had to understand that this was the price he must pay for the extra years he had already lived, as well as for the years that still remained ahead of him.

But then, ten, fifteen or eighteen years ago, when the term "radiation sickness" did not exist, X-ray radiation had seemed such a straightforward, reliable and foolproof method, such a magnificent achievement of modern medical tech-

nique, that it was considered retrograde, almost a sabotage of public health, to refuse to use it and to look for other, parallel or roundabout methods. They were afraid only of acute, immediate damage to tissue and bone, but even in those days they rapidly learned to avoid that. So—they irradiated! They irradiated with wild enthusiasm! Even benign tumors. Even small children.

And now these children had grown up. Young men and young women, sometimes even married, were coming with irreversible mutilations of those parts of the body which had been so zealously irradiated.

\* \* \*

But these incidents had greatly shocked Ludmila Afanasyevna. They had left her with a gnawing feeling of deep-rooted and unpardonable guilt. And it was right there that Kostoglotov had struck home today.

She crossed her arms, hugging her shoulders, and walked round the room from door to window and back again, across the free strip of floor between the two apparatuses that were now switched off.

Was it possible? Could the question arise of a doctor's *right* to treat? Once you began to think like that, to doubt every method scientifically accepted today simply because it might be discredited or abandoned in the future, then goodness knows where you'd end up. After all there were cases on record of death from aspirin. A man might take the first aspirin of his life and die of it! By that reasoning it became impossible to treat anyone. By that reasoning all the daily advantages of medicine would have to be sacrificed.

It was a universal law: everyone who *acts* breeds both good and evil. With some it's more good, with others more evil.

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b.

### Talcott Parsons The Social System\*

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There seem to be four aspects of the institutionalized expectation system relative to the sick role. First, is the exemption from normal social role responsibilities, which of course is relative to the nature and severity of the illness. This exemption requires legitimation by and to

the various alters involved and the physician often serves as a court of appeal as well as a direct legitimizing agent. It is noteworthy that like all institutionalized patterns the legitimation of being sick enough to avoid obligations can not only be a right of the sick person but an obligation upon him. . . .

The second closely related aspect is the institutionalized definition that the sick person cannot be expected by "pulling himself together" to get well by an act of decision or will. In this sense also he is exempted from responsibility—he is in a condition that must "be taken care of." His "condition" must be changed, not merely his "attitude." Of course the process of recovery may be spontaneous but while the illness lasts he can't "help it." This element in the definition of the state of illness is obviously crucial as a bridge to the acceptance of "help."

The third element is the definition of the state of being ill as itself undesirable with its obligation to want to "get well." The first two elements of legitimation of the sick role thus are conditional in a highly important sense. It is a relative legitimation so long as he is in this unfortunate state which both he and alter hope he can get out of as expeditiously as possible.

Finally, the fourth closely related element is the obligation—in proportion to the severity of the condition, of course—to seek *technically competent* help, namely, in the most usual case, that of a physician and to *cooperate* with him in the process of trying to get well. It is here, of course, that the role of the sick person as patient becomes articulated with that of the physician in a complementary role structure.

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By the same institutional definition the sick person is not, of course, competent to help himself, or what he can do is, except for trivial illness, not adequate. But in our culture there is a special definition of the kind of help he needs, namely, professional, technically competent help. The nature of this help imposes a further disability or handicap upon him. He is not only generally not in a position to do what needs to be done, but he does not "know" what needs to be done or how to do it. It is not merely that he, being bedridden, cannot go down to the drug store to get what is needed, but that he would, even if well, not be qualified to do what is needed, and to judge what needs to be done. There is, that is to say, a "communication gap."

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\* Glencoe, Ill.: The Free Press 436-437, 441-443, 445-453, 463-465 (1951). Copyright © 1951 by Talcott Parsons. Reprinted by permission.



This disqualification is, of course, not absolute. Laymen do know something in the field, and have some objective bases of judgment. But the evidence is overwhelming that this knowledge is highly limited and that most laymen *think* they know more, and have better bases of judgment than is actually the case. . . .

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[T]he situation of illness very generally presents the patient and those close to him with complex problems of emotional adjustment. It is, that is to say, a situation of strain. Even if there is no question of a "psychic" factor in his condition, suffering, helplessness, disablement and the risk of death, or sometimes its certainty, constitute fundamental disturbances of the expectations by which men live. They cannot in general be emotionally "accepted" without the accompaniments of strain with which we are familiar and hence without difficult adjustments unless the patient happens to find positive satisfactions in them, in which case there is also a social problem. The significance of this emotional factor is magnified and complicated insofar as defensive and adjustment mechanisms are deeply involved in the pathological condition itself.

The range of possible complexities in this sphere is very great. The problems are, however, structured by the nature of the situation in certain relatively definite ways. Perhaps the most definite point is that for the "normal" person illness, the more so the greater its severity, constitutes a frustration of expectancies of his normal life pattern. He is cut off from his normal spheres of activity and many of his normal enjoyments. He is often humiliated by his incapacity to function normally. His social relationships are disrupted to a greater or a less degree. He may have to bear discomfort or pain which is hard to bear, and he may have to face serious alterations of his prospects for the future, in the extreme but by no means uncommon case the termination of his life.

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. . . There are two particularly important broad consequences of the features of the situation of the sick person for the problem of the institutional structuring of medical practice. One is that the combination of helplessness, lack of technical competence, and emotional disturbance makes him a peculiarly vulnerable object for exploitation. It may be said that the exploitation of the helpless sick is "unthinkable." That happens

to be a very strong sentiment in our society, but for the sociologist the existence of this sentiment or that of other mechanisms for the prevention of exploitation must not be taken for granted. There is in fact a very real problem of how, in such a situation, the very possible exploitation is at least minimized.

The other general point is the related one that the situation of the patient is such as to make a high level of rationality of judgment peculiarly difficult. He is therefore open to, and peculiarly liable to, a whole series of ir- and non-rational beliefs and practices. The world over the rational approach to health through applied science is, as we have noted, the exception rather than the rule, and in our society there is, even today, a very large volume of "superstition" and other non- or irrational beliefs and practices in the health field. This is not to say that the medical profession either has a monopoly of rational knowledge and techniques, or is free of the other type of elements, but the volume of such phenomena outside the framework of regular medical practice is a rough measure of this factor. This set of facts then makes problematical the degree to which the treatment of health problems by applied science has in fact come to be possible. It can by no means be taken for granted as the course which "reasonable men," i.e., the normal citizen of our society will "naturally" adopt.

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The role of the physician centers on his responsibility for the welfare of the patient in the sense of facilitating his recovery from illness to the best of the physician's ability. In meeting this responsibility he is expected to acquire and use high technical competence in "medical science" and the techniques based upon it. The first question to ask about his situation, therefore, concerns the relation of these technical tools to the tasks he is called upon to perform and the responsibilities he is expected to live up to.

In a certain proportion of cases the doctor has what may be called a perfectly straightforward technological job. His knowledge and skill give him quite adequate tools for accomplishment of his ends; it is only necessary to exercise sufficient patience, and to work steadily and competently at the task. This would, it is true, leave the "penumbra" of emotional reactions of patients and their families for him to deal with, and his own emotional reactions to such things as severe suffering and imminence of death might well pose certain problems of emotional adjust-



ment to him. But with these qualifications it would be much like any other high level technical job.

But in common with some and not other technical jobs there is in this case a shading off into cases with respect to which knowledge, skill and resources are not adequate, with hard, competent work, to solve the problem. There are two main aspects to this inadequacy. On the one hand there are cases, a good many of them, where the upshot of a competent diagnosis is to expose a condition which is known, in the given state of medical knowledge and technique, to be essentially uncontrollable. This is true both in the individual case and generally. Though there is a fundamental relationship between knowledge and control, this is a general and not a point-for-point relationship. Optimistic biases are very general and fundamental in human social orientations, perhaps particularly in our society and certainly in relation to health. It is, therefore, very common that the initial effect of a given advance in knowledge is to demonstrate the impossibility of controlling things which were thought to be readily controllable, to expose unfavorable factors in the situation which were not previously appreciated, and to show the fruitlessness of control measures in which people had previously had faith.

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The absolute limits of the physician's control—which of course are relative to the state of medical science at the time and his own assimilation of it—are not the only source of frustration and strain. Within these limits there is a very important area of uncertainty. As in so many practical situations, some of the factors bearing on this one may be well understood, but others are not. The exact relation of the known to the unknown elements cannot be determined; the unknown may operate at any time to invalidate expectations built up on analysis of the known. Sometimes it may be known *that* certain factors operate significantly, but it is unpredictable whether, when and how they will operate in the particular case. Sometimes virtually nothing is known of these factors, only that the best laid plans mysteriously go wrong. In general the line between the spontaneous forces tending to recovery—what used to be called the *vis medicatrix naturae*—and the effects of the physician's "intervention" is impossible to draw with precision in a very large proportion of cases.

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The primary definition of the physician's responsibility is to "do everything possible" to forward the complete, early and painless recovery of his patients. The general effect of the existence of large factors of known impossibility and of uncertainty in the situation with which he has to cope is to impose strain upon him, to make it more difficult for him to have a "purely rational" orientation to his job than if his orientation were such as to guarantee success with competent work. This is true of his own orientation without taking account of reciprocal interactions with his patients and their intimates.

But the function of "doing everything possible" is institutionalized in terms of expectations, and these expectations are most vividly and immediately embodied, besides in the physician's own attitude system, in the attitudes of precisely this group of people. But compared to most such groups their involvement is, because of the considerations analyzed above, peculiarly intensive, immediate, and likely to contain elements of emotional disturbance which are by definition, tendencies to deviant behavior. Hence the elements of strain on the physician by virtue of these impossibility and uncertainty components of his situation are particularly great. Non- and irrational mechanisms [are] prominent in the reactions of sick people to their situations, and those of their families. In spite of the discipline of his scientific training and competence, it would be strange if, in view of the situation, physicians as a group were altogether exempted from corresponding tendencies. In fact that magic frequently appears in situations of uncertainty is suggestive. [I]t is clear from the above that quite apart from the operation of so-called psychic factors in the disease process itself, the strains existing on *both* sides of a doctor-patient relationship are such that we must expect to find, not merely institutionalization of the roles, but special mechanisms of social control in operation.

Factors of impossibility, and uncertainty in situations where there is a strong emotional interest in success, are common in many other fields of applied science—the military field is an outstandingly important example. There are, however, certain other features of the situation of the physician which are not common to many other fields which share those so far discussed. The engineer, for example, deals primarily with non-human impersonal materials which do not have "emotional" reactions to what he does with them. But the physician deals with human beings,

and does so in situations which often involve "intimacies," that is, in contexts which are strongly charged with emotional and expressively symbolic significance, and which are often considered peculiarly "private" to the individual himself, or to especially intimate relations with others.

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Modern developments in psychology, particularly psychoanalysis, have made us aware that in addition to resistances to access to the body, and to confidential information, anyone taking a role like that of the physician toward his patients is exposed to another sort of situational adjustment problem. That is, through processes which are mostly unconscious the physician tends to acquire various types of projective significance as a person which may not be directly relevant to his specifically technical functions, though they may become of the first importance in connection with psychotherapy. The generally accepted name for this phenomenon in psychiatric circles is "transference," the attribution to the physician of significances to the patient which are not "appropriate" in the realistic situation, but which derive from the psychological needs of the patient. For understandable reasons a particularly important class of these involves the attributes of parental roles as experienced by the patient in childhood. . . .

If all these factors be taken together it becomes clear that, in ways which are not true of most other professional functions, the situation of medical practice is such as inevitably to "involve" the physician in the psychologically significant "private" affairs of his patients. Some of these may not otherwise be accessible to others in any ordinary situation, others only in the context of specifically intimate and personal relationships. What the relation of the physician's role to these other relationships is to be, is one of the principal functional problems which underly the structuring of his professional role.

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[T]he sick person is peculiarly vulnerable to exploitation and at the same time peculiarly handicapped in arriving at a rationally objective appraisal of his situation. In addition, the physician is a technically competent person whose competence and specific judgments and measures cannot be competently judged by the layman. The latter must therefore take these judgments and measures "on authority." But in this

type case there is no system of coercive sanctions to back up this authority. All the physician can say to the patient who refuses to heed his advice is "well, it's your own funeral"—which it may be literally. All this of course is true of a situation which includes the potential resistances which have been discussed above.

These different factors seem to indicate that the situation is such that it would be particularly difficult to implement the pattern of the business world, where each party to the situation is expected to be oriented to the rational pursuit of his own self-interests, and where there is an approach to the idea of "caveat emptor." In a broad sense it is surely clear that society would not tolerate the privileges which have been vested in the medical profession on such terms. The protection of the patient against the exploitation of his helplessness, his technical incompetence and his irrationality thus constitutes the most obvious functional significance of the pattern. In this whole connection it is noteworthy how strongly the main reliance for control is placed on "informal" mechanisms. The law of the state includes severe penalties for "malpractice" and medical associations have relatively elaborate disciplinary procedures, but these quite definitely are not the principal mechanisms which operate to ensure the control of self-orientation tendencies. . . .

Here it may be noted that the collectivity-orientation of the physician is protected by a series of symbolically significant practices which serve to differentiate him sharply from the businessman. He cannot advertise—he can only modestly announce by his "shingle" and the use of his M.D. in telephone directories and classified sections, that he is available to provide medical service. He cannot bargain over fees with his patients—a "take it or leave it" attitude is enjoined upon him. He cannot refuse patients on the ground that they are poor "credit risks." He is given the privilege of charging according to the "sliding scale," that is, in proportion to the income of the patient or his family—a drastic difference from the usual pricing mechanism of the business world. The general picture is one of sharp segregation from the market and price practices of the business world, in ways which for the most part cut off the physician from many immediate opportunities for financial gain which are treated as legitimately open to the businessman. . . .

[T]he definition in terms of collectivity-orientation is expected to be reciprocal. The most usual formulation for this is that the patient is

expected to "have confidence" in his physician and, if this confidence breaks down, to seek another physician.

This may be interpreted to mean that the relationship is expected to be one of mutual "trust," of the belief that the physician is trying his best to help the patient and that conversely the patient is "cooperating" with him to the best of his ability. It is significant for instance that this constitutes a reinforcement of one of the principal institutional features of the sick role, the expectation of a desire to get well. It makes the patient, in a special sense, responsible to his physician. But more generally, . . . collectivity-orientation is involved in all cases of institutionalized authority, that is authority is an attribute of a status in a collectivity. In a very special and informal sense the doctor-patient relationship has to be one involving an element of authority—we often speak of "doctor's orders." This authority cannot be legitimized without reciprocal collectivity-orientation in the relationship. To the doctor's obligation to use his authority "responsibly" in the interest of the patient, corresponds the patient's obligation faithfully to accept the implications of the fact that he is "Dr. X's patient" and so long as he remains in that status must "do his part" in the common enterprise. He is free, of course, to terminate the relationship at any time. But the essential point is the sharp line which tends to be drawn between being X's patient, and no longer being in that position. In the ideal type of commercial relationship one is not A's customer to the exclusion of other sources of supply for the same needs.

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## NOTES

### NOTE I.

RENÉE C. FOX  
EXPERIMENT PERILOUS\*

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[P]hysicians are confronted with two basic types of uncertainty. One of these derives from limitations in the current state of medical knowledge. There are many questions to which no physician, however well trained, can yet provide answers. The second type of uncertainty results from incomplete or imperfect mastery of avail-

able knowledge. No one can have at his command all the information, lore, and skills of modern medicine.

In turn, these forms of uncertainty to which all physicians are subject are connected with another set of problems which they also inevitably face: problems of therapeutic limitation. Since the knowledge and skills of the physician are not always adequate, there are many times when his most vigorous efforts to understand illness and to rectify its consequences are of little or no avail.

What the physician *can* do to help a patient, then, is often limited. What he *ought* to do is frequently not clear. And the consequences of his clinical actions cannot always be accurately predicted. Yet, in the face of these uncertainties and limitations, the physician is expected to institute measures which will facilitate the diagnosis and treatment of the problems the patient presents.

Largely because what the physician decides to do (and not to do) on behalf of a patient is generally based on less than perfect knowledge, it has been said that "in a sense [his] every clinical act is an investigation," and that "medical experimentation on human beings, in its broadest meaning and for the good of the individual patient, takes place continually in every doctor's office."

The rapidity with which new diagnostic and therapeutic procedures and new drugs have been appearing in the last few swiftly moving decades is also responsible for the fact that the practicing physician is often cast in the role of experimenter. It is hard for the physician simply to keep abreast of these developments, and even more difficult for him to appraise them. The available reports about their benefits and dangers are tentative and far from consistent, since only gradually, and to a considerable extent on the basis of a trial-and-error empiricism, can relatively definitive judgments about a new procedure or drug be reached. Thus, although typically, the physician in practice has published literature and the informally transmitted opinions and experiences of colleagues to inform him about the properties of new techniques and drugs, the degree of uncertainty about their benefits, limitations and hazards which still remain is often large enough to warrant calling "experimental" some of the clinical trials he conducts on patients. Before he carries out such trials, the practicing physician, like the research physician, has the problem of trying to determine whether the dangers of the contemplated proce-

\* Glencoe, Ill.: The Free Press 237-240 (1959). Copyright © 1959 by The Free Press. Reprinted by permission of The Macmillan Company.

cedure or drug are sufficiently less than the hazards of the patient's disease, and its potential benefits sufficiently greater than both, to justify asking the patient to undergo it. If they seem to be, and he therefore decides to carry out such a procedure on a patient or administer such a drug, the practicing physician, like the clinical investigator, sometimes finds himself in a situation where the measures he employed fail to benefit the patient clinically, or have negative effects on him. Thus, physicians in practice, as well as research physicians, experience some of the uncertainties, limitations, moral ambiguities, and untoward consequences connected with the application of advanced medical knowledge and techniques to the medical problems of patients.

Not only medicine, of course, but all scientific activity has social consequences, harmful as well as beneficent. However technical or abstract the experiments which biological and physical scientists conduct in their laboratories and field stations may seem, ultimately they may affect the lives of men and women in powerful ways—both for good and for evil. The "pure" scientist, however, is in several respects further removed and better insulated from the ultimate outcome of his actions than the physician is. On the whole, his attention is confined to the test tube and microscope; his research is conducted on lifeless matter or on animals; and the social implications of what he does are for the most part indirect and long-range.

The special difficulty of the physician—the problem that distinguishes him from most other scientists, be they in the fields of pure or applied science—is that the material on which he works is the disease-stricken human being. Thus, the decisions the physician makes, the procedures he carries out, the drugs he prescribes have a proximate, visible, flesh-and-blood impact on the patients under his care. To a significant extent, whether patients get better, get worse, or whether their conditions remain stubbornly fixed is contingent upon what the physician is or is not able to do for them. Because the welfare of the patient is this directly associated with his actions, the human consequences of his uncertainty, limitation, and fallibility are more apparent to the physician than to most other scientists. It is harder for him to forget or systematically ignore the fact that what he does as a scientist makes an impact on people. Furthermore, the people whom he affects are not remote, anonymous entities. They are his patients: the individuals whom he sees, to whom he talks, and on whom he

carries out various procedures, in his office, in their homes and in the hospital.

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#### NOTE 2.

FRED DAVIS  
UNCERTAINTY IN MEDICAL PROGNOSIS,  
CLINICAL AND FUNCTIONAL.\*

Medical sociology is indebted to Talcott Parsons for having called attention to the important influence of uncertainty on the relationship between doctor and patient in the treatment of illness and disease. This is described as a primary source of strain in the physician's role, not only because clinically it so often obscures and vitiates definitive diagnoses and prognoses, but also because in an optimistic and solution-demanding culture such as ours it poses serious and delicate problems in the communicating of the unknown and the problematic to the patient and his family . . . As a ready-made explanation of a disturbing element in the relationship between doctor and patient, the concept—uncertainty—stands in danger of being applied in a catch-all fashion whenever . . . communication from doctor to patient is characterized by duplicity, evasion, or other forms of strain. That other factors, having relatively little to do with uncertainty, can also systematically generate strain in the relationship may unfortunately be ignored because of the disposition to subsume phenomena under preexistent categories.

The present paper examines the scope and significance of uncertainty as evidenced in the treatment of a particular disease. Specifically, it seeks to distinguish between "real" uncertainty as a clinical and scientific phenomenon and the uses to which uncertainty—real or pretended "functional" uncertainty—lends itself in the management of patients and their families by hospital physicians and other treatment personnel. . . .

The disease in question is paralytic poliomyelitis, and the subjects are fourteen Baltimore families, in each of which a young child had contracted the disease. These were studied longitudinally over a two-year period. . . .

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[O]ne must assume that the doctor's knowledge of the disease and its physical effects is

\* 66 *American Journal of Sociology* 41-47 (1960). Reprinted by permission.

more accurate, comprehensive, and profound than that of the parents. The problem, then, could be stated: How much information was communicated to the parents? How was it communicated? And what consequences did this communication have on the parents' expectations of the child's illness and prospects for recovery? And, since in paralytic poliomyelitis (as in many other diseases and illnesses) uncertainty does affect the making of diagnoses and prognoses, an attempt was made to assess the scope, significance, and duration of uncertainty for the doctor. This then provided some basis for inferring the extent to which the parents' knowledge and expectations, or lack thereof, could also be attributed ultimately to uncertainty.

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[T]he pathological course of paralytic poliomyelitis is such that, during the first weeks following onset, it is difficult in most cases for even the most skilled diagnostician to make anything like a definite prognosis of probable residual impairment and functional disability. . . .

During this initial period of the child's hospitalization, therefore, the physician is hardly ever able to tell the parents anything definite about the child's prospects of regaining lost muscular function. In view of the very real uncertainty, to attempt to do so would indeed be hazardous. To the parents' insistent questions, "How will he come out of it?" "Will he have to wear a brace?" "Will his walk be normal?" and so on, the invariable response of treatment personnel was that they did not know and that only time would tell. Thus during these first weeks the parents came to adopt a longer time perspective and more qualified outlook than they had to begin with.

By about the sixth week to the third month following onset of the disease, however, the orthopedist and physiotherapist are in position to make reasonably sound prognoses of the amount and type of residual handicap. This is done on the basis of periodic muscle examinations from which the amount and rate of return of affected muscular capacity is plotted. . . .

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By this time, therefore, the element of clinical uncertainty regarding outcome, so conspicuously present when the child is first stricken, is greatly reduced for the physician, if not al-

together eliminated.<sup>7</sup> Was there then a commensurate gain in the parents' understanding of the child's condition after this six-week to three-month period had passed? Did they then, as did the doctors, come to view certain outcomes as highly probable and others as improbable?

[T]he answer to these questions is that, except for one case in which the muscle check pointed clearly to full recovery, the parents were neither told nor explicitly prepared by the treatment personnel to expect an outcome significantly different from that which they understandably hoped for, namely, a complete and natural recovery for the child. This does not imply that the doctors issued falsely optimistic prognoses or that, through indirection and other subtleties, they sought to encourage the parents to expect more by way of recovery than was possible. Rather, what typically transpired was that the parents were kept in the dark. The doctors' answers to their questions were couched for the most part in such hedging, evasive, or unintelligibly technical terms as to cause them, from many such contacts, to expect a more favorable recovery than could be justified by the facts then known. As one treatment-staff member put it, "We try not to tell them too much. It's better if they find out for themselves in a natural sort of way."

Indeed, it was disheartening to note how, for many of the parents, "the natural way" consisted of a painfully slow and prolonged dwindling of expectations for a complete and natural recovery. This is ironical when one considers that as early as two to three months following onset the doctors and physiotherapists were able to tell members of the research team with considerable confidence that one child would require bracing for an indefinite period; that another would never walk with a normal gait; that a third would require a bone-fusion operation before he would be able to hold himself erect;

<sup>7</sup> As in nearly all applied fields of endeavor, medicine necessarily deals in probabilities rather than absolutes. Hence some measure of uncertainty is always present, the crucial question being the matter of degree and not the mere presence. Admittedly, no hard-and-fast lines can be drawn at the point at which uncertainty acquires therapeutic significance; but, if the concept is to have any analytical value at all, it cannot be applied to all instances of illness in which it is possible to concede the existence of some degree of uncertainty, however slight. If this were done, there would not be an instance to which it did not apply.

and so on. By contrast, the parents of these children came to know these prognoses much later, if at all. And even then their understanding of them was in most instances partial and subject to considerable distortion.

But what is of special interest here is the way in which uncertainty, a *real* factor in the early diagnosis and treatment of the paralyzed child, came more and more to serve the purely managerial ends of the treatment personnel in their interaction with parents. Long after the doctor himself was no longer in doubt about the outcome, the perpetuation of uncertainty in doctor-to-family communication, although perhaps neither premeditated nor intended, can nonetheless best be understood in terms of its functions in the treatment system. These are several, and closely connected.

Foremost is the way in which the pretense of uncertainty as to outcome serves to reduce materially the expenditure of additional time, effort, and involvement which a frank and straightforward prognosis to the family might entail. The doctor implicitly recognizes that, were he to tell the family that the child would remain crippled or otherwise impaired to some significant extent, he would easily become embroiled in much more than a simple, factual medical prognosis. Presenting so unwelcome a prospect is bound to meet with a strong—and, according to many of the treatment personnel, “unmanageable”—emotional reaction from parents. . . . Moreover, to the extent to which the doctor feels some professional compunction to so inform the parents, the bustling, time-conscious work milieu of the hospital supports him in the convenient rationalization that, even were he to take the trouble, the family could not or would not understand what he had to tell them anyway. Therefore, in hedging, being evasive, equivocating, and cutting short his contact with the parents, the doctor was able to avoid “scenes” with them and having to explain to and comfort them, tasks, at least in the hospital, often viewed as onerous and time-consuming.

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[I]t must in fairness be recognized that there is still little agreement within medical circles on what practice should be in these circumstances. (The perennial debate on whether a patient and his family should be told that he is dying of cancer, and when and how much they should be told, is an extreme though highly relevant case in point.) And perhaps the easiest

recourse of the hospital practitioner—who, organizationally, is better barricaded and further removed from the family than, for example, the neighborhood physician—is to avoid it altogether.

Clearly, then, clinical uncertainty is not responsible for all that is not communicated to the patient and his family. Other factors, interests, and circumstances intrude in the rendering of medical prognoses, with the result that what the patient is told is uncertain and problematic may often not be so at all. And, conversely, what he is made to feel is quite certain may actually be highly uncertain. . . .

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#### NOTE 3.

##### WISCONSIN BAR ASSOCIATION INTERVIEWING CLIENTS\*

Most lawyers spend too much time in interviewing clients. This is very wasteful and costly to the lawyer. The use of time records will tend to regulate this difficulty. Also, the use of checklists in interviews will help to prevent drawn-out conferences with clients.

The following items will prove helpful in client interviews:

1. Develop and use definite systems for keeping interview time at a minimum. For example, upon signal, the secretary can interrupt the interview with a message requiring the lawyer's attention. Mainly, however, limitation of interview time is a matter of sticking firmly to the matter at hand and not letting the interview wander to a point where the client is giving the advice and opinion rather than receiving it.
2. Be affable and polite. Clients respond to friendly treatment and a smile.
3. Get at the client's problem immediately and stick to it. Don't bother to explain the reasoning processes by which you arrive at your advice. The client expects you to be an expert. This not only prolongs the interview, but generally confuses the client. The client will feel better and more secure if told in simple straightforward language what to do and how to do it, without an explanation of *how* you reached your conclusions.
4. Do something for your client, if possible. Perhaps a quick telephone call can be made or and on-the-spot letter dictated. A lawyer is trained in these matters and can do them easily.

\* R.M.-11 (Sept. 11, 1959). Reprinted by permission of the Wisconsin Bar Association.

Many times a lawyer can do in minutes a job which would require hours of time and effort of the client.

5. Take every opportunity to explain in simple language the functions of various courts and the basic rules of conduct and procedure in the courts. If the client understands that not all cases are matters for long, formal jury trials, he will approach the law with less fear and trembling. The client should know that most legal matters never result in trial and that most court procedures are rather informal in nature. It is natural for persons to be more at ease when dealing with matters with which they have some basic knowledge. Therefore, for the client's peace of mind, he should be made familiar with the basic workings of the court in which his matter is being handled and of his own role in the proceedings if he is required to make an appearance in court.

6. Quit when you're ahead. When you have reached the conclusion of the conference, break it off clearly. Otherwise it will drag on as new themes and situations develop. Tactfully but clearly wind up the conference and usher the client out.

c.  
**Raymond S. Duff and August B. Hollingshead**  
**Sickness and Society\***

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When an individual becomes a patient he is confronted with the necessity of dealing with a physician or physicians. The association brought into existence by the mutual interest of the patient and his physician in the illness—the doctor-patient relationship—is symbolized by *sponsorship*, the method through which the physician assumes responsibility and discharges obligations to care for the patient. Sponsorship is believed to have its roots in the mutual images and expectations of patients and physicians. Ideally, the doctor-patient relationship revolves around only the sick or troubled person who is a client (patient) and the professional person (physician) whom he consults. Crucial decisions regarding diagnosis and treatment of disease should not be influenced by extraneous matters. However, physicians and patients are inextricably parts of the

society to which both belong. The broader social contexts of the patient and his family, the physician and his family, the hospital, insurance companies, the institutions of government, religion, education, and so on are related in one way or another with the interactions that take place between physicians and patients. The sick person looks to the physician for advice and help. The physician, in performing his professional role, is concerned directly with the problem the patient brings to him and indirectly with his own interests: his practice, possibly learning or research, or some combination of these pursuits. To a certain extent, therefore, each partner in this relationship must look after his own interests. The patient has an illness: the physician has a career. Each partner must make tentative judgments about the other. The patient accepts the competence of the physician to diagnose and treat his problem, and the physician accepts the sponsorship of the patient.

Reciprocal expectations between patient and physician were based upon the respective images which patients and physicians had of each other. Ward patients recognized their low status and expected to be used as teaching subjects and, to some extent, as research material. The doctors reciprocated these expectations and offered little apology since these patients, though being charged for their hospital services were not being billed for their medical care.[\*] The semiprivate and private patients did not expect to be used as research subjects. Furthermore, the semiprivate and private patients were fully aware that they had physician sponsors who were responsible to them for the diagnosis and treatment of, and information, about their diseases. They expected to pay their physicians directly

[\*] Persons who were housed on the ward accommodations seldom realized initially that they had to endure the indignities of ward treatment in order to benefit from the advances of medical technology which were probably developed in the first place on some hospital ward. They resented being passed over by powerful professors in the School of Medicine who moved around them on the wards, treated them as clinical material, and "talked their Latin" as they discussed the case with the medical students and the house staff in front of the patients. These patients and their families knew that a gulf of social distance separated them from the important figures inside and outside the hospital. They did not fully understand what went on around them, but they had an awareness of their ignorance and they were humiliated at the treatment they had to endure to get medical attention [p. 119].

\* New York: Harper & Row 124-134, 140-145 (1968). Copyright © 1968 by Raymond S. Duff and August B. Hollingshead. Reprinted by permission of Harper & Row, Publishers, Inc.



for the personal services they had received from them.

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#### *Committee Sponsorship*

One of the resident physicians used the term "committee doctor" when describing the physician-patient relationship in the ward accommodations. This gave us our cue for the label used here. *Committee sponsorship* was found to apply to all ward patients and to no others. . . . The patient in committee sponsorship had no single continuing doctor on whom he could depend. Care of the patient was vested in the always-present and ever-changing committee whose members—doctors at the house-staff and medical-student levels—rotated within the institution during their training years and then, in most instances, left it. These physicians and students identified professionally with the School of Medicine and looked to it primarily for aid in the development of their careers. They were most impressed and influenced by teachers and researchers who necessarily had little time for patient care and whose major interest was new knowledge through research. Learning was the overriding consideration.

In the School of Medicine, diseases were the necessary "clinical material" for the teaching and research program. In a conference room in the Medical Center, used for teaching students and house officers, a sign posted above a blackboard admonishes: "Think Pathology!" The hospital was the place in which the students and house officers saw patients. The patient was the vehicle for the study of disease. To be sure, these young physicians realized that the problems of patients were great; they realized the chief task of a physician is to treat disease, but they were students who had to learn how to become physicians before they could treat people. When we asked them about the patients, they usually knew the nature of the patient's disease and something about his ongoing treatment but they knew little about the patients as human beings; thus, our questions embarrassed them. Some told us we were choosing the less ill patients who required less time to care for or that they had been assigned to the patient in question for only a few days and had had little contact with him. One intern was more precise in his views; he ended a rather nonproductive interview with this comment: "I cannot answer your questions. You're interested in patients. I'm interested in the disease in the body in the bed."

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Mrs. O'Pell, a 56-year-old woman of Irish birth, was selected to illustrate some facets of committee sponsorship. Mrs. O'Pell had been in reasonably good health most of her life except for her nervousness and hypochondriacal tendencies. She did not trust the general practitioners, whom she saw for these complaints, to deal with her more complicated problems. For these she went to the clinic. She considered the clinic and ward accommodations as places where poor people get care. She viewed the doctors as being much above and distant from her, and she communicated poorly with them; they responded in general by ignoring her as a person.

One week prior to her admission she had some teeth extracted under general anesthesia in this hospital. The day following the procedure, she developed pains in her chest and a sore throat. She visited the Emergency Room where some X rays were taken and "pain killers" prescribed. Still feeling ill the next day, she came again to the Emergency Room and was sent to the medical clinic for further examinations. She was sent home again although the chest pain was not alleviated. She returned the following day to the medical clinic and decided to "sit them out". . . . Finally, a doctor did see her and decided to admit her to the hospital.

Up to this point, Mrs. O'Pell had been seen by two dentists, four medical students, two interns, and one assistant resident, all of whom she viewed as inexperienced students. She attributed her illness to improper anesthesia and poor dental technique of a "young student, that Puerto Rican dentist." When she was admitted to the hospital she was seen by three more members of the committee—a medical student, an intern, and an assistant resident. The medical student spent more time with her than the others so she felt that he was her physician more than anyone else. When he left for the weekend she was worried, but she assumed that he had left orders to be carried out in his absence. The student, however, spent little time with her, knew nothing about her family, and felt he did not understand her situation well. The family, while visiting and through telephone calls to the hospital, tried to get information by seeking a doctor to answer their questions. They never succeeded in finding a doctor who would listen to them.

Mrs. O'Pell realized the doctors were teaching one another and learning on her. In her opinion the pelvic examination was "quite a production." The doctors talked to one another but not



to her. Three thoracenteses were done in just this way—one doctor teaching another. She said that one doctor seemed to be especially knowledgeable about "needles," while the other one seemed to know more about examining her. . . .

Shortly after her admission to the hospital, the resident in radiology concluded from his study of the X ray that the problem was pulmonary embolus. The intern reported this diagnosis to Mrs. O'Pell explaining that a clot which had probably formed in her leg had broken off and lodged in her lungs. Although she did not tell the doctors, she was extremely frightened by this diagnosis because a close friend had died suddenly and unexpectedly of this condition.

After Mrs. O'Pell was in the hospital two days, the attending physician (a faculty member in the School of Medicine), who was introduced to her as an outstanding international authority on diseases of her kind, visited her. He examined her briefly, listened to her history, and informed her that the doctors were doing the "right things" for her. He left before Mrs. O'Pell could ask him any questions. When she asked the medical students and the intern again what her diagnosis really was, they said she had an embolus and that the attending physician concurred with their diagnosis and treatment. In reality, the attending physician thought the cause of the illness was not embolus but aspiration during the dental extraction while under general anesthesia.

\* \* \*

After discharge from the hospital, Mrs. O'Pell consulted a general practitioner for his interpretation of the cause of her illness. He examined her legs carefully, found no evidence of phlebitis, and said that embolization probably did not account for the illness. This was not reassuring to Mrs. O'Pell because, although she appreciated his interest in her, she discounted his competence. She then saw another physician who told her that she was very lucky that the clot had struck her lungs instead of her heart or her brain because then she would not have survived. Thinking of illness and death, Mrs. O'Pell decided to visit Ireland, the land of her birth, while she was still able to travel. . . .

From the viewpoint of the doctors, Mrs. O'Pell was a model patient, cooperative and forbearing. She made no demands and seemed to accept without any major challenge their explanation of her illness. She responded dramatically to treatment which was most gratifying to all, and finally she was discharged as "cured" from the

medical clinic. The doctors felt that they had learned much while practicing splendid medicine. They knew nothing of Mrs. O'Pell's doubts and fears about her illness. Although the pulmonary disease was treated successfully regardless of its nature, the management of this patient did involve some risks of treatment (anticoagulant therapy) which she almost certainly did not require and which was costly to her and her family. The management by the physicians had the effect of increasing the severity of her hypochondriasis; she went to more doctors, incurring higher costs, and her fears were never successfully relieved.

\* \* \*

We found many handicaps to a cooperative relationship between doctors and patients in the committee sponsorship. The patients viewed themselves as uneducated and incapable of understanding the explanations of the professionals; patients often referred to the doctors "talking among themselves" and using "their Latin" or "their Greek." Most patients believed the doctors withheld information from them. Several patients told us: "Doctors have their secrets." The patients were embarrassed by their ignorance and remained silent to avoid exposure. Thus, they failed to put their questions to the doctors although they asked such questions of family members, our data collectors, and sometimes the nurses if the latter would listen. (They did not often question the nurses, however, because they thought the nurses gave them little encouragement and were probably incapable of answering them.)

These patients lacked the standing to make demands upon their physicians. They expected to be cared for by interns and medical students, yet they were resentful of being "pushed around" and ignored as individuals. Some realized that inconvenience, discomfort, and at times higher costs for decisions made in the interest of teaching and research, rather than in the interest of the patient, were necessary in order for them to get the benefits of service within the ward accommodations.

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The unsatisfactory interpersonal relationships between sick patients and student physicians in committee sponsorship had pervasive consequences. Sometimes diagnoses were less than adequate. Patients were poorly informed and often confused. Even if the treatment was

successful, the patients resented the attitudes of the hospital staff: student physicians, nurses, nursing students, aides, and so on. When the treatment was not successful, as was often the case among these very sick persons, their suspicions increased sharply. Weakened and frightened, they had to accept what was offered to them; there was almost no alternative choice. Both sponsor and patient recognized the distance between them; neither was comfortable with it, but only the patient had to endure it without hope for ameliorative change.

In summary, interest in the patient as a human being, though present occasionally in exemplary ways, could not be sustained when there was so little in common between the providers and recipients of service and when the patient was in no position to pay for the physician's time and hence make demands upon him. Interest in the disease, lack of interest in the patient, and difficulty in communication characterized the ward accommodation in which the formal learning of medical students, house officers, and senior physicians took place. The impact of these influences was noted for all ward patients.

#### *Semicommittee Sponsorship*

Semicommittee sponsorship involved a private physician nominally, but a large segment of responsibility for the diagnosis and treatment of the disease, as well as communication with the patient, was assumed by house officers. The private physician admitted the patient to the hospital and may have influenced the diagnostic and treatment choices of the timing of discharge, but he was not close to the patient or his care. The patients, the house staff, and the private physicians recognized this pattern of sponsorship. Private physicians in semicommittee sponsorship were often general practitioners, very busy internists, or surgeons who spent little time with their patients. These physicians often exhibited a lesser competence in diagnosis and treatment procedures, particularly on the medical service, than others described in the casual and committed sponsorships.

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#### *Committed Sponsorship*

Committed sponsorship involved a determined assumption of responsibility for the patient by the physician. The physician's interest in the patient extended beyond his interest in the disease. These patients were of higher social sta-

tus and some of them treated their physicians as subordinates. The physicians showed their interest and assumed responsibility accordingly. . . .

Dr. Sail, a successful practitioner of medicine, is an example of the optimal complex of medical and social relations characteristic of committed sponsorship. He developed a hernia and was admitted by a surgeon for treatment. These two physicians had dealt with each other professionally about patients of mutual interest and had met frequently on social occasions, but they were not close friends. Dr. Sail was admitted at his convenience and placed on the operating schedule so he would get fresh staff and the first choice of anesthesiologists. The surgeon saw to it that the admissions office, the floor nurses, the house staff, the operating room staff, and the anesthesiologists were informed of the admission of his special patient. Subsequent to the operation, the surgeon visited him frequently in his room, explained all treatments that were given, and supervised them diligently. Dr. Sail was given more attention than he wanted or needed by the nurses on the floor as well as by private-duty nurses caring for other patients. (These nurses either had been his patients or had cared for his patients at another time.) Mrs. Sail was kept informed of the progress of the surgery and the course of her husband's treatment. Dr. Sail's recovery was rapid and uneventful.

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Although committed sponsorship was almost always dictated and enjoyed by the patients, it was not always an enlightened relationship which served the patients' best health interests. For example, Mrs. Leadon was an anxious, emotionally unstable individual who had sought out the services of many physicians. In the year prior to her admission she managed to persuade the physicians to do a gastrointestinal series, two breast biopsies, and several examinations. One physician who saw her reported that her cysts were benign and, although they should be checked occasionally for any suspicious changes, they should be left alone. She was dissatisfied with his evaluation and went to see a doctor who had removed a breast cancer for a friend of hers in the recent past. She cultivated the doctor and he seemed to enjoy her company. He admitted her to the hospital for still another biopsy.

In her shopping for physicians, Mrs. Leadon revealed enough about herself to indicate

that she needed medical care but never enough so that the physicians became aware that her behavior was a manifestation of an emotional disturbance. She enjoyed the physicians' examinations and discussed them freely. She told us one physician was a friend who "lives near by and does not examine me internally because he knows me too well and gets embarrassed." Mrs. Leadon enjoyed being seductive with her friends and particularly with her physicians. She not only demanded examinations but further operations. In the hospital her performance was childlike, exhibitionistic, and seductive. Although she made the most of her opportunity to perform, she was frightened of the anesthesia and surgery. She felt, however, that it was a necessary price she had to pay to be relieved, even temporarily, from the continuous adversities of her troubled life at home. The physician, in admitting her to the hospital, provided removal from the troubled home atmosphere, interest as a physician and a male, and dramatic validation of the sickness as evidenced by further surgery.

The surgical resident who saw Mrs. Leadon at admission was unaware of any of this background. He casually commented that he did not think another breast biopsy was really necessary at that time. Mrs. Leadon was infuriated; she told him to leave her room and not return. Meanwhile, her surgeon, a private practitioner, came in daily, held hands with her for a few minutes, and listened to her chat. She exhibited herself in varying ways to him, which he seemed to enjoy. Near the end of the hospitalization when she discovered the breast scar was longer than she had expected and when she was feeling very depressed at having to leave the hospital to return home, her surgeon visited and discovered her weepy mood. He put his arm around her shoulder and she immediately began to smile. He said, "You're happy; you're cheerful; you're content. We should do this more often." He joked with her for a few minutes and then left. Mrs. Leadon concluded that her surgeon was "very nice."

In talking to us Mrs. Leadon described herself as being "kind of nutty" or "mental." She discussed her favorite movie actors, all of whom were paragons of masculinity, and also she described her attraction to "powerful movies like those with miracles and the fantastic." She was extremely anxious about instruction for her children in sexual matters, courtship, and so on. She indicated her pleasure with her surgeon because

she felt he had agreed to admit her for further breast biopsies at her desire. She thought this was what he meant when he said, "We should do this more often."

Mrs. Leadon's committed sponsorship was a continuing and close one, but we have to ask *to what it was committed*. The physician failed to perceive the main problem, or he chose to ignore it. He appeared to be concerned primarily with pleasing and enjoying his appealing, seductive patient and perhaps, in part, with the promotion of his practice of surgery.

Physician-patient relationships for six of the twenty-three private patients involved in committed sponsorships were similar to the interactions that prevailed between Mrs. Leadon and her physicians. The physicians were drawn into these sponsorships by powerful patients who communicated selectively and in some instances dictated diagnosis or therapy or both. Committed sponsorship, though very favorable for some patients, was not favorable for others. It may have promoted the practices of physicians and led to at least superficial satisfaction of patients but, as we have indicated in the example of Mrs. Leadon, the relationships were often less than therapeutic.

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[C]ommitted sponsorship offered the best opportunity for the physician and patient to join in the diagnosis and treatment of health problems. Patients and physicians in general were pleased with their relationship, but in the majority of cases committed sponsorship simply made the technical aspects of patient care more tolerable, if not pleasant, for the patients and physicians, while about 25 per cent of the committed sponsorships constituted a threat to the patient. We wish to state clearly that the *physician by himself* was never found to be entirely responsible for this threat; it is equally clear at the same time that the *patient by himself* was not fully responsible. This type of sponsorship, in which the patient demanded and paid for the physician's time, was open to influences of the patient or physician, or both, which focused on the importance of pleasantries and good manners as opposed to solving the patient's problems. In only one of the 24 cases was this sponsorship an exemplary one, devoted to the task of professional management of both technical and personal aspect of patient care. The influences and fear of mental illness, plus the narrow training and cor-

respondingly narrow framework of medical practice, may have contributed largely to these disturbing situations. Superficially, we found little to suggest that the patients or their physicians understood these dilemmas or wanted a change in their relationships. However, in the more problematic relationships, the deeper feelings of patients indicated dissatisfaction and annoyance. They knew something was not right but they kept their feelings to themselves. They did not communicate their dissatisfaction to their spouses or to their physicians.

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**L. J. Henderson**  
**Physician and Patient as a Social System\***

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. . . A patient sitting in your office, facing you, is rarely in a favorable state of mind to appreciate the precise significance of a logical statement, and it is in general not merely difficult but quite impossible for him to perceive the precise meaning of a train of thought. It is also out of the question that the physician should convey what he desires to convey to the patient, if he follows the practice of blurting out just what comes into his mind. The patient is moved by fears and by many other sentiments, and these, together with reason, are being modified by the doctor's words and phrases, by his manner and expression. This generalization appears to me to be as well founded as the generalizations of physical science.

If so far I am right, I think it is fair to set up a precept that follows from all this as a rule of conduct: The physician should see to it that the patient's sentiments do not act upon his sentiments and, above all, do not thereby modify his behavior, and he should endeavor to act upon the patient's sentiments according to a well-considered plan. . . .

However, in this case the application of science to practice is peculiarly difficult. If I am to speak about it, I must in the first place beg explicitly to disclaim any skill of my own. . . . Accordingly, what I am now to say to you is, in the main, second-hand knowledge that I have cribbed from others. It represents, so far as I can understand what I have seen and heard, the soundest judgment, based upon experience, skillful per-

formance and clear analysis in this field. In order to be brief and clear, I shall permit myself the luxury of plain assertion.

In talking with the patient, the doctor must not only appear to be, but must be, really interested in what the patient says. He must not suggest or imply judgments of value or of morals concerning the patient's report to him or concerning the patient's behavior. (To this there is one exception: When the patient successfully presents a difficult objective report of his experiences, it is useful to praise him for doing well what it is necessary that he should do in order to help the physician to help him.) In all those matters that concern the psychological aspects of the patient's experience few questions should be asked and, above all, no leading questions. There should be no argument about the prejudices of the patient, for, at any stage, when you are endeavoring to evoke the subjective aspect of the patient's experience or to modify his sentiments, logic will not avail. In order to modify the sentiments of the patient, your logical analysis must somehow be transformed into the appropriate change of the patient's sentiments. But sentiments are resistant to change. For this reason, you must so far as possible utilize some part of the sentiments that the patient has in order to modify his subjective attitude.

When you talk with the patient, you should listen, first, for what he wants to tell, secondly, for what he does not want to tell, thirdly, for what he cannot tell. He does not want to tell things the telling of which is shameful or painful. He cannot tell you his implicit assumptions that are unknown to him, such as the assumption that all action not perfectly good is bad, such as the assumption that everything that is not perfectly successful is failure, such as the assumption that everything that is not perfectly safe is dangerous. We are all of us subject to errors of this kind, to the assumption that quantitative differences are qualitative. Perhaps the commonest false dichotomy of the hypochondriac is the last of those that I have just mentioned: the assumption that everything not perfectly safe is dangerous.

When you listen for what the patient does not want to tell and for what he cannot tell, you must take especial note of his omissions, for it is the things that he fails to say that correspond to what he does not want to say plus what he cannot say. . . .

[B]eware of your own arbitrary assumptions. Beware of the expression of your own feel-

\* 212 *New England Journal of Medicine* 819, 821-823 (1935). Reprinted by permission.

ings. In general, both are likely to be harmful, or at least irrelevant, except as they are used to encourage and to cheer the patient. Beware of the expression of moral judgments. Beware of bare statements, of bare truth, or bare logic. Remember especially that the principal effect of a sentence of confinement or of death is an emotional effect, and that the patient will eagerly scrutinize and rationalize what you say, that he will carry it away with him, that he will turn your phrases over and over in his mind, seeking persistently for shades of meaning that you never thought of. Try to remember how as a very young man you have similarly scrutinized for non-existent meaning the casual phrases of those whom you have admired, or respected, or loved.

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### NOTES

#### NOTE 1.

ELIOT FRIEDSON

#### PATIENTS' VIEWS OF MEDICAL PRACTICE\*

. . . In the Family Health Maintenance Demonstration many patients would not accept the services of the social worker in spite of their need and the recommendation of the physician and nurse. In the Montefiore Hospital Medical Group a sizable proportion of patients chose to avoid services to which they were entitled by contract. A lesser but nonetheless important proportion of Demonstration patients used outside services even when they were enrolled in a program with which they expressed overwhelming general satisfaction. Analysis indicated that the patient rejected professional services when they did not fit into his scheme of things—when they were isolated from the steps he goes through in seeking help, when they contradicted his own and his lay consultants' conception of illness and treatment, when they were insulated from the way by which he and his lay consultants try to establish their reliability, and when they required him to sacrifice personal convenience. The professional expects patients to accept what he recommends on his terms; patients seek services on their own terms. In that each seeks to gain his own terms, there is conflict.

How typical of the doctor-patient relationship is conflict? The profession itself contends, as Hughes observed, "that there is no conflict of

interest or perspective between professional and client—or at least . . . none between the good professional and the good client." It may be that the professionals of the Demonstration and the Medical Group are not all they should be, but they all have excellent credentials and those observed at work seemed to possess admirable skill and conscientiousness. It may also be that patients in The Bronx are unusually demanding and arrogant, but, except for one or two, those interviewed seemed to have only the best of intentions. It is quite likely that the particular situation studied stimulated more overt conflict than is present in other situations, but the nature of the conflict itself did not seem unusual.

Struggle between patient and doctor seems to have gone on throughout recorded history. Almost 2500 years ago, the Hippocratic corpus collected doctors' complaints about the nonprofessional criteria that people used to select their physicians, criticism of patients for insisting on "out of the way and doubtful remedies" or on overconventional remedies like "barley water, wine and hydromel," and for disobeying the doctor's orders.

The patients who have left us documents often treat the physician as a potential danger to which one must respond cautiously and whom one must always be ready to evade. Patients have circulated stories about the occasions on which they successfully cured themselves, or continued to live for a long time in defiance of medical prognoses. This sort of literature may be represented by the Roman "epigram about a doctor Marcus who touched a statue of Zeus, and although Zeus was made of stone he nevertheless died," and by Benvenuto Cellini's little story:

I put myself once more under doctor's orders, and attended to their directions, but grew worse. When fever fell upon me, I resolved on having recourse again to the wood: but the doctors forbade it, saying that if I took it with the fever on me, I should not have a week to live. However, I made my mind up to disobey their orders, observed the same diet as I had formerly adopted, and after drinking the decoction four days, was wholly rid of fever. . . . After fifty days my health was re-established.

Physicians have left us instructive essays on "decorum"—practical guides to the physician for managing his relations with the patient in such a way that threats to his authority are minimized. . . .

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Struggle between physician and patient has not been restricted to times past. [C]ontempo-

\* New York: Russell Sage Foundation 171-175, 177-178, 180-181, 183 (1961). Reprinted by permission.

rary studies . . . reveal that elsewhere, as in The Bronx, patients do not always do what physicians tell them to do. They persist in diagnosing and dosing themselves and in assigning great weight to lay advice and their own personal dispositions. It is difficult to get them to cooperate wholly with health programs that, professionals believe, are for their own good.

That the problem continues is somewhat paradoxical, for it seems unquestionable that the medical practitioner has reached an all-time peak of prestige and authority in the eyes of the public. The physician of today is an essentially new kind of professional whose scientific body of knowledge and occupational freedom are quite recent acquisitions. His knowledge is now far more precise and effective than it has ever been in the past, since for the first time it could be said that from "about the year 1910 or 1912 . . . [in the United States] a random patient with a random disease consulting a doctor chosen at random stood better than a 50-50 chance of benefiting from the encounter." The physician has obtained unrivaled power to control his own practice and the affairs that impinge upon it, and the patient now has severely limited access to drugs for self-treatment and to nonmedical practitioners for alternative treatment. But the ancient problem continues.

\* \* \*

What also happens is that more of reality than proves to be appropriate tends to be subsumed under the ordinary and commonly used categories. This again seems to be in the very nature of professional practice—if *most* patients have upper-respiratory infections when they complain of sneezing, sounds in the head, a running nose and fatigue, then an upper-respiratory infection is probably involved when *one* particular person makes the complaint. It could, indeed, be an allergy or even approaching deafness, but it is not probable—that is to say, it was not commonly the case in the past. The physician cannot do otherwise than make such assumptions, but by the statistical nature of the case he cannot help being wrong sometimes.

These problems of diagnosis are not only problems for the doctor but for the patient as well. All the patient knows is what he feels and what he has heard. He feels terrible, his doctor tells him that there's nothing to worry about, and a friend tells him about someone who felt the same way and dropped dead as he was leaving the consulting-room with a clean bill of health. For the patient the problem is, When are subjec-

tive sensations so reliable that one should insist on special attention, and when can one reasonably allow them to be waved away as tangential, ordinary and unimportant; when is the doctor mistaken? The answer to these questions is never definite for any individual case, and indeed cannot be resolved decisively except by subsequent events. All of us know of events that have contradicted the judgment of the physician, and, of course, many others that have contradicted the patient.

The situation of consultation thus proves to involve ambiguities that provide grounds for doubt by the patient. Furthermore, those ambiguities are objective. Most reasonable people will agree that the doctor is sometimes wrong, whether by virtue of overlooking the signs that convert an ordinary-appearing case into a special case or by virtue of the deficiencies of the knowledge of his time. He is less often wrong now than he was a hundred years ago, but frequency is not really the question for the individual. Even if failure occurs once in ten thousand cases, the question for the patient is whether it is he who is to be that one case, a question that no one can answer in advance. If the evidence of his senses and the evidence of his knowledge and that of his intimate consultants are contradicted by the physician, the patient may understandably feel it prudent to seek another physician or to evade the prescriptions he has already obtained.

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[T]he well-educated patient in The Bronx [fairly well versed in modern medicine, on occasion cooperates] admirably with the physician, but on occasion he is also quite active in evaluating the physician on the basis of his own knowledge and "shopping around" for diagnoses or prescriptions consonant with his knowledge. He is more confident and cooperative in routine situations, perhaps, but he is also more confident of his own ability to judge the physician and dispose himself accordingly. A less-educated patient may be far more manageable.

The dilemma in patient education is now clear. When he lacks health education the prospective patient is unlikely to seek the aid of a professional consultant, and he is unable to give the doctor a history or cooperate with the treatment. When he is well educated the prospective patient is confident of his ability to treat himself "scientifically," and when he sees a doctor he feels more confident of his own ability to judge the doctor's services.

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. . . In The Bronx questions arose when the consultant did not act as he was expected to, when the diagnosis seemed implausible, when the prescription seemed intolerable and unnecessary, and when "cure" was slow or imperceptible. They became pressing when the problem of consultation assumed what seemed to be serious proportions. What was needed to sustain the relationship was a stronger sort of confidence than supported initial consultation.

It may be that this stronger sort of confidence is in the minds of those who make a special connection between professions and client confidence. Certainly it is true that three of the old, established professions deal with some of the most anxiety-laden topics of existence—the body, the soul, human relations, and property. Anxiety inherent in those topics, a stronger confidence is required for entrusting oneself to doctors, clergymen, and lawyers than to plumbers, piano-tuners, and fitting-room tailors. However, we have enough evidence from The Bronx study to know that in the early stages of illness there is not enough anxiety even to motivate search for professional help. One first tries tinkering with his piano himself before deciding to call in a professional tuner; one first tries tinkering with his organs himself before calling in a doctor. In the later stages of illness, when anxiety does occur, it can as well interfere with as sustain confidence. Consultants with professional standing thus *claim* confidence, but do not necessarily get it.

#### NOTE 2.

MEDICAL AUDIT UNIT OF THE TEAMSTER  
CENTER PROGRAM  
A STUDY OF THE QUALITY OF HOSPITAL CARE  
SECURED BY A SAMPLE OF TEAMSTER  
FAMILY MEMBERS IN NEW YORK CITY\*

\* \* \*

The present report is a study of the quality of medical care received by . . . a sample of patients who had a claim paid by Blue Cross in May 1962 to hospitals in New York City. . . .

Based upon written consents, photostatic copies of the hospital records of 78 per cent of the original sample were obtained. The records were reviewed by thirteen clinicians with recognized professional standing in their specialties.

\* M. A. Morehead, M.D., M.P.H., Director. New York: Columbia University School of Public Health and Administrative Medicine 7-8 (1964). Reprinted by permission.

The surveyors were asked to judge on the basis of their knowledge and experience the quality of the medical care rendered. [I]n order to assess the reproducibility of evaluation results, two surveyors independently reviewed each record. . . .

. . . Sixteen per cent of the admissions were to voluntary hospitals affiliated with medical schools, 40 per cent to voluntary hospitals with programs approved by the American Medical Association for the training of interns and/or residents. Fourteen per cent were to the smaller voluntary hospitals without approved training programs, and 24 per cent were to proprietary hospitals. Six per cent of the admissions were to municipal hospitals.

Eighteen per cent of the admissions were to the ward services of voluntary or municipal hospitals where the patients were under the care of house staff. . . .

\* \* \*

In the opinion of the reviewing surveyors, only 57 per cent of the care given in the total of all admissions reviewed represented "optimal" medical care; 43 per cent of the care was believed to have been performed in a "less than optimal" fashion when viewed in light of the standards of present day medical practice.

There was variation by specialty in the proportion of medical care considered as "optimal"—obstetrics/gynecology, 80 per cent; general surgery, 57 per cent; pediatrics, 43 per cent; and general medicine, only 31 per cent. The other specialty areas all had a higher proportion of care considered "optimal." The handling of the ophthalmology cases was particularly outstanding; the orthopedic cases were also felt to have received a very satisfactory level of medical care.

\* \* \*

When the findings of the quality of medical care were examined in relation to the type of hospital, the qualifications of the physicians and the type of case, it was found that the highest proportion of medical care judged as "optimal" was provided by the voluntary hospitals affiliated with medical schools, regardless of the recorded qualifications of the physicians or the type of case. Eighty-six per cent of such admissions were considered to have received "optimal" medical care. The "optimal" performance ratings decreased among the different hospital classifications so that less than half (47 per cent) of the care given in the proprietary hospitals was considered "optimal."

Sixty-six per cent of the admissions under



the care of house staff were judged as having received "optimal" care. The "optimal" performance ratings decreased by the other three classifications of physicians . . .—Class I, 65 per cent; Class II, 52 per cent; Class III, 33 per cent.

The principal reason that medical care was considered "less than optimal" was the failure to adequately determine the cause of the patients' presenting symptoms so that rational, as opposed to symptomatic, therapy could be given. Preoperative management and the techniques of the surgical procedure were important reasons in the admissions where surgery was performed and for whom care was judged "less than optimal."

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#### NOTE 3.

CHARLES S. BRANT AND BERNARD KUTNER  
PHYSICIAN-PATIENT RELATIONS IN A TEACHING  
HOSPITAL\*

\* \* \*

The study was limited to the surgical service. . . For this type of patient, hospitalization is experienced as a radical disjunction from deeply ingrained habits, life patterns and expectations.

The study procedures consisted of personal interviews with fifty patients, unselected except for diagnosis, conducted both pre-operatively and post-operatively. . . The interviews were aimed at elicitation of feelings about hospitalization, knowledge of and attitudes toward diagnosis and contemplated surgery, and understanding of the work of physicians and nurses. A large portion of the surgical residents were also interviewed for the purpose of learning their views and attitudes concerning relationships with patients and the role of the physician in the other than purely technical aspects of patient care. A sample of the graduate nurses on the surgical service were interviewed in order to gain insight into their perceptions of the psycho-social needs of patients and to obtain their observations and impressions as to how these needs are manifested by patients, and how they are managed by physicians and nurses. Finally, direct observations were made of physician-patient interaction in treatment situations at the bedside and during ward rounds.

Some principal findings of the study may be summarized as follows:

1. Despite the acknowledged desirability of

such a procedure, rarely do the house staff physician and the patient meet in a private, unhurried conference to discuss the diagnosis and the plan of therapy. Usually, the physician informs the patient at the bedside briefly and in general terms that an operation on a given organ is necessary because it is diseased or is not functioning properly. Signed consent for the procedure is then requested.

2. Patients seldom attempt to ask the house staff physicians about the impending surgery at the time they are informed of it, yet, many patients admit that questions come to mind which they want to ask. The tendency is very common for the patient to feel that he should not "bother the doctor" by asking questions. House staff physicians interpret the silence and passivity of patients as meaning that the patients have no immediate problems, and, therefore, they see no need to elicit from patients whatever questions may be troubling them.

3. Experienced graduate nurses on the surgical wards tend to agree that patients frequently feel isolated, are often in a state of anxious uncertainty about their conditions and do not understand the events occurring in the course of their hospitalization. Patients frequently put questions to nurses about diagnosis, prognosis, tests and medications which nurses feel they are not qualified to answer, or which they feel, as nurses, they are not free to answer. When they refer these questions to house staff physicians, in some instances nurses are requested to answer the patient's questions themselves; in others, the physician may indicate he will do so himself. In the latter situation, the intention is sometimes forgotten, owing to the pace and multiplicity of tasks in the house officer's total schedule of work, and his common tendency to give low priority to talking with patients.

4. The paucity and infrequency of communication from the professional personnel about their illnesses, therapy and impending events leads patients sometimes to acquire misinformation and misinterpretations by asking questions of other patients who have seemingly similar illnesses or have had apparently similar surgery.

\* \* \*

6. Patients rarely know in advance of normal, predictable post-operative events such as the routine stay in the recovery room following surgery and preceding return to the ward, the expectation of some pain at the operative site for a time or the necessity of early ambulation. Some

\* 32 *Journal of Medical Education* 703-707 (1957). Reprinted by permission.



patients presume that the total time off the ward was spent in the operating room in a lengthy, extensive and difficult procedure, misinterpret post-operative pain as surgical failure, and regard the effort of the nurse or aide to have them leave their bed a few days post-operatively as callousness if not sadism.

7. In amputation of the lower extremities due to peripheral vascular diseases, the patient seldom acquires a thorough understanding (to the limits of his individual ability to understand, of course) of the compelling necessity of this drastic procedure. His normal anxieties concerning the operation and his probable future adjustments do not usually undergo thorough discussion with the house staff physician. Few amputees who possess good rehabilitation potential acquire this hopeful information pre-operatively. Seldom are they made aware before operation, of the time, effort and services available to deal with re-ambulation, rehabilitation and prostheses.

8. In general, house staff physicians on the surgical service do not often conceive of the physician-patient relationship as an integral, important part of their role. There is little agreement among them concerning the communicative aspect of their relation to the surgical patient, and a tendency to view this as quite incidental and peripheral to their "real" concerns. Some house staff physicians believe that the teaching hospital does not provide the proper setting or amount of time for developing their relations with patients, but that once they enter private practice this phase of their work will develop naturally or spontaneously.

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#### NOTE 4.

D. OGSTON AND G. M. MCANDREW  
ATTITUDES OF PATIENTS TO  
CLINICAL TEACHING\*

Few clinical teachers would dispute the value of repeated personal contact with patients in the training of medical students. . . . In view of the paucity of objective information about the attitudes of patients to their use in clinical teaching, this study was undertaken to obtain the views of an unselected group of patients who had been used for teaching purposes in the general medical wards of a teaching hospital.

The hospital in which this study was carried

out (Aberdeen Royal Infirmary) belongs to a group serving a population of about 500,000. The wards are used for the clinical instruction of medical students attending the University of Aberdeen. There is no non-teaching hospital of comparable size in the region.

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At the time of this survey no formal notification of the possibility of clinical teaching was given to patients before admission; this is now included in the admission booklet issued to patients.

The normal practice is the allocation of one or two students to a patient for one hour to obtain a history and carry out a physical examination followed by an hour in which one patient is selected for presentation and discussion with four to 10 students and a member of the teaching staff. This normally takes place at the bedside. Students attend the wards for such clinical instruction 5 days in the week.

The medical students were in the third to sixth year of the medical curriculum. Twenty-five of the 94 students who had spent time in the wards during the period of this study were women, and nine were coloured. The number of students by whom each patient was examined varied from one to 12 (mean 3.4).

\* \* \*

Forty-three patients out of the 100 interviewed had not realized that they might be seen by students during their stay in hospital. Thirty-seven of the remaining 57 patients expected to be seen by students because of their experience in previous admissions to the same hospital. The remaining 20 patients had heard of the presence of students from relatives or friends. One patient had, in addition, heard a discussion about patients' attitudes to medical students on the radio.

With a single exception all the patients considered that it was reasonable that students be allowed to examine them during their stay in hospital; 40 specifically volunteered the comment that 'students must learn.' Qualifying comments included: 'if well enough' (four patients), 'not too often,' and 'if permission asked first.'

\* \* \*

Seventy-three per cent of the patients interviewed did not know of their right to refuse to be examined by students, but only one patient said that he would have refused if he had known of this right on admission. Eleven of the patients who were unaware that they could refuse con-

\* 1 *British Journal of Medical Education* 316-319 (1967). Reprinted by permission.

sidered that refusal would prejudice the medical staff against them.

Eight patients (five women and three men) declined to be seen by students on at least one occasion. The reason given by six patients was that they felt unwell, one patient did not like the manner of a student, and one patient finally refused after being seen by six students over the first 14 days of his admission. This patient indicated that he was unwilling to help medical students to complete their medical training because they would then emigrate. Six of the eight patients were unaware of their right to refuse and their refusal took the form of a request to their allocated student not to be examined on a particular day.

\* \* \*

Although our findings indicate that patients accept the need for their participation in the clinical training of medical students and that the great majority cooperate willingly, there is a small minority who, while not refusing, find that examination by students is an unpleasant experience. While there is general recognition of the value of teaching devices such as closed-circuit television, these do not eliminate the need for the personal examination of patients by students, and in a period of increasing student numbers it is essential that as many patients as possible cooperate for this purpose. There is thus a conflict between the need for patients for clinical instruction and the personal feelings of a relatively small number of patients. The right of patients to refuse to be examined by students is beyond question; the problem concerns those who do not refuse but find examination distasteful—usually because of embarrassment at any exposure of the body or because of colour prejudice. Such attitudes are not readily changed; we consider, therefore, that the only reasonable course is to exclude such patients from clinical teaching.

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**Andrew Watson**  
**The Lawyer as Counselor\***

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[T]he role a person occupies is determined by the society in which it exists. The practitioner himself has little to do with its definition, nor

can he freely modify it without running into serious difficulties. For example, lawyers have had considerable to do with shaping the physician's role through the laws of malpractice, contract, and assault and battery, which limit and define the ways in which a doctor may and must relate to a patient. Physicians may challenge and protest these definitions, but they have no freedom whatsoever to change them. While these more tangible aspects of role are also present for the lawyer, I wish to focus upon some aspects of his role which are not so frequently the subject of comment.

First of all, a lawyer as a professional, in contrast to an artist or artisan, has several characteristics which place upon him very specific role demands. First of all, he is the possessor and purveyor of highly technical knowledge and skill, which is the product of a long and involved educative process. Second, this technical information is so completely unintelligible and beyond the scope of judgment to laymen, that a client has no capacity to evaluate the professional skill or effectiveness of the lawyer and his work. Third, because of the two factors mentioned above, the client therefore places himself entirely in the hands of the lawyer and must *trust* that his best interest will be protected by him. This weighty obligation is the motive force for the development by all truly professional groups, of a code of ethics which is self-administered by the professional group and which is dedicated to the protection of those served. These ethical limitations set the standards of practice and are the principal method for controlling professional efficiency. It should be clear upon a moment's reflection, that it is virtually impossible to control professional standards from without, since non-professionals will always suffer the same incapacity to assay professional activity accurately, as do patients or clients.

Clearly this places lawyers as professionals, in a posture which shall cause them to be both revered and feared. Those who function in a manner which looks both omniscient and omnipotent, stand in close psychological proximity to the status of gods, and it is no accident that ancient civilizations inevitably deified both lawgivers and healers. All who have functioned in a professional role have no doubt experienced many occasions in which they fairly glowed under the worshipful attitudes of patients or clients. I do not suggest that we should deny ourselves this pleasure, but I would urge that we be aware of the other side of this emotional axis. Gods

\* 5 *Journal of Family Law* 7, 8-16 (1965).  
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with power to produce reverence must also be feared, and wherever there is fear, there will be latent hatred. Just as the ancient gods suffered frequent assaults from mortals below, so professionals will be common targets for the irrational anger created and propagated by the helplessness of the client relationship. This fear and potential fury may be assumed omnipresent as a potent source of complication in the lawyer-client relationship. In fact, one can safely assert that there are *always* problems in professional relationships due to this normal, irrational force, but skillful and competent professionals know how to deal with it either intuitively or consciously. . . .

The lawyer's contribution to the professional relationship is not only composed of all the personal emotional factors present in the client, but he also provides the complication produced by his need to perform a highly technical task. Because he has selected his own professional activity, there will also be a series of emotional needs, some of which are largely unconscious, which caused him to choose the profession of law. Though we do not yet have well established information about these factors, it would appear that many select law as a vocation because it gives them opportunities to operate from a position of power and authority, as they organize, conceptualize, and manipulate the social forces known as law. To comment about this is not to criticize these impulses, for they are present in every human being in some degree. It is merely to emphasize that each of us, in selecting our vocation, responds to inner needs and desires by seeking out tasks which provide probabilities for such gratification. These tasks may be of enormous social value, but the professional role necessitates, or at least makes desirable, some awareness of the manner in which these internal forces may enter into professional activities.

Another emotional factor which will be present in many professionals, is a powerful desire to be helpful to others and thus secure a supply route to sources of approval, affection, or love. All human beings need such guarantees of attachment to the group, and professional activities are one of the surest sources of supply for this need. However, such emotional need may become of such overriding importance, that it can distort the professional relationship and produce inappropriate decisions and actions. One of the burdens of professionalism is to make occasional moves which by their nature are bound to

be unpopular, even when they are desirable and ultimately helpful. This is reflected in the oft repeated protest of punishing parents, that "this will hurt me more than you." No child nor any client can recognize this while suffering pain or frustration, and it is only by hindsight and mature reflection that the truth of the statement and the reason of its offering can become known.

Another important need which can be gratified in the law and which may be a well-forged portion of a lawyer's identity, is the wish to create orderliness in ideas, institutions, and relationships. While clearly of enormous social importance, if too urgent a pressure, it can result in premature limitation of hypothesis and result in constriction of viewpoint. No doubt all lawyers recall if their memory is jogged, early frustrations in their legal studies, when they first discovered that the "known certainties of the law" even when stated by the venerable Coke, is chimerical at best. However, after overcoming initial panic, most learned how to be orderly about disorder, and thus restored a sense of well-being. I would suggest, parenthetically, that perhaps some of this need for order still rests latently in most, where it may occasionally cause problems through forcing premature decisions about clients, their needs, and their wishes.

Because of the psychological components of the lawyer's self-image described above, as well as other and perhaps more subtle ones, each lawyer has tended to select an area of professional activity which best suits the balance of his individual needs. This empirically and perhaps gropingly effected result has placed each lawyer in his position of best strength as well as greatest weakness. Strength, because it facilitates the use of the sharpest tools possessed; weakness, because it is closest to the built-in blindspots which emanate from the largely unconscious forces which led to the position from which each functions, and by which each is invisibly bound. To get behind this invisible net should be part of the lifetime educational goal of every professional. It is the road to both professional success as well as a sense of personal well-being.

\* \* \*

[B]ecoming involved with clients produces high probability that even such concrete matters as choice of legal tactics, whether or not to take a client, whether to negotiate or go to trial, which witnesses to use and which to shelve, which jurors to empanel and which to avoid, and, in the last analysis, even which aspect of law to

practice, will at least be partially determined by emotions of which the lawyer will be only vaguely aware or completely ignorant.

These omnipresent and disturbing emotional forces in the professional relationship will place the legal counselor under constant internal pressure to resolve questions in a way which will tend to make him more comfortable, with at least some semblance of control in the variables. In practice, this results in premature decisions with curtailment of fact gathering. Tactical and strategic decisions will be drawn from insufficient and sometimes less-than-optimal data. Decisions of this sort can only result in poor practice which is likely to be less than effective for clients and far from satisfying to the lawyer. Let me emphasize that these premature maneuvers are not the function of slovenliness, technical ineptitude, or lack of a conscientious interest in clients. Rather, they are due to the inner needs of a lawyer to alleviate anxiety by "settling matters." Also it should be emphasized, that many lawyers do not "feel" this anxiety since their training and experience has provided them with excellent means for avoiding this sensation and thus missing the more obvious clues to conflict in the situation. The desirable approach sounds paradoxical, since the optimal psychological posture for pursuing these professional problems is to be able to sustain comfortably, the discomfort of open-ended situations. . . .

At this point, let me enumerate and describe briefly, some of the built-in emotional difficulties in the practice of law. . . .

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The legal counselor in his professional operations has from the offset a potentially difficult problem in that he must serve the best interest of his client, and at the same time maintain his responsibility to the bar. Thus he serves two masters who may have different goals and this places him in the psychologically difficult position of balancing and judging the merits and relationships between these competing claims. I do not suggest that this is inappropriate or undesirable, but merely wish to point out that if one were to contrive a psychological situation which would produce great stress and anxiety, this set of circumstances could hardly be improved upon. This is greatly augmented by the fact that law, ethics of practice, and the authority of judges on the bench, all have great psychological potential for being cloaked in [a] kind of blind and irrational authority, . . . . When this occurs, rational ap-

proaches tend to be immobilized and press one toward automatic submission to the authority. When this happens and is side by side with the ideal of serving the client, internal conflict may be stirred up sufficiently to produce paralysis.

It is my impression from observing the legal process, and most especially the relationship of lawyers to clients, that a great many decisions are made in response to these blind internal forces and are detrimental to clients, or are seriously questionable in relationship to responsibility to the bar. Many lawyers resist discussing some of the more difficult questions which involve potential conflict in this area. They reject them with what I would call pseudo-callousness in order to avoid this stress. For example, the problem of representing the unpopular client is often brushed aside with a rationalization. I do not personally believe that lawyers are less interested than most in the problems of other persons, and in many instances they are the most sensitive members of the community in regard to such questions. I merely put the notion that they appear to fall strikingly into two groups which represent both sides of this question. They may be too quick and too casual in their judgments in order to avoid the difficult problems involved. On the other hand, they may have suffered through such trial-by-fire, and through maturing will take the issues on rationally and with enormous social responsibility. At any rate, it behooves the laymen to appreciate the fantastic difficulty that such a professional responsibility carries with it.

Another operation of the legal counselor, which can provide difficulty, stems from ramifications of the adversary method of trial. The necessity for aggressive statement of position is one of the built-in and fundamental ethical demands of the method, and it is essential that the lawyer argue his client's position as aggressively as possible with the facts he has at hand. This must occur regardless of whether or not he agrees with the position or action of his client. There is much psychological difficulty in this situation since it may result in the active espousal of a question which is diametrically opposed to one's own inner value system. One must possess considerable emotional maturity to accomplish this goal without adopting some kind of psychological defense maneuver to deal with the anxiety created by holding a double position. . . .

Another source of potential difficulty for lawyers is that they always represent a partisan interest. While there are clear and cogent rea-

sons for this, it also carries built-in difficulties for the lawyer who must often function as a negotiator. To do this successfully he must be able to identify himself emotionally with both sides of a question in order to understand it well, and to negotiate skillfully. . . .

The last example of difficulties which the lawyer-counselor needs to handle has to do with the kind of intimacy which is involved in professional relationships. Due to the psychological tendency on the part of the client to invest the counselor with all sorts of power, authority, and a nearly magical belief in their helpfulness, there will also be a powerful tendency to bestow affection. These feelings largely are unrelated to truly personal involvement and are mostly a function of the relationship itself. Therefore, for a lawyer to take advantage of them would be quite as unethical as making personal use of the client's money or property which had been entrusted to him in the course of carrying out the professional role. These powerful and yet irrational emotions can be disquieting to say the least. They are capable of producing a variety of defensive maneuvers which may range from callous advantage-taking, to total withdrawal carried out in blind, rationalized ways, or in the context of overt anger caused by the threat which such feelings may pose. One need not feel guilty about sensing internal responses to these emotional manifestations, nor indeed is it unlikely or inappropriate that one should gain some pleasure and satisfaction from the allegation of affection displayed by the client. It is only in the area of action that the professional obligation exists, and it is therefore a matter of professional concern that these feelings be handled in a way which will not interfere with professional obligations to the client.

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#### NOTES

##### NOTE 1.

DOUGLAS E. ROSENTHAL  
CLIENT PARTICIPATION IN PROFESSIONAL  
DECISION—THE LAWYER-CLIENT  
RELATIONSHIP IN PERSONAL INJURY CASES\*

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I do not think it unfair to say that at the national, state and local levels, the "organized

bar" has been complacent about the problem of professional self-policing. The comprehensive review of lawyer performance commissioned by the American Bar Association shortly after World War II came to the conclusion that criticism of the profession is largely unfounded and that, while there is room for improvement, the primary fault of the profession is not performance but "public relations." Consistent with the traditional approach to professional performance, the bar has defined the problem as one best dealt with by screening out the untrained and the unfit, treating proficient performance as synonymous with a lawyer being a person of "integrity and good character." The law schools are encouraged to teach legal ethics. Bar associations are encouraged to develop and implement effective certification procedures for admitting new lawyers, and practicing attorneys are encouraged to cooperate in "purging the profession of the unworthy." However, these traditional principles are not implemented. While many law schools teach ethics courses, they are not designed to relate to the specific and real world pressures and dilemmas of practice. Students are taught almost nothing about how to deal with clients. Students end their law school careers more cynical than when they started and consistently report that their law school experience did not prepare them for the realities of law practice. . . . [I]nstead of reporting ethical violations and gross incompetence of colleagues (as mandated by Disciplinary Rule 1-103 of the Code of Professional Responsibility), lawyers ignore them. Even Henry Drinker, an approving friend of the legal profession, has remarked that

One of the principle features resulting in just public criticism of the bar is the unwillingness of lawyers to expose the abuses of which they know that certain of their brethren are guilty, as well as the reluctance of judges to disbar, suspend, or even publicly reprimand such lawyers. . . . [M]ost of the abuses of which lawyers are guilty could be eliminated if the bar and the courts were constantly alert and willing to do their full duty in this regard.

While the bar has been more active in establishing principles of professional responsibility than in enforcing them, the existing legal doctrine defining lawyer duties and client rights is neither clear nor extensive, nor especially protective of the client. Three overlapping standards have been used by different courts to define the performance owed a client by his attorney. A lawyer is responsible

\* Yale University: Doctoral Dissertation 194-206 (1970). Reprinted by permission of the author who retains all rights.

for any loss to his client which proximately results from a want to that degree of knowledge and skill (1) ordinarily possessed by others of his profession similarly situated, or (2) from the omission to use reasonable care and diligence, or (3) from the failure to exercise in good faith his best judgment in attending to the litigation committed to his care.<sup>13</sup>

The first standard has been incorrectly interpreted as setting as the norm the average conduct of general practice attorneys in the same community. Such an interpretation sets a lower standard of care for the attorney than the reasonable man standard of negligence applied to his client. A reasonable man is expected to perform prudently. The law says a person is not freed from liability for negligence on the mere showing that his conduct was average or normal. The third standard implies, among other things, that if a lawyer is specially trained in a specific area of the law, he may be held to a higher standard of care—the skill and knowledge reasonable for a specialist. Though widely approved, it has yet to be applied in a specific case.

The reasonable care standard is usually applied to the omissions of attorneys rather than to their affirmative acts. Attorneys have been held not to have exercised reasonable care for failing to push a claim diligently, for failing properly to arrange for witnesses at the trial, for incorrectly serving a summons and complaint, for defective pleadings and for not researching the relevant law of a foreign jurisdiction. An attorney may be liable for failing to follow with reasonable promptness and care the explicit instructions of his client—even in the honest belief that the instructions were not in the client's best interest. The attorney is clearly constrained from acting in his client's behalf without his permission only with respect to three issues: he may not settle the claim unilaterally; he may not farm out the case without the client's consent; and he may not unilaterally waive judgment and seek a new trial. More than 70 years ago, New York courts were divided on whether an attorney could stipulate not to appeal a decision without his client's permission. No more recent case has settled the matter.

These rulings may appear to constitute strong protection for the client's right to competent representation. In fact they fall far short of extensive protection. An important erosion of the due care standard has come with the application of the principle that an attorney is freed from

liability where the law is considered uncertain or unsettled. Judges and juries are invited to view a negligent act or judgment as an honest mistake. In a recent California case, for example, an attorney was not held liable for damages resulting from drafting a will which excluded an intended beneficiary from an inheritance. The court so held notwithstanding that the particular error had been the subject of prior authoritative California court decisions interpreting the relevant statute. The court felt that the area of law was sufficiently difficult, anyway, that the honest mistake was not negligence. Ironically, though "ignorance of the law will not excuse" the client, it will excuse the lawyer who is trained to learn the law. Further examples of lawyer omissions that have been judicially sanctioned include: failure to bring a timely appeal—where this was not specifically directed by the client,—failure to receive the client's permission to waive trial by jury and failure to get the client's approval for extending the adversary's permissible time to serve the complaint.

All of the principles promulgated by the organized bar which we have considered to this point are consistent with the traditional professional model. Active client participation is disfavored. As one judge put it, "clients should not be forced to act as hawklike inquisitors of their own counsel, suspicious of every step and quick to switch lawyers. The legal profession knows no worse headache than the client who mistrusts his attorney." Because clients are passive and dependent they deserve the bar's vigilance in weeding out the unscrupulous and incompetent and in establishing principles of lawyer responsibility to be enforced against specific cases of lawyer non-conduct and mis-conduct. Even if implementation is not as extensive and effective as it should be, the ideals are legitimate and consistent. One reason, however, why these principles are not better enforced is the fear that granting relief for every attorney error will tie up the courts with endless petitions from "overly litigious or chronically dissatisfied persons who refuse to accede to the sound judgment of their lawyers." The bar—especially the judiciary—is torn between the policies of putting an end to litigation and guaranteeing clients effective legal representation. To resolve this conflict, the Supreme Court has abandoned the traditional model. The Warren Court has held that the client must be presumed to control his lawyer's conduct. The lawyer is merely the client's passive agent. Therefore any errors of judgment or performance by the lawyer are presumed to be approved by the client. In

<sup>13</sup> *Hodges v. Carter*, 239 N.C. 517, 80 S.E.2d 144, 146 (1954).

the leading case of *Link v. Wabash Railroad Company*, the majority said,

Petitioner voluntarily chose this attorney as his representative in the action, and he cannot now avoid the consequences of the acts or omissions of this freely selected agent. Any other notion would be wholly inconsistent with our system of representative litigation.<sup>34</sup>

In dissent, Justice Black (joined by Chief Justice Warren) reminded the Court that lawyers want and encourage clients to be passive and are therefore being unfair in punishing them for this trusting behavior.

How could [the client] know or why should he be presumed to know that it was his duty to see that the many steps a lawyer needs to take to bring his case to trial had been taken by his lawyer. . . . [S]o far as this record shows [the client] was simply trusting his lawyer to take care of his case as clients generally do.<sup>35</sup>

In the *Link* case, the court's rule denied the plaintiff, an accident victim, any recovery in his claim. As frequently happens, more than three years had elapsed between the day of the accident and the day of the ruling. There is a three-year statute of limitations for bringing negligence actions.

The policy of the *Link* case can be partially defended on the theory that the client still had the possible remedy of a malpractice suit. "Malpractice" suits are brought by aggrieved clients—represented by a second lawyer—against the attorney representing them in a prior unsuccessful lawsuit. If successful, the client may recover from his lawyer the damages to which he was entitled had his prior claim been handled properly. In reality, there are serious hurdles impairing the client's chances for success. First of all, there is also a three-year statute of limitations on suits claiming attorney negligence. The three-year period begins to run from the time the negligent act is committed, not from the time that the client discovers it. Even if the lawyer conceals the negligence from his client until the statute has run, New York courts have held that the lawyer is not liable for fraudulent concealment (with its longer six-year statute of limitations). Thus in the *Link* case, the client would have been denied a malpractice remedy. More than three years had elapsed since his attorney failed to meet the initial trial date. The second hurdle for the client

is that in the malpractice action he must show not only that the first attorney was negligent but that the result would have been different *but for* that negligence. As has been noted, the client must win a "suit within a suit." Courts, so inclined, are given the "easy out" of finding that while, yes, the attorney was negligent, his negligence did not proximately cause the client's damages since the client might well have lost the first suit anyway. A third hurdle is that in many communities (though probably not in Manhattan) it is difficult to find attorneys who will represent clients in malpractice suits against colleagues. The fourth hurdle is the bias of the judges who hear malpractice cases. In the words of a *Columbia Law Review* comment,

Although the overwhelming majority of decisions indicate that the question of negligence is one of fact, judicial reluctance actually to submit the issue to the jury is manifest. Allocating responsibility between judge and jury in attorney malpractice suits raises questions even more delicate and complex than those presented in ordinary negligence cases, which normally involve mixed questions of law and fact. The nature of the relationship between bench and bar inevitably influences judicial attitudes. . . . Notwithstanding their frequent statements that attorneys occupy a position with respect to those they serve similar if not identical to that of members of the medical profession, the courts have treated attorney malpractice suits as *sui generis*. The majority of decisions reflect a superficial analysis that is almost certainly colored by the fraternal concern of the judiciary for members of the practicing bar. . . . The defense that "errors of judgment" were made has generally been sustained uncritically. The defendant should be required to demonstrate that his choice of alternatives, though subsequently mistaken, was not unreasonable, for only then is the allegation of negligence rebutted.<sup>36</sup>

Even if the client overcomes these hurdles and wins the malpractice suit he may well find that the liable lawyer is not sufficiently covered by malpractice insurance to pay damages. The costs of malpractice insurance are steadily rising as more suits are undertaken. The one most likely to be underprotected—or not protected at all—is the general practice attorney who is also the one most likely to make a mistake in representation. Some local bar associations have established client security funds, with contributions provided by member attorneys, to pay defaulted claims brought against guilty attorneys. Unfortunately, for the victim of ordinary negligence, client security funds are only used to pay the

<sup>34</sup> 370 U.S. 626, at 633, 634, 8 L.Ed.2d 734, 82 S.Ct. 1386 (1962).

<sup>35</sup> *Ibid.*, at 643 (dissenting opinion).

<sup>36</sup> *Attorney Malpractice*, 63 COLUMBIA LAW REVIEW 1309, 1312 (1963).



victims of serious misconduct—misappropriation of trust funds, fraud, forgery, larceny and the like.

There is a third possible "remedy" for aggrieved clients who fail to recover damages. They may receive the moral satisfaction of seeing the attorney "disciplined" by the court. Judicial discipline for negligence rarely if ever exceeds the simple wrist-slap of "censure." The attorney is free to continue his practice otherwise unimpaired. It has been found that the effects of a malpractice suit on the practices of a sample of 58 Connecticut physicians were insignificant. The same is probably true for the practice of censured attorneys.

In sum, current legal doctrine is designed to frustrate the client seeking effective representation. If he behaves passively and trusts his lawyer, as encouraged under the traditional approach, he will be held liable for all but the most serious forms of misconduct by his attorney, thereby forfeiting his claim. If he behaves actively and tries to monitor the actions of his attorney he risks incurring the wrath of the lawyer and being branded a troublemaker. The legal profession departs from advocacy of the traditional model of client passivity only so far as to deny the client the very protection the profession claims to provide. Since laymen cannot rely upon the legal profession to insure that all practicing lawyers give good service, choosing a lawyer becomes a real problem.

\* \* \*

#### NOTE 2.

LINK V. WABASH RAILROAD CO.  
370 U.S. 626, 646-49 (1962)

MR. JUSTICE BLACK with whom THE CHIEF JUSTICE concurs, dissenting.

\* \* \*

[T]o say that the sins or faults or delinquencies of a lawyer must always be visited upon his client so as to impose tremendous financial penalties upon him, as here, is to ignore the practicalities and realities of the lawyer-client relationship. Lawyers everywhere in this country are granted licenses presumably because of their skill, their integrity, their learning in the law and their dependability. While there may be some clients sophisticated enough in the affairs of the world to be able to select the good from the bad among this mass of lawyers throughout the country, this unfortunately cannot always be

the case. The average individual called upon, perhaps for the first time in his life, to select a lawyer to try a lawsuit may happen to choose the best lawyer or he may happen to choose one of the worst. He has a right to rely at least to some extent upon the fact that a lawyer has a license. From this he is also entitled to believe that the lawyer has the ability to look out for his case and that he should leave the lawyer free from constraint in doing so. Surely it cannot be said that there was a duty resting upon Link, a layman plaintiff, to try to supervise the daily professional services of the lawyer he had chosen to represent him. How could he know, even assuming that it is true, that his lawyer was a careless man or that he would have an adverse effect upon the trial judge by failing to appear when ordered? How could he know or why should he be presumed to know that it was his duty to see that the many steps a lawyer needs to take to bring his case to trial had been taken by his lawyer? Why should a client be awakened to his lawyer's incapacity for the first time by a sudden brutal pronouncement of the court: "Your lawyer has failed to perform his duty in prosecuting your case and we are therefore throwing you out of court on your heels"? So far as this record shows, the plaintiff never received one iota of information of any kind, character or type that should have put him on notice as an ordinary layman that his lawyer was not doing his duty.

Any general rule that clients must always suffer for the mistakes of their lawyers simply ignores all these problems. If a general rule is to be adopted, I think it would be far better in the interest of the administration of justice, and far more realistic in the light of what the relationship between a lawyer and his client actually is, to adopt the rule that no client is ever to be penalized, as this plaintiff has been, because of the conduct of his lawyer unless notice is given to the client himself that such a threat hangs over his head. Such a rule would do nothing more than incorporate basic constitutional requirements of fairness into the administration of justice in this country.

The Court seems to find some reason for holding that this plaintiff can be penalized without notice because of a program certain courts have adopted to end congestion on their dockets by setting down long-pending cases for trial. It is of course desirable that the congestion on court dockets be reduced in every way possible consistent with the fair administration of justice. But that laudable objective should not be sought



in a way which undercuts the very purposes for which courts were created—that is, to try cases on their merits and render judgments in accordance with the substantial rights of the parties. Where a case has so little merit that it is not being prosecuted, a trial court can of course properly dispose of it under fair constitutional procedures. There is not one fact in this record, however, from which an inference can be drawn that the case of Link against the Wabash Railroad Company is such a case. When we allow the desire to reduce court congestion to justify the sacrifice of substantial rights of the litigants in cases like this, we attempt to promote speed in administration, which is desirable, at the expense of justice, which is indispensable to any court system worthy of its name.

\* \* \*

### 3.

#### **The Professional in Practice— Can Power Be Shared?**

- a. **Thomas S. Szasz and Marc H. Hollender**  
**A Contribution to the Philosophy of**  
**Medicine—The Basic Models of the**  
**Doctor-Patient Relationship\***

\* \* \*

The three basic models of the doctor-patient relationship, which we will describe, embrace modes of interaction ubiquitous in human relationships and in no way specific for the contact between physician and patient. The specificity of the medical situation probably derives from a combination of these modes of interaction with certain technical procedures and social settings.

1. *The Model of Activity-Passivity.*—Historically, this is the oldest conceptual model. Psychologically, it is not an interaction, because it is based on the effect of one person on another in such a way and under such circumstances that the person acted upon is unable to contribute actively, or is considered to be inanimate. This frame of reference (in which the physician does something to the patient) underlies the application of some of the outstanding advances of modern medicine (e.g., anesthesia and surgery, antibiotics, etc.). The physician is active; the pa-

tient, passive. This orientation has originated in—and is entirely appropriate for—the treatment of emergencies (e.g., for the patient who is severely injured, bleeding, delirious, or in coma). “Treatment” takes place irrespective of the patient’s contribution and regardless of the outcome. There is a similarity here between the patient and a helpless infant, on the one hand, and between the physician and a parent, on the other. It may be recalled that psychoanalysis, too, evolved from a procedure (hypnosis) which was based on this model. Various physical measures to which psychotics are subjected today are another example of the activity-passivity frame of reference.

2. *The Model of Guidance-Cooperation.*—This model underlies much of medical practice. It is employed in situations which are less desperate than those previously mentioned (e.g., acute infections). Although the patient is ill, he is conscious and has feelings and aspirations of his own. Since he suffers from pain, anxiety, and other distressing symptoms, he seeks help and is ready and willing to “cooperate.” When he turns to a physician, he places the latter (even if only in some limited ways) in a position of power. This is due not only to a “transference reaction” (i.e., his regarding the physician as he did his father when he was a child) but also to the fact that the physician possesses knowledge of his bodily processes which he does not have. In some ways it may seem that this, like the first model, is an active-passive phenomenon. Actually, this is more apparent than real. Both persons are “active” in that they contribute to the relationship and what ensues from it. The main difference between the two participants pertains to power, and to its actual or potential use. The more powerful of the two (parent, physician, employer, etc.) will speak of guidance or leadership and will expect cooperation of the other member of the pair (child, patient, employee, etc.). The patient is expected to “look up to” and to “obey” his doctor. Moreover, he is neither to question nor to argue or disagree with the orders he receives. This model has its prototype in the relationship of the parent and his (adolescent) child. Often, threats and other undisguised weapons of force are employed, even though presumably these are for the patient’s “own good.” It should be added that the possibility of the exploitation of the situation—as in any relationship between persons of unequal power—for the sole benefit of the physician, albeit under the guise of altruism, is ever present.

\* 97 *Archives of Internal Medicine* 585, 586–587 (1956). Reprinted by permission of the American Medical Association.

3. *The Model of Mutual Participation.*—Philosophically, this model is predicated on the postulate that equality among human beings is desirable. It is fundamental to the social structure of democracy and has played a crucial role in occidental civilization for more than two hundred years. Psychologically, mutuality rests on complex processes of identification—which facilitate conceiving of others in terms of oneself—together with maintaining and tolerating the discrete individuality of the observer and the observed. It is crucial to this type of interaction that the participants (1) have approximately equal power, (2) be mutually interdependent (i.e., need each other), and (3) engage in activity that will be in some ways satisfying to both.

This model is favored by patients who, for various reasons, want to take care of themselves (at least in part). This may be an overcompensatory attempt at mastering anxieties associated with helplessness and passivity. It may also be "realistic" and necessary, as, for example, in the management of most chronic illnesses (e.g., diabetes mellitus, chronic heart disease, etc.). Here the patient's own experiences provide reliable and important clues for therapy. Moreover, the treatment program itself is principally carried out by the patient. Essentially, the physician helps the patient to help himself.

In an evolutionary sense, the pattern of mutual participation is more highly developed than the other two models of the doctor-patient relationship. It requires a more complex psychological and social organization on the part of both participants. Accordingly, it is rarely appropriate for children or for those persons who are mentally deficient, very poorly educated, or profoundly immature. On the other hand, the greater the intellectual, educational, and general experiential similarity between physician and patient the more appropriate and necessary this model of therapy becomes.

\* \* \*

b. **Robert Rubenstein and Harold Lasswell**  
**The Sharing of Power in a**  
**Psychiatric Hospital\***

\* \* \*

The great social movements of our time concern the demand for full participation as equals in the affairs of the community by the

disadvantaged. Sermons and speeches have long acknowledged the justice of this demand, but the insistence that we take the democratic ideology seriously and live by it is revolutionary. All of us may be viewed in some context as disadvantaged. Two prominent examples are Negroes and women; two groups less aware of being deprived are students and patients.

\* \* \*

Patients are . . . among the disadvantaged. It is customary to view them as having to be taken care of because they are ill, incapacitated, or defective. Hospitalized psychiatric patients, the subgroup we shall focus upon, have been defeated in long, protracted, at times subtle, at times violent, power struggles within their families, and by friends and colleagues in school, work, and the other communities in which they have unsuccessfully sought to participate. Unable to find a way to share effectively in the decision-making processes of the family, they remain in unresolved conflict with those exercising power and at odds with their critical decisions, their perspectives, preferred outcomes, and strategies.

\* \* \*

Conventional psychiatric institutions reinforce the self-image of the hospitalized as losers, sufferers, and victims. Decisions about fundamental and pressing issues in the lives of patients are decided by others; the individuals most concerned participate not at all. In an authoritarian hospital, the roles of doctors, nurses, and patients are clearly defined. The "good patient" is compliant, cooperative, accepting, unquestioning, the recipient of the good, established, known care from doctors, nurses, and other staff members. He is regarded as a troublemaker, uncooperative, and cantankerous if he questions procedures, seeks information about why this is being done and that isn't, or presumes to take a more active position by volunteering judgments about what is wrong with him and what should be done, or the nature of the difficulties and the treatment of other patients. One part of the hospital—the staff—does things to that other part of the hospital—the patients—"to get 'them' well." The patients comply with these implicit expectations by assuming the passive role of those to whom things are done by others.

Once more, society and the doctors, experts with extraordinary authority over the lives of others, justify such exemptions from democratic practices and drastic usurpation of rights

\* New Haven: Yale University Press 2-6 (1966). Reprinted by permission.

by describing the mentally ill as fragile, childlike, irresponsible, and dangerous to themselves and others. Not protecting them, failing to administer their affairs as dependents, would be a breach of professional obligation. But now the possibility is being considered that the traditional medical model is not appropriate for reprocessing these defeated and disadvantaged, and new institutions specifically elaborated in response to their needs are developing. This shift was prompted both by convincing demonstrations of the beneficial effects of patients participating actively with staff in determining and assessing what happens in the hospital and by conflicts of conscience among those exercising power, the doctors who knew, but had not previously been forced to acknowledge, the necessity of extending throughout the patient's experience the dignity, respect, responsibility, autonomy, and self-determination long acknowledged as central to psychoanalytic treatment. [P]atients respond completely and responsibly to such opportunities and expectations; in such an atmosphere they assert their dissatisfaction with being the passive recipients of the ministrations of others. That fact renders the doctor's authoritarian position uncertain and conflictful. He cannot continue to violate the canons of shared power and fulfill his obligations as a physician, for these deviations from democratic practice are no longer justifiable on therapeutic grounds.

\* \* \*

c.

**Douglas E. Rosenthal**  
**Client Participation in Professional Decision**  
**—The Lawyer-Client Relationship in**  
**Personal Injury Cases\***

The traditional model proposes that the client who participates actively in problem-solving, who shares in controlling the decisions, will end up in a poorer position—and certainly no better off—than the client who passively delegates decision-making to the professional. The participatory model posits the opposite. . . . I will test the proposition that active clients get poorer results than passive clients. . . . The data about client participation and case worth is taken from extended personal interviews with 59 Manhattan residents who brought accident claims and who were represented by an attorney. These clients had their claims terminated during 1968 and

each received at least \$2,000 in compensation for their accident. . . .

\* \* \*

Inspection of the interview protocols points up six distinguishable types of client activity that tend to have an impact on decisions made in pursuing a case. The first is seeking out quality medical attention for recuperation and making sure that medical expenses and related non-medical expenses are included in provable claim damages. The second is impressing one's wishes and concerns about the claim upon the lawyer—having the client's wishes reflected in the handling of the claim, and having the lawyer's support in dealing with client anxieties. Third is the client persuading the lawyer to give special attention to his case. Fourth is helping the lawyer to marshal evidence to build a solid claim that will be worth his extra time and energy. Fifth is a client tactic of continually appraising his lawyer's performance according to criteria of responsiveness, thoroughness and consistency and, if dissatisfied, "comparison shopping" to support additional demands or for a replacement. Sixth is bargaining with the lawyer about the fee. Any client who took all six of these types of actions would have initiated the search for information bearing on his problem, would have alerted his attorney to his special interests, would have received some feedback on how well the attorney was responding to these concerns and would have had the opportunity for an informed intermediate review before a final settlement was irrevocably made. Any client who took none of these steps would exhibit a passive problem-solving strategy. Only one client sampled, Mr. Bates, a civil engineer, reported having taken all six types of action. Almost one third (19/59) of the client sample reported taking none of them. In Table II-1, we see how frequent are the various forms of client action as reported by the interviewees (listed in rank order of frequency).

\* \* \*

Plaintiffs' trial lawyers share a consensus view that there is an objective value that can be placed, approximately, on various types of liability claims. This view facilitates compromise settlements. The going values are based on prior settlements, recent jury verdicts in similar types of cases and some rules-of-the-game, such as that a fair settlement in a strong case should not depart too greatly from a figure that reflects the victim's out-of-pocket expenses multiplied by 3. Recent jury awards within various jurisdic-

\* Yale University: Doctoral Dissertation 48-64 (1970). Reprinted by permission of the author who retains all rights.

TABLE II-1  
Frequencies of Types of Client Activity

<i>activity type</i>	<i>number of clients reporting it</i>	<i>percentage of sample</i>
1. expresses a special want or concern	25	42%
2. makes follow-up demands for attention	19	32%
3. marshals information to aid lawyer	14	24%
4. seeks quality medical attention	12	20%
5. seeks second legal opinion	10	15%
6. bargains about the fee	7	12%

tions are codified and reported by national research services. Thus it is possible to find value precedents for most types of injuries. Nonetheless, it remains an open question just how objective and consensual expert case evaluation can be.

One analytical way to make use of an "objective" consensus value on case result is to have a disinterested panel of experts make a case by case appraisal. This was done for each of the 59 cases in the sample. A fact sheet was prepared based upon the more important determinants of any case's worth. It was submitted to a panel of three experienced plaintiff's personal injury attorneys. They were asked to put an exact dollar value on each claim—based on the given facts—at three different time periods: one year after the accident, four years after the accident (approximately pre-trial), and what could be expected from a jury's trial judgment. . . .

. . . If client activity does influence case result, we should expect to find a statistically significant rank order correlation between the two variables [client participation and case result]. I have used Kendall's Tau to determine the rank order correlation for the 43 cases where the mean panel evaluation is acceptable, and find a moderately strong positive relationship: contrary to the expectation from the traditional professional model, active clients not only do not get worse results, but actually get better recoveries from their legal claims.

But this is only the first step in casting doubt on the traditional hypothesis. It may well be expected that client activity only masks some deeper more important explanation of good case outcome. In fact, there are three additional factors which might be thought to have a significant causal impact on case outcome. These factors are the social status of the client, the dollar worth of the claim, and the "perfection" of the liability issue in the claim.

. . . If social position significantly influences

case result we would expect to find a high positive rank order correlation between the status and case result scales. Surprisingly, the two factors are not significantly related. A client's high social standing does not noticeably improve his chances of receiving a good claim disposition. Therefore, even though client status is significantly related to active client participation . . . it is the client's activity rather than his status which better explains the success of his claim.

Though not intuitively obvious, the worth of a case and the perfection of the liability issue in it are significant rival explanations of why some clients get better case results than others. Ironically, the greater the dollar amount of recovery that can be anticipated in a claim (case worth), the smaller the chances of making a good recovery. Also, and more plausibly, the stronger the evidence that the one accused of causing the accident was negligent and that the accident victim was free from contributory negligence, the greater the chances of a successful recovery.

There is a statistical procedure for performing a partial correlation of client activity with case result, controlling for each of these three alternative variables one at a time. If client activity is not a valid independent causal variable, the correlation should "wash out." Computation of the partial Kendall correlation reveals that client activity is indeed a potent explanatory factor. At the level of aggregate analysis, active client participation definitely pays off. . . .

\* \* \*

d.

**Washington-Greene Legal Aid Society  
Application  
45 D. & C. 2d 563 (Pa. 1968)**

\* \* \*

SWEET, P. J.—This matter comes before the court on the application of the O. E. O.

Legal Services for a nonprofit corporation charter. The Washington-Greene Legal Aid Society has asked us, pursuant to the Act of May 5, 1933, P. L. 289, 15 PS §7201 et seq., to grant them powers of a nonprofit corporation "... to make available legal aid to all residents in the Counties of Washington and Greene, Commonwealth of Pennsylvania, who, because of their financial inability are unable to procure such legal aid, and to undertake educational programs in which indigent residents may be instructed in and advised of their fundamental private legal rights and obligations, to the end that their performance, motivation and productivity as citizens may be improved and their respect for the law increased."

Because of the controversial nature of this application, the amount of money involved and the relatively novel nature of the proposal, we ordered a hearing, "as to the necessity, the legality, the propriety and the wisdom of the activities of the proposed corporation."

\* \* \*

The application before us ... has some non-lawyers representative of the poor in a position of control of policy, but it provides for a director of legal service employees who will exercise executive authority in the office. Probably this is the best compromise that could be planned.

It seems fairly clear that the Canon of Ethics does not prohibit such an organization as this. Canon 35 says that while a lawyer should not be controlled by any corporate lay agency which comes between him and his client that "charitable societies rendering aid to the indigent are not deemed such intermediaries."

Liberals favoring these offices may find some delicious irony in the fact that the American Bar Association once approved a plan by which attorneys associated with the Liberty League publicly advertised free legal services to those challenging the constitutionality of New Deal Legislation. A.B.A. Opinions of the Committee on Professional Ethics and Grievances #148 (1935) said in part the canon "... certainly was never aimed at a situation such as this, in which a group of lawyers announced that they are willing to devote some of their time and energy to the interests of indigent citizens whose constitutional rights are believed to be infringed."

\* \* \*

The most ambitious scholarly study of this field which we have been able to find is "Neigh-

borhood Law Offices," 80 *Harv. L. Rev.* 805, February (1967). This characteristically documented article points out the inadequacy of existing charitable legal aid services. It suggests the importance of the neighborhood concept in the furnishing of legal services to the poor and makes a careful distinction between the service function and the nonservice function of the new legal services programs in their component role in the war on poverty. "While the service function—the representation by the neighborhood lawyer of individual clients without regard for broader reform—is the least novel aspect of the New Wave, it is nevertheless crucial to any neighborhood law office program. The service function occupies the great majority of the working hours of most of the lawyers and is the day-to-day means of building the community's trust and confidence in the program, so that the neighborhood concept may become a reality." A fairly large percentage of the article is devoted to the nonservice functions. These are categorized (A) law reform, (B) community action, and (C) community education. "In order to restore the overall integrity of the adversary system, OEO-funded legal services programs are required to list as one of their goals the reform of substantive law in the interest of the poor." This is squarely based on the guidelines which say, "that advocacy of appropriate reforms ... should be among the services afforded by the program."

\* \* \*

We also find in the *Harvard Law Review* article considerable stress laid on participation of the poor. It is critical of the technique, used by our applicant here, in placing responsibility for the poor on the Board of Trustees. While the N.Y.S. case, *supra*, was critical of any non-lawyer control over lawyers the *Harvard Law Review* article tends the other way. "But there are disadvantages in giving the poor full control of a legal services program. The attorneys have special responsibilities to their clients, and the poor on the board may not understand the lawyer-client relationship, expecting the attorney to be dedicated to the interest of the poor as a class or even to the more narrow interests of the representatives. Although the attorney should be able to resist pressures to sacrifice his client's interest, it would be preferable not to put him in such a position. It seems undesirable, however, to attempt to avoid this pressure by giving the organized bar control of the program; local bar associations tend to be critical of innovation and close to the established

interests in society with whom the poor are often in conflict. The adverse effect that control by the organized bar might have on the nonservice functions outweighs the possible advantages of a professionally supervised program."

\* \* \*

It seems to us that we should grant the application. There is little reason to question the legality of the plan nor are we convinced that it will be injurious to the community. At this point, however, a word of caution, precatory perhaps, but none the same strongly felt, seems in order. We are gravely disturbed by the implications of the "non-service" function referred to indirectly in the application as education, training and research activities. The business of a lawyer engaged by such a nonprofit corporation is to represent indigent persons one by one as they have a need for representation. It is not a class representation of the indigent, as such, nor a collective representation, nor are the poor a single and collective client. . . .

\* \* \*

We take this occasion to remind the public generally that any person aggrieved by the operations of this society has recourse to the courts. It would be invidious indeed were O.E.O. Legal Services organized to prevent injustice to the poor only to become an instrument of malevolence and illegality toward any person, however affluent.

\* \* \*

#### NOTES

##### NOTE 1.

##### THE NEW PUBLIC INTEREST LAWYERS\*

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Though there are a great variety of checks and controls the lawyer may adopt in his selection of cases, the issues are focused most clearly by comparing two models: "independence" and "community control." The "independent" lawyer relies solely on personal values and his own sense of what is important. Monroe Freedman, director of the Philip Stern Community Law Office in Washington, justified the independence model, arguing that lawyers totally released from external constraints serve a highly useful social

purpose: they provide a perspective on the legal process and social change which cannot be achieved by those concerned with their own immediate problems.

I do not mean to say that people in the community, groups in the community, don't know what they want or are not entitled to have representation on what they want or think they want. I am saying that there is a place for lawyers who don't serve that function. It's in part a matter of expertise and values and of the public not knowing what they're going to need in the future. I'll use a dirty word. It's very elitist, and although I'm a very strong believer in participatory democracy, it doesn't prevent me from being an elitist.

Set against the "independence" model is the "community control" model, which subordinates the lawyer's decisions on cases to handle to control by those whose interests are at stake in his work. This model is usually implemented by a board composed of true representatives of the constituency—rather than lawyers or inveterate board sitters—who have effective power to control the lawyer's allocation decisions. The contemporary movement for decentralization and democratization of institutions urges that people are the best judges of their own best interests and ought to participate in decisions which affect their lives—and thus it supports the notion that the lawyer should be accountable to those he serves. Since lawyers for the underrepresented are by definition scarce, a lawyer's decision to handle one kind of case rather than another is also a decision setting priorities in the community's legal representation. The "community control" model asserts that those decisions are properly the people's.

The two models are true alternatives where—as in Neighborhood Legal Services—the question is how best to serve the interests of a poor community. Here there exists a group which is able, through representatives, to exercise control over the lawyer; yet the lawyer can also, while still claiming to serve the group's best interests, be independent of it in allocating his services among various types of cases. The dilemma is well focused: who should decide how to allocate the community's legal resources, the lawyer or the community?

The justification for independence in this situation is paternalistic; the lawyer is properly a social engineer, deciding what is really important for a community or constituency, because he is skilled, rational, and benevolent. Independence allows rational action and the use of expertise

\* 79 *Yale Law Journal* 1069, 1132-35 (1970). Reprinted by permission of The Yale Law Journal Company and Fred B. Rothman and Company.

free from the need to spend energy and time dealing with people who may be uneducated, prone to irrationality, unaware of their best interests, and difficult to organize. But this notion of benevolent expertise is undercut by the community's conflicting claim of "expertise" in matters which concern it, and by the danger that, in following his own instincts, the lawyer may be misled by the limitations of his perspective, and mistake personal interests for those of the group on whose behalf he speaks. Thus the lawyer's personal interest in furthering his career or his organization, or in amusing himself, and the limitations of his perspective because of his class and racial background, may undermine the goals to which he professes devotion.

No one really knows which of these models will produce "better" decisions, nor is there agreement on what "better" means. Given what have been publicized as failures of expertise in the country's recent history, however, and given the demoralizing powerlessness so many citizens feel today, the arguments seem stronger in the direction of community control; where there is popular participation, decisions are at least likely to be perceived as more "legitimate," and, even should there be increased in-fighting among groups, community organization is likely to be furthered. This is not to say that lawyers whose selection of cases is not controlled by the community are somehow illegitimate advocates of the underrepresented. They may be handling matters affecting the community to which no other lawyer is giving consideration; and they may even be handling matters which *they* perceive are the *most* important for the community. But where a lawyer, a foundation, or a program administrator is faced with the choice between the independence and community control models of service, we can favor the latter, insisting that only it will offer the poor more than "expert"—though presumably benevolent—manipulation of their future.

\* \* \*

#### NOTE 2.

STEPHEN WEXLER  
PRACTICING LAW FOR POOR PEOPLE\*

\* \* \*

The dominant attitude in law school is that the client is a troublesome pain-in-the-neck. Occasionally, the law student hears hints that he should present his clients with the legal alternatives, among which the client should choose. Many lawyers are now aware that people should control their lawyer, and are beginning to present alternatives from which their clients can choose. But the control which poor people should exercise over their lawyer is much greater than that of merely selecting among his proposals. Because he does know more about the possibilities in the law, the lawyer should present new knowledge and options to his clients; but, because they know what is helpful to them and possible for them, they can and must structure their own alternatives and make their own choices. The lawyer should not push his clients toward or away from jail. "Jail" must, of course, be read as a metaphor for the whole range of possible consequences of possible actions, of which jail is only the worst.

The last portion of the preceding sentence makes clear why a lawyer must not lead his clients. For me, as for most lawyers, jail is the worst possible consequence of a political action. But it is clear that jail is not the worst consequence. Welfare recipients have lost the only money they have to live on because they protested the policies of welfare departments; people have lost their homes; children have been unable to obtain medical care; Fred Hampton was shot.

\* \* \*

\* 79 *Yale Law Journal* 1049, 1063-64 (1970). Reprinted by permission of the Yale Law Journal Company and Fred B. Rothman and Company.





## CHAPTER FOUR

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# Perspectives on Decisionmaking

The case studies in the first two chapters revealed considerable confusion about the rights and duties of the various participants as well as the absence of a conceptual framework to guide the human experimentation process. Before turning to a detailed analysis of the authority of the participants at each stage—formulation, administration, and review—of the experimentation process, we pause briefly to examine some theories of decisionmaking. These theories raise fundamental questions for students of human experimentation about the extent of man's ability to plan rationally for human institutions.

At one extreme the pure market system of "free contract" suggests that all control over experimentation should be left to individual investigators and the subjects whom they persuade to join their projects. At the other extreme, some theorists maintain that decisions are better made collectively after a thorough analysis of all relevant values and a comparison of the extent to which alternative plans maximize these values. Under this system, for example, a central planning agency would decide which experiments to pursue and would prescribe in detail the rights and duties of all participants. Other analysts agree that some planning is needed, but believe that limitations on time, resources, and rationality preclude "comprehensive" planning. Their approach would lead to intervention directed at specific evils in the process or to marginal changes in regulations which are easily modified in the light of experience.

This chapter does not present a complete examination of competing decisionmaking theories. Rather, a sampling of the literature is offered to stimulate questions as the student

turns to Parts Two, Three, and Four of this book and analyzes specific proposals for exercising control over human experimentation, especially by the professions and the state. Thus, the materials should facilitate consideration of the advantages and disadvantages of various decisionmaking models for each stage in the human experimentation process. Throughout the remaining chapters we return not only to the decisionmaking questions raised in this chapter but also to an examination of the values on which such decisions rest. For example, the discussion of individual liberty in Chapter Eight bears on both the value of freedom and the kinds of regulations society may wish to pursue to implement this value.

In studying these materials, on the basis of problems already raised about human experimentation, consider the following questions:

1. What are the limits of planning by man for man?
2. What values of man and society are reinforced or undermined by these theories of decisionmaking?
3. What impact would the adoption of the various decisionmaking theories have had on the resolution of the problems which surfaced in the Jewish Chronic Disease Hospital and Wichita Jury Recording cases?

## A.

### Planning by Individuals—The Market System

#### 1.

#### Cooperation without Coercion?

##### a.

#### Milton Friedman Capitalism and Freedom\*

\* \* \*

Fundamentally, there are only two ways of coordinating the economic activities of millions. One is central direction involving the use of coercion—the technique of the army and of the modern totalitarian state. The other is voluntary cooperation of individuals—the technique of the market place.

The possibility of coordination through voluntary cooperation rests on the elementary—yet frequently denied—proposition that both parties to an economic transaction benefit from it, *provided the transaction is bilaterally voluntary and informed.*

Exchange can therefore bring about coordination without coercion. A working model of a society organized through voluntary exchange is

a *free private enterprise exchange economy*—what we have been calling competitive capitalism.

In its simplest form, such a society consists of a number of independent households—a collection of Robinson Crusoes, as it were. Each household uses the resources it controls to produce goods and services that it exchanges for goods and services produced by other households, on terms mutually acceptable to the two parties to the bargain. It is thereby enabled to satisfy its wants indirectly by producing goods and services for others, rather than directly by producing goods for its own immediate use. The incentive for adopting this indirect route is, of course, the increased product made possible by division of labor and specialization of function. Since the household always has the alternative of producing directly for itself, it need not enter into any exchange unless it benefits from it. Hence, no exchange will take place unless both parties do benefit from it. Cooperation is thereby achieved without coercion.

Specialization of function and division of labor would not go far if the ultimate productive unit were the household. In a modern society, we have gone much farther. We have introduced enterprises which are intermediaries between in-

\* Chicago: University of Chicago Press, 13-15, 23-24, 33-34 (1962). © 1962 by the University of Chicago. Reprinted by permission.

dividuals in their capacities as suppliers of service and as purchasers of goods. And similarly, specialization of function and division of labor could not go very far if we had to continue to rely on the barter of product for product. In consequence, money has been introduced as a means of facilitating exchange, and of enabling the acts of purchase and of sale to be separated into two parts.

Despite the important role of enterprises and of money in our actual economy, and despite the numerous and complex problems they raise, the central characteristic of the market technique of achieving coordination is fully displayed in the simple exchange economy that contains neither enterprises nor money. As in that simple model, so in the complex enterprise and money-exchange economy, cooperation is strictly individual and voluntary *provided*: (a) that enterprises are private, so that the ultimate contracting parties are individuals and (b) that individuals are effectively free to enter or not to enter into any particular exchange, so that every transaction is strictly voluntary.

It is far easier to state these provisos in general terms than to spell them out in detail, or to specify precisely the institutional arrangements most conducive to their maintenance. Indeed, much of technical economic literature is concerned with precisely these questions. The basic requisite is the maintenance of law and order to prevent physical coercion of one individual by another and to enforce contracts voluntarily entered into, thus giving substantive meaning to "private." Aside from this, perhaps the most difficult problems arise from monopoly—which inhibits effective freedom by denying individuals alternatives to the particular exchange—and from "neighborhood effects"—effects on third parties for which it is not feasible to charge or recompense them. . . .

So long as effective freedom of exchange is maintained, the central feature of the market organization of economic activity is that it prevents one person from interfering with another in respect of most of his activities. The consumer is protected from coercion by the seller because of the presence of other sellers with whom he can deal. The seller is protected from coercion by the consumer because of other consumers to whom he can sell. The employee is protected from coercion by the employer because of other employers for whom he can work, and so on. And the market does this impersonally and without centralized authority.

Indeed, a major source of objection to a free economy is precisely that it does this task so well. It gives people what they want instead of what a particular group thinks they ought to want. Underlying most arguments against the free market is a lack of belief in freedom itself.

The existence of a free market does not of course eliminate the need for government. On the contrary, government is essential both as a forum for determining the "rules of the game" and as an umpire to interpret and enforce the rules decided on. What the market does is to reduce greatly the range of issues that must be decided through political means, and thereby to minimize the extent to which government need participate directly in the game. The characteristic feature of action through political channels is that it tends to require or enforce substantial conformity. The great advantage of the market, on the other hand, is that it permits wide diversity. It is, in political terms, a system of proportional representation. Each man can vote, as it were, for the color of tie he wants and get it; he does not have to see what color the majority wants and then, if he is in the minority, submit.

It is this feature of the market that we refer to when we say that the market provides economic freedom. . . .

\* \* \*

There are clearly some matters with respect to which effective proportional representation is impossible. I cannot get the amount of national defense I want and you, a different amount. With respect to such indivisible matters we can discuss, and argue, and vote. But having decided, we must conform. It is precisely the existence of such indivisible matters—protection of the individual and the nation from coercion are clearly the most basic—that prevents exclusive reliance on individual action through the market. If we are to use some of our resources for such indivisible items, we must employ political channels to reconcile differences.

The use of political channels, while inevitable, tends to strain the social cohesion essential for a stable society. The strain is least if agreement for joint action need be reached only on a limited range of issues on which people in any event have common views. Every extension of the range of issues for which explicit agreement is sought strains further the delicate threads that hold society together. If it goes so far as to touch an issue on which men feel deeply yet differently, it may well disrupt the society. Funda-

mental differences in basic values can seldom if ever be resolved at the ballot box; ultimately they can only be decided, though not resolved, by conflict. The religious and civil wars of history are a bloody testament to this judgment.

The widespread use of the market reduces the strain on the social fabric by rendering conformity unnecessary with respect to any activities it encompasses. The wider the range of activities covered by the market, the fewer are the issues on which explicitly political decisions are required and hence on which it is necessary to achieve agreement. In turn, the fewer the issues on which agreement is necessary, the greater is the likelihood of getting agreement while maintaining a free society.

\* \* \*

Freedom is a tenable objective only for responsible individuals. We do not believe in freedom for madmen or children. The necessity of drawing a line between responsible individuals and others is inescapable, yet it means that there is an essential ambiguity in our ultimate objective of freedom. Paternalism is inescapable for those whom we designate as not responsible.

The clearest case, perhaps, is that of madmen. We are willing neither to permit them freedom nor to shoot them. It would be nice if we could rely on voluntary activities of individuals to house and care for the madmen. But I think we cannot rule out the possibility that such charitable activities will be inadequate, if only because of the neighborhood effect involved in the fact that I benefit if another man contributes to the care of the insane. For this reason, we may be willing to arrange for their care through government.

Children offer a more difficult case. The ultimate operative unit in our society is the family, not the individual. Yet the acceptance of the family as the unit rests in considerable part on expediency rather than principle. We believe that parents are generally best able to protect their children and to provide for their development into responsible individuals for whom freedom is appropriate. But we do not believe in the freedom of parents to do what they will with other people. The children are responsible individuals in embryo, and a believer in freedom believes in protecting their ultimate rights.

To put this in a different and what may seem a more callous way, children are at one and the same time consumer goods and potentially responsible members of society. The freedom of

individuals to use their economic resources as they want includes the freedom to use them to have children—to buy, as it were, the services of children as a particular form of consumption. But once this choice is exercised, the children have a value in and of themselves and have a freedom of their own that is not simply an extension of the freedom of the parents.

The paternalistic ground for governmental activity is in many ways the most troublesome to a liberal [\*]; for it involves the acceptance of a principle—that some shall decide for others—which he finds objectionable in most applications and which he rightly regards as a hallmark of his chief intellectual opponents, the proponents of collectivism in one or another of its guises, whether it be communism, socialism, or a welfare state. Yet there is no use pretending that problems are simpler than in fact they are. There is no avoiding the need for some measure of paternalism. As Dicey wrote in 1914 about an act for the protection of mental defectives, “The Mental Deficiency Act is the first step along a path on which no sane man can decline to enter, but which, if too far pursued, will bring statesmen across difficulties hard to meet without considerable interference with individual liberty.” There is no formula that can tell us where to stop. We must rely on our fallible judgment and, having reached a judgment, on our ability to persuade our fellow men that it is a correct judgment, or on their ability to persuade us to modify our views. We must put our faith, here as elsewhere, in a consensus reached by imperfect and biased men through free discussion and trial and error.

\* \* \*

b.

### Sumner H. Slichter Modern Economic Society†

\* \* \*

The reasoning in support of the belief that freedom of enterprise is the maximum of satisfaction at the minimum of cost is very simple. Each individual, it is said, is better able than any one else to judge his own interests. If men are at liberty to spend their money as they choose,

[\*] Friedman uses the term “liberal” in its classical nineteenth century meaning, not in the meaning it has come to have in the United States in recent decades.

† New York: Henry Holt & Co. 43–46, 49 (1928). Reprinted by permission.

they will naturally purchase those things that will yield them the most satisfaction. Consequently the very commodities which give consumers the greatest pleasure are the most profitable for business enterprises to produce. Likewise, if men are free to use such methods of production as they wish, they will select those which involve the least cost per unit of output. With the goods which give the greatest gratification being made by the methods which are least costly, it follows, according to the theory, that there will be the maximum surplus of satisfaction over sacrifice.

But if this result is to follow, two things would appear to be necessary: (1) goods must go to the consumers who will derive the greatest pleasure from them, and (2) the tasks of making goods must be assigned to the workers who can perform them with the least sacrifice for each unit of product. Does freedom of enterprise cause either goods or jobs to be distributed in this manner?

[U]nder a system of free enterprise goods tend to get into the hands of those who offer the best prices for them. But how then can they be consumed so as to yield the maximum of satisfaction? Are the people who are willing and able to pay most for goods also those who will derive the most satisfaction from using them? If they are not, it would appear possible to increase the surplus of satisfaction over sacrifice by causing goods to be distributed more in accordance with needs and less in accordance with ability to pay. We have no way of comparing the amount of pleasure which two persons derive from consuming an article. And yet it seems ridiculous to assert that ability to derive satisfaction from goods is proportionate to ability to pay for them. . . .

We are no better able to compare the pains suffered by different persons than we are the pleasures which they enjoy. Nevertheless it does not appear probable that freedom of enterprise necessarily causes jobs to be distributed so as to result in a minimum sacrifice for each unit of output—so that, for example, persons who can do heavy work with least fatigue will be given heavy work. Rather jobs tend to go to those who are willing to do the most work for the least money. . . .

In face of the fact that ability to derive pleasure from goods does not appear to correspond to capacity to pay for them and that jobs are not necessarily given to the men who can do them with the least sacrifice for each unit of product, how can it be asserted that industrial liberty results in a maximum of satisfaction over

sacrifice? But the exponents of free enterprise are not without a reply. To interfere with liberty in order to bring about a distribution of goods upon the basis of needs rather than ability to pay, or in order to cause jobs to be assigned to those who perform them with least sacrifice, might have the *immediate* effect of increasing the surplus of satisfaction over sacrifice. But this result, it is said, would be short lived. Men have the greatest incentive to improve their efficiency when they are free to compete for any jobs which they desire and to spend their income as they see fit. Were this incentive diminished by distributing jobs to those who could perform them with the least sacrifice and goods to those who would derive the most pleasure from them, output would inevitably decline. What would be gained by a different distribution of goods and jobs would be lost through smaller production.

\* \* \*

The theory of free enterprise does not, it is important to emphasize, assert that restraints upon human selfishness are not needed. It simply assumes that they are provided by *competition*. This, according to the theory, is the great regulative force which establishes effective control over economic activities and gives each of us an incentive to observe the interests of others. Thus business establishments are deterred from furnishing adulterated or poorly made goods by the fear that customers may shift their patronage to rivals. Likewise the enterprises which fail to protect their men against accidents or industrial disease or which work them unusually hard are penalized by the refusal of laborers to work for them except at a higher wage than other employers pay.

The mere existence of competition, however, is not enough. For it to perform satisfactorily the protective function attributed to it, certain very definite conditions must be present.

To begin with, an appreciable proportion of buyers and sellers must be willing to discriminate against those sellers or buyers who ignore, and in favor of those who take account of, the welfare of others. Otherwise, of course, no one has an economic incentive to pay attention to the well-being of his fellows. Assume, for example, that an enterprise pollutes a stream by dumping refuse and chemicals into it. From the standpoint of the firm, this may be an economical method of production. But from the standpoint of the community it is an expensive one, because it kills the fish, spoils the stream for bathing, and makes it

foul and ill-smelling. But competition will not stop the pollution unless an appreciable number of consumers, wage earners, or investors refuse to deal with the firm which is responsible—that is, unless a substantial number of consumers refuse to buy from it, or wage earners to work for it, or investors to put money into it. But if the enterprise charges no more than its rivals for goods of equal grade, offers equally attractive conditions of employment, and pays as high dividends, who has an interest in discriminating against it? Perhaps the very fact that the enterprise pollutes the stream enables it to offer better terms than its rivals. Or take the case of child labor—another method of production cheap in dollars and cents but expensive in terms of human cost. If the firms which employ children are able, *because of that very fact*, to sell for less or to pay higher wages to adults or higher profits to investors, who is going to discriminate against them? Under these circumstances, does not competition positively encourage the employment of children?

But willingness to discriminate between those who consider the interests of others and those who do not is insufficient. Competition protects consumers against inferior ware only when they know good quality from bad; it protects laborers from unguarded machines only when they know which employers have and which have not guarded their machines. In other words, competition is an efficient protective agency only when buyers or sellers have the information necessary to make intelligent choices. It fails, for example, to protect consumers against milk from tubercular cattle because the ordinary buyer of milk has no way of distinguishing the milk of healthy cows from that of diseased.

\* \* \*

Perhaps the most striking aspect of the theory of free enterprise is its assertion that intervention of the government in economic activities is unnecessary. The theory, as we have said, does not deny that restraints on human selfishness are needed. It simply asserts that we can trust competition to provide them. But closer inquiry reveals that the defenders of free enterprise do not trust competition to do all things. However much they trust it to guard the lives and limbs of workmen against dangerous machinery or to protect consumers against injurious foods, they do not rely upon it to enforce contracts or to prevent fraud. But the same reasoning which is used to

prove that the government need not intervene on behalf of wage earners and consumers can be employed to show that laws are not required to guard business men against fraud or breach of contractual obligations. Would not a customer who refused to pay his bills soon experience difficulty in getting dealers to sell to him, and would not an enterprise which violated its contracts find other concerns unwilling to deal with it? Is not the aid of the courts in these matters as superfluous as laws to protect workmen against dangerous machines or consumers against adulterated wares?

## NOTE

W. J. M. MacKENZIE  
POLITICS AND SOCIAL SCIENCE\*

The invisible hand of perfect competition was deemed to maximize utility by making decisions which were in a sense not decisions at all. Part of the puzzle is that in common use the word "decision" has something to do with an individual choosing, according to criteria which he has chosen or at least accepted, between options present to his mind. A market in a sense sums individual decisions, and it is easy to slip into talking of market "decisions," as one slips into talking of computer "decisions." But one of the blessed characteristics of a market is that the outcome is no one's "decision": it is an "event." As has been said often, seriously or in sarcasm, it is a situation almost too good to be true, that nature should thus optimize and distribute for us, without human responsibility.

## 2.

### Free Choice about Risktaking?

Guido Calabresi  
The Costs of Accidents—A Legal and  
Economic Analysis†

\* \* \*

[T]he primary way in which a society may seek to reduce accident costs is to discourage activities that are "accident prone" and substitute

\* Baltimore: Penguin Books 139 (1967). © W. J. M. MacKenzie, 1967. Reprinted by permission.

† New Haven: Yale University Press 68-94 (1970). Reprinted by permission.

safer activities as well as safer ways of engaging in the same activities. But such a statement suggests neither the degree to which we wish to discourage such activities nor the means for doing so. [W]e certainly do not wish to avoid accident costs at all costs by forbidding all accident-prone activities. Most activities can be carried out safely enough or be sufficiently reduced in frequency so that there is a point at which their worth outweighs the costs of the accidents they cause. Specific prohibition or deterrence of most activities would cost society more than it would save in accident costs prevented. We want the fact that activities cause accidents to influence our choices among activities and among ways of doing them. But we want to limit this influence to a degree that is justified by the cost of these accidents. The obvious question is, how do we do this?

There are two basic approaches to making these difficult "decisions for accidents," and our society has always used both, though not always to the same degree. The first, which I have termed the specific deterrence or collective approach . . . involves deciding collectively the degree to which we want any given activity, who should participate in it, and how we want it done. These decisions may or may not be made solely on the basis of the accident costs the activity causes. The collective decisions are enforced by penalties on those who violate them.

The other approach . . . involves attempting instead to decide what the accident costs of activities are and letting the *market* determine the degree to which, and the ways in which, activities are desired given such costs. Similarly, it involves giving people freedom to choose whether they would rather engage in the activity and pay the cost of doing so, including accident costs, or, given the accident costs, engage in safer activities that might otherwise have seemed less desirable. I call this approach general, or market, deterrence.

The crucial thing about the general deterrence approach to accidents is that it does not involve an a priori collective decision as to the correct number of accidents. General deterrence implies that accident costs would be treated as one of the many costs we face whenever we do anything. Since we cannot have everything we want, individually or as a society, whenever we choose one thing we give up others. General deterrence attempts to force individuals to consider accident costs in choosing among activities. The problem is getting the best combination of choices available. The general deterrence ap-

proach would let the free market or price system tally the choices.

\* \* \*

The general deterrence approach treats accident costs as it does any other costs of goods and activities—such as the metal, or the time it takes, to make cars. If all activities reflect the accident costs they "cause," each individual will be able to choose for himself whether an activity is worth the accident costs it "causes." The sum of these choices is, *ex hypothesi*, the best combination available and will determine the degree to which accident-prone activities are engaged in (if at all), how they are engaged in, and who will engage in them. Failure to include accident costs in the prices of activities will, according to the theory, cause people to choose more accident-prone activities than they would if the prices of these activities made them pay for these accident costs, resulting in more accident costs than we want. Forbidding accident-prone activities *despite* the fact that they can "pay" their costs would, in theory, bring about an equally bad result from the resource allocation point of view. Either way, the postulate that individuals know best for themselves would be violated.

\* \* \*

For the theory to make some sense there is no need to postulate a world made up of economic men who consciously consider the relative costs of each different good and the relative pleasure derived from each. If the cost of all automobile accidents were suddenly to be paid out of a general social insurance fund, the expense of owning a car would be a good deal lower than it is now since people would no longer need to worry about buying insurance. The result would be that some people would buy more cars. Perhaps they would be teen-agers who can afford \$100 for an old jalopy but who cannot afford—or whose fathers cannot afford—the insurance. Or they might be people who could only afford a second car so long as no added insurance was involved. In any event, the demand for cars would increase, and so would the number of cars produced. Indeed, the effect on car purchases would be much the same as if the government suddenly chose to pay the cost of the steel used by automobile manufacturers and to raise the money out of general taxes. In each case the objection would be the same. In each, an economist would say, resources are misallocated in that goods are produced that the con-



sumer would not want if he had to pay the full extent of their cost to society, whether in terms of the physical components of the product or in terms of the expense of accidents associated with its production and use.

\* \* \*

To describe a world of perfect general deterrence is to refute its possibility. In the extreme, general deterrence would consider the problem of accident costs to be precisely one of market decisions to buy and sell goods, and accident costs would give rise to *no* collective decisions regarding whether activities were worthwhile. If an activity could pay for the accidents it caused and make a go of it, it would be considered worthwhile; if it could not pay such costs, it would be priced out of the market. But for this to work properly, activities would have to be able to "buy" willing victims. Just as the potential injurer would have to decide whether the accident was worth its costs to him, so the victim would have to decide whether the payment to be received in compensation made the accident worthwhile from his point of view. The costs of accidents would then be determined freely by the market, and there would be no need for collective intervention.

Obviously this is a highly improbable situation. In the extreme, it presupposes such perfect knowledge that all "accidents" would be come intentional killings, mayhems, or taking of property by the injurer; and suicides or sales of person or property by the victim. Short of the extreme, it presupposes at least a *statistical* intention to injure and a *statistical* willingness to be injured (for a price) that does not and cannot represent the attitudes of actual injurers and victims in our society. An examination of each of these "theoretical" worlds of general deterrence will disclose what an actual world of optimal general deterrence would look like.

The first of these theoretical worlds requires perfect knowledge, so that both the victim and the injurer would know that a particular act performed at a particular time in a particular way would result in a particular injury. Victim and injurer could then bargain without collective intervention for an appropriate price to be paid for the injury. But it is virtually inconceivable that with such perfect knowledge the parties would not find it cheaper to act in a slightly different way and avoid the injury. In other words, with such perfect knowledge there would be no "accidents" and virtually no injuries—general deter-

rence would work perfectly. The few injuries that would remain would be acts of madmen, and intolerable—or perfect martyrdoms, and divine.

It might be thought that even without perfect knowledge, pure general deterrence, requiring no collective determination of accident costs, could exist so long as we have statistically certain injurers and victims. People willing to take a risk for a price would form a supply of victims, and people willing to pay the price in order to undertake activities which injure would form a demand side for the victims.

\* \* \*

It may appear that potential victims do undertake some risky enterprises in exchange for payments that compensate them for the risk of being injured. Thus it may seem that both the supply and the demand sides of an injury market exist, and that no collective decisions are necessary to the determination of the costs of accidents. Closer examination, however, casts severe doubt on the feasibility of this statistical world of pure general deterrence.

It depends on market bargains—establishing the value of accident costs—between *potential* injurers and *potential* victims *before* the accident. In practice this presupposes that the potential victims are already in a bargaining relationship with the potential injurers *before* the accident and can therefore ask for payment for being potential victims. Market determination of accident costs is not meaningful in any practical sense in situations where the injuring party has no bargaining relationship with the potential victims before the accident. For example, while it may appear that coal miners receive, and mine owners pay, wages that reflect the costs of mine accidents as estimated by both sides, it is hard to see how a similar bargain could be struck between drivers and pedestrians. In theory, drivers might seek out all the pedestrians whom they could conceivably injure, offer them an amount of money in exchange for taking the risk, receive counteroffers from them, and ultimately strike bargains establishing market values for the costs of car-pedestrian accidents. But such bargains are inconceivable in reality.

Even in the case where potential victims are in a bargaining relationship with potential injurers before the accident, it is usually unrealistic to treat market determination of the value of accident costs as adequate.

In the first place, adequate market determination of accident costs requires freedom on



the part of victims to refuse the bargain and thus avoid the risk of injury. If the organization of our society is such that the only alternative work available to coal miners is the almost equally dangerous occupation of lumberjacking, it is hard to accept the valuation of accident costs arrived at through the "free" bargains between miners and mine owners as a satisfactory estimate of the accident costs involved.

In the second place, free market determination of the value of accident costs will lead to an acceptable result only if the potential injurers and victims are reasonably aware of and take account of the risks, i.e., only if they have adequate statistical knowledge of the risks involved and act on that knowledge. Injurers may often obtain and act on such knowledge, but as we have seen in our discussion of why private insurance is not likely to bring about adequate loss spreading, victims are unlikely to do either. Virtually all the arguments made there apply here: it may be very expensive for potential victims to obtain adequate knowledge of what the risk is; they may be psychologically incapable of viewing themselves as *actual* victims; they may suffer from the Faust complex and inevitably choose the good life now and regret it later; and they may not be the only ones to bear the costs when they occur.

In the third place, statistical willingness to take risks does not give an adequate value for what an accident costs if it actually occurs. This is because the value individuals give to a particular accident depends on the likelihood of its occurring. A man may take \$1,000 for one chance in a thousand of being killed, thus seeming to value his life at \$1,000,000, and still require much more than \$10,000 for one chance in a hundred of being killed. And if he accepted \$50,000 for one chance in a hundred, thus valuing his life at \$5,000,000, this would be no indication that he would accept \$2,500,000 for one chance in two of being killed, or \$5,000,000 in exchange for the certainty of death. [T]his does not mean we should not try to give values to lives and limbs, but especially when coupled with the other factors I have just mentioned, it makes any purely market determination of the costs of accidents entirely inadequate.

For all these reasons, even a society that is basically committed to the general notion that individuals know best for themselves, and hence to general deterrence, will not leave valuations of accident costs to the free market. Some of the problems with free market determination of the

value of accident costs will be mitigated in situations where the bargaining on the part of potential victims is carried out by representatives (such as unions) who presumably are aware of the risks and do not suffer from the same psychological disadvantages. But this mitigation will occur, at best, only in some of the bargaining situations, which themselves are only a part of the world of accident costs.

\* \* \*

Of course, even if we thought that the problems with free market accident cost determination could all be overcome, we might still find a world of total general deterrence not to our liking. The allocation of losses necessary in such a world might result in intolerable concentrations of losses, the administrative costs of establishing and running this world might be too high, and our "moral" sense might be offended by activities that could pay their way under such a system. As a result, collective judgments would still be required. In other words, a world of total general deterrence might not be desirable because it does not allow room for our other goals.

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#### NOTE

FRANK H. KNIGHT  
FREEDOM AND REFORM\*

\* \* \*

Individual freedom of patients to select their doctors, and of men generally to be doctors, is exactly the meaning of what is called free competition or, more accurately, the open-market organization. Social planners usually misconceive these facts. There is no implication of competition in the psychological sense of a feeling of rivalry, or action motivated by this feeling; on the contrary, such feeling and action are definitely irrational, in the instrumental or means-and-end definition. Economic competition is one of the unfortunate accidents of terminology; what it means is simply the freedom of individuals to cooperate through exchange with the others who offer (or accept) the best terms. The idea of "bargaining" is likewise misleading. In an effective market there is no bargaining in the sense of higgling or discussion or influence, and in most real markets there is very little.

\* New York: Harper and Brothers 360-364 (1947). Copyright, 1947, by Harper & Row, Publishers, Inc. Reprinted by permission.

To discuss market competition as a method of organized action would mean summarizing the science of economics. It may be viewed either as an alternative to collective rationality or as an alternative form of the latter or method of achieving the same general result. We can only assert "dogmatically" that the theoretically perfect, or "perfectly competitive" market has certain consequences for the given individuals involved. It leads to the "maximum" of efficiency consistent with individual freedom, in the only sense in which association can be free, i.e., based on rational mutual consent, and also results in "justice" in the sense of exchange of equal values. That is, the return to each participant is equal to his contribution to the total result of the joint activity; each takes out the equivalent of what he puts in, in the only possible meaning of quantitative equivalence.

This of course is "theory." On the other side we note two general facts, also without explanation or argument. The first is that real markets are more or less imperfect; the second, the far more important fact that the ethical quality of the result is limited by the consideration that the individuals are taken as given, specifically with respect to their desires and their possession and control of productive capacity in all forms. The main mechanical limitations are monopoly and the business cycle. Popular ideas about monopoly fantastically exaggerate both its amount and the evil of that which exists, particularly when society is not already demoralized by crisis conditions. A considerable amount of monopoly is inevitable and more is natural and useful in a free and progressive economy. In extreme cases, where conditions make reasonably effective competition impossible or grossly wasteful (such as public utilities, railways, etc.) public authority always steps in to "regulate" the industry or to operate it directly. As to cycle phenomena, the main fact is that depressions benefit virtually no one and accordingly do not arise out of conflicts of interest. Consequently, the remedy does not present an ethical problem but is purely a matter of science and political competence.

The major ethical problem of economic organization arises out of the grossly unequal distribution of economic capacity, and consequently of the product, among individuals, and the fact that distribution is determined for the most part by forces beyond the control of the disadvantaged individuals and classes, while the working of the free exchange system naturally tends toward increasing inequality. The simple

and obvious remedy for inequality, insofar as it is unjust and is practically remediable, is not planning by a central authority, but progressive taxation, particularly of inheritances, with use of the proceeds to provide services for the poorer people. Particularly in point are relief of destitution, health measures, and educational opportunities for the young. This remedy has long been widely applied. . . .

. . . This points in a very different direction from central planning, which obviously means curing the root evil of excessively unequal distribution of economic power through an enormously greater concentration—in economic terms, a universal monopoly. And if any prediction is possible, this would not tend either to equalize the distribution of income itself, or to increase efficiency, while freedom, the greatest of the fundamental human values, would be largely sacrificed. Within wide limits, free government is to be preferred to good government (in any other meaning) or freer to better; and the reasonable inference from history, current experience, and reasoning in general terms is that planning by any central authority would sacrifice the one and mean a loss rather than a gain in terms of the other. Any government which had the task of managing the economic life of a modern nation, to say nothing of the world, would have to be a dictatorship and to repress the primary freedoms of thought, communication and association. This would be true even if it were staffed with people who personally abhorred power—and the contention that power would fall into the hands of such people will appeal only to the most romantic credulity.

However, the problem is complex, and certainly calls for a combination of practically all conceivable forms of solution. The positive values in what we call economic life itself are more aesthetic, social and cultural than really individual; and even where they seem to be individualistic, there are limits to the possibility of allowing each individual to be his own judge of what is good for him, or the means of achieving it. The real defects of the "competitive" economic order as revealed by objective analysis are largely due to limitations of the rationality of individuals and free groups, and a general replacement of free action and voluntary association with political compulsion would certainly mean a decrease and not an increase in rationality. Centralization of authority within any political unit would be achieved through the domination, by force, emotional appeal, or outright trickery, of

some particular interest group, and under conditions which probably mean mobilization for war against opposed internal interests or externally against other units. . . .

Yet there is a place for centralized planning under authority, with some use of force, of innumerable activities and with an infinite variety in the scope of the units. All government is by nature central planning. But there is a vast difference in principle between general laws, of the nature of traffic regulations or rules of the game, and concrete prescription of where, when, and

how to travel or what game to play. The main difficulty is that planning always means replanning, and the imposition of some particular plan out of an infinite number of possibilities, and under some particular authority, among innumerable claimants. Most of the possibilities under both heads have both merits and limitations, and the real question is how far to go at the same time in most of the possible directions.

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## B.

### Collective Planning—The Rational-Comprehensive Approach

#### 1.

#### Cooperation through Comprehensive Analysis?

##### a.

#### Harold D. Lasswell The Political Science of Science\*

\* \* \*

[The] task is to construct a continuing institutional activity by which central theory is related continuously to events as they unfold. . . .

The limited degree of success achieved by the profession in perfecting or in encouraging the body politic to perfect such an institutional process had adverse consequences for our role in regard to nuclear weapons. Long before atomic weapons were introduced we were well aware of the importance of scientific knowledge for the technology of fighting. But we did not correctly anticipate the approximate timing of the impact of nuclear physics upon military technology. Although we were equipped to assess the political consequences of sudden and stupendous increases of fighting effectiveness we did not foresee that such an emergent was imminent. Since technical developments were not explicitly anticipated we did not clarify in advance the main policy alternatives open to decision makers in this country or elsewhere. We did not create a literature or a body of oral analysis that seriously anticipated these issues. As political scientists we should have anticipated fully both the bomb and the significant problems of policy that came with it.

I do not want to create the impression that all would have been well if we had been better political scientists, and that we must bear upon our puny shoulders the burden of culpability for the situation of the world today. We are not so grandiose as to magnify our role or our responsibility beyond all proportion. Yet I cannot refrain from acknowledging, as I look back, that we left the minds of our decision makers flagrantly unprepared to meet the crisis precipitated by the bomb. I have no desire to hold a kangaroo court on President Truman's momentous decision or upon his principal advisers; or to give credence to the insinuation that "results" had become necessary in the face of Congressional restiveness about the cost of research and development. In the light of hindsight (that should have been foresight) I want to underline the probability that the new weapon was introduced in a manner that contributed unnecessarily to world insecurity. Perhaps the critics are right who say that the bomb should have been demonstrated on an uninhabited island before the live drops were made on Hiroshima and Nagasaki. More important is the question of how formal and effective control might have been extended beyond the decision makers of a single power. At least some members of the winning coalition might wisely have been brought into a system that operated through a common agency of inspection and direction.

Plainly there were not enough political scientists trained in physics, or sufficiently aware of the implication of impending scientific developments, to do much forward thinking and planning. This points to a failure of professional recruitment and training, and calls in question the then-prevailing conception of the political

\* 50 *The American Political Science Review* 961-979 (1956). Reprinted by permission.

scientist's role. . . . Part of our role, as the venerable metaphor has it, is scanning the horizon of the unfolding future with a view to defining in advance the probable import of what is foreseeable for the navigators of the Ship of State. It is our responsibility to flagellate our minds toward creativity, toward bringing into the stream of emerging events conceptions of future strategy that, if adopted, will increase the probability that ideal aspirations will be more approximately realized.

An implication for our future relation to science and armament is that we need to develop more political scientists who have the competence to infer the weapon implications of science and technology. It then becomes possible to anticipate the implications for collective policy.

Even a moderate degree of cross-disciplinary training or continuing contact should have enabled us to prepare for the advent of nuclear fission (and fusion). The *Review of Modern Physics* carried an article by Louis Turner of Princeton University in January 1940 in which 133 papers were appraised. They began with Fermi's original report of 1934 and came down to the Hahn-Strassman-Meitner researches which made explicit the import of Fermi's original experiment. In passing it may be noted that the contributions of a dozen nations were catalogued in Turner's review. Not more than half a dozen of the 133 papers were by American authors. Perhaps American political scientists may be partially absolved for lack of foresight under these circumstances. But the over-all record of the profession is not thereby improved, since I do not find that colleagues in other countries were any more in touch than we were. Incidentally, it is worth recording that a standard college textbook in physics included a chapter in which the implications of current research were clearly spelled out. Ernest Pollard of Yale University referred in 1940 to the possibility of nuclear reactors that might generate electrical power or detonate as immensely destructive bombs; or that might produce radioactive substances for research and industrial processes or for a new and frightful kind of chemical warfare. I note further that at the time of the Fermi-Dunning experiment at the Columbia University cyclotron in early 1939 some science writers (especially of the *New York Times*) were quite definite about what was at stake.

\* \* \*

In many ways the most disturbing result of the laggard position of political scientists in com-

prehending science and technology is that we have displayed no intellectual initiative in furnishing guidance to those who are in command of modern knowledge and its instrumentalities. Alert businessmen have long been on the lookout for promising applications in the marketplace. The professional military man is now accustomed to take the initiative. The question for us as political scientists is whether we have given enough serious attention to the task of reducing the human cost of whatever violence we cannot dispense with.

As an exercise in this line of thought I invite you to use your imagination to ask what an instrument of coercion would look like that incapacitates without killing, mutilating or in any way imposing permanent incapacity. You and I will probably come up with the same answer: a gas or a drug or a beam that when applied will induce sleep or a similar state of suspension. We spent several billion dollars on A and H bombs; and it is commonly said, with some plausibility, that scientists and engineers give you what you pay for. Our suggestion (and I repeat an old proposal) is that we go down the alphabet to the P bomb, the "paralysis bomb." The technical difficulties in the way of paralyzing a city or a region are very great, given current means of delivering a concentrated gas. Possibly the instrument can be a "P beam," a paralyzing beam of sound or of some other kind capable of accomplishing the purpose.

Without being in the least committed to the specific devices referred to, I nevertheless assert that in the future we need not remain as passive as we have been in approaching the problem of harmonizing considerations of humanity with the use of whatever coercion cannot be avoided.

\* \* \*

As clarifiers of the goals and alternatives implicit in a decision process and as advisers of the participants we have an opportunity to reduce the amount of unnecessary friction by establishing a frame of reference in advance of the facts. When factual details appear they will of course exhibit some novel elements; common goals and principles will not. The members of the world community have a long history of accommodating "exclusive" claims and "sharing" claims with one another (as new resources provide new base values for the participants in the world arena).

It is, of course, essential that in taking advantage of this opportunity we deal with the entire context of value goals and principles as they

relate to potential facts. I have referred to sets of doctrines that in all probability will be invoked when claims are made. The chief function of these formulations is to guide the attention of decision makers to the context in which pertinent activities occur. Formulas assist in recognizing and evaluating the consequences for international public order of accepting the exclusive or the sharing claim in particular cases or categories of cases.

\* \* \*

When we think configuratively about the problems raised in reference to the new resources it is clear that instead of relying on blanket principles (like "freedom of the seas" or "freedom of the air") the most fruitful policy alternatives are likely to emerge when we anticipate the appearance of characteristic factual contexts, and consider how the values chiefly at stake in them can be maximized. Hence we would not expect to apply the same prescription (1) to the sharing of air space for weather observation (where equipment is used that is expressly designed for the purpose and perhaps registered, and when the information obtained is made public) and (2) to the sharing of air space for projects of weather or climate control that may be deleterious to local values.

The contextual (or, synonymously, the configurative) approach is a challenge to imagine the full range of possible means of anticipating and resolving difficulties. On the most uncertain matters it is appropriate to call attention to the need of exploring the possibilities of agreement in advance of conflict. The inference is that no time should be lost, for instance, in putting into the hands of the UN the facilities for research, development and operation of satellites, "space platforms" and travel beyond the limits of the earth's atmospheric and gravitational fields. Doubtless the USA and the USSR will continue to compete with independent programs. Since the polar powers have a stake in moderating the conflict in which they are engaged in the hope of eventual harmony through agreement, not catastrophe, a practical method would appear to be to strengthen the "third factor," especially when both powers are also included within it.

The rapid introduction of new resources under present conditions calls for some degree of community and regional planning; and planning poses thorny questions about the structure and ideology of society. To an increasing extent questions of this kind need to be answered directly rather than by default. . . . For instance,

we have not explored the principles of proportion that are most likely to consolidate or to sustain at various stages of industrial growth the perspectives and operational technique of popular government. Shall we, for example, rely upon a 30-40-50 rule to guide public policy in regard to the permissible degree of market control permitted to private interests? (For example: When one interest has 30 per cent control of output, shall it be subject to special regulations designed to nullify the side-effects of power that go along with economic control? When one interest rises to 40 per cent shall we put governmentally appointed trustees on the Board of Directors? At 50 per cent shall government trustees predominate?)

Whatever the workable rules of proportion may be in representative contexts it is evident that we need to guide our studies of trend correlation and of comparative cases in order to improve the available bases of inference in such matters.

The same approach—the search for rules of proportion—applies to every institutional and personality pattern in a body politic. What are the optimum proportions of community resources to devote to elementary, intermediate, advanced and ultra-advanced education? To research and development in science and technology? To positive and negative sanctions for correctional and other purposes?

One way to jar "cakes of custom" out of the mind is to draft specifications for the first Mayflower expedition to establish continuing occupation outside the earth. (Possibly it could be "Noah's Jet"? ) What proportion of men, women and children of which culture or combination of earth cultures shall we select? What ideological traditions, secular and sacred? What class backgrounds (elite, mid-elite, mass)? What individual and group interests? What personality structures?

By asking questions of this kind we are in a position to assess our present stock of knowledge concerning the interdependence of institutions specialized to power, and all other institutions in the social process of any community, together with the forms of personality involved. These, of course, are the recurring issues of political science and historical interpretation as well as policy.

. . . As political scientists we are perhaps even less well prepared to anticipate developments in genetics, experimental embryology and related disciplines. Taken together these fields signify that, as Julian Huxley has often put it,

man is on the threshold of taking evolution into his own hands. By influencing the genes that constitute the key units in man's biological inheritance we affect the entire potential of future generations.

\* \* \*

It has been pointed out that perhaps the most satisfactory index of genetic damage is the sum of tangible defects existing among living individuals. We are speaking of such stigmata as "mental defects, epilepsy, congenital malformations, neuromuscular defects, hematological and endocrine defects, defects in vision or hearing, cutaneous and skeletal defects, or defects in the gastrointestinal or genitourinary tracts." We are informed that about 2 per cent of the live births in the United States have defects that are of "simple genetic origin and appear prior to sexual maturity." If mankind were subjected to a "double dosing" of radiation the present level of genetic defects would rise, and would eventually be doubled.

Regulatory measures are obviously needed against wars and weapon tests; and they are essential to the disposition of nuclear waste from industrial plants. (It has been remarked that a nuclear power plant is to be viewed as a large-scale production of both highly poisonous gas and explosives under a single roof.)

The principal questions to which I desire to call attention pose issues of a relatively new and different order. Some of these questions have already come up in controversies over artificial insemination. They have embarrassed the champions of the orthodox prescriptions that prevail in several fields (theology, ethics, jurisprudence). Shall we call a child legitimate whose biological father is not identical with the sociological father? Even with the consent of the latter? With spermatozoa from a known or unknown source? (A possible international question is whether a nation state like the United States can claim the child as a citizen if the spermatozoa employed originated with an American mail order house and was sent by air mail for use abroad.)

Poignant as these issues are in specific cases they do not confront us with the consequences for public order that are to be anticipated if the progress of biology separates insemination and child-bearing from genital contact. The assumption is often made that the continuation of sexual rectitude and even civic order depends

upon charging every genital contact with the blessings and perils of procreation. The impending improvement of oral contraceptives, joined with other recent advances, are factors that already suggest the wisdom of other norms and sanctions of public order.

Other developments are threatening current ratios of the influence and power of the sexes. Given the millions and millions of spermatozoa produced by one male and the technique of canning by refrigeration, any very large number of males becomes relatively redundant for purposes of procreation. Must the male rest his future upon other values such as the strictly aesthetic appeal of the male contour? Before the female of the species becomes too complacent in this context it may be worth recalling the significance of some current experiments for the removal of the primordial female function from the body and into other receptacles. (Women, too, may have to rely upon their charm, a role for which their experience has provided extensive preparation.)

Apparently we are closer than most of us like to think to the production of species that occupy an intermediate position between man and the lower animals (or even plants). It is sometimes said, even in august quarters, that "one has not yet succeeded in making a species from another species." Theodosius Dobzhansky notes, however, that "the feat of obtaining a new species was accomplished more than a quarter of a century ago." In recent decades a fair number of new species have been brought into being. It is also true that some species that exist in nature have been recreated experimentally. A garrison police regime fully cognizant of science and technology can, in all probability, eventually aspire to biologize the class and caste system by selective breeding and training. Such beings can, in effect, be sown and harvested for specialized garrison police services or for other chosen operations.

Great strides have been taken in brain design. Experimental models of robots have been built who solve problems of a rather complex order in a given environment. Some of these machines look after themselves to a degree, obtaining and using the raw materials required for energy and repairs. Already it is claimed that the function of reproducing its kind, and of interacting with others, can be in-built.

The question then rises: Given our concern for human dignity when do we wisely extend all or part of the Universal Declaration of Human Rights to these forms? When do we accept the

humanoids—the species intermediate between lower species and man, and which may resemble us in physique as well as in the possession of an approximately equivalent central nervous and cortical system—as at least partial participants in the body politic? And at what point do we accept the incorporation of relatively self-perpetuating and mutually influencing “super-machines” or “ex-robots” as beings entitled to the policies expressed in the Universal Declaration?

It is obvious that we are not too well equipped by cultural tradition to cope with these problems. A trait of our civilization is the intense sentimentalization of superficial differences in the visible format of the groupings to be found even within the human species. Recall the theologians, ethicists and jurists who have devoted themselves to the elaboration of symbols to show that the white race alone is genuinely human and hence solely entitled to the dignity of freedom. Recall, too, the counter-assertions, nourished in the soil of humiliation, that have arisen among ethnic groups that seek to overcome their contempt for themselves by dragging down the pretensions of the white imperialist.

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The most disturbing question, perhaps, arises when we reflect upon the possibility that super-gifted men, or even new species possessing superior talent, will emerge as a result of research and development by geneticists, embryologists or machine makers. In principle, it is not too difficult to imagine a superior form. For instance, our sensory equipment does not enable us to take note of dangerous radiation levels in the environment. We have no inborn chattering of a Geiger counter.

I spoke before of taking the intellectual initiative for the use of science and technology for the fuller realization of our value goals. It is plain that if we bring certain kinds of living forms into the world we may be introducing a biological elite capable of treating us in the manner in which imperial powers have so often treated the weak. A question is whether the cultivation of superior qualities ought to be limited to intellectual capability. The answer, I feel confident you will agree, is in the negative. We need to be sufficiently vigilant to prevent the turning loose on the world of a hyper-intelligent species driven by an instinctual system especially inclined toward predation. The blood-stained story of our own species is only too familiar (the

stories about succulent missionaries whose bodies were more readily incorporated than their messages are not wholly without foundation). Can we improve the prospects of developing a form of intelligent life copied not after our own image, but after the image of our nobler aspirations?

It is not to be overlooked that the problem of human capability can become acute if in the years ahead we escape from our present habitat on the earth, or are visited by other forms of intelligent life. There are, after all, untold millions of environments resembling our solar system, and it would be more remarkable to find that but one planet is inhabited by a complex living form than to encounter parallel developments. It would of course be embarrassing, at least, to discover that we are the savages or that we are put together on a markedly inferior biological plan.

The fact is that many of the problems to which I have been referring will be upon us long before we can make great changes in the ideological outlook or the socio-political patterns of life in this country or elsewhere. The same point applies to ourselves in our role as individuals and as members of the political science profession. Considering our present predispositions how can we improve the likelihood of contributing to the decision process at every level, from the neighborhood to the world as a whole?

It is abundantly clear that the impact of science and technology does not occur in a social vacuum, but in a context of human identifications, demands and expectations. I make the modest proposal that it is appropriate for political scientists, in company with other scientists and scholars dealing with human affairs, to improve our procedures of continuous deliberation upon the potential impacts of science and technology upon human affairs. No doubt the American Political Science Association and other professional societies constitute an appropriate network for the purpose. We can sustain continuing conferences devoted to the examination of emerging developments. As fellow professionals we have special responsibility for giving thought to the aggregate effects of any specific innovation.

Our first professional contribution, it appears, is to project a comprehensive image of the future for the purpose of indicating how our overriding goal values are likely to be affected if current policies continue.

A closely related contribution consists in clarifying the fundamental goal values of the body politic. We are accustomed to confront po-



litical ideologies with new factual contingencies and to suggest appropriate specific interpretations. We also confront political doctrines with rival doctrines, and with comprehensive theological and metaphysical systems. I have called attention to the point that the basic value systems of European civilization, in particular, are likely to be exposed to sweeping challenge as biology and engineering narrow the obvious differences between man and neighboring species, and between man and centrally operating machines. The crisis will be peculiarly sharp if we create or discover forms of life superior to man in intellect or instinctual predispositions. Our traditions have not been life-centered, but man-centered. We possess various paranoid-like traditions of being "chosen." Clearly a difficult task of modifying these egocentric perspectives lies ahead.

The third task is historical and scientific. It is historical in the sense that by mobilizing knowledge about the past we are enabled to recognize the appearance of new patterns and the diffusion or restriction of the old. It is scientific in the sense that we summarize the past in order to confirm (or disconfirm) propositions about the interplay of predisposition and environment. If we are to serve the aims of historic recognition and of scientific analysis, one of our professional responsibilities is to expedite the development of more perfect institutions specialized to continual self-observation on a global scale. Self-observation requires guidance by a system of theoretical models of the political process in which a continuing gradation is maintained between the most inclusive model and submodels adjusted to more limited contexts in time and space. Continual self-observation renders it necessary at each step through time to reevaluate the appropriateness of the operational indices for the variables and concepts employed at the most recent step. In this way all the concepts that figure in systematic, descriptive political science can be kept chronically pertinent to the ordering of political events as the future unfolds.

The fourth task is inventive and evaluative. It consists in originating policy alternatives by means of which goal values can be maximized. In estimating the likely occurrence of an event (or event category), it is essential to take into account the historical trends and the scientifically ascertained predispositions in the world arena or any pertinent part thereof.

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## NOTE

STUART CHASE  
DEMOCRACY UNDER PRESSURE—  
SPECIAL INTERESTS VS. THE PUBLIC WELFARE\*

\* \* \*

Sometimes I have a clear picture of the way the Agenda . . . could be presented to the people. I see perhaps a hundred leading Americans, men and women, meeting in some high, quiet place to prepare it. . . . They are scientists, judges, teachers, university people, philosophers of business, lovers of the land, statesmen; and they think in terms of the whole community.

I picture them as people without ideologies or dogmatic principles, aware of their own shortcomings and the general inadequacy of mankind, as Wells put it. They are accustomed to approach a question with the scientific attitude, and to look at all the major characteristics of a situation before leaping to a conclusion. They are aware of the pitfalls of language.

\* \* \*

They ought, I think, to go up into the mountains somewhere.

\* \* \*

They could hold general meetings in the big Lodge, while subcommittees, working on detail problems, could meet wherever they pleased.

\* \* \*

It ought to clear the brain. The meeting should be held in summer rather than winter, with wild flowers, not snow. The delegates would do better to take their exercise on horseback, or fishing, rather than risk their tibias on the Canyon run.

I can see the Chairman getting to his feet in front of the big blue tapestry in the Lodge dining room to open the conference.

. . . I shall not quote him directly, but paraphrase his address, as I imagine it.

\* \* \*

We who are meeting here, I take it, represent no economic interest except that of the consumer, which means everybody. We are not specifically for "labor," for "capital," for farmers,

\* New York: Twentieth Century Fund, 133-142 (1945). © 1945 by The Twentieth Century Fund, New York. Reprinted by permission.



for organized medicine, for Wall Street, the West Coast, the export trade, the department stores, or for the manufacturers of Shocking Radiance perfume.

We are not in favor of "capitalism," "Socialism," "Fascism," "Communism," "individualism," or saving the world by the introduction of planned parenthood. We have gone through these vague ideologies and come out on the other side. We are in favor of keeping our minds open and the machines running. We want the community to go on, not to stop dead in its tracks as in 1929.

\* \* \*

We must have first-rate men in government, and public service made an attractive career to keen youngsters. We need a more enlightened civil service, better rules for tenure, many more schools of public administration.

\* \* \*

We want to offer reasoned suggestions as to which public activities should be centralized and handled from Washington, and which should be decentralized and handled regionally, like the TVA, or by the states, or by local governments. We want to know why we should tolerate 165,000 units of government at all levels.

We want to develop some pretty clear ideas about the three major forms of government control: regulation, control-without-ownership, and outright ownership. Which is best for a given activity?

\* \* \*

These are some of the concrete matters we are going to take up, the Chairman went on. In order to handle them wisely, we must keep in mind some longer-range principles. We must remember that it is the era of abundance we are trying to adjust to.

\* \* \*

Since 1929, the Chairman went on, any expectation of free, unmanaged economies is academic. We all know that, in our minds if not in our emotional nervous systems. Men cannot return to free, unmanaged economies so long as inanimate energy and mass production dominate human activity. Furthermore, I do not know how many of us, when we get right down to it, would like the London of Adam Smith. We have to cope with the age that is here. To run away from it is to become impotent. The parade back

to unlimited free enterprise is not an inspiring spectacle. It leaves young people confused and baffled. They want leaders, not retreaters.

Economic systems must now be managed. Have people in the democracies the brains to work out a kind of management which deals only with a few key functions and leaves most activities in private hands?

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Americans, the Chairman continues, were not brought up to plan for, or even think about, their national survival. It was taken for granted.

\* \* \*

Our forefathers set up an elaborate plan in 1787. They gave it a push and let it go. The expanding frontier carried it on for a hundred and fifty years. Lincoln had to do some managing, and so did Woodrow Wilson. But the New Deal marked the first time it was ever necessary to make over-all plans coordinating banks, farmers, and employment.

\* \* \*

The Chairman paused again . . . My time is about up. This isn't a speech but some ideas thrown out to get us started. A preliminary draft prepared by the steering committee is now before you. Each delegate has his copy. Your task is to round out this preliminary draft; take it as far as you can, as deep as you can, while holding general agreement. We want to obtain maximum agreement among ourselves. None of us belongs to pressure groups, but some of us have pet ideas. I implore you to drop them if they stand in the way of agreement. It isn't you who must be vindicated, it is your country. Broader still, it is democracy which must be vindicated.

b.

### **Bertrand Russell The Scientific Outlook\***

. . . The scientific society, as I conceive it, is one which employs the best scientific technique in production, in education, and in propaganda. But in addition to this, it has a characteristic which distinguishes it from the societies of the past, which have grown up by natural causes,

\* New York: W. W. Norton & Co., Inc. 203-205, 227-228, 233-235, 260-261, 268-269 (1931). Reprinted by permission of George Allen & Unwin, Ltd. and W. W. Norton & Co., Inc. Copyright 1931, renewed 1959 by Bertrand Russell.

without much conscious planning as regards their collective purpose and structure. No society can be regarded as fully scientific unless it has been created deliberately with a certain structure in order to fulfill certain purposes. . . .

[S]cientific technique has so enormously increased the power of governments that it has now become possible to produce . . . profound and intimate changes in social structure. . . . Science first taught us to create machines; it is now teaching us by Mendelian breeding and experimental embryology to create new plants and animals. There can be little doubt that similar methods will before long give us power, within wide limits, to create new human individuals differing in predetermined ways from the individuals produced by unaided nature. And by means of psychological and economic technique it is becoming possible to create societies as artificial as the steam engine, and as different from anything that would grow up of its own accord without deliberate intention on the part of human agents.

Such artificial societies will, of course, until social science is much more perfected than it is at present, have many unintended characteristics, even if their creators succeed in giving them all the characteristics that were intended. The unintended characteristics may easily prove more important than those that were foreseen, and may cause the artificially constructed societies to break down in one way or another. But I do not think it is open to doubt that the artificial creation of societies will continue and increase so long as scientific technique persists. The pleasure in planned construction is one of the most powerful motives in men who combine intelligence with energy; whatever can be constructed according to a plan, such men will endeavour to construct. So long as the technique for creating a new type of society exists there will be men seeking to employ this technique. They are likely to suppose themselves actuated by some idealistic motive, and it is possible that such motives may play a part in determining what sort of society they shall aim at creating. But the desire to create is not itself idealistic, since it is a form of the love of power, and while the power to create exists there will be men desirous of using this power even if unaided nature would produce a better result than any that can be brought about by deliberate intention.

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When I speak of scientific government I ought, perhaps, to explain what I mean by the

term. . . . I should define a government as in a greater or less degree scientific in proportion as it can produce intended results: the greater the number of results that it can both intend and produce, the more scientific it is. . . .

Owing to the increase of knowledge, it is possible for governments nowadays to achieve many more intended results than were possible in former times, and it is likely that before very long results which even now are impossible will become possible. The total abolition of poverty, for example, is at the present moment technically possible; that is to say, known methods of production, if wisely organized, would suffice to produce enough goods to keep the whole population of the globe in tolerable comfort. But although this is technically possible, it is not yet psychologically possible. International competition, class antagonisms, and the anarchic system of private enterprise stand in the way, and to remove these obstacles is no light task. The diminution of disease is a purpose which in Western nations encounters fewer obstacles and has therefore been more successfully pursued, but to this purpose also there are great obstacles throughout Asia. Eugenics, except in the form of sterilization of the feeble-minded, is not yet practical politics, but may become so within the next fifty years. [I]t may be superseded, when embryology is more advanced, by direct methods of operating upon the foetus.

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The man who dreams of a scientifically organized world and wishes to translate his dream into practice finds himself faced with many obstacles. There is the opposition of inertia and habit: people wish to continue behaving as they always have behaved, and living as they always have lived. There is the opposition of vested interest: an economic system inherited from feudal times gives advantages to men who have done nothing to deserve them, and these men, being rich and powerful, are able to place formidable obstacles in the way of fundamental change. In addition to these forces, there are also hostile idealisms. Christian ethics is in certain fundamental respects opposed to the scientific ethic which is gradually growing up. Christianity emphasizes the importance of the individual soul, and is not prepared to sanction the sacrifice of an innocent man for the sake of some ulterior good to the majority. Christianity, in a word, is unpolitical, as is natural since it grew up among men devoid of political power. The new ethic which is gradually growing in connexion with

scientific technique will have its eye upon society rather than upon the individual. It will have little use for the superstition of guilt and punishment, but will be prepared to make individuals suffer for the public good without inventing reasons purporting to show that they deserve to suffer. In this sense it will be ruthless, and according to traditional ideas immoral, but the change will have come about naturally through the habit of viewing society as a whole rather than as a collection of individuals. We view a human body as a whole, and if, for example, it is necessary to amputate a limb we do not consider it necessary to prove first that the limb is wicked. We consider the good of the whole body a quite sufficient argument. Similarly the man who thinks of society as a whole will sacrifice a member of society for the good of the whole, without much consideration for that individual's welfare. This has always been the practice in war, because war is a collective enterprise. Soldiers are exposed to the risk of death for the public good, although no one suggests that they deserve death. But men have not hitherto attached the same importance to social purposes other than war, and have therefore shrunk from inflicting sacrifices which were felt to be unjust. I think it probable that the scientific idealists of the future will be free from this scruple, not only in time of war, but in time of peace also. . . .

But, the reader will say, how is all this to come about? Is it not merely a phantasy of wish-fulfillment, utterly remote from practical politics? I do not think so. The future which I foresee is, to begin with, only very partially in agreement with my own wishes. I find pleasure in splendid individuals rather than in powerful organizations, and I fear that the place for splendid individuals will be much more restricted in the future than in the past. . . .

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. . . The impulse towards scientific construction is admirable when it does not thwart any of the major impulses that give value to human life, but when it is allowed to forbid all outlet to everything but itself it becomes a form of cruel tyranny. There is, I think, a real danger lest the world should become subject to a tyranny of this sort. . . .

Science in the course of the few centuries of its history has undergone an internal development which appears to be not yet completed. One may sum up this development as the passage from contemplation to manipulation. The love of knowledge to which the growth of science is

due is itself the product of a twofold impulse. We may seek knowledge of an object because we love the object or because we wish to have power over it. The former impulse leads to the kind of knowledge that is contemplative, the latter to the kind that is practical. In the development of science the power impulse has increasingly prevailed over the love impulse. The power impulse is embodied in industrialism and in governmental technique. It is embodied also in the philosophies known as pragmatism and instrumentalism. Each of these philosophies holds, broadly speaking, that our beliefs about any object are true in so far as they enable us to manipulate it with advantage to ourselves. This is what may be called a governmental view of truth. Of truth so conceived science offers us a great deal; indeed there seems no limit to its possible triumphs. To the man who wishes to change his environment science offers astonishingly powerful tools, and if knowledge consists in the power to produce intended changes, then science gives knowledge in abundance.

\* \* \*

. . . The new powers that science has given to man can only be wielded safely by those who, whether through the study of history or through their own experience of life, have acquired some reverence for human feelings and some tenderness towards the emotions that give colour to the daily existence of men and women. I do not mean to deny that scientific technique may in time build an artificial world in every way preferable to that in which men have hitherto lived, but I do say that if this is to be done it must be done tentatively and with a realization that the purpose of government is not merely to afford pleasure to those who govern, but to make life tolerable for those who are governed. Scientific technique must no longer be allowed to form the whole culture of the holders of power, and it must become an essential part of men's ethical outlook to realize that the will alone cannot make a good life. Knowing and feeling are equally essential ingredients both in the life of the individual and in that of the community. Knowledge, if it is wide and intimate, brings with it a realization of distant times and places, an awareness that the individual is not omnipotent or all-important, and a perspective in which values are seen more clearly than by those to whom a distant view is impossible. Even more important than knowledge is the life of the emotions. A world without delight and without affection is a world destitute of value. These things the scien-

tific manipulator must remember, and if he does his manipulation may be wholly beneficial. All that is needed is that men should not be so intoxicated by new power as to forget the truths that were familiar to every previous generation. Not all wisdom is new, nor is all folly out of date.

Man has been disciplined hitherto by his subjection to nature. Having emancipated himself from this subjection, he is showing something of the defects of slave-turned-master. A new moral outlook is called for in which submission to the powers of nature is replaced by respect for what is best in man. It is where this respect is lacking that scientific technique is dangerous. So long as it is present, science, having delivered man from bondage to nature, can proceed to deliver him from bondage to the slavish part of himself. The dangers exist, but they are not inevitable, and hope for the future is at least as rational as fear.

## 2.

### Rational Choice about Alternatives?

a.

#### Directive Committee on Regional Planning—Yale University The Case for Regional Planning\*

\* \* \*

To the most general question: Why plan? the answer is that man is in some measure a rational animal and plans because it is by planning that he is able to insure the fullest achievement of his specific goals and basic values. By trial and error and success, he has learned that it is by clear vision of goals, careful calculation of probabilities, and intelligent appraisal of alternative courses of action that both individuals and human communities, of whatever size or purpose, most effectively achieve their goals, whether individual or communal. Experience has taught him that he plans, whether he knows it or not, and that the more conscious he is of his planning and the more systematically he appraises its results, the better he is able to adapt it to his purposes. The reason why contemporary people give so much conscious thought and effort to planning is, therefore, to make certain that all

of their planning, whether for peace or general welfare or individual happiness, is as effective as contemporary science and knowledge will permit.

To the slightly less general question: Why plan through government? the answer is that government is the one general institution of society established and maintained for the very purpose of planning and acting for communal ends. It is the institution especially designed to marshal all the other institutions, to keep them from interfering with each other, and to promote their most efficient contribution to the values of the community. Government is the intelligence-receiving, communicating, directing, central nervous system of community life. By appropriate techniques of representation, government can, further, be made the most amenable of all society's institutions to democratic control, and hence can best be trusted with that power over people and resources which is necessarily involved in the shaping of a community's physical environment and in providing its public services, by whomsoever these tasks may be performed.

\* \* \*

Planning is the rational adaptation of means to ends. It is a process of thought, a method of work, the way in which man makes use of his intelligence. People always act with some anticipations of the future—with some picture, however cloudy, of the ends they are seeking; with some notion, however inaccurate, of the conditions that determine the extent to which they can achieve their ends; and with some appraisal, however inadequate, of what are appropriate means to attain their ends under such conditions. It is the function of planning to make such anticipations of the future—such prevision of goals, such calculation of probabilities, and such appraisal of alternative courses of action—as clear, as realistic, and as effective as possible.

This conscious application of intelligence to the task of creating appropriate means to attain defined ends, which we call planning, is a process that is characteristic of man—however irrational he may appear or be on occasion—in all of his activities. It extends from the shaping of individual careers to the conduct of all his communities or associations, public and private. It is a process which is applied, with varying degrees of success, from the small rural community, at one extreme, through the various levels of local, state, or national government and through the great private or public-private associations of

\* Myres S. McDougal and Maurice E. H. Rotival, Chairmen. New Haven: Yale University Press 7-18, 19-20 (1947). Reprinted by permission.

business to, at the other extreme, world society. When planning is thus understood, there can be no question of a choice between planning and not planning. Planning in this sense is not a political philosophy which can be accepted or rejected. It is a mode of exercising foresight in action and is indispensable to effective action in any walk of life. Differences between political philosophies are to be found in the ends to which planning is put (as well as in, of course, the means by which such ends are effected) and not in the fact of planning. Planning can be used to implement any political philosophy. Under contemporary world conditions, it is especially needed to implement democracy.

In government, specifically, it is the function of planning to apply scientific knowledge and common sense to the creation and execution of programs, designed to achieve the general purposes and specific goals for which any particular institution of government exists.

\* \* \*

*The first task of the planner in any community or governmental institution is, therefore, to clarify and define objectives.*

In a democratic society the basic values, with which planning for any community or institution must begin, are the more general values common to the individual citizens of such a society. In highest abstraction these values may be described in terms of a wider sharing of power, respect, knowledge, income, safety, health, and character and of all other values that contribute to the dignity of the individual and the possibilities of his maturing his latent talents, without discrimination, into socially valued expression.

\* \* \*

It is obvious, however, that . . . specific goals for community action cannot be completely clarified without realistic knowledge of the conditions which determine or limit the possibilities of the community's achievement of basic values.

*The second task of the planner, which must be carried out concomitantly with the first is, therefore, to study such determining conditions.*

These conditions include many interdependent variables, such as the numbers and characteristics of the people, the structure and functioning of external political and economic power, the efficiency of the community's institutions and technology, and the resources, natural and man-made, that are available for development. The exact interrelation of these conditioning factors

varies from community to community and can seldom be precisely known, but the planner assumes that, given a certain stability in population and in the effects of power exercised from outside the area, the extent to which the people of any particular area achieve their values depends upon the efficiency with which they apply their institutions and technology to the development of their resources. Even common observation suggests that certain variations in a people's organizational practices and in their design and use of natural resources greatly affect the degree of their achievement of their values and that man has a very large capacity to adapt natural resources and forces to his ends. It is with these modifiable factors in organizational practice and in the design and use of resources that the planner must work.

One of the variables which most directly affects the extent to which a community can achieve its values in the future is the extent to which it has achieved them in the past. The degree to which any of the basic values of a community are achieved conditions in part the achievement of all the other values. Thus, the extent to which knowledge, respect, and character are shared affects the extent to which real income can be produced and shared, and vice versa. It is important, therefore, for the planner first to inquire to what extent the people of the area under study have hitherto realized their various major values.

\* \* \*

In exploring adequately the extent to which any given community is achieving its basic values, the planner must of course study all of the major institutions and organizational practices of the community under observation. Since, however, the community's over-all achievement is so largely dependent upon the efficiency of its component institutions and the practices within them, these institutions and practices call for a more direct investigation, with attention focused squarely upon them and their functioning.

\* \* \*

*The third task of the planner, which must be kept constantly in mind while goals are being clarified and conditions determined, is to appraise or devise appropriate means for securing the goals that are finally established.*

In a democratic society, people plan to achieve most of their values by voluntary, private, or civic undertakings and to preserve the

widest possible zones of action for private decision. The function of government is to create the conditions—to supply the basic framework of efficient material environment and public services and regulation—under which voluntary, private activity and individual development can flourish. In a federal nation of interdependent regions such as the United States, however, the creation of such conditions calls for planning and action at all levels of government—national, regional, state, and local.

\* \* \*

*A fourth and final task is to assist in carrying recommendations into action.*

His contribution to the implementation of plans is as indispensable as his contribution to their conception. . . .

[He must also] observe closely all day-to-day activities in the execution of programs, conducting what amounts to a continuous survey of the extent to which the action being taken is effective in securing the specific goals and basic values sought. This process of evaluation is indeed a part of his general function of determining the conditions which affect the achievement of specific goals and basic values and here leads directly to subsequent recommendations for action. It is exactly for this reason that the planning process is a continuing one and the role of the planner a permanent one. The purposes of planning cannot be achieved in one blow by a single exercise of the four separate steps involved in the process of planning. The continuing achievement of the major values of any community requires a continuous exercise of all its powers of foresight and rational decision.

\* \* \*

It might, therefore, be helpful if a reasonably routinized order of work could be adopted.

\* \* \*

The first chapter in a study of the order proposed would investigate the people and their values and aspirations. This would comprehend all the ascertainable facts about population, including numbers, births, deaths, biological traits, health, personality types, spatial distribution and migrations; about groups, classes, and skills and about movements and conflicts between groups; about the basic disciplines imposed by the family and other small in-groups for the conditioning of character; about the intellectual, spiritual, es-

thetic, moral, and philosophical concepts, attitudes, and aspirations of the people; and finally, about the details of the standard of living, the real income, achieved in the community.

A second chapter would study institutions and resources. To make these manageable, a seamless web of areal activities and resources must be broken up into specific types of component functional activities. Any classification of components of a community organism adequate to satisfy all of man's basic needs must be somewhat arbitrary. Each component must also consist, if it is to represent reality, of a complex of people pursuing values through organizational practices applied to specific resource bases.

\* \* \*

A third chapter would single out design, as a most influential variable, for especial emphasis. . . . It would consider explicitly how norms and practices might be modified to secure a more efficient use of resources in promoting major purposes.

The fourth and final chapter would concentrate on government, potentially the most quickly responsive of all man's instruments of social change. This would survey institutions, rules, and practices at all levels of areal organization and seek to determine how these might be modified to implement any new moulding . . . to promote a fuller and richer achievement of major objectives.

\* \* \*

The planning process follows also an order in time, representing degrees in the advancement of the work. Certain commonly accepted divisions are in terms of analysis, diagnosis, synthesis, and action.

Analysis represents the systematic study of conditions, following the sequence outlined above. Diagnosis is the careful appraisal of such conditions to determine which are most critical in effect and easiest of modification. Synthesis is an integration of all the relevant factors to achieve a final choice of means, a concrete vision of the kind of community desired and how to effect it—a vision so concrete that it can be embodied in plans, drawings, and programs for action. Action is what is required, in effective time sequence, at each level of government or by voluntary agencies to achieve ends sought.

\* \* \*

b.

**Harold Lasswell and Myres S. McDougal**  
**Law, Science and Policy—**  
**The Jurisprudence of a Free Society\***

\* \* \*

. . . A relevant jurisprudence must, in sum, seek a comprehensiveness and realism in focus which will encourage both a systematic, configurative examination of all the significant variables affecting decision and the rational appraisal of the aggregate value consequences of alternatives in decision.

\* \* \*

. . . In any community, this process of authoritative and controlling decision, as an integral part of a more comprehensive process of effective power, can be seen to be composed of two different kinds of decisions: first, the decisions which establish and maintain the most comprehensive process of authoritative decision and, secondly, the flow of particular decisions which emerge from the process so established for the regulation of all the other community value processes. The first of these types of decision may be conveniently described as "constitutive," and the second as "public order."

For the comprehensive and economic description of a process of decision, as of other social processes, it is necessary to employ some systematic set of terms (the precise words do not matter if equivalences can be made clear) to refer to the participants in the process, their perspectives (demands, identifications, expectations), the situations of intersection, the base values at the disposal of participants, the strategies employed in management of base values, and the immediate outcomes and long-term effects achieved.

In the terms we find convenient, the "constitutive process" of a community may be described as the decisions which identify and characterize the different authoritative decision-makers, specify and clarify basic community policies, establish appropriate structures of authority, allocate bases of power for sanctioning purposes, authorize procedures for making the different kinds of decisions, and secure the continuous performance of all the different kinds of decision functions (intelligence, promotion,

prescription, etc.) necessary to making and administering general community policy.

In complementary terms, the "public order" decisions of a community may be described as those, emerging in continuous flow from the constitutive process, which shape and maintain the protected features of the community's various value processes. These are the decisions which determine how resources are allocated and developed, and wealth produced and distributed; how human rights are promoted and protected or deprived; how enlightenment is encouraged or retarded; how health is fostered, or neglected; how rectitude and civic responsibility are matured; and so on through the whole gamut of demanded values.

It will be obvious in any community that an intimate relationship exists between constitutive process and public order. The economy and effectiveness of the constitutive process a community can achieve vitally affects the freedom, security, and abundance of its public order; while the quality of the public order a community attains, in turn, affects the viability of the constitutive process it can maintain. By distinguishing, however, between these two different types of decisions, and seeking systematic coverage of both, inquiry may avoid destructive fixation upon the mere application of allegedly given rules and vacuous controversies about the differences between "political" and "legal" decisions, and may appropriately extend its concern to all relevant features of the processes by which law is made and applied and to their consequences for preferred public order.

The conventional description of the different phases in authoritative decision which we describe as "authority functions" is in such terms as "legislative," "executive," "judicial," and "administrative" but these terms would appear to refer more to authority structures than functions. Inquiry seeking both greater precision and comprehensiveness in describing authority functions might distinguish the following (or their equivalents):

- Intelligence: Obtaining information about the past, making estimates of the future, planning.
- Promoting: Urging proposals.
- Prescribing: Projecting authoritative policies.
- Invoking: Confronting concrete situations with provisional char-

\* Unpublished manuscript 23, 40-54 (1970).  
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- acterization in terms of a prescription to concrete circumstances.
- Applying: Final characterization and execution of a prescription in a concrete situation.
- Terminating: Ending a prescription or arrangement within the scope of a prescription.
- Appraising: Comparison between goals and performance.

\* \* \*

Every phase in the processes of authoritative decision is affected both by the past distribution of values and by the perspectives (demands, identifications, and expectations) of participants about future distribution. The outcomes of processes of authoritative decision, in turn, not only directly affect the future distribution of values among the claimants and others but, in total impact and in the long run, determine and secure a community's public order.

For comprehensive and precise description of the social process context of decision, any categorizations of values and institutional practices which can be given detailed operational indices in terms of specific, empirical relations between human beings can be made to serve the purposes of policy-oriented inquiry. The most general conceptualization we recommend is in terms of eight value-institution categories made familiar by contemporary social science:

- |                |   |
|----------------|---|
| Power:         | government, law, politics.  |
| Wealth:        | production, distribution, consumption.  |
| Respect:       | social class and caste.   |
| Well-being:    | health, safety, comfort arrangements.   |
| Affection:     | family, friendship circles, loyalty.  |
| Skill:         | artistic, vocational, professional training and activity.                           |
| Rectitude:     | churches and related articulators and appliers of standards of responsible conduct. |
| Enlightenment: | mass media, research.   |

When these or equivalent value-institutional categories are employed, in appropriately detailed phase analysis, to describe the events in social process which precipitate claims to authori-

tative decision, the claims which participants make about such precipitating events and relevant policies in their appeals to decision, and the choices which the established decision-makers actually make in their prescriptions and applications of policy, then effective comparisons can be made through time within single communities, and across the boundaries of communities, for study of the factors that affect decision and of the public order consequences of decision.

\* \* \*

Every phase of decision process, whether of constitutive process or relating to public order, and every facet of conditioning context, will be examined for opportunities in innovation which may influence decision toward greater conformity with clarified goal. Assessment of particular alternatives will be made in terms of gains and losses with respect to all clarified goals and disciplined by the knowledge acquired of trends, conditioning factors, and future probabilities. All the other intellectual tasks will be synthesized and brought to bear upon search for integrative solutions characterized by maximum gains and minimum losses. Special procedures for encouraging creativity will be employed, including expansions and contractions of the focus of attention, alternation of periods of intensive concentration and inattention, free association, and experiment with random combinations.

C.

#### Gordon S. Fulcher Common Sense Decision-Making\*

[W]e shall devote most of our attention to thoughtful decision-making, especially to procedures useful for making very important, difficult, and complex decisions. Such procedures can readily be modified for use when making less important, less difficult, and less complex decisions. . . .

The *main factors* which may be involved in thoughtful decision-making are the following:

1. *Problem situation* with which the decision will deal: a situation which is unsatisfactory in some respect and is a problem because the proper action to take, if any, is not obvious.
2. *Purpose* to be achieved if practicable; the end to be aimed at.
3. *Available alternative decisions*; alternative means for dealing with the situation so as to

\* Evanston: Northwestern University Press 7-12 (1965). Reprinted by permission.



achieve the desired purpose. Since each decision is a choice between alternative possible decisions, at least two such alternatives should be known, but one may be to take no action.

4. *Probable consequences* of each alternative. Since each alternative will have, if chosen, its natural consequences, a choice between the alternatives is, in effect, a choice between their consequences.

5. *Values* to the decision-maker of the probable consequences of the alternatives. A comparison of such values is necessary to determine which alternative is likely to have the most desirable consequences to him or, if the decision-maker is faced with a choice of evils, the least undesirable consequences.

... If one step is devoted to each of the five factors listed above and one step is added to check the evidence, assumptions, and reasoning on which a tentative decision, reached after taking the first five steps, is based, we arrive at the following list of steps which may be required in making a thoughtful decision:

*To determine the available alternatives:*

1. Investigation of the problem situation to determine the unsatisfactory features, the causes of these, and other pertinent facts.

2. Selection of the purpose to be achieved, the goal to have in mind, the end to be aimed at.

3. Determination of the courses of action or policies available to achieve the purpose selected, the means available to achieve the goal.

*To make the best choice between the alternatives:*

4. Prediction of the probable consequences of each of the alternatives or the probable differences in the consequences of the alternatives, so that the decision-maker may know what difference it is likely to make if he chooses one rather than the other.

5. Evaluation of these predictions by the decision-maker so as to determine which alternative, if chosen, is likely to have for him the most desirable results, or the least undesirable results if it is a choice of evils, and thus achieve the purpose most satisfactorily.

*To check the tentative decision:*

6. Checking of the evidence, the assumptions, and the reasoning on which the decision is based so as to make reasonably sure that the decision is the best decision available, a sound decision.

Each step may, of course, involve minor conclusions, judgments, and decisions. For example, step 1 may involve judgments as to which features of the problem situation are most unsatisfactory and conclusions as to the causes of the unsatisfactory features. Step 3 may involve decisions as to which alternatives should receive further consideration. Also, step 5 naturally involves judgments as to the relative desirability of various predicted consequences; and all the steps may involve decisions as to how to obtain needed facts, ideas, and predictions. Thus, making a sound decision to deal with an important personal or social problem may be very complex. For such problems the advantage to the decision-maker of training and practice in the art of decision-making should be obvious.

The numerical sequence of the steps indicates the order in which they are normally completed, since after each has been checked, there is usually no need for further consideration of any preceding steps.

The relative importance of the six steps varies greatly for different types of a problem situation as well as for different situations of the same general type. When the unsatisfactory features of the situation are not well enough known or the causes are not well enough understood, the first step may be the most important. When no satisfactory alternative is known, the third step may be the most important. When all available alternatives are clear or easily known from past experience, the first three steps are not needed, and either the fourth or the fifth step may be the most important.

*Best decisions.* From the point of view of the decision-maker, the best decision he can make to deal with a certain problem situation is, naturally, the decision he would make if he knew all the factors involved—including all available alternatives and their probable consequences—well enough to be sure which alternative would have the most desirable results according to his highest standards. . . .

*Sound decisions.* Even when a decision-maker cannot be sure which decision is the best to deal with the situation, he can arrive at a sound decision, as we shall use the term, by making a reasonable effort to determine the available alternatives, to predict the probable consequences, and to make a sound choice between them. In other words, a decision will be considered sound if after reasonable effort the decision-maker is reasonably sure that the decision is a logical decision based on sound premises, one for

each of the factors involved, as listed below. The premises are:

1. That the pertinent facts and causal factors of the problem situation are sufficiently well known;
2. That the purpose selected is the best under the circumstances;
3. That the alternatives considered include the best available;
4. That the predicted probable consequences are sufficiently complete, accurate, and far-sighted; and
5. That the evaluation of the expected consequences is sufficiently objective, conscientious, and idealistic.

### NOTES

#### NOTE 1.

DAVID BRAYBROOKE AND CHARLES E. LINDBLOM  
A STRATEGY OF DECISION\*

\* \* \*

[The] more sophisticated lines of thought, which many social scientists have followed—and philosophers before them—find their fullest expression in what we shall call “the rational-deductive ideal” . . . Many people, social scientists among them, are inclined to think that the most rational and satisfactory procedure for evaluating policies would be something like that described in the following instructions, assuming that they could be carried out:

Let ultimate values be expressed in general principles satisfactory to everybody who is ready to attend to the arguments identifying them—or, if there is no hope of that, satisfactory at least to those who are now undertaking a specific job of evaluation. Let these principles, which may embody notions of happiness, welfare, justice, or intuitive notions of goodness, be stated so exactly that they may be arranged intelligibly in an order of priority that indicates precisely which principles govern the application of others and when. Then derive within the limits of such a system intermediate principles that are suitable for application in particular cases, and that—allowing for rare cases of equality in net benefits—will indicate unambiguously which of alternative policies is to be chosen, according to the values they would promote.

The intermediate principles of such a system would specify the sort of information that would be decisive for rating any policy above or below its alternatives. If these principles are formulated as hypothetical propositions (which is the most convenient way to formulate them), they take something like the following form:

In conditions C, D, E, etc. (themselves derived from the ultimate principles), if such-and-such are the facts about Policy P and such-and-such are the facts about Policy Q, then P is better than Q.

For example, given that due process is observed and compensation is paid and (perhaps certain other conditions), if Policy P would remove certain dangers to the health and safety of the community caused by an existing use of private property, while Policy Q, although it would improve certain recreational facilities, would leave those dangers (and the existing use of private property) untouched, then P is better than Q. Once we substitute, in this intermediate principle, “compelling the dye-works in the town center to shut down,” for “Policy P” and explain that the dangers consist of fumes and excessive traffic, the way is open for us to move directly from these facts to recommending the policy of shutting down the dye-works rather than Policy Q (“spending the money on a new playground”).

Ideally, the system would be complete, not necessarily in the sense that it mentioned every contingency but in the sense that its ultimate principles were rich enough to supply the intermediate principles or the sequence of intermediate principles that one would need to decide any case that might come up. With such a system, the uncertainties of evaluation would have been mastered on the values side. For, on the values side, determination of policy becomes simply a matter of calculation, a question of feeding in the observed facts and thinking consistently through a sequence of logical transformations. One discovers the facts, looks up (or derives) the relevant hypotheticals, and deduces by strict logic which policy is to be selected. Nowadays, the work does not even seem impossibly tedious; one imagines that a suitably programmed computer could do it.

The rational-deductive ideal, so conceived, represents an ideal of science transferred to the field of values. For on the facts side too, the traditional ideal of science, going back to Plato and Aristotle, is the ideal of a complete deductive system—as a way of organizing knowledge (not, of course, for empirical scientists, as the sole way

\* New York: The Free Press of Glencoe 9-12 (1963). Reprinted by permission of The Macmillan Company. Copyright © 1963 by The Free Press of Glencoe, a Division of The Macmillan Company.

of obtaining it). If it is fantastic to imagine having such a system for all phenomena, it has not necessarily been thought fantastic to have it for a specific range of phenomena. The triumph that men attributed to Newton was a triumph of this kind—invested with fantasy by Laplace's boast of being able to calculate the positions and velocities of all the particles in the universe at any time if he knew their positions and velocities at any other.

In ethics, all manner of philosophers have subscribed to this ideal, beginning with the man who first conceived it, Plato. Both Aristotle and St. Thomas had reservations about the precision with which what can be known of values determines what is to be done in particular cases. In Aristotle's case, at least, if these reservations are explored, they lead away from the rational-deductive ideal. But it is fair to say that the historical effect of both men's writings has been to promote the view that, insofar as there is genuine knowledge in ethics, it can be elaborated in the form of a deductive system. John Locke also held this ideal; in ethics, furthermore, he was no more of an empiricist than Plato, for he believed that the content of ethics could be established with absolute certainty by *a priori* reasoning. Kant thought that we can discover *a priori* a universally effective method of testing decisively every moral judgment. If it is made an axiom that one ought to do whatever the test of the Categorical Imperative requires, then this universal method furnishes a system containing every judgment that passes the test.

The ethical discussions of most philosophers, from the Middle Ages through modern times, have concentrated on ethics in the narrow sense, which is concerned with the actions that it is our duty to do, the policies that it is our duty to choose. One has to guess from this what they would say of ethics in the larger sense, which involves choices among actions and policies all of which are morally permissible, although they differ in benefits. In the case of one major philosopher—the doggedly fact-minded Bentham—we do not have to guess. Bentham thought that the way to discover moral principles was to consider what could be consistently and effectively recommended to people, taking people as they are. His manifold lexicographical investigations aside, the system that he championed was meant to be fundamentally very simple. At the same time, it was meant to be a complete system, which applied to every choice of policy; it not only went beyond ethics in the

"duty" sense, but it subordinated "duty" to the criteria laid down for the larger sphere. The principle of utility, accompanied by the felicific calculus, is offered as a way of determining every conceivable policy question.

The example of Bentham—and, in another way, the example of Kant—shows that the ethical system offered to satisfy the rational-deductive ideal need not be a complicated one. If the right key can be found, the whole plan of the system will open up immediately—or so it may seem before getting down to cases. Whether the complications appear in the basic formulation of the system or only in its applications, however, people who subscribe to the rational-deductive ideal are ready to confront them bravely. In an intrepid statement of the ideal, the sociologist Florian Znaniecki writes,

You cannot isolate . . . arbitrarily one practical cultural problem and its solution from the rest of the human cultural world; you must take into consideration all the other practical cultural problems which are connected with it now and may become connected with it as an actual consequence of your activity—your own problems, those of the individuals and groups whose cooperation you must enlist, and those of the wider society whom you wish to influence through those individuals or groups. Otherwise, divergent, perhaps conflicting, standards of valuation and norms of conduct will continually interfere with the planful realization of your cultural "end." This "end" as a value and the activity pursuing it must be incorporated into an axiological and normative system organizing conceptually all the values and activities which are or will be connected with it in the active experience of all the people who are or will be involved in the realization of your plan.

Znaniecki will not allow anything relevant to be excluded from consideration. He will not spare himself or other evaluators even the difficulties involved in obtaining agreement on the system from all the people who will be affected by its determinations.

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#### NOTE 2.

ABRAHAM KAPLAN  
SOME LIMITATIONS ON RATIONALITY\*

Few would argue that the human animal has become more rational in the twentieth century, but there can be no doubt that more than

\* Carl J. Friedrich, ed.: *Nomos VII—Rational Decision*. New York: Atherton Press 55–59 (1964). Copyright 1964 by Atherton Press. Reprinted by permission of Aldine-Atherton, Inc.

ever before we know what we're missing. Within our lifetimes the advances in the exact formulation and analysis of what constitutes rational behavior have been greater, I venture to say, than in the whole of history. The theory of games, of information, and of decision-making and associated techniques like linear programming, operations research, and data-processing all have provided us, not just with an impressive new vocabulary, but with new and profound insights into the nature of rational choice, incomparably richer and more subtle than those underlying the *mos geometricus* which defined rationality from Plato to Spinoza and beyond. At the same time a new technology has sprung up, whose electronic components and magnetic tapes allow an exploitation of the theoretical possibilities incomparably beyond the capacity of a brain of flesh and blood; and if machines do not think, whatever it is they do puts our thinking to shame, given the same data and problems. More and more areas of rational decision have been brought into the domain of such theory and practice, and its frontier is expanding. A brave new world of rationality is in the making.

\* \* \*

Rationality, I should suppose, is more than a matter of acting so as to secure the values pursued. Would we not also want to say that the reason judges them to be worth pursuing? I need not rehearse here the decades of discussion of the mutuality of ends and means. What seems to me unexceptionable is that rationality is not limited to a choice among means. The paranoid who waits till dark before turning on his persecutors may be a master strategist; he is surely not a paragon of rationality. A theory which demands only consistency of preference scales (a stable transitivity of utilities) is grossly inadequate to the political process. Political theorists have recognized at least since Burke that the decision-maker has a responsibility beyond giving the people what they want; he owes them also his own best judgment of what they *should* want. It could be said, to be sure, that rationality is not the only desideratum for political decisions, and that what I see as a problem for rationality lies, in fact, beyond its limits. But that Satan has a fine mind and is lacking only in heart is more than I am willing to admit; I believe he is a fool from beginning to end.

In politics, above all, decision-making cannot escape the responsibility of judging the rela-

tive worth of disparate and perhaps conflicting values. Even decisions affecting only ourselves may involve this problem, for we are individuals only by courtesy; in truth the individual is a congress of selves, each pursuing values to which the other selves may be indifferent or hostile—if, indeed, they are even aware of the pursuit. . . .

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Given determinate values as well as the probabilities of their attainment by alternative courses of action, there is still the question of the criterion by which a rational choice is to be made. Risks can vary though expectations remain constant; what is a rational valuation of the element of risk itself? Gambling has a positive utility for players of Russian roulette, a negative utility for the young man in de Maupassant's story who is so afraid he might die in a scheduled duel that he kills himself the night before. The political game of "Chicken!" is grossly irrational, Bertrand Russell has tirelessly argued; under the designation of "brinkmanship" it has been defended by others as rational indeed. Agreement on expected outcomes might still lead to different decisions. The mathematical expectation is the same whether we match pennies or hundred-dollar bills, it being zero in both cases; for my part, the smaller stake is the more rational. This example is complicated by uncertainties as to the utility of money, but the excitement of the gamble itself also plays a part.

A criterion must also make some assessment of the value of the future, that is, of how utilities are affected by the mere fact that they lie in the future. A miser saves all he can, spends only what he must; a child does just the reverse. We might all agree with Aristotle that the rational course lies somewhere between these extremes; but where? I am not raising the problem of hedging against inflation, for example; expected changes of this kind can be discounted. The question is how to allocate resources between consumption and capital goods—as it were, how to choose between consuming something now or more later, how to decide how much the generation of the revolution should sacrifice to ensure the great leap forward. For a democracy the formulation is that the welfare of the unborn must be taken into account even though they cast no vote. What is the basis of a rational assignment of such utilities or, for that matter, a rational assignment of the utilities for our own future selves? Is it rational to decide now in

terms of what I may want then? Love's way never changes of promising never to change, but those unblinded by love know better.

NOTE 3.

FELIX E. OPPENHEIM  
RATIONAL DECISIONS AND  
INTRINSIC VALUATIONS\*

I am an advocate of birth control. Moreover, I maintain that, in view of certain further goals, it is *rational* for people in overpopulated countries to practice contraception and for government to encourage the practice. I have then the burden of proving that birth control is *desirable* under those conditions. I may justify contraception by pointing out that such a policy is required to reduce the population to the point where the necessities of life become available to all and that this condition is in turn a necessary means to "the greatest happiness of the greatest number." Now, means-end statements can be translated without loss of meaning into cause-effect statements which can, in principle, be empirically tested. Accordingly, if my prediction of the causal chain (contraception = decrease of population = increase of the average living standard = general well-being and happiness) is empirically warranted and if I am committed to the principle of utility, then my advocacy of contraception is rational, and it will be rational for any government which aims at maximizing the general welfare and happiness to adopt a policy of contraception in case of actual or threatened overpopulation.

Now, suppose someone disagrees with me. Suppose, however, he does not challenge my factual allegations. He concedes that failure to check the threatened population explosion will with practical certainty lead to the greatest unhappiness of the greatest number. He realizes the ineffectiveness of the rhythm method and the futility of preaching continence. He nevertheless opposes contraception, and his sole argument is that such practices are contrary to God's will. Our disagreement then boils down to the question: Should everyone (including every government) aim at maximizing the general welfare, or at complying with allegedly divine commands, even if in conflict with the principle of utility?

Professor Kaplan points out the unsatisfactory state of the theory of rational choice even for the purpose "of acting so as to secure the values pursued." But, on the other hand, he is convinced "that rationality is not limited to the choice among means" but applies also to the determination of "the relative worth of disparate and perhaps conflicting values" in the sense of ultimate goals. He would presumably claim that it is either rational or irrational to adopt the principle of utility as an aim in itself. He thereby espouses the meta-ethical theory of value cognitivism, according to which not only extrinsic, but also intrinsic, value judgments have cognitive status. Accordingly, the intrinsic value judgment that the greatest happiness principle is worth implementing for its own sake would be either demonstrably true or demonstrably false. If true, it would follow that it is rational to pursue this goal and irrational to be guided by any conflicting value, for example, my opponent's.

I cannot think of any scientific argument by which either my opponent or I could justify our respective intrinsic valuations. He might contend that an action is demonstrably rational if and only if it does not deviate from the moral law ordained by God and that the practice of contraception violates this principle and is therefore irrational as well as immoral. This argument may be *persuasive*, but only to those who happen to believe in an anthropomorphic God issuing commands—commands incompatible with birth control. However, an argument which is in principle acceptable only to some is not *valid* in the intersubjective, scientific sense. But I deny just as categorically the possibility of validating the greatest happiness principle or any other intrinsic value judgment. I agree with the meta-ethical position of value noncognitivism. Intrinsic valuations are a matter of subjective commitment, not of objective truth. Neither my adoption of the utilitarian standard nor my opponent's adherence to a particular religious faith can be called *either* rational *or* irrational. Both are commitments and, as such, nonrational.

I nevertheless agree with Professor Kaplan that rationality is not limited to the choice of means, and value noncognitivism does not entail that it is. To arrive at a rational decision, one does not start with the arbitrary selection of some ultimate end and then proceed to the choice of whatever means are most conducive to its realization. Means have consequences other than the goal, and the negative utility of the

\* Carl J. Friedrich, ed.: *Nomos VII—Rational Decision*. New York: Atherton Press 217-219 (1964). Copyright 1964 by Atherton Press. Reprinted by permission of Aldine-Atherton, Inc.

former may outweigh the positive utility of the latter. A rational actor must therefore predict (with as high a degree of probability as possible) the *total* outcome of each alternative action open to him in the given situation. Then he must establish a preference rank order among these total outcomes. The preferred outcome may include elements which the actor would disvalue if he considered them in isolation, and it need not include his original, but tentative, ultimate goal. Intrinsic valuations in connection with rational choice do not pertain to separate goals but to total outcomes. I have thus oversimplified the previous example. I must ask the following, more complicated question. What do I prefer on the whole: government-sponsored birth control *and* a higher living standard *and* increased promiscuity *and* a disregarding of certain religious beliefs and so on; or the outlawing of information about contraception *and* increased misery *and* the upholding of a particular faith and the like? This is the type of question which the theory of rational choice cannot answer; it does cover all other steps of the decision-making process.

\* \* \*

d.

**Karl R. Popper**  
**The Open Society and Its Enemies\***

[Utopian engineering] may be described as follows. Any rational action must have a certain aim. It is rational in the same degree as it pursues its aim consciously and consistently, and as it determines its means according to this end. To choose the end is therefore the first thing we have to do if we wish to act rationally; and we must be careful to determine our real or ultimate ends, from which we must distinguish clearly those intermediate or partial ends which actually are only means, or steps on the way, to the ultimate end. If we neglect this distinction, then we must also neglect to ask whether these partial ends are likely to promote the ultimate end, and accordingly, we must fail to act rationally. These principles, if applied to the realm of political activity, demand that we must determine our ultimate political aim, or the Ideal State, before taking any practical action. Only when this ultimate aim is determined, in rough outlines at least, only when we are in the posses-

sion of something like a blueprint of the society at which we aim, only then can we begin to consider the best ways and means for its realization, and to draw up a plan for practical action. These are the necessary preliminaries of any practical political move that can be called rational, and especially of social engineering.

\* \* \*

Before proceeding to criticize Utopian engineering in detail, I wish to outline another approach to social engineering, namely that of piecemeal engineering. It is an approach which I think to be methodologically sound. The politician who adopts this method may or may not have a blueprint of society before his mind, he may or may not hope that mankind will one day realize an ideal state, and achieve happiness and perfection on earth. But he will be aware that perfection, if at all attainable, is far distant, and that every generation of men, and therefore also the living, have a claim; perhaps not so much a claim to be made happy, for there are no institutional means of making a man happy, but a claim not to be made unhappy, where it can be avoided. They have a claim to be given all possible help, if they suffer. The piecemeal engineer will, accordingly, adopt the method of searching for, and fighting against, the greatest and most urgent evils of society, rather than searching for, and fighting for, its greatest ultimate good. This difference is far from being merely verbal. In fact, it is most important. It is the difference between a reasonable method of improving the lot of man, and a method which, if really tried, may easily lead to an intolerable increase in human suffering. It is the difference between a method which can be applied at any moment, and a method whose advocacy may easily become a means of continually postponing action until a later date, when conditions are more favourable. And it is also the difference between the only method of improving matters which has so far been really successful, at any time, and in any place . . . and a method which, wherever it has been tried, has led only to the use of violence in place of reason, and if not to its own abandonment, at any rate to that of its original blueprint.

In favour of his method, the piecemeal engineer can claim that a systematic fight against suffering and injustice and war is more likely to be supported by the approval and agreement of a great number of people than the fight for the establishment of some ideal. The existence of social evils, that is to say of social conditions under which many men were suffering, can be

\* Princeton: Princeton University Press, Vol. 1, 157-163 (5th rev. ed., 1966). Copyright © 1962, 1966 by Karl Raimund Popper. Reprinted by permission of the author, Princeton University Press, and Routledge & Kegan Paul, Ltd., London.

comparatively well established. Those who suffer can judge for themselves, and the others can hardly deny that they would not like to change places. It is infinitely more difficult to reason about an ideal society. Social life is so complicated that few men, or none at all, could judge a blueprint for social engineering on the grand scale; whether it be practicable; whether it would result in a real improvement; what kind of suffering it may involve; and what may be the means for its realization. As opposed to this, blueprints for piecemeal engineering are comparatively simple. They are blueprints for single institutions, for health and unemployed insurance, for instance, or arbitration courts, or anti-depression budgeting or educational reform. If they go wrong, the damage is not very great, and a re-adjustment not very difficult. They are less risky, and for this very reason less controversial. But if it is easier to reach a reasonable agreement about existing evils and the means of combating them than it is about an ideal good and the means of its realization, then there is also more hope that by using the piecemeal method we may get over the very greatest practical difficulty of all reasonable political reform, namely, the use of reason, instead of passion and violence, in executing the programme. There will be a possibility of reaching a reasonable compromise and therefore of achieving the improvement by democratic methods. ("Compromise" is an ugly word, but it is important for us to learn its proper use. *Institutions* are inevitably the result of a compromise with circumstances, interests, etc., though as *persons* we should resist influences of this kind.)

[T]he Utopian attempt to realize an ideal state, using a blueprint of society as a whole, is one which demands a strong centralized rule of a few, which therefore is likely to lead to a dictatorship. . . . One of the difficulties faced by a benevolent dictator is to find whether the effects of his measures agree with his good intentions. . . . The difficulty arises out of the fact that authoritarianism must discourage criticism; accordingly, the benevolent dictator will not easily hear of complaints concerning the measures he has taken. But without some such check, he can hardly find whether his measures achieve the desired benevolent aim. The situation must become even worse for the Utopian engineer. The reconstruction of society is a big undertaking which must cause considerable inconvenience to many, and for a considerable span of time. Accordingly, the Utopian engineer will have to be deaf to many complaints; in fact, it will be part of his business to suppress unreasonable objections. . . . But with

it, he must invariably suppress reasonable criticism also. . . . The very sweep of . . . a Utopian undertaking makes it improbable that it will realize its ends during the lifetime of one social engineer, or group of engineers. And if the successors do not pursue the same ideal, then all the sufferings of the people for the sake of the ideal may have been in vain.

A generalization of this argument leads to a further criticism of the Utopian approach. This approach, it is clear, can be of practical value only if we assume that the original blueprint, perhaps with certain adjustments, remains the basis of the work until it is completed. But that will take some time. It will be a time of revolutions, both political and spiritual, and of new experiments and experience in the political field. It is therefore to be expected that ideas and ideals will change. What had appeared the ideal state to the people who made the original blueprint, may not appear so to their successors. If that is granted, then the whole approach breaks down. The method of first establishing an ultimate political aim and then beginning to move towards it is futile if we admit that the aim may be considerably changed during the process of its realization. It may at any moment turn out that the steps so far taken actually lead away from the realization of the new aim. And if we change our direction according to the new aim, then we expose ourselves to the same risk again. In spite of all the sacrifices made, we may never get anywhere at all. . . .

[T]he Utopian approach can be saved only by the Platonic belief in one absolute and unchanging ideal, together with two further assumptions, namely (a) that there are rational methods to determine once and for all what this ideal is, and (b) what the best means of its realization are. Only such far-reaching assumptions could prevent us from declaring the Utopian methodology to be utterly futile. But even Plato himself and the most ardent Platonists would admit that (a) is certainly not true; that there is no rational method for determining the ultimate aim, but, if anything, only some kind of intuition. Any difference of opinion between Utopian engineers must therefore lead, in the absence of rational methods, to the use of power instead of reason, i.e., to violence. If any progress in any definite direction is made at all, then it is made in spite of the method adopted, not because of it. The success may be due, for instance, to the excellence of the leaders; but we must never forget that excellent leaders cannot be produced by rational methods, but only by luck.



It is important to understand this criticism properly; I do not criticize the ideal by claiming that an ideal can never be realized, that it must always remain a Utopia. This would not be a valid criticism, for many things have been realized which have once been dogmatically declared to be unrealizable, for instance, the establishment of institutions for securing civil peace, i.e. for the prevention of crime *within* the state; and I think that, for instance, the establishment of corresponding institutions for the prevention of international crime, i.e. armed aggression or blackmail, though often branded as Utopian, is not even a very difficult problem. What I criticize under the name Utopian engineering recommends the reconstruction of society as a whole, i.e. very sweeping changes whose practical consequences are hard to calculate, owing to our limited experiences. It claims to plan rationally for the whole of society, although we do not possess anything like the factual knowledge which would be necessary to make good such an ambitious claim. We cannot possess such knowledge since we have insufficient practical experience in this kind of planning, and knowledge of facts must be based upon experience. At present, the sociological knowledge necessary for large-scale engineering is simply non-existent.

In view of this criticism, the Utopian engineer is likely to grant the need for practical experience, and for a social technology based upon practical experiences. But he will argue that we shall never know more about these matters if we recoil from making social experiments which alone can furnish us with the practical experience needed. And he might add that Utopian engineering is nothing but the application of the experimental method to society. Experiments cannot be carried out without involving sweeping changes. They must be on a large scale, owing

to the peculiar character of modern society with its great masses of people. An experiment in socialism, for instance, if confined to a factory, or to a village, or even to a district, would never give us the kind of realistic information which we need so urgently.

[T]he Utopian engineer . . . is convinced that we must recast the whole structure of society, when we experiment with it; and he can therefore conceive a more *modest* experiment only as one that recasts the whole structure of a *small* society. But the kind of experiment from which we can learn most is the alteration of one social institution at a time. For only in this way can we learn how to fit institutions into the framework of other institutions, and how to adjust them so that they work according to our intentions. And only in this way can we make mistakes, and learn from our mistakes, without risking repercussions of a gravity that must endanger the will to future reforms. Furthermore, the Utopian method must lead to a dangerous dogmatic attachment to a blueprint for which countless sacrifices have been made. Powerful interests must become linked up with the success of the experiment. All this does not contribute to the rationality, or to the scientific value, of the experiment. But the piecemeal method permits repeated experiments and continuous readjustments. In fact, it might lead to the happy situation where politicians begin to look out for their own mistakes instead of trying to explain them away and to prove that they have always been right. This—and not Utopian planning or historical prophecy—would mean the introduction of scientific method into politics since the whole secret of scientific method is a readiness to learn from mistakes.

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## C.

### Collective Planning—The Step-by-Step Approach

#### Choice by Mutual Adjustment?

a.

#### Charles E. Lindblom The Science of "Muddling Through"\*

Suppose an administrator is given responsibility for formulating policy with respect to in-

flation. He might start by trying to list all related values in order of importance, e.g., full employment, reasonable business profit, protection of small savings, prevention of a stock market crash. Then all possible policy outcomes could be rated as more or less efficient in attaining a maximum of these values. This would of course require a prodigious inquiry into values held by members of society and an equally prodigious set of calculations on how much of each value is

\* 29 *Public Administration Review* 79-88 (Spring, 1959). Reprinted by permission.



equal to how much of each other value. He could then proceed to outline all possible policy alternatives. In a third step, he would undertake systematic comparison of his multitude of alternatives to determine which attains the greatest amount of values.

In comparing policies, he would take advantage of any theory available that generalized about classes of policies. In considering inflation, for example, he would compare all policies in the light of the theory of prices. Since no alternatives are beyond his investigation, he would consider strict central control and the abolition of all prices and markets on the one hand and elimination of all public controls with reliance completely on the free market on the other, both in the light of whatever theoretical generalizations he could find on such hypothetical economies.

Finally, he would try to make the choice that would in fact maximize his values.

An alternative line of attack would be to set as his principal objective, either explicitly or without conscious thought, the relatively simple goal of keeping prices level. This objective might be compromised or complicated by only a few other goals, such as full employment. He would in fact disregard most other social values as beyond his present interest, and he would for the moment not even attempt to rank the few values that he regarded as immediately relevant. Were he pressed, he would quickly admit that he was ignoring many related values and many possible important consequences of his policies.

As a second step, he would outline those relatively few policy alternatives that occurred to him. He would then compare them. In comparing his limited number of alternatives, most of them familiar from past controversies, he would not ordinarily find a body of theory precise enough to carry him through a comparison of their respective consequences. Instead he would rely heavily on the record of past experience with small policy steps to predict the consequences of similar steps extended into the future.

Moreover, he would find that the policy alternatives combined objectives or values in different ways. For example, one policy might offer price level stability at the cost of some risk of unemployment; another might offer less price stability but also less risk of unemployment. Hence, the next step in his approach—the final selection would combine into one the choice among values and the choice among instruments for reaching values. It would not, as in the first method of policy-making, approximate a more mechanical process of choosing the means that best satisfied

goals that were previously clarified and ranked. Because practitioners of the second approach expect to achieve their goals only partially, they would expect to repeat endlessly the sequence just described, as conditions and aspirations changed and as accuracy of prediction improved.

For complex problems, the first of these two approaches is of course impossible. Although such an approach can be described, it cannot be practiced except for relatively simple problems and even then only in a somewhat modified form. It assumes intellectual capacities and sources of information that men simply do not possess, and it is even more absurd as an approach to policy when the time and money that can be allocated to a policy problem is limited as is always the case. . . . It is the second method that is practiced.

. . . This might be described as the method of *successive limited comparisons*. I will contrast it with the first approach, which might be called the rational-comprehensive method. More impressionistically and briefly—and therefore generally used in this article—they could be characterized as the branch method and root method, the former continually building out from the current situation, step-by-step and by small degrees; the latter starting from fundamentals anew each time, building on the past only as experience is embodied in a theory, and always prepared to start completely from the ground up.

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#### *Intertwining Evaluation and Empirical Analysis*

The quickest way to understand how values are handled in the method of successive limited comparisons is to see how the root method often breaks down in its handling of values or objectives. The idea that values should be clarified, and in advance of the examination of alternative policies, is appealing. But what happens when we attempt it for complex social problems? The first difficulty is that on many critical values or objectives, citizens disagree, congressmen disagree, and public administrators disagree. Even where a fairly specific objective is prescribed for the administrator, there remains considerable room for disagreement on sub-objectives. . . .

Administrators cannot escape these conflicts by ascertaining the majority's preference, for preferences have not been registered on most issues; indeed, there often are no preferences in the absence of public discussion sufficient to bring an issue to the attention of the electorate. Fur-

thermore, there is a question of whether intensity of feeling should be considered as well as the number of persons preferring each alternative. By the impossibility of doing otherwise, administrators often are reduced to deciding policy without clarifying objectives first.

Even when an administrator resolves to follow his own values as a criterion for decisions, he often will not know how to rank them when they conflict with one another, as they usually do.

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A more subtle third point underlies both the first two. Social objectives do not always have the same relative values. One objective may be highly prized in one circumstance, another in another circumstance. If, for example, an administrator values highly both the dispatch with which his agency can carry through its projects and good public relations, it matters little which of the two possibly conflicting values he favors in some abstract or general sense. Policy questions arise in forms which put to administrators such a question as: Given the degree to which we are or are not already achieving the values of dispatch and the values of good public relations, is it worth sacrificing a little speed for a happier clientele, or is it better to risk offending the clientele so that we can get on with our work? The answer to such a question varies with circumstances.

The value problem is, as the example shows, always a problem of adjustments at a margin. But there is no practicable way to state marginal objectives or values except in terms of particular policies. That one value is preferred to another in one decision situation does not mean that it will be preferred in another decision situation in which it can be had only at great sacrifice of another value. Attempts to rank or order values in general and abstract terms so that they do not shift from decision to decision end up by ignoring the relevant marginal preferences. The significance of this third point thus goes very far. Even if all administrators had at hand an agreed set of values, objectives, and constraints, and an agreed ranking of these values, objectives, and constraints, their marginal values in actual choice situations would be impossible to formulate.

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In summary, two aspects of the process by which values are actually handled can be distinguished. The first is clear: evaluation and empirical analysis are intertwined; that is, one chooses among values and among policies at one

and the same time. Put a little more elaborately, one simultaneously chooses a policy to attain certain objectives and chooses the objectives themselves. The second aspect is related but distinct: the administrator focuses his attention on marginal or incremental values. . . .

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As to whether the attempt to clarify objectives in advance of policy selection is more or less rational than the close intertwining of marginal evaluation and empirical analysis, the principal difference established is that for complex problems the first is impossible and irrelevant, and the second is both possible and relevant. The second is possible because the administrator need not try to analyze any values except the values by which alternative policies differ and need not be concerned with them except as they differ marginally. His need for information on values or objectives is drastically reduced as compared with the root method: and his capacity for grasping, comprehending, and relating values to one another is not strained beyond the breaking point.

#### *Relations between Means and Ends*

Decision-making is ordinarily formalized as a means-ends relationship: means are conceived to be evaluated and chosen in the light of ends finally selected independently of and prior to the choice of means. This is the means-ends relationship of the root method. But it follows from all that has just been said that such a means-end relationship is possible only to the extent that values are agreed upon, are reconcilable, and are stable at the margin. Typically, therefore, such a means-ends relationship is absent from the branch method, where means and ends are simultaneously chosen.

Yet any departure from the means-ends relationship of the root method will strike some readers as inconceivable. For it will appear to them that only in such a relationship is it possible to determine whether one policy choice is better or worse than another. How can an administrator know whether he has made a wise or foolish decision if he is without prior values or objectives by which to judge his decisions? The answer to this question calls up the third distinctive difference between root and branch methods: how to decide the best policy.

#### *The Test of "Good" Policy*

In the root method, a decision is "correct," "good," or "rational" if it can be shown to attain

some specified objective, where the objective can be specified without simply describing the decision itself. Where objectives are defined only through the marginal or incremental approach to values described above, it is still sometimes possible to test whether a policy does in fact attain the desired objectives; but a precise statement of the objectives takes the form of a description of the policy chosen or some alternative to it. To show that a policy is mistaken one cannot offer an abstract argument that important objectives are not achieved; one must instead argue that another policy is more to be preferred.

... But what of the situation in which administrators cannot agree on values or objectives, either abstractly or in marginal terms? What then is the test of "good" policy? For the root method, there is no test. Agreement on objectives failing, there is no standard of "correctness." For the method of successive limited comparisons, the test is agreement on policy itself, which remains possible even when agreement on values is not.

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If agreement directly on policy as a test for "best" policy seems a poor substitute for testing the policy against its objectives, it ought to be remembered that objectives themselves have no ultimate validity other than they are agreed upon. Hence agreement is the test of "best" policy in both methods. But where the root method requires agreement on what elements in the decision constitute objectives and on which of these objectives should be sought, the branch method falls back on agreement wherever it can be found.

In an important sense, therefore, it is not irrational for an administrator to defend a policy as good without being able to specify what it is good for.

#### *Non-Comprehensive Analysis*

Ideally, rational-comprehensive analysis leaves out nothing important. But it is impossible to take everything important into consideration unless "important" is so narrowly defined that analysis is in fact quite limited. Limits on human intellectual capacities and on available information set definite limits to man's capacity to be comprehensive. . . .

In the method of successive limited comparisons, simplification is systematically achieved in two principal ways. First, it is achieved through limitation of policy comparisons to those policies that differ in relatively small degree from policies presently in effect. Such a limitation im-

mediately reduces the number of alternatives to be investigated and also drastically simplifies the character of the investigation of each. For it is not necessary to undertake fundamental inquiry into an alternative and its consequences; it is necessary only to study those respects in which the proposed alternative and its consequences differ from the status quo. . . .

\* \* \*

Since the policies ignored by the administrator are politically impossible and so irrelevant, the simplification of analysis achieved by concentrating on policies that differ only incrementally is not a capricious kind of simplification. In addition, it can be argued that, given the limits on knowledge within which policy-makers are confined, simplifying by limiting the focus to small variations from present policy makes the most of available knowledge. Because policies being considered are like present and past policies, the administrator can obtain information and claim some insight. Non-incremental policy proposals are therefore typically not only politically irrelevant but also unpredictable in their consequences.

\* \* \*

The second method of simplification of analysis is the practice of ignoring important possible consequences of possible policies, as well as the values attached to the neglected consequences. If this appears to disclose a shocking shortcoming of successive limited comparisons, it can be replied that, even if the exclusions are random, policies may nevertheless be more intelligently formulated than through futile attempts to achieve a comprehensiveness beyond human capacity. Actually, however, the exclusions, seeming arbitrary or random from one point of view, need be neither.

\* \* \*

Suppose that each value neglected by one policy-making agency were a major concern of at least one other agency. In that case, a helpful division of labor would be achieved, and no agency need find its task beyond its capacities. The shortcomings of such a system would be that one agency might destroy a value either before another agency could be activated to safeguard it or in spite of another agency's efforts. But the possibility that important values may be lost is present in any form of organization, even where agencies attempt to comprehend in planning more than is humanly possible.

The virtue of such a hypothetical division of labor is that every important interest or value has its watchdog. And these watchdogs can protect the interests in their jurisdiction in two quite different ways: first, by redressing damages done by other agencies; and, second, by anticipating and heading off injury before it occurs.

\* \* \*

Mutual adjustment is more pervasive than the explicit forms it takes in negotiation between groups; it persists through the mutual impacts of groups upon each other even where they are not in communication. For all the imperfections and latent dangers in this ubiquitous process of mutual adjustment, it will often accomplish an adaptation of policies to a wider range of interests than could be done by one group centrally.

Note, too, how the incremental pattern of policy-making fits with the multiple pressure pattern. For when decisions are only incremental—closely related to known policies, it is easier for one group to anticipate the kind of moves another might make and easier too for it to make correction for injury already accomplished. . . .

#### *Succession of Comparisons*

The final distinctive element in the branch method is that the comparisons, together with the policy choice, proceed in a chronological series. Policy is not made once and for all; it is made and re-made endlessly. Policy-making is a process of successive approximation to some desired objectives in which what is desired itself continues to change under reconsideration.

\* \* \*

In the first place, past sequences of policy steps have given him knowledge about the probable consequences of further similar steps. Second, he need not attempt big jumps toward his goals that would require predictions beyond his or anyone else's knowledge, because he never expects his policy to be a final resolution of a problem. His decision is only one step, one that if successful can quickly be followed by another. Third, he is in effect able to test his previous predictions as he moves on to each further step. Lastly, he often can remedy a past error fairly quickly—more quickly than if policy proceeded through more distinct steps widely spaced in time.

\* \* \*

Successive limited comparisons is, then, indeed a method or system; it is not a failure of

method for which administrators ought to apologize. None the less, its imperfections, which have not been explored in this paper, are many. For example, the method is without a built-in safeguard for all relevant values, and it also may lead the decision-maker to overlook excellent policies for no other reason than that they are not suggested by the chain of successive policy steps leading up to the present. Hence, it ought to be said that under this method, as well as under some of the most sophisticated variants of the root method—operations research, for example—policies will continue to be as foolish as they are wise.

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b.

#### **Charles E. Lindblom The Policy-Making Process\***

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The limits on analyses . . . are of a particular kind. They indicate how far man might go, *if he tried*, toward settling policy disputes by investigating their merits—that is, by studying and reasoning about policy instead of fighting over it. Often, however, he does not even try. Why?

*Irrationality.* Men turn an indifferent or hostile eye on policy analysis because they are not wholly rational. Because, specifically, it is easier to feel than to think. Because they cling to beliefs that serve the needs of their personalities. Because words or symbols with which they talk about politics come to be more dear to them than the things to which the symbols refer. Because sometimes it pains them to change their minds. Because they have picked up all kinds of beliefs from their families, friends, churches, and other groups—beliefs that give them a comforting orientation to the world about them and which they consequently dare not challenge. Because it may not have occurred to them that policy analysis is of potential great value.

\* \* \*

*Assaults on the mind.* Moreover, man is forever assaulted by a barrage of communications from other men who want to manipulate him. If he wants to pursue analysis, or encourage those who do, he must fight off the seductive irrational and nonrational appeals of political parties, candidates for office, interest groups, and propagandists of other kinds. They everywhere tug at his

\* Englewood Cliffs, N.J.: Prentice-Hall, Inc. 18-20, 24-29, 116-117 (1968). © 1968. Reprinted by permission.

attention and try to commit his mind before he has had time to think. They are always at his ear.

*Men who "know" what they want.* And those at his ear may not want to analyze policy either, for they may have decided that they already know what they want. Senator Joseph McCarthy wanted no analysis of the threat of internal communism in the U.S.; he wanted to proceed directly against a whole class of people he indiscriminately associated with communism, socialism, liberalism, and internationalism. Similarly, most taxpayers' councils scattered around the United States want only limited analysis of government fiscal policy; on their basic antagonism to government expenditures they have already made up their minds: on that issue, they feel, the less discussion and the less study, the better.

*Reasoned grounds for rejection.* Even those people most interested in analysis will know that analysis will always be influenced by the biases of the analysts and by their incompetences, and that hence it is not always to be trusted. And they will know that, since most analysis takes place in organizations, it will always be marred by organizational biases, rigidities, and other incompetence. Take for example the unhappy failure of organizations to define relevant problems. An organization like the Air Force is primarily established to exploit the usefulness of aircraft for national defense. Its policy problems therefore revolve around the question: How best to use aircraft for defense? As an organization, however, it is most unlikely ever to ask the question (which as time passes and new techniques of warfare are developed comes to be a critical question): Should aircraft give way to missiles? And the organization may even try to suppress the question elsewhere in the Department of Defense.

Organizational obstacles to satisfactory analysis constitute a subject in themselves. Differences of rank in organizations obstruct communication; the generalist's rivalry with the specialist sows distrust and becomes a source of bias; the organization's hiring policies may not attract competent personnel; promotion may be based on fitting in with the organization rather than on analytical skill; and so on. No few examples, however, can represent the luxuriant variety of organizational barriers to analysis.

\* \* \*

It is not . . . man's use of language, of quantification, of other universal tools, or of ideology that changes the character of policy analysis in

surprising ways. We begin to see a somewhat unexpected new face on policy analysis only when we look at certain strategies or dodges that man has developed for dealing with very complex problems—strategies that are especially well adapted to public policy analysis. Some of the most important are as follows:

#### *Satisficing*

In the conventional ideal of a rational decision, a decision maker maximizes something—utility or want satisfaction, income, national security, the general welfare, or some other such value. But, as we have already noted, an exhaustive search for the maximum, for the best of all possible policies, is not usually worth what it costs, and may in fact be impossible of accomplishment. An alternative strategy, therefore, is not to try too hard, to decide instead on some acceptable level of goal accomplishment short of maximization, and then pursue the search until a policy is found that attains that level. One "satisfices" instead of maximizes.

#### *The Next Chance*

Sometimes policy analysts deliberately make little mistakes to avoid big ones. One can deliberately choose a policy (knowing that it is not quite the right policy) that leaves open the possibility of doing better in a next step, instead of a policy designed to be on target but difficult to amend. While an Indian civil servant, for example is inclined to shoot for his target with little thought of a second chance, an American civil servant never expects to be wholly right and values a second chance. In as relatively simple a policy problem as routing New Haven traffic, to try out one-way traffic going south and stand ready, if that is unsuccessful, to try a northbound flow may be better than to gamble, through an *a priori* study of traffic flows, on a permanent installation of expensive controls to inaugurate southbound one-way movement.

#### *Feedback*

A policy analyst may want to deal inconclusively with a problem—that is, keep a next chance open because he thinks that with the passage of time he will come to know more. But if he can choose a policy that will, as in the traffic example, itself feed back information necessary to a better choice of policy, so much the better. Policy feedback is of course a commonplace phenomenon: it is hard to imagine a policy that feeds back no useful information at all. Monetary and fiscal policy is an example of especially

quick and powerful feedback because of its immediate impact on business activity. But policy-making systems differ in sensitivity to feedback and in the skill with which they choose policies in order to induce feedback. A policy chosen because it is ideologically correct—like Soviet policy on collective farms—may persist for years in spite of failure, with its advocates blind to feedback.

#### *Remediability*

In the classical model of rational decision making a policy analyst concerned about American Negroes would be required to formulate in his mind an organized set of policy aspirations and to specify for various dates in the future the income, educational status, and other social and cultural goals at which policy should aim. In actual fact, some policy analysts greatly simplify this otherwise impossible goal-setting task by refusing to look very far ahead—focusing instead on the removal of all-too-observable disadvantages now suffered by the Negroes. That is, if they cannot decide with any precision the state of affairs they want to achieve, they can at least specify the state of affairs from which they want to escape. They deal more confidently with what is wrong than with what in the future may or may not be right.

Critics will say that policy would be more rational if it were guided by positive instead of negative objectives, but it is not at all certain that positive objectives could win assent, or that they would be as operational as negative objectives. . . . If in this sense policy analysts look backward instead of forward, they sometimes gain rather than lose competence.

#### *Seriality*

A policy analyst who appreciates a next chance, exploits feedback, and keeps his eye on ills to be remedied will come to take for granted that policy making is typically serial, or sequential. He will see that policy making is typically a never-ending process of successive steps in which continual nibbling is a substitute for a good bite. He will design policy not merely on the expectation of a second step but on the projection of a third, or a fourth—of an endless series. In this style of policy analysis, he sees possibilities for revising both policies and objectives, and he comes to treat policy making as open-ended in all its aspects. He and any political system of this style may therefore develop a high level of flexibility, resilience and pertinacity that greatly

raises his or its ability to make good policy in the face of complexity. In a system in which policy making is frankly recognized to be serial or sequential, the whole system may be tailored to rapid sequences so that, though no one policy move is great, the frequency of small moves makes rapid social change possible.

In the U.S., policy analysts nibble endlessly at taxation, social security, national defense, conservation, foreign aid and the like. Policy analysts assume that these problems are never solved, and hold themselves in readiness to return to them again and again. That kind of persistence in policy making has transformed the society. America, observers say, has gone through an industrial revolution, an organizational revolution, a revolution in economic organization (from *laissez faire* to a highly regulated economy) and a revolution in the role of the family—but all through policy sequences so undramatic as to obscure the magnitude of change.

#### *Bottlenecks*

Every policy analyst—and you and I in our personal problems—makes frequent use of the tactic of bottleneck breaking to simplify complex problems. On a superficial view of policy making, a bottleneck is nothing more than clear evidence of a breakdown in decision making. If something is running behind schedule, or something necessary to action is missing, or there is a congestion, we say a bottleneck exists. But since bottlenecks are inevitable for complex policy making, policy analysts have discovered how to use them to make the best of a less-than-ideal situation.

\* \* \*

At an extreme, one can see the two contrasting possibilities for policy analysis: on the one hand, plan everything to fit with everything else; on the other hand, plan to break specific bottlenecks as they arise. The first is impossible; the second, though far from ideal, works.

#### *Incrementalism*

Usually—though not always—what is feasible politically is policy only incrementally, or marginally, different from existing policies. Drastically different policies fall beyond the pale. That aside, a preoccupation with no more than incremental or marginal changes in policy often serves for still other reasons to raise the level of competence of policy. Where applicable, such a strategy:

1. concentrates the policy maker's analysis on familiar, better-known experience;
2. sharply reduces the number of different alternative policies to be explored; and
3. sharply reduces the number and complexity of factors he has to analyze.

\* \* \*

What is the ordinary interpretation put on these strategies or dodges? On superficial examination they are often dismissed as irrational. For they are seen as indecisiveness, patching up, timidity, triviality, narrowness of view, inclusiveness, caution, and procrastination. But we have seen them to be useful devices for stretching man's analytic capacities. Man has had to be devilishly inventive to cope with the staggering difficulties he faces. His analytical methods cannot be restricted to tidy scholarly procedures. The piecemealing, remedial incrementalist or satisficer may not look like an heroic figure. He is nevertheless a shrewd, resourceful problem-solver who is wrestling bravely with a universe that he is wise enough to know is too big for him.

\* \* \*

We now come back to face up to the fact that, however extended, policy analysis is inadequate. If it is not possible through analysis to find policies that are everywhere accepted because proved to be correct, what can be done? Someone has to take on the task of deciding on policy for society. But because no one can perform the task of making a decision on policy without the "power" to do so, the more usual way to put the point is to say that, in the absence of universal agreement on what is to be done, someone has to either seize or be given "power" to decide.

In actual fact, of course, "power" is always held by a number of persons rather than by one; *hence policy is made through the complex processes by which these persons exert power or influence over each other.* What is the character of this play of power in policy making? And how is policy analysis incorporated into it?

\* \* \*

"Power" to decide. . . . Some person or persons must simply make policy choices for the society, the rest of the population simply accepting the decisions. The policy-making task or function has to be seized or assigned.

Whoever takes on the task will of course employ analysis up to a point. He must, however,

come to a decision—by guess, considered judgment, or whim.

The task or responsibility cannot be laid upon one man or small group. Even an authoritarian ruler needs a structure of subordinate colleagues to assist him in policy making. In the democracies, almost every adult is offered a share of the task, and many accept. For example—though examples do not do justice to the complexities of task definition and assignment—some (President, Prime Minister, or Cabinet) accept the principal responsibility for initiating policy decisions; others the (legislative) task of amending, ratifying, or rejecting these policy decisions; others the (judicial) task of testing policy decisions for consistency with constitutional rules; and others, the task of deciding who will be a member, say, of the legislative group—a task they are able to discharge through voting.

*Rules and authority.* Such a process—both the relatively simpler one of authoritarian policy making and the intricate one of democratic policy making—works because somehow people perform the tasks they accept, and others accept the results. But why do people accept? They may be terrorized into so doing by someone or some group that can command a personal guard, police force, or army. In democratic societies, however, they do so for a variety of other reasons. Some people like the tasks that have been assigned, or the money, prestige, and sense of power that go with them. Others who like to be left free of responsibility are willing to accept what is decided upon. Some people simply believe that, since some assignment of tasks is necessary, they ought morally to perform their assigned task and go along with others who perform theirs. Others perform their tasks, or accept the results of those who do, because they fear the enmity of their neighbors and associates if they fail to do so, or because those who do accept their assignments have organized routines for fining, jailing, or otherwise punishing others.

Whatever the reasons, most people adopt a rule of performing an accepted task according to the task's specifications, and a rule of accepting the decisions of those to whom tasks have been assigned. They do not ask themselves at every opportunity: "Could I get away with defaulting?" Some of the rules they accept are those of obedience on specified matters to specified categories of persons, thus establishing the authority of those persons.

*Specialization.* A proliferation of specialized tasks in policy making arises both as a method of



raising the competence of policy makers (since no one participant in the process can be competent in all areas and on all aspects of policy making) and as a method of limiting the power or influence of any one policy maker. In democratic societies, an especially intricate specialization of function—carried to an extreme in the American pattern of checks and balances—greatly constrains the power or influence of any one policy maker.

*Cooperation.* Hence in all policy-making systems, but especially in democratic systems, policies can be made only through the cooperation of many participants, each of whom performs a task that is necessary, but itself insufficient, to establish a policy decision. Policy making is a cooperative collective effort, and policy a joint output, beyond the capacity of any one person or any small group of those to whom policy-making tasks are assigned.

c.

**Aaron Wildavsky**  
**The Politics of the Budgetary Process\***

Budgeting is incremental, not comprehensive. The beginning of wisdom about an agency budget is that it is almost never actively reviewed as a whole every year in the sense of reconsidering the value of all existing programs as compared to all possible alternatives. Instead, it is based on last year's budget with special attention given to a narrow range of increases or decreases. Thus the men who make the budget are concerned with relatively small increments to an existing base. Their attention is focused on a small number of items over which the budgetary battle is fought. As Representative Norrel declared in testifying before the House Rules Committee, "If you will read the hearings of the subcommittees you will find that most of our time is spent in talking about the changes in the bill which we will have next year from the one we had this year, the reductions made, and the increases made. That which is not changed has very little, if anything, said about it." Most appropriations committee members, like Senator Hayden in dismissing an item brought up by the Bureau of Indian Affairs, "do not think it is necessary to go into details of the estimate, as the committee has had this appropriation before it for many years." Asked to defend this procedure,

a budget officer (or his counterparts in the Budget Bureau and Congress) will say that it is a waste of time to go back to the beginning as if every year was a blank slate. "No need to build the car over again."

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If a normative theory of budgeting is to be more than an academic exercise, it must actually guide the making of governmental decisions. The items of expenditures that are passed by Congress, enacted into law, and spent must in large measure conform to the theory if it is to have any practical effect. This is tantamount to prescribing that virtually all the activities of government be carried on according to the theory. For whatever the government does must be paid for from public funds; it is difficult to think of any policy that can be carried out without money.

The budget is the lifeblood of the government, the financial reflection of what the government does or intends to do. A theory that contains criteria for determining what ought to be in the budget is nothing less than a theory stating what the government ought to do. If we substitute the words "what the government ought to do" for the words "ought to be in the budget," it becomes clear that a normative theory of budgeting would be a comprehensive and specific political theory detailing what the government's activities ought to be at a particular time. A normative theory of budgeting, therefore, is utopian in the fullest sense of that word: its accomplishment and acceptance would mean the end of conflict over the government's role in society.

By suppressing dissent, totalitarian regimes enforce their normative theories of budgeting on others. Presumably, we reject this solution to the problem of conflict in society and insist on democratic procedures. How then arrive at a theory of budgeting that is something more than one man's preferences?

The crucial aspect of budgeting is whose preferences are to prevail in disputes about which activities are to be carried on and to what degree, in the light of limited resources. The problem is not only "how shall budgetary benefits be maximized?" as if it made no difference who received them, but also "who shall receive budgetary benefits and how much?" One may purport to solve the problem of budgeting by proposing a normative theory (or a welfare function or a hierarchy of values) which specifies a method for maximizing returns for budgetary expendi-

\* Boston: Little, Brown & Co. 15, 128-138 (1964). Copyright © 1964 by Little, Brown & Company, Inc. Reprinted by permission.



tures. In the absence of ability to impose a set of preferred policies on others, however, this solution breaks down. It amounts to no more than saying that if you can persuade others to agree with you, then you will have achieved agreement. Or it begs the question of what kind of policies will be fed into the scheme by assuming that these are agreed upon. Yet we hardly need argue that a state of universal agreement has not yet arisen.

Another way of avoiding the problem of budgeting is to treat society as a single organism with a consistent set of desires and a life of its own, much as a single consumer might be assumed to have a stable demand and indifference schedule. Instead of revenue being raised and the budget being spent by and for many individuals who may have their own preferences and feelings, as is surely the case, these processes are treated, in effect, as if a single individual were the only one concerned. This approach avoids the central problems of social conflict, of somehow aggregating different preferences so that a decision may emerge. How can we compare the worth of expenditures for irrigation to certain farmers with the worth of widening a highway to motorists and the desirability of aiding old people to pay medical bills as against the degree of safety provided by an expanded defense program?

The process we have developed for dealing with interpersonal comparisons in government is not economic but political. Conflicts are resolved (under agreed-upon rules) by translating different preferences through the political system into units called votes or into types of authority like a veto power. There need not be (and there is not) full agreement on goals or the preferential weights to be accorded to different goals. Congressmen directly threaten, compromise, and trade favors in regard to policies in which values are implicitly weighted, and then agree to register the results according to the rules for tallying votes.

The burden of calculation is enormously reduced for three primary reasons: first, only the small number of alternatives politically feasible at any one time are considered; second, these policies in a democracy typically differ only in small increments from previous policies on which there is a store of relevant information; and, third, each participant may ordinarily assume that he need consider only his preferences and those of his powerful opponents since the American political system works to assure that

every significant interest has representation at some key point.

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The basic idea behind program budgeting is that instead of presenting budgetary requests in the usual line-item form, which focuses on categories like supplies, maintenance, and personnel, the presentation is made in terms of the end-products, of program packages like public health or limited war or strategic retaliatory forces. The virtues of the program budget are said to be its usefulness in relating ends to means in a comprehensive fashion, the emphasis it puts upon the policy implications of budgeting, and the ease with which it permits consideration of the budget as a whole as each program competes with every other for funds. Interestingly enough, the distinguishing characteristics of the program procedure are precisely the reverse of those of the traditional practice. Federal budgeting today is incremental rather than comprehensive, calculated in bits and pieces rather than as a whole, and veils policy implications rather than emphasizing them.

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The incremental, fragmented, non-programmatic, and sequential procedures of the present budgetary process aid in securing agreement and reducing the burden of calculation. It is much easier to agree on an addition or reduction of a few thousand or a million than to agree on whether a program is good in the abstract. It is much easier to agree on a small addition or decrease than to compare the worth of one program to that of all others. Conflict is reduced by an incremental approach because the area open to dispute is reduced. In much the same way the burden of calculation is eased because no one has to make all the calculations that would be involved in a comprehensive evaluation of all expenditures.

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Procedures that de-emphasize overt conflicts among competing programs also encourage secret deliberations, non-partisanship, and the recruitment of personnel who feel comfortable in sidestepping policy decisions most of the time.

\* \* \*

Consider by contrast some likely consequences of program budgeting. The practice of

focusing attention on programs means that policy implications can hardly be avoided. The gains and the losses for the interests involved become far more evident to all concerned. Conflict is heightened by the stress on policy differences and increased still further by an in-built tendency to an all-or-nothing, "yes" or "no" response to the policy in dispute. The very concept of program packages suggests that the policy in dispute is indivisible, that the appropriate response is to be for or against rather than bargaining for a little more or a little less. Logrolling and bargaining are hindered because it is much easier to trade increments conceived in monetary terms than it is to give in on basic policy differences. Problems of calculation are vastly increased by the necessity, if program budgeting is to have meaning, of evaluating the desirability of every program as compared to all others, instead of the traditional practice of considering budgets in relatively independent segments. Conflict would become much more prevalent as the specialist whose verdict was usually accepted in his limited sphere gave way to the generalist whose decisions were fought over by all his fellow legislators who could claim as much or (considering the staggering burden of calculation) as little competence as he. The Hobbesian war of all against all, though no doubt an exaggeration, is suggestive on this score.

I wish to make it clear that I am not saying that the traditional method of budgeting is good because it tends to reduce the amount of conflict. Many of us may well want more conflict in specific areas rather than less. What I am saying is that mitigation of conflict is a widely shared value in our society, and that we ought to realize that program budgeting is likely to affect that value.

## NOTES

### NOTE 1.

WILLIAM M. CAPRON  
THE IMPACT OF ANALYSIS ON BARGAINING  
IN GOVERNMENT\*

Does the current addition to Washington's alphabetic vocabulary—PPBS—signify that a real and important change is occurring in the Federal government's decision-making process? Or do the techniques, devices, and ground rules

summed up in the terms for which those initials stand—Planning - Programming - Budgeting System—merely represent a systematic eruption which will leave unaffected the real elements—and the actual results of—the "bargaining," or decision-making process, in government?

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[O]ne of the hallmarks of good systems analysis as it has come to be practiced is that simultaneously with a definition and testing of alternative means is the refinement and specification of objectives. Furthermore, the *comprehensiveness* of any given systems analysis will depend on the ingenuity of the analyst, the kind of data available to him, and the amount of resources that are at his command in undertaking the analysis. I would urge even if Lindblom's preferred approach—successive limited comparisons—is selected as appropriate to the case in hand that it should be undertaken *systematically* with assumptions clearly specified. This is particularly necessary since in this approach, as he points out, many of the interrelationships with other parts of the system are ignored. It is important that those who will use the results of the "analysis" have called to their attention the limited nature of the analysis so that the limited, partial and incomplete nature of the argument will be understood. I am not so concerned with "comprehensiveness" or the lack thereof, but rather with the use of a very casual and inarticulate "analysis" in place of a specific, "spelled-out" analysis. The "consumer" of the results should be in a position to judge whether or not the particular analysis is in fact useful to him—whether he wants to be guided, in whole or in part, by the results of that "analysis."

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... I must refer to one other theme which is frequently emphasized by the skeptics, namely, the value in many areas of government activity in *not* being explicit about objectives. Representatives of this view have pointed out that at least in some instances agreement on specific programs is possible, even though the interests of various affected groups in the program may be not only quite different but, in terms of their overall value schemes, antithetical. From this one *might* draw the inference that an attempt at articulating an analysis which identifies objectives will actually make agreement on programs and on budgets *more* difficult than reliance on implicit reasoning and bargaining to arrive at

\* A. A. Altshuler, ed.: *The Politics of the Federal Bureaucracy* 196-207 (1968). New York: Dodd, Mead & Co. Reprinted by permission.

the program's contours and level. It is, moreover, pointed out that the implicit "analysis" in a bargaining system with various interests and values "taken care of" by the representation of these interests and values by one or more players at the bargaining table is a good and workable system. I would agree that, by and large, the system has been pretty good and pretty workable and I further agree that one can undoubtedly identify specific cases (especially where feelings run high) which might be put back rather than forward by an attempt to subject the program to an explicit analysis—or at least to make that analysis public. (But the Executive Branch can develop its position based on analysis without injecting analysis into public debate.) However, I am not persuaded by this view as a regular and basic guide. For one thing, the fact that there are different interests and different values concerned with particular programs does not mean that systematic analysis will necessarily make agreement on specific program decisions and specific budget decisions impossible. It is possible, for example, to reflect explicitly the degree and extent to which different objectives or values will be realized under different alternative approaches and different levels in a given program area. Thus, the interested parties will be able to identify the extent to which *their* own particular interests—their own particular weighing of the outcomes—will be achieved.

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... PPBS was designed and is being pushed as a technique or set of techniques which will improve the *Presidential* budget decision process. The President, having made his decisions, can forward his recommendations to the Congress in a variety of forms. It is worth emphasizing that in any case the *implementation* of the President's budget once the Congress has authorized and appropriated funds requires expression of these budgetary decisions in the familiar "object class," input-oriented, and organizational-unit oriented terms of the traditional budget. There is no special difficulty or extra burden placed on the Executive Branch in translating the results of the program budget and the decisions reflected therein to the Congress in the familiar terms which they seem, at least up until now, to prefer. Thus, I see no particular technical difficulty in acceding to the apparent will of Congress that the familiar budget structure be maintained with regard to their deliberations.

There is, however, one central and sensitive

point involved in the implications of PPB for Congressional-Executive relations: a key element in the new system is its emphasis on *multi-year* programming and budgeting. The standard pattern is that each program be developed in terms of a five-year program plan and that this be translated into a five-year financial plan.

... Even though the President decides not to submit formally the five-year program and financial plans to the Congress, there is little question, given the facts of life in Washington, that the existence of these plans will not only be well known but that they will, one way or another—above or below the table—come into the hands of the Congress. I recognize that there is a certain amount of risk for the President in this situation. Only by the repeated and steadfast reiteration of the fact that the plans for each program beyond the next budget year—the year for which he must make specific recommendations—are only tentative and do *not* represent any kind of Presidential determination or commitment, can he avoid creating the impression that he is committed for the future.

\* \* \*

#### NOTE 2.

ALLEN SCHICK  
SYSTEMS FOR ANALYSIS—  
PPB AND ITS ALTERNATIVES\*

\* \* \*

... Despite all the talk about cost-benefit analysis, there are too many conceptual and operational difficulties to the implementation of useful benefit analysis at this time. Economists who have joined the analytic staffs have had to trim their sails and put a good deal of their methodological equipment into storage. It is not that the problems confronting Government are simple; they are too difficult to solve with the high-powered methods now at hand. Before benefits can be measured, they have to be identified. Some scale of values must be set. The question of values is especially troublesome, for each discipline and interest has its own way of seeing and evaluating things.

If policy analysis were focused on public benefits, it would be appropriate to have a system structured according to the purposes of Gov-

\* *The Analysis and Evaluation of Public Expenditures—The PPB System*, Joint Economic Committee, Subcommittee on Economy in Government, 91st Congress, 1st Session 825-827, 832-833 (1969)

ernment. An end product program structure would facilitate the comparison of alternative program opportunities on some homogeneous value scale. Such is not the case, however. Most policy analysis deals not with benefits, but with program effectiveness. Only implicitly does the analyst put a value on the program he is studying. For example, a billion-dollar health care program might be adjudged the most cost effective if it yields a lower infant mortality rate than any alternative billion-dollar program. Unlike benefit analysis which begins with some social value, effectiveness analysis begins with a concrete set of objectives that are embodied in specific programs or with a problem that concerns policymakers. In appraising a health care program, one need not place some value on the life of an infant. One need only assume that more lives saved is preferable (i.e., more effective) to fewer lives saved.

\* \* \*

... Unless the categories are designed with sensitive attention to problems as they are perceived by top officials and unless they are revised frequently to reflect changing perspectives, the program categories will hinder rather than abet useful policy analysis. It is very doubtful that this kind of categorization can be devised. The analyses undertaken in HEW ignored the boundaries imposed by the program categories. Problems don't come packaged according to some grand formulation of governmental ends. The analyst must pursue his problem in whatever format is appropriate, regardless of the constraints of the data system. Sometimes he will want to look at health from the viewpoint of target groups—expectant mothers, the needy, the elderly. Other times, he will want to study

health in terms of diseases—heart, kidney, cancer, and so on.

\* \* \*

The attempt to link analysis to budgeting is a logical recognition of the place and potency of the budget process in public policymaking.

... Yet it is appropriate to question the connection to budgeting and to raise the possibility of some alternative outlet for analysis.

[B]udgeting is nonanalytic and ... a rigid integration of analysis and budgeting will not be successful.

[T]he cause of analysis would be better served if analytic work were addressed to the processes of program determination and legislative recommendation. These processes are not well formalized, but they are the processes which deal with the big issues, which mark departures from the status quo and changes in direction. The overwhelming weight of the budget process favors the continuation of what is already on the books. When a President wishes to launch new programs, he is impelled to rely on task forces, advisory staff, and ad hoc arrangement. All these are lacking sustained analytic focus, but perhaps they are more useful than the budget process. In the crowded months of the budget cycle, there just isn't enough time or inclination to consider the bigger issues, to look beyond the present and the certain to the future and the speculative.

While analysis can be channeled to both planning and budgeting, I would urge that attention be given to the neglected opportunities for planning. We tend to rely too heavily on an overburdened budget process and not enough on other decisional institutions.

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## **PART TWO**

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# **The Authority of the Investigator as Guardian of Science, Subject and Society**

This Part explores the role of the investigator in the human experimentation process. Unlike theoretical scientists whose freedom to pursue their studies, though sometimes challenged, is generally accepted in contemporary society, investigators involved in human research often find their freedom encumbered by the rights and interests of their subjects. Consequently, the student of human experimentation must confront issues which would be only remotely relevant, and perhaps even overreaching, to someone examining decisionmaking in theoretical science.

To familiarize the student with the range of activities investigators pursue with human subjects, research studies from a variety of disciplines are presented in this Part. Among the analytic tasks which emerge from an examination of these materials are: (1) to identify those decisions of investigators which conflict with the interests and values of subject and society; (2) to determine whether the investigator, once aware of these conflicts, can resolve them on his own initiative to the satisfaction of society and subject; (3) if not, to explore those consequences to subject and society which should affect his authority; and (4) to decide by what rules and procedures the extent of his authority should be established.

We begin this inquiry with a question: Can complete authority over experimentation be left to the investigator, placing trust in his professional conscience? Materials from set-

tings in which investigators were permitted to work with considerable or even unlimited freedom are presented in Chapter Five, in order to explore the problems created by relying on an investigator's personal and professional conscience, ambition, and training, without providing him with rules and procedures to guide his personal choices. Abandoned to their scientific curiosity, some researchers are impelled to proceed in disregard of the consequences for themselves or mankind, while other researchers, abandoned to their ethical doubts, hold back on experiments of great potential value.

If investigators are to be responsive to the rights and interests of others, the nature and extent of their authority must be defined. This involves determining how to minimize intentional and unintentional harm to subject, science, and society. Thus, Chapters Six and Seven seek first to define categories of harm to which research may expose subjects and society and then to identify additional elements of experimental design and objectives (*e.g.*, the subject's awareness of participating in an experiment, the subject's understanding of its risks, or the benefits of an experiment to subject, science, and society) which may aggravate or mitigate an experiment's harmful consequences. Both the nature of harm and the conditions under which it may arise must be examined in any attempt to define the proper scope of an investigator's authority for the formulation, administration, and review of experimentation with human beings.

Throughout we ask:

1. What are the professional and personal goals of the investigator, and what consequences to subjects and society result from the pursuit of these goals as well as the means employed to pursue them?
2. What interests of investigator, subject, and society are affected by these consequences?
3. Which of these consequences and interests can be taken into account by the investigator alone, and how should they affect his decisions?
4. In what areas are investigators competent to make informed judgments, and what are the limits of their personal and professional competence?
5. What aspects of experimental design and objectives should either extend or limit the investigator's authority and who else, if anyone, should participate in his decisionmaking?
6. To what extent should the investigator's authority be affected by a coexisting intention to benefit subjects and society?

## CHAPTER FIVE

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# Experimentation without Restriction

What consequences ensue for society and subjects if no external restraints are imposed on an investigator's utilization of human subjects for research purposes? Perhaps the most extreme example of experimentation without restrictions occurred in Nazi Germany. In the concentration camps "political," "racial," and "military" prisoners, considered unworthy of the protections ordinarily afforded to citizens of any society, were made available to physicians for research. This environment provided the investigators with an opportunity to carry out studies with unlimited freedom. In scrutinizing these materials from the vantage point of experimentation without restriction, we disregard their cultural origins and focus on those aspects which may offer insights relevant to our society.

The legal proceedings against the Nazi doctors at Nuremberg exposed in their starkest form the inherent conflicts between scientific interests and societal interests (seen as the interest of the world community). We do not suggest that other cases comparable to the concentration camp experiments in magnitude or cruelty can be found elsewhere in the literature. Yet examples from the pre- and post-World War II periods demonstrate that the actions of the Nazi physicians were not isolated instances of "crimes against humanity." Whenever subjects are too helpless or ignorant to resist participation, the investigator is in a position to pursue his scientific interests constrained only by his personal and professional conscience and values. Thus, similar transgressions occurred prior to the Nuremberg trials and continue to occur, though because they are less dramatic their existence is more likely to be denied.

In examining these materials consider the following questions:

Do investigators require external guides in order to make decisions about the limits of human experimentation? If so, what form should they take?

A.

## Prologue—Experiments Prior to 1939

### Vikenty Veressayev The Memoirs of a Physician\*

... I will now occupy myself with a question to which but one answer is possible, and that a perfectly straight one. It deals with gross and entirely conscious disregard for that consideration which is due to the human being.

I approach the subject with regret, but it is impossible to pass it by.

"A certain Dr. Koch," we read in the Russian medical paper, *Physician*, "has published a pamphlet, entitled, ... Medical Experiments on Living Man, than which nothing were better calculated to further undermine the respect for, and confidence of the laity in, our profession. The author essays to prove that 'vivisection has long crossed the thresholds of our hospitals'—in other words, that experiments similar to those conducted upon the lower animals in the laboratory, are practised on living man in our infirmaries. ..."

[U]nfortunately there is much substantial truth even in the title of Dr. Koch's booklet alone. In proof of the above it would be easy enough to adduce a very long array of facts—facts of such a nature, too, that they could not be bracketed in inverted commas, for this simple reason—they are substantiated in black and white by the perpetrators themselves.

As we proceed, I shall point out the original sources of my information with every possible care, that the reader may verify my statements.

I shall restrict myself to the venereal diseases. ... I was compelled in my choice to single out the above, because they furnish us with the greatest wealth of the facts I wish to draw public attention to. For venereal complaints are the exclusive lot of man, and not a single one of them can be transmitted to the lower animals.<sup>1</sup>

Owing to this, many questions which, in other branches of medicine, find their answer in experiments on animals, can, in venerology, only be decided through human inoculation, and venerologists have not hesitated to take the plunge. ...

\* \* \*

The specific micro-organism of gonorrhoea was discovered by Neisser in 1879. His experiments, conducted with exemplary care, tended to prove, with a considerable degree of probability, that the gonococcus he had discovered was the specific agent of that disease. But in bacteriology the proof positive of the specific quality of any micro-organism is only absolute when obtained through inoculation; if, on inoculating an animal with a pure culture of the micro-organism, we call forth a given disease, this fact proves that the above micro-organism is the specific agent of the latter. Unfortunately, not a single animal, as we already know, is liable to gonorrhoea. Either the discovery had to remain doubtful, or else it was necessary to inoculate man. For himself, Neisser chose the first alternative.

His followers were not so nicely conscientious. The first to inoculate man with gonococcus was Dr. Max Bockhart, assistant to Professor Rinecker.

"Geheimrath von Rinecker," writes Bockhart, "held the view, that the discovery of the causes of venereal disease was only possible through the inoculation of human beings."<sup>2</sup> Acting upon the suggestion of his patron, Bockhart inoculated a patient suffering from creeping paralysis in its last stages with a pure culture of gonococcus: a few months previously the patient had lost his sense of feeling and his death was awaited very shortly.

\* Translated from Russian by Simeon Linden. New York: Alfred A. Knopf, 332-366 (1916). [Wherever possible the references in Dr. Veressayev's book were checked against the original sources; their accuracy was confirmed in every instance.]

<sup>1</sup> It has been possible to infect monkeys with syphilis.

<sup>2</sup> "Beitrag zur Aetiologie des Harnröhrentripers": *Vierteljahrsschr. für Dermatol. und Syphilis*, 1883, p. 7.



The inoculation proved successful, but the discharge was very insignificant. To increase it, the patient was given half a litre of beer. "The success was brilliant," writes Bockhart; "the discharge became very copious. . . . Ten days after inoculation the patient died of a paralytic fit. Autopsy showed acute gonorrhoeic inflammation of the urethra and bladder, with incipient kidney mortification, and a large number of abscesses in the left kidney; numerous gonococci were found in the pus taken from these abscesses."<sup>3</sup>

The methods of pure culture employed by Bockhart were very crude, and his experiment had but small scientific value. The first undoubtedly pure culture of gonococcus was obtained by Ernst Bumm.<sup>4</sup> To prove that it was the specific agent, Bumm, by means of a platinum wire, introduced the culture into a woman's urethra, which had been found perfectly healthy after repeated examinations. Typical urethritis developed which required six weeks for its cure (*op. cit.*, p. 147). Studying the various peculiarities of his cultures, Bumm inoculated his gonococcus upon another woman in the same manner, obtaining an identical result (p. 150). Here we must note that, more than twenty years previously, Noeggerath proved how serious and painful were the effects—especially in the case of women, following so-called "innocent" gonorrhoea. . . . Bumm himself declares, in the preface to his work, that "gonorrhoeic infection is one of the most important causes of painful and serious affections of the sexual organs";<sup>5</sup> which knowledge did not, however, deter him from subjecting two of his patients to such a risk. It is true that, according to his accounts, "every measure of precaution (?) against infection of the sexual organs" was taken, but such precautions are extremely unreliable. We may further add that even gonorrhoeic affection of the urethra alone is sufficient to cause the most painful complications later.

<sup>3</sup> "Beitrag zur Aetiologie des Harnröhrentripers": *Vierteljahrsschr. für Dermatol. und Syphilis*, 1883, pp. 7–10.

<sup>4</sup> E. Bumm, "Die Micro-organismen der gonorrhoeischen Schleimhautrekrankheiten." 2. Ausg. Wiesbaden, 1887.

<sup>5</sup> *Op. cit.*, p. iv.

<sup>7</sup> Provisional report in the *Deutsche med. Wochenschrift*, 1891, No. 50 ("Reinzuchtung des Gonococcus Neisser mittels des Plattenverfahrens"). Detailed description in *Archiv für Gynaecologie*, Bd. 42 (1892): "Die ascendierende Gonorrhoe beim Weibe."

The next step in the culture of the gonococcus was made by Dr. Ernst Wertheim,<sup>7</sup> who succeeded in obtaining a pure culture on plates. "To prove conclusively," writes Wertheim, "that the colonies growing on the plates were really those of Neisser's gonococci, it was naturally necessary to perform inoculation upon the urethra of man." Wertheim inoculated four paralytic patients with his culture and also a certain S. (an idiot of thirty-three). "Fairly abundant discharge was still noticeable in S. two months after inoculation."<sup>8</sup> Wertheim made no further experiments "owing to lack of suitable material."<sup>9</sup>

Wertheim's methods were verified by other investigators . . . Karl Menge . . . inoculated a woman suffering from a vesico-vaginal carcinomatous fistula, with gonococcus; it was he also, who inoculated a woman, suffering from tumour on the brain, with gonorrhoea, two days before her death.<sup>11</sup>

But especially comprehensive were the experiments of Finger, Ghon and Schlangenhäufen.<sup>12</sup> They inoculated fourteen patients, all of them hopeless cases, chiefly consumptives, who mostly died from three to eight days after inoculation. "Extremely valuable histological material was furnished by the patient F.D., 21, who died three days after inoculation. Taking into consideration," remarked the joint authors, "the short duration of the process, which lasted but three days, one is surprised at its intensity, which caused such deep histological changes."

Gonorrhoea is one of the commonest causes of inflammation of the eyes in newly born infants. Many investigators studied the relationship of gonococcus to eye-disease in newly born children. E. Fraenkel inoculated the eyes of infants, which could not have lived in any case, with the inflammatory secretions of gonorrhoeic patients. One of the infants lived for ten days after inoculation, developing typical purulent inflammation of the eyes.<sup>13</sup>

Tischendorff inoculated the eyes of atrophic

<sup>8</sup> *Archiv*, pp. 17, 28, 33–34, 37, 39.

<sup>9</sup> I will here draw attention to the fact of Wertheim having injected pure culture of gonococcus into his own body—each time with positive results.

<sup>11</sup> "Ein Beitrag zur Kultur des Gonococcus": *Centralblatt für Gynaecologie*, 1893, No. 8.

<sup>12</sup> "Zur Biologie des Gonococcus": *Archiv für Dermatologie und Syphilis*, Bd. 28, 1894, pp. 304–306, 317–324.

<sup>13</sup> "Bericht über eine bei Kindern beobachtete Endemie infectiöser Kolpitis": *Virchow's Archiv* Bd. 99, Heft 2 (1885), pp. 263–264.

children with gonorrhoeic discharge of little girls suffering from that disease: purulent inflammation, with characteristic gonococci, was the result.<sup>14</sup> Kroner inoculated six adult blind persons with the muco-purulent discharge of pregnant and parturient women (with negative results).<sup>15</sup>

\* \* \*

[W]e pass on to syphilis. Without going far back into antiquity, I shall give an account of the history of that disease dating from the times of the celebrated French syphilologist, Philippe Ricord.

Ricord cleared up many obscure problems of his specialty and entirely reconstructed the science of venerology. But, of course, he did not escape error. One of his most lamentable mistakes was the affirmation that syphilis was not contagious in its secondary stage. This mistake was due to the fact that while Ricord performed endless inoculations upon venereal patients, he never ventured to experiment upon the healthy.<sup>17</sup> Let us see how this fallacy was set right.

One of the first to express himself in favour of secondary syphilis being contagious was the Dublin physician, William Wallace, in his highly instructive "lectures on Cutaneous and Venereal Disease."<sup>18</sup> These lectures are remarkable for the classical shamelessness with which their author tells us of his criminal experiments in inoculating healthy people with syphilis.

\* \* \*

In his . . . lectures, he gives a detailed account of his inoculations performed upon five healthy individuals from 19 to 35 years of age. All developed characteristic syphilis.<sup>19</sup>

<sup>14</sup> "Verhandlungen der 57 Versammlung deutscher Naturforscher u. Aerzte in Magdeburg, 1884": *Archiv für Gynaecologie*, Bd. 25 (1885), p. 114.

<sup>15</sup> *Ibid.*, p. 113.

<sup>17</sup> Rinecker, referring to this fact, very justly remarks: "It is hard to understand why Ricord condemned the inoculation of the healthy so absolutely; taking into consideration the vast number of his experiments, he could not have remained in ignorance of the fact that the inoculation of the sick is not infrequently dangerous to the latter." The sum-total of Ricord's gonorrhoeic inoculations, as well as those of syphilis and soft ulcer, amounted to seven hundred.

<sup>18</sup> W. Wallace, "Lectures on Cutaneous and Venereal Diseases": *The Lancet* for 1835-36 vol. ii., p. 132.

<sup>19</sup> "Clinical Lectures on Venereal Diseases": *The Lancet* 1836-1837, vol. ii., pp. 535, 536, 538, 620, 621.

In his twenty-second lecture Wallace declared that the facts above mentioned were "only a portion, yes, a very small portion of those of a similar kind which I could adduce."<sup>20</sup> In his twenty-third he again lays stress on the circumstance that the experiments described were only a small part of those he had conducted.<sup>21</sup>

"Is it permissible to expect more convincing proofs of the contagiousness of the secondary stage of syphilis?" queries Dr. Schnepf,<sup>22</sup> writing on the subject of these experiments. "No further experiments on the healthy are required. Wallace's make them entirely superfluous. The problem is solved, science desires no more victims; all the worse for those who close their eyes to this fact."

But the orgy was only about to commence. . . . In 1851 the "remarkable epoch-making" experiments of Waller were published. This is how he described them:—

"First experiment: Durst, a boy of 12, registration number 1396, suffered for a number of years from sores on the head. Otherwise quite healthy, never had rash or scrofula. As his disease required his detention in hospital for several months, and as he had not suffered from syphilis in the past, I found him to be very suitable for inoculation, which was performed on August 6th. The skin of the right thigh was incised and the pus taken from a syphilitic patient introduced into the fresh and slightly bleeding wounds. I rubbed the matter into the abrasions with a spatula, then I rubbed the scarified surface with lint soaked in the same matter, and having covered it with the same lint, applied a bandage." About the beginning of October the child developed a typical syphilitic rash.<sup>23</sup>

"Second experiment: Friedrich, 15, registration number 5676, suffered for the last seven years from lupus of the right cheek and the chin. Up to now the patient had not had syphilis and was therefore eligible for inoculation. This was performed on July 27th. I introduced the blood of a syphilitic woman into fresh incisions made on the left thigh and then dressed the wounds with lint soaked in the same blood." About the

<sup>20</sup> *Ibid.*, p. 539.

<sup>21</sup> *Ibid.*, p. 615.

<sup>22</sup> "De la contagion des accidents consecutifs de la syphilis": *Annales des maladies de la peau et du syphilis*. Publ. par A. Cazenave. Vol. iv., 1851-52, p. 44.

<sup>23</sup> Waller, "Die Contagiosität der secundären Syphilis": *Vierteljahrsschr. für d. prakt. Heilkunde*. Prag. 1851, Bd. I. (xxix), pp. 124-126.

beginning of October the success of the inoculation was beyond a doubt.<sup>24</sup>

"I showed both patients expressly to the director of the hospital, Riedel," adds Waller, "to the head physicians of the hospital (Boehm and others), to many of the city physicians, to several professors (Jackisch, Kubik, Oppolzer, Dietrich and others), to almost all the hospital physicians of the city and to many foreigners. All unanimously substantiated the accuracy of my diagnosis of the syphilitic rash and declared themselves ready, if necessary, to step forward as witnesses of the reality of the results of my inoculations."

Is not this a complete and accurate . . . criminal report? All the details of the "case" are communicated, the victims are indicated and the witnesses cited name by name. . . . If the public prosecutor had peeped into this province, he would have found his task wonderfully simplified.

Waller's were the signal for general and universal experiments for the verification of the contagiousness of secondary syphilis.

In March, 1852, Professor Rinecker inoculated a boy of 12, suffering from incurable St. Vitus' dance, with the pus taken from a syphilitic patient. After the lapse of a month the inoculated part developed infiltration and induration. There were no constitutional symptoms in this case.<sup>25</sup>

In 1855, at a convocation of Pfalz doctors, while the contagiousness of secondary syphilis was under discussion (in connection with Waller's experiments), the assembly was acquainted by its secretary with the contents of a communication received from an absent colleague.

"A peculiar coincidence made it possible for the above-mentioned colleague to carry out experiments in connection with the contagiousness of secondary syphilis, without infringing the laws of humanity." These experiments consisted of the following.<sup>26</sup> The discharge of flat moist condylomata and the secretion of the fissures of a female syphilitic patient, were inoculated upon eleven persons—three women of 17,

20 and 25 years of age respectively, and eight men of ages varying from 18 to 28 years. All developed syphilis. The pus of syphilitic ulcers was used for inoculating three women of 24, 26 and 35 years of age respectively. All three developed syphilis. Sores on the feet of six patients were smeared with blood taken from a syphilitic patient; three of the above contracted syphilis. The blood of a syphilitic patient was introduced into the wounds left after wet cupping of three persons. There was no result.<sup>27</sup>

Thus *twenty-three persons* were inoculated; seventeen of these developed syphilis; and it was found possible to do all this "without infringing the laws of humanity!" Truly, a wonderful coincidence! As we proceed, we shall see that such "coincidences" are not rare in syphilology. . . . The identity of the author of these experiments never transpired; he found it best to keep his infamous name forever in the dark, and he is known in science to this day as the "*Anonimus of Pfalz*."

The same question of the contagiousness of secondary syphilis was the subject of the researches of Professor H. von Hubbenet. Among others, he made the following experiments:—

1. "F. Susikoff, medical orderly, 20 years of age, in February, 1852, underwent inoculation with mucous papulae of a syphilitic patient, while in blooming health. I blistered his left thigh, and, after thus removing the cuticle, transferred the matter of the mucous papule, by means of a spatula, to the raw surface, and applied lint dressing impregnated with the same secretion. . . . Roseolae appeared on his chest and abdomen in five weeks. From that moment the syphilitic affection made rapid progress. I kept the patient in this condition for a week longer, to enable me to demonstrate him before as large a number of physicians as possible, and thus allow them to assure themselves of the actuality of the fact. At last I applied the mercurial treatment, and the patient was cured in three months."

2. "Private Timothy Maximoff, 33, admitted to the surgical clinic on January 13th, 1858, suffering from an inveterate fistula of the urethra. As according to every calculation the patient was to remain in hospital for a considerable period, and there was thus sufficient time at our disposal to await results, this case struck me

<sup>24</sup> *Ibid.*, pp. 126–128.

<sup>25</sup> "Über die Ansteckungsfähigkeit der constitutionellen Syphilis": *Verhandlungen der phys. medic. Gesellschaft in Würzburg*, Bd. III. (1852), p. 391. In the clinic of the same Prof. Rinecker two physicians, Drs. Warneri of Lausanne and W. P., consented to be inoculated and both developed syphilis.

<sup>26</sup> *Ibid.*

<sup>27</sup> "Auszüge aus den Protocollen des Vereins pfälzischer Aerzte vom Jahre 1855": *Aerztliche Intelligenzblatt*, 1856 No. 35, pp. 425, 426.

as being a suitable one for experiment. On March 14th, inoculation with the matter taken from the ulcerated tonsils of Private Nesteroff was performed. . . . By May 22nd characteristic roseolae. . . . Mercurial treatment started on June 2nd, and in six weeks the patient was cured."<sup>28</sup>

Commenting upon these descriptions, Professor V. A. Manassein expresses himself as follows: "We do not know what to be more amazed at: the cold-blooded way in which the experimenter allows syphilis to develop more acutely for the purposes of clearer illustration and 'so as to show the patient to a larger number of physicians;' or at that logic of the superior, which permits him to subject a subordinate to the dangers of a serious and, not infrequently, fatal disease, without so much as obtaining his consent thereto! I should very much like to know whether Professor Hubbenet would inoculate his own son with syphilis, even were he to acquiesce!"<sup>29</sup>

Professor von Hubbenet concludes his article with the following words: "I consider it necessary to remark that, having carried out a multitude of ineffective experiments on sick persons, I was perfectly convinced that, in the case of the healthy, I would meet with the same lack of success: this conviction alone made it possible for me to proceed with these dangerous experiments." Needless to say that a professor and specialist could not have been ignorant of Waller's successful inoculations. Besides, Prof. Hubbenet performed his first successful inoculation in 1852, while his last dates 1858. Are we to believe that in 1858 the professor proceeded with his inoculations full of the same "conviction"?

The publication of these observations, continues Hubbenet, "will perhaps restrain others, even with such a sceptical nature as my own, from making further experiments, often leading to the complete wrecking of the lives of the persons subjected to them. It would add considerably to my peace of mind in respect to the victims' fate, if these experiments were to spread the conviction that the secondary stage is contagious. If they lead to the establishing of such an important truth, the sufferings of a few individuals were not too high a price to be paid by

mankind for the attainment of such a truly beneficial and practical result."

If that is the case, it is hard to understand why Professor Hubbenet did not inoculate *himself* with syphilis. Perhaps, after all, such a price would have been too high to pay even "in the cause of humanity."

In 1858 the French Government applied to the Parisian Medical Academy for elucidation on the still contested question of the contagiousness of secondary syphilis. A commission was nominated and Dr. Gibert was appointed as its referee. Among other things, he stated that with a view to clear this question up, Dr. Auzias-Turenne had inoculated two adult patients suffering from lupus, and that both developed syphilis.

The referee himself inoculated two patients, also suffering from lupus, and in both cases he obtained syphilis.<sup>30</sup>

Gibert's report gave rise to stormy and lengthy debates in the Academy; Ricord, who had hitherto obstinately denied the contagiousness of secondary syphilis, notwithstanding overwhelming confirmatory evidence, entered the lists with great heat, but was compelled, in the end, to confess his mistake, and went over to the opposite camp.

Thus the most powerful and authoritative opponent of the new view taken by science was vanquished. But, nevertheless, experiments, now absolutely unwarrantable, went on and on. . . . In 1859 Guyenot inoculated T.B.B., a boy of ten, suffering from sores of the head, with the secretions of syphilitic plaques, and obtained syphilis.<sup>31</sup>

In the same year Professor Baerensprung successfully inoculated Bertha B., a girl of eighteen, with syphilitic pus. It was also he who inoculated the prostitute Marie G. with the secretions of hard chancre.<sup>32</sup>

Prof. Lindwurm, in 1860-1861, inoculated five women lying in his hospital, aged 18, 19, 30, 45 and 71 years respectively, with syphilis.

<sup>28</sup> *Bulletin de l'Académie Impériale de Médecine*, Tome xxiv. Paris. 1858-1859, pp. 888-890.

<sup>31</sup> "Nouveau fait d'inoculation d'accidents Syphil. Secondaires": *Gaz. hebdomad. de med. et de chirurgie*, 1859, No. 15. Guyenot was terribly punished for his experiment: the Tribunal of Correction of Lyons condemned him to a fine of one hundred francs!

<sup>32</sup> "Mitteilungen aus der Klinik für syphil. Kranke." *Annalen des Charité-Krankenhauses* Bd. IX. Heft I (1860), pp. 167, 168.

<sup>28</sup> Prof. H. v. Hubbenet, "Observations and Experiments in Syphilis": *The Medical-Military Journal*, Part 77 (1860), pp. 423-427.

<sup>29</sup> "Lectures on General Therapeutics," Part I. St. P. 1879, p. 66.

We quote the description of the last of these experiments: "Mary E., aged 71 years, suffering for many years from an extensive and deep ulcer in the forehead. Both *sinera frontalia*, thanks to the destruction of the front walls, are open; the bottom of the ulcer is covered deeply with granulations, through which the probe easily reaches the bone, and, in places, traverses the latter. . . . On May 27th, 1861, the blood of a syphilitic patient was injected subcutaneously between the shoulder blades." The patient developed syphilis.<sup>33</sup>

According to Zeissel, Dr. Rosnerom, acting under Prof. Hebra's directions, made the following experiments: "(1) The secretion of a flat condyloma, located on the breast of a certain wet-nurse, was inoculated upon a patient of 50, suffering from acute itch."—Syphilis. (2) A wet-nurse, suffering from innocuous syphilis, was inoculated in the forearm with chancrous pus. This woman, impregnated with syphilis, developed characteristic pustules. The pus of the latter was used to inoculate a certain leprous patient, who had not previously suffered from syphilis. . . . This inoculation also was successful."<sup>34</sup>

Dr. Puche inoculated a patient lying at the *Hôpital du Midi*, in the ventral regions, with the secretion of an indurated ulcer of a syphilitic patient, but without results. Three weeks later Puche inoculated his victim with the matter of another syphilitic. This time the experiment was crowned with success: the patient contracted syphilis.<sup>35</sup>

To settle the question once and for all whether a person who had once had syphilis could contract it again, Prof. Vidal de Cassi made the following experiments. "M., age 37." (Had been cured of syphilis, entered hospital with paralysis of the lower extremities, formerly employed in a tannery and afterwards as a watchman.) "The patient began to recover but wished to remain in hospital for a certain time longer, in expectation of a government post. In January, 1852, small blisters were applied to each thigh because of the inactivity of the bladder; when the skin was removed, the wounds were dressed with lint soaked in matter taken

from the mucous papules of another patient. This inoculation was barren of results. Later I proposed that the experiment should be repeated. On April 12th, 1852, when the patient began to complain of difficulty in breathing, blisters were applied to the upper parts of his arms; these were dressed on April 13th with lint saturated in the pus of the mucous papules of another patient. April 15th, the wounds on each arm had become covered with a greyish membrane, suppuration very copious and of disgusting odour; lint saturated with the same pus as previously was freshly applied to the wounds," etc.<sup>36</sup> Vidal was very dissatisfied with the squeamishness of those savants who did not venture upon such experiments. "Unfortunately," he remarks, "the cleverest of syphilologists, who could be of the greatest service to science thanks to their logic and clinical observations, regard experiment as immoral, and neglect it accordingly."<sup>37</sup>

\* \* \*

For the sake of ascertaining whether the milk of women suffering from syphilis was infectious. . . .

. . . Dr. R. Voss . . . inoculated three prostitutes, "having obtained their consent," with the milk of a syphilitic patient.

First experiment: P.A., aged thirteen, a peasant from the Province of Novgorod; had had syphilis, but was cured. On September 25th, 1875, the milk of a syphilitic patient was injected into her back. The only result was an abscess the size of a "small fist."

Second experiment: Natalie K., age fifteen, had taken up prostitution but recently. Admitted with urethritis and vaginitis. Milk of a syphilitic patient injected. No result.

Third experiment: Lubov U., age sixteen, a prostitute; admitted into hospital suffering from urethritis; never had syphilis. September 27th, a full Pravaz syringe of milk from a syphilitic patient injected beneath the left shoulder blade. *The girl developed syphilis.*<sup>38</sup>

Dr. Voss, as also Prof. Gay, assures us that his victims gave their consent to these ex-

<sup>33</sup> "Über die Verschiedenheit der syphilitischen Krankheiten": *Würzburger medicinische Zeitschrift* Bd. III., pp. 146-148, 174 (1862).

<sup>34</sup> Herrmann Zeissel, "Guide to the Study of General Syphilis." St. Petersburg, 1866, p. 29.

<sup>35</sup> Henry Lee, "Hunterian Lectures on Syphilis": *The Lancet* 1875, vol. ii, p. 122.

<sup>36</sup> Prof. A. Vidal, "On Venereal Disease" Transl. from the French, St. Petersburg, 1857, pp. 560-561.

<sup>37</sup> Prof. A. Vidal, "On Venereal Disease" Transl. from the French, St. Petersburg, 1857, p. 31.

<sup>38</sup> "Ist die Syphilis durch Milch übertragbar?" *St. Petersburg med. Wochenschrift*, 1876, No. 23. In the original all three girls were named in full.

periments. Is this mockery? The eldest of the girls was but sixteen years of age! Even if their consent had really been obtained, did these children know what they were agreeing to, could any importance have been attached to their acquiescence?

... In conclusion, I will only quote a few more experiments from other spheres of medicine. Although the latter are comparatively rarer (thanks to the possibility of experimenting upon animals), nevertheless their absolute number is more than sufficient.

While investigating the channels of human infection with worms, Professors Grassi and Calandruccio administered a pill, containing the germs of ascaris, to a boy of seven, who had not hitherto suffered from worms: in the course of three months the child evacuated 143 ascaris of lengths varying from 18 to 23 centimetres each.<sup>50</sup>

\* \* \*

In March, 1887, a woman, suffering from cancer of the mammary gland, applied to the surgeon, Eugene Hahn, of Berlin. The performance of an operation was impossible. "Not wishing to divulge before the patient the hopelessness of her condition by declining to operate upon her, and so as to relieve and reassure her by the psychical illusion of having performed the operation," Dr. Hahn removed a portion of the tumour of the patient's diseased breast and . . . transplanted it into the other healthy one; the inoculation was successful.<sup>55</sup>

\* \* \*

Dr. N. A. Finn studied the question of the infectiousness of typhoid fever in one of the military hospitals of the Caucasus. Following his instructions, assistant house-physician Artemovitch injected the blood of typhoid patients subcutaneously into the systems of seventeen healthy soldiers. Not one of those inoculated contracted the disease; "only ten of them developed ordinary abscesses at the places of puncture." In addition, twenty-eight young and healthy soldiers were placed by Dr. Finn in the same ward with typhoid patients. They lay in hospital in the vicinity of the sick, "for four or five days, the beds being moved close together,

and sometimes they were covered with the blankets of the typhoid patients."<sup>56</sup>

During December, 1887, Dr. Stickler read a paper before the Academy of Medicine of New York on preventive inoculation against scarlet fever. He had observed that persons who had contracted hoof and other kindred diseases from the lower animals became immune to scarlet fever. To verify his observations, Stickler inoculated children with the blood of sick horses and cows. After this he placed the children on bedding which had been in the use of scarlet fever patients and made them inhale the air exhaled by the latter; these children numbered twenty. Stickler also injected the blood of scarlet fever patients into the systems of the twenty children. Of their number several did not take the fever at all, the rest developed it in very mild form; there were no serious cases.<sup>57</sup>

Professor Roberts Bartholow of Ohio, U.S.A., attended a female patient, the posterior part of whose cerebrum had become exposed, owing to cancer of the cranial integuments. The professor took advantage of this rare case for the purpose of conducting a series of experiments of electric irritation of his patient's brain. Galvanic irritation of the *dura mater* proved to be painless, while faradic current caused muscular contraction throughout the opposite side of the body. After this he "passed an isolated needle into the left posterior lobe; the other isolated needle was placed in contact with the *dura mater*. When the circuit was closed, muscular contractions of the right upper and lower extremities ensued; faint but visible contractions of the left orbicularis palpebrarum and dilation of the pupils also ensued. Notwithstanding the very evident pain from which she suffered, she smiled as if amused."

The same experiment was repeated upon the right cerebral hemisphere. "When the needle entered the brain substance she complained of acute pain in the neck. In order to develop more decided reactions, the strength of the current was increased. When communication was made

<sup>50</sup> Prof. B. Grassi, "Trichocephalus u. Ascaris-entwicklung": *Centralbl. für Bacteriol. u. Paras.*, 1887 Bd. I., p. 131.

<sup>55</sup> E. Hahn, "Über Transplantation der carcin. Haut." *Berlin. klin. Woch.* 1888 No. 21.

<sup>56</sup> The minutes of the meetings of the *Imperial Caucasian Medical Society* for 1878-1879, No. 8, p. 107. Drs. Finn and Artemovitch also injected the blood of typhoid patients into their own systems.

<sup>57</sup> Summary of Stickler's paper, as it appeared in one of the American medical journals. The *Centralblatt für Bacteriologie u. Parasitenkunde*, Bd. IV., 1888, p. 369, remarks: "The results obtained are, in any case, sufficiently important to encourage further research in the same direction."

with the needles, her countenance exhibited great distress, and she began to cry. Her eyes became fixed, with pupils widely dilated, lips blue, and she frothed at the mouth. She lost consciousness and was violently convulsed on the left side. The convulsion lasted five minutes, and was succeeded by coma. She returned to consciousness in twenty minutes from the beginning of the attack." After the lapse of a certain time the experiment was repeated once more with a weaker current, and three days later her condition was decidedly worse. In the evening she "had a convulsive seizure, lasting about five minutes. After this attack she relapsed into profound unconsciousness and was found to be completely paralysed on the right side."

The unfortunate woman died soon afterwards. According to Professor Bartholow's opinion her death was caused by the original disease.<sup>58</sup>

... The existence of a few hundred doctors, to whom the sick are merely so many objects for experiment, does not justify the branding of the entire profession. As a parallel, I might bring forward a no less array of facts, which would show that, in the past, doctors have conducted—and continue to do so now—no less dangerous experiments *upon their own persons*. Thus, Pettenkoffer's and Emerich's experiments are still fresh in the memory of all: both swallowed

pure cultures of cholera bacilli, after having had the acids of the stomach neutralised with soda. This was repeated by Professor Metschnikoff, Drs. Hasterlick and Latapie. Drs. Borgioni, Warneri, and Lindemann, and many others, inoculated themselves with syphilis; young and healthy, in the name of Science, they faced experiments which crippled and ruined their entire lives. To conclude that the entire medical body is made up of heroes, because a few devoted men martyred themselves in the name of Science, was as erroneous as to write all doctors down brutes, callous of their patients' interest, in consequence of the comparative few having conducted criminal experiments as described. But the latter establish one thing beyond all vestige of a doubt—and that is the shameful indifference with which the medical world contemplates such atrocities. For this martyrology of the unhappy patients offered up as victims to science was not compiled by any underhand means—the culprits publicly blazoned their own infamy in black and white. One would suppose that the mere fact of publication of such experiments would make their repetition utterly impossible, the first to attempt anything of the kind being cast forever from the medical corporation! But, unfortunately, this is not so. With heads proudly erect, these bizarre disciples of science proceed upon their way without encountering any effective opposition, either from their colleagues or the medical press. . . .

\* \* \*

[T]he moment has also arrived for society to take its own measures of self-protection against those zealots of science who have ceased to distinguish between their brothers and guinea-pigs, without waiting for the faculty to emerge from its lethargy.

## NOTES

### NOTE 1.

R. J. V. PULVERTAFT  
THE INDIVIDUAL AND THE GROUP  
IN MODERN MEDICINE\*

\* \* \*

... It is in fact remarkable how irresponsibly attempts were made at the end of the last century to produce venereal disease experimentally. Urethritis was produced with pure cul-

<sup>58</sup> See *British Medical Journal*, 1874, vol. i. p. 687. In reviewing the above communication which appeared in an American contemporary, the *British Medical Journal* censured the author for his experiments. Bartholow wrote a letter to the editor, in which he sought to vindicate his action by remarking that his patient was bound to die very soon and that she had agreed to the experiments, which, according to his opinion, presented no danger. "Notwithstanding my sanguine expectation that small isolated needle electrodes could be introduced without injury into the cerebral substance," wrote the professor, "I now know that I was mistaken. To repeat such experiments with the knowledge we now have that injury will be done by them would be in the highest degree criminal. I can only now express my regret that facts which I hoped would further, in some slight degree, the progress of knowledge were obtained at the expense of some injury to the patient." According to the journal's opinion, this letter was "one which is likely to disarm further criticism," and the editor found it both sincere and worthy of the author's profession, and even . . . humane (p. 728). All this was said without a trace of irony. On the whole, however, Bartholow's experiments aroused the indignation of the entire medical press.

\* 2 *The Lancet* 841 (1952). Reprinted by permission.



tures of the gonococcus in both men and women by at least five separate workers. It is hard to believe that they were not aware of the implications both to the human beings infected and to their children. . . .

. . . I once met a lawyer who held office during the prosecution of Germans for war crimes, and asked him how a nation so renowned for its humanitarian services could have perpetrated such horrors. "There is only one step to take," he answered. "You may not think it possible to take it; but I assure you that men I thought decent men did take it. You have only to decide that one group of human beings have lost human rights."

\* \* \*

#### NOTE 2.

##### MYRON PRINZMETAL ON THE HUMANE TREATMENT OF CHARITY PATIENTS\*

During my training in medical school, as well as during my residency in St. Louis, fellowships at Harvard, New York City, and London, and visits to Vienna, Budapest, and other European medical centers, I was always horrified by the inhumane manner in which charity patients were treated. Occasionally, private patients were similarly abused.

I distinctly remember the degradation of a poor man with a prolapsed rectum who was asked to defecate in a wastebasket before the class in the proctology clinic, which included women medical students. I remember the Ne-

groes—two in a bed—called only by their first names. I remember a poor young woman at the San Francisco County Hospital who was stripped from neck to knees in front of the entire class. Cringing with shame and embarrassment, she closed her eyes.

The most detailed histories, including terms such as cancer, cardiac murmurs, leukemia, were on occasions given while the patient was in the room. X-rays demonstrating cancers, enlarged hearts, and other serious diseases were described in great detail. This was followed by long discussions and arguments among the doctors, all within hearing distance of the terrified patient.

The doctors and medical students would then consult, palpate, listen to the heart, argue over electrocardiograms, before the patient was rolled out.

\* \* \*

New drugs, new experimental surgical procedures were commonly tested on charity patients, who rarely understood what was being done to them. Often they were maimed or killed—experimental "animals" sacrificed in the interests of medical progress. In former years, American doctors would actually pay the authorities in certain European capitals for the privilege of doing completely unnecessary operations on peasants.

In a word—some patients were not treated like animals, but often worse than animals—for some of them understood.

\* \* \*

## B.

### United States v. Karl Brandt†

#### 1.

##### Indictment

The United States of America, by the undersigned Telford Taylor, Chief of Counsel for War Crimes, duly appointed to represent said Government in the prosecution of war criminals, charges that the defendants herein participated

in a common design or conspiracy to commit and did commit war crimes and crimes against humanity, as defined in Control Council Law No. 10, duly enacted by the Allied Control Council on 20 December 1945. . . .

\* \* \*

Count Two [and Three]—War Crimes [and Crimes against Humanity]

Between September 1939 and April 1945 all of the defendants herein unlawfully, willfully, and knowingly committed war crimes [and crimes against humanity], as defined by Article II of Control Council Law No. 10, in that they

\* 6 *Medical Tribune* 15 (September 22, 1965). Reprinted by permission.

† *Trials of War Criminals Before the Nuremberg Military Tribunals. Volumes I and II, The Medical Case.* Washington, D.C.: U.S. Government Printing Office (1948).



were principals in, accessories to, ordered, abetted, took a consenting part in, and were connected with plans and enterprises involving medical experiments without the subjects' consent, upon [German civilians and] civilians and members of the armed forces of nations then at war with the German Reich . . . in the course of which experiments the defendants committed murders, brutalities, cruelties, tortures, atrocities, and other inhuman acts. Such experiments included, but were not limited to the following:

*High-Altitude Experiments.* From about March 1942 to about August 1942 experiments were conducted at the Dachau concentration camp, for the benefit of the German Air Force, to investigate the limits of human endurance and existence at extremely high altitudes. The experiments were carried out in a low-pressure chamber in which the atmospheric conditions and pressures prevailing at high altitude (up to 68,000 feet) could be duplicated. The experimental subjects were placed in the low-pressure chamber and thereafter the simulated altitude therein was raised. Many victims died as a result of these experiments and others suffered grave injury, torture, and ill-treatment. . . .

*Freezing Experiments.* From about August 1942 to about May 1943 experiments were conducted at the Dachau concentration camp, primarily for the benefit of the German Air Force, to investigate the most effective means of treating persons who had been severely chilled or frozen. In one series of experiments the subjects were forced to remain in a tank of ice water for periods up to 3 hours. Extreme rigor developed in a short time. Numerous victims died in the course of these experiments. After the survivors were severely chilled, rewarming was attempted by various means. In another series of experiments, the subjects were kept naked outdoors for many hours at temperatures below freezing. The victims screamed with pain as parts of their bodies froze. . . .

*Malaria Experiments.* From about February 1942 to about April 1945 experiments were conducted at the Dachau concentration camp in order to investigate immunization for and treatment of malaria. Healthy concentration-camp inmates were infected by mosquitoes or by injections of extracts of the mucous glands of mosquitoes. After having contracted malaria the subjects were treated with various drugs to test their relative efficacy. Over 1,000 involuntary subjects were used in these experiments. Many of the victims died and others suffered severe pain and permanent disability. . . .

\* \* \*

*Sulfanilamide Experiments.* From about July 1942 to about September 1943 experiments to investigate the effectiveness of sulfanilamide were conducted at the Ravensbrueck concentration camp for the benefit of the German Armed Forces. Wounds deliberately inflicted on the experimental subjects were infected with bacteria such as streptococcus, gas gangrene, and tetanus. Circulation of blood was interrupted by tying off blood vessels at both ends of the wound to create a condition similar to that of a battlefield wound. Infection was aggravated by forcing wood shavings and ground glass into the wounds. The infection was treated with sulfanilamide and other drugs to determine their effectiveness. Some subjects died as a result of these experiments and others suffered serious injury and intense agony. . . .

\* \* \*

*Epidemic Jaundice Experiments.* From about June 1943 to about January 1945 experiments were conducted at the Sachsenhausen and Natzweiler concentration camps, for the benefit of the German Armed Forces, to investigate the causes of, and inoculations against, epidemic jaundice. Experimental subjects were deliberately infected with epidemic jaundice, some of whom died as a result, and others were caused great pain and suffering. . . .

\* \* \*

*Spotted Fever [Typhus] Experiments.* From about December 1941 to about February 1945 experiments were conducted at the Buchenwald and Natzweiler concentration camps, for the benefit of the German Armed Forces, to investigate the effectiveness of spotted fever and other vaccines. At Buchenwald numerous healthy inmates were deliberately infected with spotted fever virus in order to keep the virus alive; over 90 percent of the victims died as a result. Other healthy inmates were used to determine the effectiveness of different spotted fever vaccines and of various chemical substances. In the course of these experiments 75 percent of the selected number of inmates were vaccinated with one of the vaccines or nourished with one of the chemical substances and, after a period of 3 to 4 weeks, were infected with spotted fever germs. The remaining 25 percent were infected without any previous protection in order to compare the effectiveness of the vaccines and the chemical substances. As a result, hundreds of the persons experimented upon died. . . .

*Experiments with Poison.* In or about December 1943, and in or about October 1944, experiments were conducted at the Buchenwald concentration camp to investigate the effect of various poisons upon human beings. The poisons were secretly administered to experimental subjects in their food. The victims died as a result of the poison or were killed immediately in order to permit autopsies. In or about September 1944 experimental subjects were shot with poison bullets and suffered torture and death. . . .

\* \* \*

Between June 1943 and September 1944 the defendants Rudolf Brandt and Sievers . . . were principals in, accessories to, ordered, abetted, took a consenting part in, and were connected with plans and enterprises involving the murder of civilians and members of the armed forces of nations then at war with the German Reich and who were in the custody of the German Reich in exercise of belligerent control. One hundred twelve Jews were selected for the purpose of completing a skeleton collection for the Reich University of Strasbourg. Their photographs and anthropological measurements were taken. Then they were killed. Thereafter, comparison tests, anatomical research, studies regarding race, pathological features of the body, form and size of the brain, and other tests, were made. The bodies were sent to Strasbourg and defleshed.

\* \* \*

The said war crimes [and crimes against humanity] constitute violations of international conventions, particularly . . . of the Hague Regulations, 1907 . . . the laws and customs of war, the general principles of criminal law as derived from the criminal laws of all civilized nations, the internal penal laws of the countries in which such crimes were committed. . . .

\* \* \*

## 2.

### Opening Statement of the Prosecution by Brigadier General Telford Taylor

The defendants in this case are charged with murders, tortures, and other atrocities committed in the name of medical science. The victims of these crimes are numbered in the hundreds of thousands. A handful only are still alive;

a few of the survivors will appear in this courtroom. But most of these miserable victims were slaughtered outright or died in the course of the tortures to which they were subjected.

For the most part they are nameless dead. To their murderers, these wretched people were not individuals at all. They came in wholesale lots and were treated worse than animals. They were 200 Jews in good physical condition, 50 gypsies, 500 tubercular Poles, or 1,000 Russians. . . .

The charges against these defendants are brought in the name of the United States of America. They are being tried by a court of American judges. The responsibilities thus imposed upon the representatives of the United States, prosecutors and judges alike, are grave and unusual. It is owed, not only to the victims and to the parents and children of the victims, that just punishment be imposed on the guilty, but also to the defendants that they be accorded a fair hearing and decision. Such responsibilities are the ordinary burden of any tribunal. Far wider are the duties which we must fulfill here.

These larger obligations run to the peoples and races on whom the scourge of these crimes was laid. The mere punishment of the defendants, or even of thousands of others equally guilty, can never redress the terrible injuries which the Nazis visited on these unfortunate peoples. For them it is far more important that these incredible events be established by clear and public proof, so that no one can ever doubt that they were fact and not fable; and that this Court, as the agent of the United States and as the voice of humanity, stamp these acts, and the ideas which engendered them, as barbarous and criminal.

We have still other responsibilities here. The defendants in the dock are charged with murder, but this is no mere murder trial. We cannot rest content when we have shown that crimes were committed and that certain persons committed them. To kill, to maim, and to torture is criminal under all modern systems of law. These defendants did not kill in hot blood, nor for personal enrichment. Some of them may be sadists who killed and tortured for sport, but they are not all perverts. They are not ignorant men. Most of them are trained physicians and some of them are distinguished scientists. Yet these defendants, all of whom were fully able to comprehend the nature of their acts, and most of whom were exceptionally qualified to form a moral and professional judgment in this respect,

are responsible for wholesale murder and unspeakably cruel tortures.

It is our deep obligation to all peoples of the world to show why and how these things happened. It is incumbent upon us to set forth with conspicuous clarity the ideas and motives which moved these defendants to treat their fellow men as less than beasts. The perverse thoughts and distorted concepts which brought about these savageries are not dead. They cannot be killed by force of arms. They must not become a spreading cancer in the breast of humanity. They must be cut out and exposed, for the reason so well stated by Mr. Justice Jackson in this courtroom a year ago—

The wrongs which we seek to condemn and punish have been so calculated, so malignant, and so devastating, that civilization cannot tolerate their being ignored because it cannot survive their being repeated.

\* \* \*

I pass now to the facts of the case in hand. There are 23 defendants in the box. All but three of them—Rudolf Brandt, Sievers, and Brack—are doctors. Of the 20 doctors, all but one—Pokorny—held positions in the medical services of the Third Reich. . . .

\* \* \*

I turn now to the main part of the indictment and will outline at this point the prosecution's case relating to those crimes alleged to have been committed in the name of medical or scientific research. . . . What I will cover now comprehends all in the experiments charged as war crimes . . . and as crimes against humanity in . . . the indictment. . . .

\* \* \*

A sort of rough pattern is apparent on the face of the indictment. Experiments concerning high altitude, the effect of cold, and the potability of processed sea water have an obvious relation to aeronautical and naval combat and rescue problems. The mustard gas and phosphorous burn experiments, as well as those relating to the healing value of sulfanilamide for wounds, can be related to air-raid and battlefield medical problems. It is well known that malaria, epidemic jaundice, and typhus were among the principal diseases which had to be combated by the German Armed Forces and by German authorities in occupied territories.

To some degree, the therapeutic pattern

outlined above is undoubtedly a valid one, and explains why the Wehrmacht, and especially the German Air Force, participated in these experiments. Fanatically bent upon conquest, utterly ruthless as to the means or instruments to be used in achieving victory, and callous to the sufferings of people whom they regarded as inferior, the German militarists were willing to gather whatever scientific fruit these experiments might yield.

But our proof will show that a quite different and even more sinister objective runs like a red thread through these hideous researches. We will show that in some instances the true object of these experiments was not how to rescue or to cure, but how to destroy and kill. The sterilization experiments were, it is clear, purely destructive in purpose. The prisoners at Buchenwald who were shot with poisoned bullets were not guinea pigs to test an antidote for the poison; their murderers really wanted to know how quickly the poison would kill. This destructive objective is not superficially as apparent in the other experiments, but we will show that it was often there.

Mankind has not heretofore felt the need of a word to denominate the science of how to kill prisoners most rapidly and subjugated people in large numbers. This case and these defendants have created this gruesome question for the lexicographer. For the moment we will christen this macabre science "thanatology," the science of producing death. The thanatological knowledge, derived in part from these experiments, supplied the techniques for genocide, a policy of the Third Reich, exemplified in the "euthanasia" program and in the widespread slaughter of Jews, gypsies, Poles, and Russians. This policy of mass extermination could not have been so effectively carried out without the active participation of German medical scientists.

\* \* \*

The experiments known as "high-altitude" or "low-pressure" experiments were carried out at the Dachau concentration camp in 1942. According to the proof, the original proposal that such experiments be carried out on human beings originated in the spring of 1941 with a Dr. Sigmund Rascher. Rascher was at that time a captain in the medical service of the German Air Force, and also held officer rank in the SS. He is believed now to be dead.

The origin of the idea is revealed in a letter which Rascher wrote to Himmler in May 1941

at which time Rascher was taking a course in aviation medicine at a German Air Force headquarters in Munich. According to the letter, this course included researches into high-altitude flying and

considerable regret was expressed at the fact that no tests with human material had yet been possible for us, as such experiments are very dangerous and nobody volunteers for them. (1602-PS.)

Rascher, in this letter, went on to ask Himmler to put human subjects at his disposal and baldly stated that the experiments might result in death to the subjects but that the tests theretofore made with monkeys had not been satisfactory.

Rascher's letter was answered by Himmler's adjutant, the defendant, Rudolf Brandt, who informed Rascher that—" . . . Prisoners will, of course, gladly be made available for the high-flight researches."

. . . The tests themselves were carried out in the spring and summer of 1942, using the pressure chamber which the German Air Force had provided. The victims were locked in the low-pressure chamber, which was an airtight ball-like compartment, and then the pressure in the chamber was altered to simulate the atmospheric conditions prevailing at extremely high altitudes. The pressure in the chamber could be varied with great rapidity, which permitted the defendants to duplicate the atmospheric conditions which an aviator might encounter in falling great distances through space without a parachute and without oxygen.

. . . The first report by Rascher was made in April 1942, and contains a description of the effect of the low-pressure chamber on a 37-year-old Jew. (1971-A-PS.) I quote:

The third experiment of this type took such an extraordinary course that I called an SS physician of the camp as witness, since I had worked on these experiments all by myself. It was a continuous experiment without oxygen at a height of 12 kilometers conducted on a 37-year-old Jew in good general condition. Breathing continued up to 30 minutes. After 4 minutes the experimental subject began to perspire and wiggle his head, after 5 minutes cramps occurred, between 6 and 10 minutes breathing increased in speed and the experimental subject became unconscious; from 11 to 30 minutes breathing slowed down to three breaths per minute, finally stopping altogether.

Severest cyanosis developed in between and foam appeared at the mouth.

At 5 minute intervals electrocardiograms from three leads were written. After breathing

had stopped Ekg (electrocardiogram) was continuously written until the action of the heart had come to a complete standstill. About ½ hour after breathing had stopped, dissection was started.

\* \* \*

Another series of experiments carried out at the Dachau concentration camp concerned immunization for and treatment of malaria. Over 1,200 inmates of practically every nationality were experimented upon. Many persons who participated in these experiments have already been tried before a general military court held at Dachau, and the findings of that court will be laid before this Tribunal. The malaria experiments were carried out under the general supervision of a Dr. Schilling, with whom the defendant Sievers and others in the box collaborated. The evidence will show that healthy persons were infected by mosquitoes or by injections from the glands of mosquitoes. Catholic priests were among the subjects. The defendant Gebhardt kept Himmler informed of the progress of these experiments. Rose furnished Schilling with fly eggs for them, and others of the defendants participated in various ways which the evidence will demonstrate.

After the victims had been infected they were variously treated with quinine, neosalvarsan, pyramidon, antipyrin, and several combinations of these drugs. Many deaths occurred from excessive doses of neosalvarsan and pyramidon. According to the findings of the Dachau court, malaria was the direct cause of 30 deaths and 300 to 400 others died as the result of subsequent complications.

\* \* \*

From December 1941, until near the end of the war, a large program of medical experimentation was carried out upon concentration camp inmates at Buchenwald and Natzweiler to investigate the value of various vaccines. This research involved a variety of diseases—typhus, yellow fever, smallpox, paratyphoid A and B, cholera, and diphtheria. . . .

\* \* \*

The general pattern of these typhus experiments was as follows. A group of concentration camp inmates, selected from the healthier ones who had some resistance to disease, were injected with an anti-typhus vaccine, the efficacy of which was to be tested. Thereafter, all the persons in the group would be infected with typhus. At the same time, other inmates who

had not been vaccinated were also infected for purposes of comparison—these unvaccinated victims were called the “control” group. But perhaps the most wicked and murderous circumstance in this whole case is that still other inmates were deliberately infected with typhus with the sole purpose of keeping the typhus virus alive and generally available in the bloodstream of the inmates.

\* \* \*

The 20 physicians in the dock range from leaders of German scientific medicine, with excellent international reputations, down to the dregs of the German medical profession. All of them have in common a callous lack of consideration and human regard for, and an unprincipled willingness to abuse their power over the poor, unfortunate, defenseless creatures who had been deprived of their rights by a ruthless and criminal government. All of them violated the Hippocratic commandments which they had solemnly sworn to uphold and abide by, including the fundamental principles never to do harm —“*primum non nocere*.”

Outstanding men of science, distinguished for their scientific ability in Germany and abroad, are the defendants Rostock and Rose. Both exemplify, in their training and practice alike, the highest traditions of German medicine. Rostock headed the Department of Surgery at the University of Berlin and served as dean of its medical school. Rose studied under the famous surgeon, Enderlen, at Heidelberg and then became a distinguished specialist in the fields of public health and tropical diseases. Handloser and Schroeder are outstanding medical administrators. Both of them made their careers in military medicine and reached the peak of their profession. Five more defendants are much younger men who are nevertheless already known as the possessors of considerable scientific ability, or capacity in medical administration. These include the defendants Karl Brandt, Ruff, Beiglboeck, Schaefer, and Becker-Freyseng.

A number of the others such as Romberg and Fischer are well trained, and several of them attained high professional position. But among the remainder few were known as outstanding scientific men. Among them at the foot of the list is Blome who has published his autobiography entitled “Embattled Doctor” in which he sets forth that he eventually decided to become a doctor because a medical career would enable him to become “master over life and death.”

\* \* \*

I intend to pass very briefly over matters of medical ethics, such as the conditions under which a physician may lawfully perform a medical experiment upon a person who has voluntarily subjected himself to it, or whether experiments may lawfully be performed upon criminals who have been condemned to death. This case does not present such problems. No refined questions confront us here.

None of the victims of the atrocities perpetrated by these defendants were volunteers, and this is true regardless of what these unfortunate people may have said or signed before their tortures began. Most of the victims had not been condemned to death, and those who had been were not criminals, unless it be a crime to be a Jew, or a Pole, or a gypsy, or a Russian prisoner of war.

\* \* \*

Were it necessary, one could make a long list of the respects in which the experiments which these defendants performed departed from every known standard of medical ethics. But the gulf between these atrocities and serious research in the healing art is so patent that such a tabulation would be cynical.

\* \* \*

These experiments revealed nothing which civilized medicine can use. It was, indeed, ascertained that phenol or gasoline injected intravenously will kill a man inexpensively and within 60 seconds. This and a few other “advances” are all in the field of thanatology. . . .

Apart from these deadly fruits, the experiments were not only criminal but a scientific failure. It is indeed as if a just deity had shrouded the solutions which they attempted to reach with murderous means. The moral shortcomings of the defendants and the precipitous ease with which they decided to commit murder in quest of “scientific results,” dulled also that scientific hesitancy, that thorough thinking-through, that responsible weighing of every single step which alone can insure scientifically valid results. Even if they had merely been forced to pay as little as two dollars for human experimental subjects, such as American investigators may have to pay for a cat, they might have thought twice before wasting unnecessary numbers, and thought of simpler and better ways to solve their problems. The fact that these investigators had free and unrestricted access to human beings to be experimented upon misled

them to the dangerous and fallacious conclusion that the results would thus be better and more quickly obtainable than if they had gone through the labor of preparation, thinking, and meticulous preinvestigation.

A particularly striking example is the sea-water experiment. I believe that three of the accused . . . will today admit that this problem could have been solved simply and definitively within the space of one afternoon. On 20 May 1944 when these accused convened to discuss the problem, a thinking chemist could have solved it right in the presence of the assembly within the space of a few hours by the use of nothing more gruesome than a piece of jelly, a semipermeable membrane and a salt solution, and the German Armed Forces would have had the answer on 21 May 1944. But what happened instead? The vast armies of the disenfranchised slaves were at the beck and call of this sinister assembly; and instead of thinking, they simply relied on their power over human beings rendered rightless by a criminal state and government. . . .

\* \* \*

. . . Who could German medicine look to to keep the profession true to its traditions and protect it from the ravaging inroads of Nazi pseudo-science? This was the supreme responsibility of the leaders of German medicine—men like Rostock and Rose and Schroeder and Handloser. That is why their guilt is greater than that of any of the other defendants in the dock. They are the men who utterly failed their country and their profession, who showed neither courage nor wisdom nor the vestiges of moral character. . . .

\* \* \*

### 3.

#### Extracts from Argumentation and Evidence of Prosecution and Defense

a.

##### Testimony of Defense Expert Witness Dr. Franz Vollhardt

Direct Examination.

\* \* \*

DR. MARX: Please, would you briefly tell the Tribunal what your scientific activities have

been and in what special field you have taken a particularly great interest, and since when?

WITNESS VOLLHARDT: I am Professor of Internal Medicine at Frankfurt and predominantly I have dealt with the questions of circulation, metabolism, blood pressure, and kidney diseases.

\* \* \*

Q: Which foreign academies and foreign societies have you been a member of? . . .

A: I am Honorary Doctor of the Sorbonne, Paris, of Goettingen and Freiburg; and, as far as societies are concerned, there are a lot of them, Medical Society at Edinburgh, at Geneva, at Luxembourg. I am an Honorary Member of the University at Santiago, and so on and so forth.

\* \* \*

Q: Now, Professor, have you sufficient insight into the planning and carrying out of the so-called sea-water experiments to give an expert opinion on that subject?

\* \* \*

A: I think that scientifically speaking the planning was excellent and I have no objection to the entire plan. It was good to add a hunger-and-thirst group because we know by experience that thirst can be borne less well than hunger, and if people are suffering from hunger and thirst too, they do not suffer from hunger, but do suffer from thirst; and that resembles what shipwrecked persons would be subjected to because they only suffer from thirst. It was excellent that Wofatit was to be introduced into the experiments too, although it was expected from the beginning that this wonderful discovery would show its value. . . .

\* \* \*

Q: Could the aim of these experiments have been achieved with a semipermeable membrane?

A: I don't understand how one can imagine this. What we are concerned with is the question of how long the human body can survive without water and under the excess quantity of salt. Now, that is subject to the water content of the body and it depends first of all, upon whether water is only used by the intermediary tissues or whether the cell liquid too is being used up. In the latter case, there is a danger which becomes apparent through excess potassium quantities, and this was also continuously observed and checked during such experiments,

and there were no excess potassium quantities such as can be expected after 6 days.

Q: Nor would it be right to say that these experiments were not planned scientifically and medically, is that correct?

A: Absolutely not.

Q: Could they have been planned differently?

A: I couldn't imagine how.

Q: Were these experiments in the interests of active warfare, or in the interests of the care of shipwrecked sailors or soldiers?

A: The latter.

Q: In other words, for aviators and sailors who were shipwrecked or might be shipwrecked?

A: Towards the end of the war there was an increase in the number of pilots shot down as well as of shipwrecked personnel, and it was, therefore, the duty of the hygiene department concerned to consider the question of how one could best deal with such cases of shipwrecked personnel. . . .

\* \* \*

Q: Now, Professor, the experiments we were talking about; did they have a practical valuable aim and did they show a corresponding result?

A: Yes, that is correct. For instance an important observation was made which Eppinger had expected; he wanted to see if the kidneys did concentrate salt under such extreme conditions to an even higher extent than one expected previously. One thought that it would be something like 2.0 percent but 2.6 or 2.7 percent and record figures of 3.0, 3.5, 3.6, and 4 percent are shown, so that the fortunate man who is in a position to concentrate 3.6 percent or 4 percent of salt would be able to live on sea water for quite a long period.

\* \* \*

Finally, one unsuspected fact was shown which may be connected with this, and that is that the drinking of small quantities of sea water up to 500 cc. given over a lengthy period turned out to be better than unalleviated thirst.

\* \* \*

Q: So, you think that the result of these experiments is not only of importance in wartime, but is also of importance for the problems of seafaring nations?

A: Quite right, it is a wonderful thing for all seafaring nations.

\* \* \*

b.

### **Final Plea for Defendant Joachim Mrugowsky**

\* \* \*

The case with the typhus experiments is different. No order was given to kill a man in order to obtain knowledge. But the typhus experiments were dangerous experiments. Out of 724 experimental persons, 154 died. But these 154 deaths from the typhus experiments have to be compared with the 15,000 who died of typhus every day in the camps for Soviet prisoners of war, and the innumerable deaths from typhus among the civilian population of the occupied eastern territories and the German troops. This enormous number of deaths led to the absolute necessity of having effective vaccines against typhus in sufficient quantity. The newly developed vaccines had been tested in the animal experiments as to their compatibility.

\* \* \*

The Tribunal will have to decide whether, in view of the enormous extent of epidemic typhus, in view of the 15,000 deaths it was causing daily in the camps for Russian prisoners of war alone, the order given by the government authorities to test the typhus vaccines was justified or not. If the answer is in the affirmative, then the typhus experiments at Buchenwald were not criminal, since the prosecution did not contest that they were carried out according to the rules of medical science.

\* \* \*

c.

### **Testimony of Defendant Gerhard Rose**

Direct Examination.

\* \* \*

DR. FRITZ: What do you know about the reasons for this protest (against experiments) being ignored and the typhus experiments being carried out in spite of it?

\* \* \*

DEFENDANT ROSE: The Buchenwald experiments (with typhus vaccine) had four main results. First of all, they showed that belief in the protective effect of Weigl vaccine was a mistake, although this belief seemed to be based on long observation. Secondly, they showed that the useful vaccines did not protect against infection,



but almost certainly prevented death, under the conditions of the Buchenwald experiments. Thirdly, they showed that the objections of the biological experts to the vitelline membrane vaccines and to the lice vaccines were unjustified, and that vitelline membrane, rabbit lungs, and lice intestines were of equal value. We learned this only through the Buchenwald experiments. This left the way open to mass production of typhus vaccines.

The Buchenwald experiments showed in time that several vaccines were useless: First, the process according to Otto and Wohlrab, the process according to Cox, the process of Rickettsia Prowazeki and Rickettsia murina, that is, vaccine from egg cultures; secondly, the vaccines of the Behring works which were produced according to the Otto process, but with other concentrations; finally the Ipsen vaccines from mouse liver. The vaccines of the Behring works were in actual use at that time in thousands of doses. They always represented a danger to health. Without these experiments the vaccines, which were recognized as useless, would have been produced in large quantities because they all had one thing in common: their technical production was much simpler and cheaper than that of the useful vaccines. In any case, one thing is certain, that the victims of this Buchenwald typhus test did not suffer in vain and did not die in vain. There was only one choice, the sacrifice of human lives, of persons determined for that purpose, or to let things run their course, to endanger the lives of innumerable human beings who would be selected not by the Reich Criminal Police Office but by blind fate.

\* \* \*

d.

#### **Testimony of Prosecution Expert Witness Dr. Andrew C. Ivy**

Direct Examination.

\* \* \*

MR. HARDY: It is your opinion, then, that the state cannot assume the moral responsibility of a physician to his patient or experimental subject?

WITNESS DR. IVY: That is my opinion.

Q: On what do you base your opinion? What is the reason for that opinion?

A: I base that opinion on the principles of ethics and morals contained in the oath of Hippocrates. I think it should be obvious that a

state cannot follow a physician around in his daily administration to see that the moral responsibility inherent therein is properly carried out. This moral responsibility that controls or should control the conduct of a physician should be inculcated into the minds of physicians just as moral responsibility of other sorts, and those principles are clearly depicted or enunciated in the oath of Hippocrates with which every physician should be acquainted.

Q: Is the oath of Hippocrates the Golden Rule in the United States and to your knowledge throughout the world?

A: According to my knowledge it represents the Golden Rule of the medical profession. It states how one doctor would like to be treated by another doctor in case he were ill. And in that way how a doctor should treat his patients or experimental subjects. He should treat them as though he were serving as a subject.

Q: Several of the defendants have pointed out in this case that the oath of Hippocrates is obsolete today. Do you follow that opinion?

A: I do not. The moral imperative of the oath of Hippocrates I believe is necessary for the survival of the scientific and technical philosophy of medicine.

\* \* \*

e.

#### **Closing Brief for Defendant Siegfried Ruff**

\* \* \*

Experiments which time and again have been described in international literature without meeting any opposition do not constitute a crime from the medical point of view. For nowhere did a plaintiff arise from the side of the responsible professional organization, or from that of the administration of justice, to denounce as criminal the experiments described in literature. On the contrary, the authors of those reports on their human experiments gained general recognition and fame; they were awarded the highest honors; they gained historical importance. And in spite of all this, are they supposed to have been criminals? No! In view of the complete lack of written legal norms, the physician, who generally knows only little about the law, has to rely on and refer to the admissibility of what is generally recognized to be admissible all over the world.

The defense is convinced that the Tribunal, when deciding this problem without prejudice,



will first study the many experiments performed all over the world on healthy and sick persons, on prisoners and free people, on criminals and on the poor, even on children and mentally ill persons, in order to see how the medical profession in its international totality answers the question of the admissibility of human experiments, not only in theory but also in practice.

It is psychologically understandable that German research workers today will, if possible, have nothing to do with human experiments and will try to avoid them, or would like to describe them as inadmissible even if before 1933 they were perhaps of the opposite opinion. However, experiments performed in 1905-1912 by a highly respected American in Asia for the fight against the plague, which made him famous all over the world, cannot and ought not to be labelled as criminal because a Blome is supposed to have performed the same experiments during the Hitler period (which, in fact, however, were not performed at all); and experiments for which, before 1933, a foreign research worker, the Englishman Ross, was awarded the Nobel Prize for his malaria experiments, do not deserve to be condemned only because a German physician performed similar experiments during the Hitler regime. . . .

\* \* \*

f. **Testimony of Prosecution Expert Witness  
Dr. Andrew C. Ivy**

\* \* \*

**Cross-Examination.**

DR. SAUTER: Witness, you spoke yesterday of a number of experiments carried out in the United States and in other countries outside of Germany. For example, pellagra, swamp fever, beri-beri, plague, etc. Now, I should like to have a very clear answer from you to the following question. In these experiments which you heard of partly from persons involved in them and partly from international literature, did deaths occur during the experiments and as a result of the experiments or not? Professor, I ask you this question because you said yesterday that you examined all international literature concerning this question and, therefore, have a certain specialized knowledge on this question.

WITNESS DR. IVY: I also said that when one reviews the literature, he cannot be sure that he has done a complete or perfect job.

So far as the reports I have read and pre-

sented yesterday are concerned, there were no deaths in trench fever. There were no deaths mentioned, to my knowledge, in the article on pellagra. There were no deaths mentioned, to my knowledge, in the article on beri-beri, and there were no deaths in the article, according to my knowledge, in Colonel Strong's article on plague. I would not testify that I have read all the articles in the medical literature involving the use of human beings as subjects in medical experiments.

Q: And, in the literature which you have read, Witness, there was not a single case where deaths occurred? Did I understand you correctly?

A: Yes, in the yellow fever experiments I indicated that Dr. Carroll and Dr. Lazare died.

Q: That is the only case you know of?

A: That's all that I know of.

\* \* \*

g. **Testimony of Defendant Gerhard Rose**

**Cross-Examination.**

\* \* \*

MR. McHANEY: Now, would the extreme necessity for the large-scale production of typhus vaccines and the resultant experiments on human beings in concentration camps have arisen had not Germany been engaged in a war?

DEFENDANT ROSE: That question cannot simply be answered with "yes" or "no." It is, on the whole, not very probable that without the war typhus would have broken out in the German camps, but it is not altogether beyond the bounds of possibility because in times of peace too typhus has broken out in individual cases from time to time. The primary danger in the camps is the louse danger, and infection by lice also occurs in times of peace. If typhus breaks out in a camp that is infected with lice, a typhus epidemic can arise in peacetime too, of course.

Q: But Germany had never experienced any difficulty with typhus before the war. Isn't that right?

A: Not for many decades, no.

Q: You stated that nine hundred persons were used in Dr. Strong's plague experiments?

A: Yes, I know that number from the literature on the subject.

Q: What is the usual mortality in plague?

A: That depends on whether it is bubonic

plague or lung pest. In one, namely, bubonic plague, the mortality can be as high as sixty or seventy percent. It also can be lower. In lung pest, the mortality is just about one hundred.

Q: How many people died in Dr. Strong's plague experiments?

A: According to what his reports say, none of them died, but this result could not have been anticipated because this was the first time that anyone had attempted to inoculate living plague virus into human beings, and Strong said in his first publication in 1905 that he himself was surprised that no unpleasant incidents occurred and that there was only severe fever reaction. That despite this unexpectedly favorable outcome of Strong's experiments the specialists had considerable misgivings about this procedure can be seen first of all from publications where that is explicitly stated; for example, two Englishmen say that, contrary to expectations, these experiments went off well but nevertheless this process cannot be used for general vaccination because there is always the danger that, through some unexpected event, this strain again becomes virulent. Moreover, from other works that Strong later published it can be seen that guinea pigs and monkeys that he vaccinated with this vaccine died not of the plague, but of the toxic affects of the vaccine. All these difficulties are the reason why this enormously important discovery, which Koller and Otto made in 1903, and Strong in 1905, has only been generally applied, for all practical purposes, since 1926. That is an indication of the care and fear with which this whole matter was first approached, and Strong could not know ahead of time that his experiments would turn out well. I described here the enormous concern that Strong felt during all these months regarding the fact that that might happen which every specialist feared, viz., that the virus would become virulent again. That is an enormous responsibility.

Q: Be that as it may, nobody died. That is a fact, isn't it?

A: If anyone did die, the publications say nothing about it. There were deaths only among the monkeys and guinea pigs that are mentioned in the publication. If human beings died, there is no mention in the publication. It is generally known that if there are serious accidents in such experiments as this, they are only most reluctantly made public.

\* \* \*

## NOTE

LEO ALEXANDER  
MEDICAL SCIENCE UNDER DICTATORSHIP\*

\* \* \*

[A] series of experiments gave results that might have been an important medical contribution if an important lead had not been ignored. The efficacy of various vaccines and drugs against typhus was tested at the Buchenwald and Natzweiler concentration camps. Prevaccinated persons and non-vaccinated controls were injected with live typhus rickettsias, and the death rates of the two series compared. After a certain number of passages, the Matelska strain of typhus rickettsia proved to become avirulent for man. Instead of seizing upon this as a possibility to develop a live vaccine, the experimenters, including the chief consultant, Professor Gerhard Rose, who should have known better, were merely annoyed at the fact that the controls did not die either, discarded this strain and continued testing their relatively ineffective dead vaccines against a new virulent strain. This incident shows that the basic unconscious motivation and attitude has a great influence in determining the scientists's awareness of the phenomena that pass through his vision.

\* \* \*

## 4.

### Final Plea for Defendant Karl Brandt by Dr. Robert Servatius

\* \* \*

It is contended that the state finds its limits in the eternal basic elements of law, which are said to be so clear that anyone could discern their violation as a crime, and that loyalty to the state beyond these limits is therefore a crime. One forgets that eternal law, the law of nature, is but a guiding principle for the state and the legislator and not a counter-code of law which the subject might use as a support against the state. It is emphasized that no other state had made such decisions up to now. This is true only to a certain extent. It is no proof, however,

\* 241 *New England Journal of Medicine* 39, 43 (1949). Reprinted by permission.

that such decisions were not necessary and admissible now. There is no prohibition against daring to progress.

The progress of medical science opened up the problem of experiments on human beings already in the past century, and eventually made it ripe for decision. It is not the first time that a state has adopted a certain attitude with regard to euthanasia with a change of ideology.

Only the statesmen decide what is to be done in the interests of the community, and they have never hesitated to issue such a decision whenever they deemed it necessary in the interest of their people. Thereupon their rules and orders were carried through under the authority of the state, which is the basis of society.

Inquisition, witch trials, and revolutionary tribunals have existed in the name of the state and eternal justice, and the executive participants did not consider themselves criminals but servants of their community. They would have been killed if they had stood up against what was believed to be newly discovered eternal justice. What is the subject to do if the orders of the state exceed the customary limits which the individual himself took for inviolable according to tradition.

What did the airman think who dropped the first atomic bomb on Hiroshima? Did he consider himself a criminal? What did the statesmen think who ordered this atomic bomb to be used. We know from the history of this event that the motive was patriotism, based on the harsh necessity of sacrificing hundreds of thousands to save their own soldiers' lives. This motive was stronger than the prohibition of the Hague Convention, under which belligerents have no unlimited right in the choice of methods for inflicting damage on the enemy.

"My cause is just and my quarrel honorable," says the king. And Shakespeare's soldier answers him: "That's more than we know." Another soldier adds: "Ay, or more than we should seek after; for we know enough if we know we are the king's subjects; if his cause be wrong, our obedience to the king wipes the crime out of us."

It is the hard necessity of the state on which the defense for Karl Brandt is based against the charge of having performed criminal experiments on human beings.

Here also—in addition to the care for the population—the lives of soldiers were at stake, soldiers who had to be protected from death

and epidemics. In Professor Bickenbach's experiment, the issue was the lives of women and children who without 45 million gas masks would have been as unprotected against the expected gas attack as the Japanese were against the atomic bomb. Biological warfare was imminent, even praised abroad as cheaper and more effective than the atomic bomb.

The prosecution opposes to this necessity the condition of absolute voluntariness.

It was a surprise to hear from the expert Professor Ivy that in the penitentiaries many hundreds of volunteers were pressing for admission to experiments, and that more volunteered than could be used. I do not want to dispose of this phenomenon with irony and sarcasm. There may be people who realize that the community has the right to ask them for a sacrifice. Their feeling of justice may tell them that insistence on humanity has its limits. If humanity means the appeal to the strong not to forget the weak in the abundance of might and wealth, the weak should also make their contribution when all are in need.

But what if in the emergency of war the convicts, and those declared to be unworthy to serve in the armed forces, refuse to accept such a sacrifice voluntarily, and only prove an asocial burden to state and community and bring about the downfall of the community? Is not compulsion by the state then admissible as an additional expiation?

The prosecution says "No." According to this, human rights demand the downfall of human beings.

But there is a mixture of voluntariness and compulsory expiation, "purchased voluntariness." Here the experimental subject does not make a sacrifice out of conviction for the good of the community but for his own good. The subject gives his consent because he is to receive money, cigarettes, a mitigation of punishment, etc. There may be isolated cases of this nature where the person is really a volunteer, but as a rule it is not so.

If one compares the actual risk with the advantage granted, one cannot admit the consent of these "voluntary prisoners" as legal, in spite of all the protective forms they have to sign, for these can only have been obtained by taking advantage of inexperience, imprudence, or distress.

Looking through medical literature, one cannot escape the growing conviction that the

word "volunteer," where it appears at all, is used only as a word of protection and camouflage; it is hardly ever missing since the struggle over this problem became acute.

I will touch only briefly on what I have explained in detail in my closing brief. No one will contend that human beings really allowed themselves to be infected voluntarily with venereal disease; this has nowhere been stated explicitly in literature. Cholera and plague are also not minor inconveniences one is likely to undergo voluntarily for a trifle in the interest of science. Above all, it is not customary to hand over children for experimental purposes, and I cannot believe that in the 13 experiments carried out on a total of 223 children, as stated in Document Karl Brandt 117, . . . the mothers gave their consent. Would not the mothers have deserved the praise of the scientist for the sacrifice they trustfully made in the interest of science, praise which is otherwise liberally granted to real volunteers in reports on experiments?

Is it not likely to have been similar to the experiments carried out by Professor McCance? The German authorities who condemn the defendants in a particularly violent form have no objection to raise here against the order to hand over weakling children to a research commission for experimental purposes. The questionnaires which the Tribunal approved for me in order to get further information about this matter have not been answered as the higher authorities did not give permission for such statements to be made. This silence says enough; it is proof of what is supposed to be legal today in the line of "voluntariness."

It is repeatedly shown that the experiments for which no consent was given were permitted with the full knowledge of the government authorities. It is further shown that these experiments were published in professional literature without meeting any objection, and that they were even accepted by the public without concern as a normal phenomenon when reports about them appeared in popular magazines.

This happens at a time when the same press is stigmatizing as crimes against humanity the German experiments which were necessary in the interests of the state. Voluntariness is a fiction; the emergency of the state hard reality.

In all countries experiments on human beings have been performed by doctors, certainly not because they took pleasure in killing or tormenting, but only at the instigation and under the protection of their state, and in accordance

with their own conviction of the necessity for these experiments in the struggle for the existence of the people.

\* \* \*

## 5.

### Final Statements of the Defendants

\* \* \*

a.

#### Final Statement of Defendant Siegfried Handloser

\* \* \*

More than 150 years ago, the motto and guiding principle created for German military doctors and their successors was "Scientiae, Humanitati, Patriae" (For Science, Humanity, and Fatherland). Like the medical officers in their entirety I also have remained true to that guiding principle in thought and in deed. Realizing the outcome of the events of these recent times, may the joint endeavors of all the nations succeed in avoiding in future the immeasurable misfortune of war, the dreadful side of which nobody knows better than the military doctor.

b.

#### Final Statement of Defendant Gerhard Rose

\* \* \*

. . . Everyone who, as a scientist, has an insight into the history of dangerous medical experiments, knows with certainty the following fact. Aside from the self-experiments of doctors, which represent a very small minority of such experiments, the extent to which subjects are volunteers is often deceptive. At the very best they amount to self-deceit on the part of the physician who conducts the experiment, but very frequently to a deliberate misleading of the public. In the majority of such cases, if we ethically examine facts, we find an exploitation of the ignorance, the frivolity, the economic distress, or other emergency on the part of the experimental subjects. I may only refer to the example which was presented to the Tribunal by Dr. Ivy when he presented the forms for the American malaria experiments.

You yourselves, gentlemen of the Tribunal, are in a position to examine whether, on the basis of the information contained in these forms, individuals of the average education of an in-

mate of a prison can form a sufficiently clear opinion of the risks of an experiment made with pernicious malaria. These facts will be confirmed by any sincere and decent scientist in a personal conversation, though he would not like to make such a statement in public. . . .

\* \* \*

## 6.

### Judgment

\* \* \*

BEALS, SEBRING, CRAWFORD, J.J.: . . .

Judged by any standard of proof the record clearly shows the commission of war crimes and crimes against humanity substantially as alleged in counts two and three of the indictment. Beginning with the outbreak of World War II criminal medical experiments on non-German nationals, both prisoners of war and civilians, including Jews and "asocial" persons, were carried out on a large scale in Germany and the occupied countries. These experiments were not the isolated and casual acts of individual doctors and scientists working solely on their own responsibility, but were the product of coordinated policy-making and planning at high governmental, military, and Nazi Party levels, conducted as an integral part of the total war effort. They were ordered, sanctioned, permitted, or approved by persons in positions of authority who under all principles of law were under the duty to know about these things and to take steps to terminate or prevent them.

The great weight of the evidence before us is to the effect that certain types of medical experiments on human beings, when kept within reasonably well-defined bounds, conform to the ethics of the medical profession generally. The protagonists of the practice of human experimentation justify their views on the basis that such experiments yield results for the good of society that are unprocurable by other methods or means of study. All agree, however, that certain basic principles[\*] must be observed in order to satisfy moral, ethical, and legal concepts:

1. The voluntary consent of the human subject is absolutely essential.

This means that the person involved should have legal capacity to give consent; should be

so situated as to be able to exercise free power of choice, without the intervention of any element of force, fraud, deceit, duress, over-reaching, or other ulterior form of constraint or coercion; and should have sufficient knowledge and comprehension of the elements of the subject matter involved as to enable him to make an understanding and enlightened decision. This latter element requires that before the acceptance of an affirmative decision by the experimental subject there should be made known to him the nature, duration, and purpose of the experiment; the method and means by which it is to be conducted; all inconveniences and hazards reasonably to be expected; and the effects upon his health or person which may possibly come from his participation in the experiment.

The duty and responsibility for ascertaining the quality of the consent rests upon each individual who initiates, directs, or engages in the experiment. It is a personal duty and responsibility which may not be delegated to another with impunity.

2. The experiment should be such as to yield fruitful results for the good of society, unprocurable by other methods or means of study, and not random and unnecessary in nature.

3. The experiment should be so designed and based on the results of animal experimentation and a knowledge of the natural history of the disease or other problem under study that the anticipated results will justify the performance of the experiment.

4. The experiment should be so conducted as to avoid all unnecessary physical and mental suffering and injury.

5. No experiment should be conducted where there is an *a priori* reason to believe that death or disabling injury will occur; except, perhaps, in those experiments where the experimental physicians also serve as subjects.

6. The degree of risk to be taken should never exceed that determined by the humanitarian importance of the problem to be solved by the experiment.

Proper preparations should be made and adequate facilities provided to protect the experimental subject against even remote possibilities of injury, disability, or death.

8. The experiment should be conducted only by scientifically qualified persons. The highest degree of skill and care should be required through all stages of the experiment of those who conduct or engage in the experiment.

[\*] These ten principles are now known as the Nuremberg Code.

9. During the course of the experiment the human subject should be at liberty to bring the experiment to an end if he has reached the physical or mental state where continuation of the experiment seems to him to be impossible.

10. During the course of the experiment the scientist in charge must be prepared to terminate the experiment at any stage, if he has probable cause to believe, in the exercise of the good faith, superior skill, and careful judgment required of him that a continuation of the experiment is likely to result in injury, disability, or death to the experimental subject.

Of the ten principles which have been enumerated our judicial concern, of course, is with those requirements which are purely legal in nature—or which at least are so clearly related to matters legal that they assist us in determining criminal culpability and punishment. To go beyond that point would lead us into a field that would be beyond our sphere of competence. However, the point need not be labored. We find from the evidence that in the medical experiments which have been proved, these ten principles were much more frequently honored in their breach than in their observance. Many of the concentration camp inmates who were the victims of these atrocities were citizens of countries other than the German Reich. They were non-German nationals, including Jews and “asocial persons,” both prisoners of war and civilians, who had been imprisoned and forced to submit to these tortures and barbarities without so much as a semblance of trial. In every single instance appearing in the record, subjects were used who did not consent to the experiments; indeed, as to some of the experiments, it is not even contended by the defendants that the subjects occupied the status of volunteers. In no case was the experimental subject at liberty of his own free choice to withdraw from any experiment. In many cases experiments were performed by unqualified persons; were conducted at random for no adequate scientific reason, and under revolting physical conditions. All of the experiments were conducted with unnecessary suffering and injury, and but very

little, if any, precautions were taken to protect or safeguard the human subjects from the possibilities of injury, disability, or death. In every one of the experiments the subjects experienced extreme pain or torture, and in most of them they suffered permanent injury, mutilation, or death, either as a direct result of the experiments or because of lack of adequate follow-up care.

Obviously all of these experiments involving brutalities, tortures, disabling injury, and death were performed in complete disregard of international conventions, the laws and customs of war [and] the general principles of criminal law as derived from the criminal laws of all civilized nations. . . . Manifestly human experiments under such conditions are contrary to “the principles of the law of nations as they result from the usages established among civilized peoples, from the laws of humanity, and from the dictates of public conscience.”

\* \* \*

There is some evidence to the effect that the camp inmates used as subjects in the first series submitted to being used as experimental subjects after being told that the experiments were harmless and that additional food would be given to volunteers. But these victims were not informed that they would be artificially infected with a highly virulent virus nor that they might die as a result. Certainly no one would seriously suggest that under the circumstances these men gave their legal consent to act as subjects. One does not ordinarily consent to be the special object of a murder, and if one did, such consent would not absolve his slayer.

\* \* \*

[Sixteen of the twenty-three defendants were found guilty of war crimes and crimes against humanity. Seven, including Karl Brandt, Rudolf Brandt, and Joachim Mrugowsky, were sentenced to death by hanging; the other nine, including Siegfried Handloser and Gerhard Rose, to imprisonment varying from ten years to life.]

## C.

### Epilogue—Experiments Subsequent to 1945

In 1966 Dr. Henry K. Beecher, Dorr Professor of Research in Anesthesia at the Harvard Medical School, surveyed the tremendous postwar increase in research involving human subjects and charged that “many of the patients [who were used in these experiments]

never had the risk satisfactorily explained to them, and . . . further hundreds have not known that they were the subjects of an experiment although grave consequences have been suffered as the direct result. . . .”<sup>\*</sup> We have selected excerpts from his article, which was checked for accuracy by the editors of one of the most prestigious American medical journals, as illustrative of “the ease with which the admonition first of all to do no harm is allowed . . . to slip from the investigator’s consciousness.”<sup>†</sup> Despite his “troubling charges,” Dr. Beecher concluded that in addition to informed consent, a “more reliable safeguard [for an ethical approach to experimentation is] provided by the presence of an intelligent, informed, conscientious, compassionate, responsible investigator.”<sup>‡</sup>

Dr. Beecher’s conclusion in turn raises further questions: Does or can science produce such ideal investigators? And, if not, how can professional training be reformed to provide “reliable safeguards” for human subjects? Before turning to an analysis of the extent and limits of the investigator’s authority, we examine a few of the major documents on medical ethics as well as selected commentaries on research responsibility. In studying these documents we ask:

1. How adequately do codes of ethics guide the investigator in formulating specific rules of conduct for himself?
2. How adequate are these documents in themselves as rules of conduct?
3. To what extent are these documents counterproductive as guides or as rules of conduct for investigators?

# 1.

## The Experiments

Henry K. Beecher  
Ethics and Clinical Research\*\*

\* \* \*

Nearly everyone agrees that ethical violations do occur. The practical question is, how often? A preliminary examination of the matter was based on 17 examples, which were easily increased to 50. These 50 studies<sup>††</sup> contained references to 186 further likely examples, on the average 3.7 leads per study; they at times overlapped from paper to paper, but this figure indicates how conveniently one can proceed in a search for such material. The data are suggestive of widespread problems, but there is need for another kind of information, which was obtained by examination of 100 consecutive human studies published in 1964, in an excellent journal; 12 of these seemed to be unethical. If only

one quarter of them is truly unethical, this still indicates the existence of a serious situation. Pappworth, in England, has collected, he says, more than 500 papers based upon unethical experimentation. It is evident from such observations that unethical or questionably ethical procedures are not uncommon.

\* \* \*

## Known Effective Treatment Withheld

Example 1. It is known that rheumatic fever can usually be prevented by adequate treatment of streptococcal respiratory infections by the parenteral administration of penicillin. Nevertheless, definitive treatment was withheld, and placebos were given to a group of 109 men in service, while benzathine penicillin G was given to others.

The therapy that each patient received was determined automatically by his military serial number arranged so that more men received penicillin than received placebo. In the small

\* Henry K. Beecher: “Ethics and Clinical Research,” 274 *New England Journal of Medicine* 1354 (1966).

† “Experimentation on Man” (Editorial), 274 *New England Journal of Medicine* 1383 (1966).

‡ Beecher, at 1360.

\*\* 274 *New England Journal of Medicine* 1354–1360 (1966). Reprinted by permission.

†† [The editors of the *Journal*, “for reasons of space,” published only twenty-two studies.]



group of patients studied 2 cases of acute rheumatic fever and 1 of acute nephritis developed in the control patients, whereas these complications did not occur among those who received benzathine penicillin G.

Example 2. The sulfonamides were for many years the only antibacterial drugs effective in shortening the duration of acute streptococcal pharyngitis and in reducing its suppurative complications. The investigators in this study undertook to determine if the occurrence of the serious nonsuppurative complications, rheumatic fever and acute glomerulonephritis, would be reduced by this treatment. This study was made despite the general experience that certain antibiotics, including penicillin, will prevent the development of rheumatic fever.

The subjects were a large group of hospital patients; a control group of approximately the same size, also with exudative Group A streptococcus, was included. The latter group received only non-specific therapy (no sulfadiazine). The total group denied the effective penicillin comprised over 500 men.

Rheumatic fever was diagnosed in 5.4 per cent of those treated with sulfadiazine. In the control group rheumatic fever developed in 4.2 per cent.

In reference to this study a medical officer stated in writing that the subjects were not informed, did not consent and were not aware that they had been involved in an experiment, and yet admittedly 25 acquired rheumatic fever. According to this same medical officer *more than 70* who had had known definitive treatment withheld were on the wards with rheumatic fever when he was there.

Example 3. This involved a study of the relapse rate in typhoid fever treated in two ways. In an earlier study by the present investigators chloramphenicol had been recognized as an effective treatment for typhoid fever, being attended by half the mortality that was experienced when this agent was not used. Others had made the same observations, indicating that to withhold this effective remedy can be a life-or-death decision. The present study was carried out to determine the relapse rate under the two methods of treatment; of 408 charity patients 251 were treated with chloramphenicol, of whom 20, or 7.97 per cent, died. Symptomatic treatment was given, but chloramphenicol was withheld in 157, of whom 36, or 22.9 per cent died. According to the data presented, 23 patients died in the course of this study who would

not have been expected to succumb if they had received specific therapy.

\* \* \*

#### *Physiologic Studies*

Example 5. In this controlled, double-blind study of the hematologic toxicity of chloramphenicol, it was recognized that chloramphenicol is "well known as a cause of aplastic anemia" and that there is a "prolonged morbidity and high mortality of aplastic anemia" and that "... chloramphenicol-induced aplastic anemia can be related to dose. . . ." The aim of the study was "further definition of the toxicology of the drug. . . ."

Forty-one randomly chosen patients were given either 2 or 6 gm. of chloramphenicol per day; 12 control patients were used. "Toxic bone-marrow depression, predominantly affecting erythropoiesis, developed in 2 of 20 patients given 2.0 gm. and in 18 of 21 given 6 gm. of chloramphenicol daily." The smaller dose is recommended for routine use.

Example 6. In a study of the effect of thymectomy on the survival of skin homografts 18 children, three and a half months to eighteen years of age, about to undergo surgery for congenital heart disease, were selected. Eleven were to have total thymectomy as part of the operation, and 7 were to serve as controls. As part of the experiment, full-thickness skin homografts from an unrelated adult donor were sutured to the chest wall in each case. (Total thymectomy is occasionally, although not usually part of the primary cardiovascular surgery involved, and whereas it may not greatly add to the hazards of the necessary operation, its eventual effects in children are not known.) This work was proposed as part of a long-range study of "the growth and development of these children over the years." No difference in the survival of the skin homograft was observed in the 2 groups.

\* \* \*

Example 8. Since the minimum blood-flow requirements of the cerebral circulation are not accurately known, this study was carried out to determine "cerebral hemodynamic and metabolic changes . . . before and during acute reductions in arterial pressure induced by drug administration and/or postural adjustments." Forty-four patients whose ages varied from the second to the tenth decade were involved. They included normotensive subjects, those with essential hypertension and finally a group with



malignant hypertension. Fifteen had abnormal electrocardiograms. Few details about the reasons for hospitalization are given.

Signs of cerebral circulatory insufficiency, which were easily recognized, included confusion and in some cases a nonresponsive state. By alteration in the tilt of the patient "the clinical state of the subject could be changed in a matter of seconds from one of alertness to confusion, and for the remainder of the flow, the subject was maintained in the latter state." The femoral arteries were cannulated in all subjects, and the internal jugular veins in 14.

The mean arterial pressure fell in 37 subjects from 109 to 48 mm. of mercury, with signs of cerebral ischemia. "With the onset of collapse, cardiac output and right ventricular pressures decreased sharply."

Since signs of cerebral insufficiency developed without evidence of coronary insufficiency the authors concluded that "the brain may be more sensitive to acute hypotension than is the heart."

\* \* \*

#### *Studies to Improve the Understanding of Disease*

Example 14. In this study of the syndrome of impending hepatic coma in patients with cirrhosis of the liver certain nitrogenous substances were administered to 9 patients with chronic alcoholism and advanced cirrhosis: ammonium chloride, di-ammonium citrate, urea or dietary protein. In all patients a reaction that included mental disturbances, a "flapping tremor," and electroencephalographic changes developed. Similar signs had occurred in only 1 of the patients before these substances were administered:

The first sign noted was usually clouding of the consciousness. Three patients had a second or a third course of administration of a nitrogenous substance with the same results. It was concluded that marked resemblance between this reaction and impending hepatic coma implied that the administration of these [nitrogenous] substances to patients with cirrhosis may be hazardous.

Example 18. Melanoma was transplanted from a daughter to her volunteering and informed mother, "in the hope of gaining a little better understanding of cancer immunity and in the hope that the production of tumor antibodies might be helpful in the treatment of the cancer patient." Since the daughter died on the day after the transplantation of the tumor into her mother, the hope expressed seems to have

been more theoretical than practical, and the daughter's condition was described as "terminal" at the time the mother volunteered to be a recipient. The primary implant was widely excised on the twenty-fourth day after it had been placed in the mother. She died from metastatic melanoma on the four hundred and fifty-first day after transplantation. The evidence that this patient died of diffuse melanoma that metastasized from a small piece of transplanted tumor was considered conclusive.

#### *Technical Study of Disease*

Example 19. During bronchoscopy a special needle was inserted through a bronchus into the left atrium of the heart. This was done in an unspecified number of subjects, both with cardiac disease and with normal hearts.

The technic was a new approach whose hazards were at the beginning quite unknown. The subjects with normal hearts were used, not for their possible benefit but for that of patients in general.

\* \* \*

Example 21. This was a study of the effect of exercise on cardiac output and pulmonary-artery pressure in 8 "normal" persons (that is, patients whose diseases were not related to the cardiovascular system), in 8 with congestive heart failure severe enough to have recently required complete bed rest, in 6 with hypertension, in 2 with aortic insufficiency, in 7 with mitral stenosis, and in 5 with pulmonary emphysema.

Intracardiac catheterization was carried out, and the catheter then inserted into the right or left main branch of the pulmonary artery. The brachial artery was usually catheterized; sometimes, the radial or femoral arteries were catheterized. The subjects exercised in a supine position by pushing their feet against weighted pedals. "The ability of these patients to carry on sustained work was severely limited by weakness and dyspnea." Several were in severe failure. This was not a therapeutic attempt but rather a physiologic study.

#### *Bizarre Study*

Example 22. There is a question whether ureteral reflux can occur in the normal bladder. With this in mind, vesicourethrography was carried out on 26 normal babies less than forty-eight hours old. The infants were exposed to x-rays while the bladder was filling and during voiding. Multiple spot films were made to record

the presence or absence of ureteral reflux. None was found in this group, and fortunately no infection followed the catheterization. What the results of the extensive x-ray exposure may be, no one can yet say.

#### *Comment on Death Rates*

In the foregoing examples a number of procedures, some with their own demonstrated death rates, were carried out. The following data were provided by 3 distinguished investigators in the field and represent widely held views.

*Cardiac catheterization:* right side of the heart, about 1 death per 1000 cases; left side, 5 death per 1000 cases. "Probably considerably higher in some places, depending on the portal of entry." (One investigator had 15 deaths in his first 150 cases.) It is possible that catheterization of a hepatic vein or the renal vein would have a lower death rate than that of catheterization of the right side of the heart, for if it is properly carried out, only the atrium is entered en route to the liver or the kidney, not the right ventricle, which can lead to serious cardiac irregularities. There is always the possibility, however, that the ventricle will be entered inadvertently. This occurs in at least half the cases, according to 1 expert—"but if properly done is too transient to be of importance."

*Liver biopsy:* the death rate here is estimated at 2 to 3 per 1000, depending in considerable part on the condition of the subject.

*Anesthesia:* the anesthesia death rate can be placed in general at about 1 death per 2000 cases. The hazard is doubtless higher when certain practices such as deliberate evocation of ventricular extrasystoles under cyclopropane are involved.

\* \* \*

#### NOTES

##### NOTE 1.

LETTER FROM OWEN H. WANGENSTEEN  
TO SENATOR WALTER F. MONDALE—  
JANUARY 28, 1968\*

Your letter of January 10, 1968, has crossed my desk together with a memorandum relative to a proposal you intend bringing before the

Congress with the intent of creating a commission to adjudicate the Social and Ethical Implications of Health Science Research and Development.

Senator, I would urge you with all the strength I can muster to leave this subject to the conscionable people in the profession who are struggling valiantly to advance medicine. We are living through an era in which the innovator is often under suspicion, being second-guessed by self-appointed arbiters more versed in the art of criticism than in the subject under scrutiny. We need to take great care lest the wells of creativity and the spring of the mind of those who break with tradition are not manacled by well-intentioned but meddlesome intruders. I will only recount three items here:

First, when the Minnesota group under the leadership of Drs. C. Walton Lillehei, Richard Varco, and Richard De Wall gave intracardiac surgery its first real impetus, they and I too found it necessary to defend ourselves and our position. At meetings my colleagues were viciously attacked by the keenest exponents of cardiac surgery in America. I too became a target of these assaults. I remember well Walt Lillehei saying very patiently but firmly in reply to these charges: "Your procedure is palliative; mine is corrective. Time will dispel the difference in surgical mortality." As we all know, it has. . . .

In Paris for generations it was customary to adjudicate scientific matters by assigning them for arbitration to a commission—a thumbs-up or Pollicis Versus Phenomenon which determined the life or the death of the proposal. How progress was stifled by such structures! Antiseptic surgery might have come much earlier if a few enterprising and knowledgeable surgeons of those days had not been influenced too much by the unfavorable verdict of committees that expressed firm judgments upon proposals of which they had little or no orientation.

When anesthesia, an American invention of 1846 came it found generally warm acceptance because the world had been waiting patiently for centuries for a lenitive to pain. Even so there were those who raised the question of the morality of making a patient unconscious when he was undergoing an operative procedure.

I would urge you to leave these matters in the hands of their proponents, the persons who are actually doing the work. They know more about all this than any of us possibly could. They have wrestled with the problem day and

\* *Hearings on S.J. Res. 145 before the Subcommittee on Government Research of the Senate Committee on Government Operations, 90th Congress, 2d Session 98-99 (1968).*

night, almost invariably over many years. Theirs are not overnight judgments or convictions. In the academic community in which I have worked and spent my entire professional life of almost 50 years, you will find as warm, sympathetic human beings as are to be found on this earth. . . .

It is important that we look back as well as forward. To have no concern for history is tantamount to having a physician with total amnesia. If we leave this matter alone, it will simmer down. Discussion should not be restrained, but legislative action, never!

#### NOTE 2.

EDWARD A. SHILS  
SOCIAL INQUIRY AND THE AUTONOMY  
OF THE INDIVIDUAL \*

\* \* \*

More serious experimentation by social psychologists and sociologists in the present state of the subject must be viewed very cautiously. Many restraints should accompany its recommendation or execution. In medicine, where knowledge is admittedly fragmentary, although undoubtedly less fragmentary than in the social sciences, experimentation is guided by a strongly rooted therapeutic tradition and is almost always a link in a continuing sequence of scientific activity. One of the chief features of social psychological and sociological research today is the absence of both therapeutic intent and a tradition of cumulative scientific growth. This renders more doubtful the scientific value of the results attained by experimentation which would deal with the more vital features of individual or corporate life.

Furthermore, because the therapeutic tradition of medicine is lacking, the probability that sociological and psychological experimentation on more important variables might have harmful consequences is greater. The professional ethos formed by the weight of a powerful tradition and inculcated in medical schools, where ethical responsibility is one of the best precipitates of a course of study which includes much that is not technically necessary for the medical practitioner, is still lacking in the social and psychological sciences. This *ethos* is not by any means just a product of scientific maturity, al-

though the latter does not make a positive difference. It flows from a deeper attitude that is still lacking in the scientific study of man.

\* \* \*

## 2.

### Professional Guidelines

#### a.

#### Oath of Hippocrates (5th century B.C.)

I swear by Apollo the physician, and Aesculapius and Health, and All-heal, and all the gods and goddesses, that, according to my ability and judgment, I will keep this Oath and this stipulation—to reckon him who taught me this Art equally dear to me as my parents, to share my substance with him, and relieve his necessities if required; to look upon his offspring in the same footing as my own brothers, and to teach them this art, if they shall wish to learn it, without fee or stipulation; and that by precept, lecture, and every other mode of instruction, I will impart a knowledge of the Art to my own sons, and those of my teachers, and to disciples bound by a stipulation and oath according to the law of medicine, but to none others. I will follow that system of regimen which, according to my ability and judgment, I consider for the benefit of my patients, and abstain from whatever is deleterious and mischievous. I will give no deadly medicine to anyone if asked, nor suggest any such counsel; and in like manner I will not give to a woman a pessary to produce abortion. With purity and with holiness I will pass my life and practise my Art. I will not cut persons labouring under the stone, but will leave this to be done by men who are practitioners of this work. Into whatever houses I enter, I will go into them for the benefit of the sick, and will abstain from every voluntary act of mischief and corruption; and, further, from the seduction of females or males, or freemen or slaves. Whatever, in connection with my professional practice, or not in connection with it, I see or hear, in the life of men, which ought not to be spoken of abroad, I will not divulge, as reckoning that all such should be kept secret. While I continue to keep this Oath unviolated, may it be granted to me to enjoy life and the practice of the Art, respected by all men, in all times. But should I trespass and violate this Oath, may the reverse be my lot.

\* Daniel Lerner, ed.: *The Human Meaning of the Social Sciences*. New York: Meridian Books, Inc. 114, 146 (1959). © 1959 by the World Publishing Co. Reprinted by permission.

## NOTE

WORLD MEDICAL ASSOCIATION  
THE HIPPOCRATIC OATH  
FORMULATED AT GENEVA (SEPTEMBER 1948)

Now being admitted to the profession of medicine I solemnly pledge to consecrate my life to the service of humanity. I will give respect and gratitude to my deserving teachers. I will practice medicine with conscience and dignity. The health and life of my patient will be my first consideration. I will hold in confidence all that my patient confides in me. I will maintain the honor and the noble traditions of the medical profession. My colleagues will be as my brothers. I will not permit consideration of race, religion, nationality, party politics or social standing to intervene between my duty and my patient. I will maintain the utmost respect for human life from the time of its conception. Even under threat I will not use my knowledge contrary to the laws of humanity. These promises I make freely and upon my honor.

b.

**World Medical Association  
Declaration of Helsinki\***

It is the mission of the doctor to safeguard the health of the people. His knowledge and conscience are dedicated to the fulfillment of this mission.

The Declaration of Geneva of the World Medical Association binds the doctor with the words, "The health of my patient will be my first consideration"; and the International Code of Medical Ethics which declares that "Any act or advice which could weaken physical or mental resistance of a human being may be used only in his interest."

Because it is essential that the results of laboratory experiments be applied to human beings to further scientific knowledge and to help suffering humanity, the World Medical Association has prepared the following recommendations as a guide to each doctor in clinical research. It must be stressed that the standards as drafted are only a guide to physicians all over the world. Doctors are not relieved from criminal, civil, and ethical responsibilities under the laws of their own countries.

In the field of clinical research a funda-

mental distinction must be recognized between clinical research in which the aim is essentially therapeutic for a patient, and clinical research the essential object of which is purely scientific and without therapeutic value to the person subjected to the research.

## I. Basic Principles

1. Clinical research must conform to the moral and scientific principles that justify medical research, and should be based on laboratory and animal experiments or other scientifically established facts.

2. Clinical research should be conducted only by scientifically qualified persons and under the supervision of a qualified medical man.

3. Clinical research cannot legitimately be carried out unless the importance of the objective is in proportion to the inherent risk to the subject.

4. Every clinical research project should be preceded by careful assessment of inherent risks in comparison to foreseeable benefits to the subject or to others.

5. Special caution should be exercised by the doctor in performing clinical research in which the personality of the subject is liable to be altered by drugs or experimental procedure.

## II. Clinical Research Combined with Professional Care

1. In the treatment of the sick person the doctor must be free to use a new therapeutic measure if in his judgment it offers hope of saving life, re-establishing health, or alleviating suffering.

If at all possible, consistent with patient psychology, the doctor should obtain the patient's freely given consent after the patient has been given a full explanation. In case of legal incapacity consent should also be procured from the legal guardian; in case of physical incapacity the permission of the legal guardian replaces that of the patient.

2. The doctor can combine clinical research with professional care, the objective being the acquisition of new medical knowledge, only to the extent that clinical research is justified by its therapeutic value for the patient.

## III. Non-therapeutic Clinical Research

1. In the purely scientific application of clinical research carried out on a human being it is the duty of the doctor to remain the protector of the life and health of that person on whom clinical research is being carried out.

\* 271 *New England Journal of Medicine* 473 (1964). Reprinted by permission.

2. The nature, the purpose, and the risk of clinical research must be explained to the subject by the doctor.

3a. Clinical research on a human being cannot be undertaken without his free consent, after he has been fully informed; if he is legally incompetent the consent of the legal guardian should be procured.

3b. The subject of clinical research should be in such a mental, physical, and legal state as to be able to exercise fully his power of choice.

3c. Consent should as a rule be obtained in writing. However, the responsibility for clinical research always remains with the research worker; it never falls on the subject, even after consent is obtained.

4a. The investigator must respect the right of each individual to safeguard his personal integrity, especially if the subject is in a dependent relationship to the investigator.

4b. At any time during the course of clinical research the subject or his guardian should be free to withdraw permission for research to be continued. The investigator or the investigating team should discontinue the research if in his or their judgment it may, if continued, be harmful to the individual.

c.

### **American Medical Association Principles of Medical Ethics\***

#### *Introduction*

\* \* \*

Ethical principles are basic and fundamental. Men of good conscience inherently know what is right or wrong, and what is to be done or to be avoided. Written documents attempt to express for the guidance of all what each knows to be true. Thus the Principles of Medical Ethics are truly guides to good conduct.

\* \* \*

#### *Principles of Medical Ethics*

##### *Preamble*

These principles are intended to aid physicians individually and collectively in maintaining a high level of ethical conduct. They are not laws but standards by which a physician may determine the propriety of his conduct in his relation-

ship with patients, with colleagues, with members of allied professions, and with the public.

##### *Section 1*

The principal objective of the medical profession is to render service to humanity with full respect for the dignity of man. Physicians should merit the confidence of patients entrusted to their care, rendering to each a full measure of service and devotion.

##### *Section 2*

Physicians should strive continually to improve medical knowledge and skill, and should make available to their patients and colleagues the benefits of their professional attainments.

##### *Section 3*

A physician should practice a method of healing founded on a scientific basis; and he should not voluntarily associate professionally with anyone who violates this principle.

##### *Section 4*

The medical profession should safeguard the public and itself against physicians deficient in moral character or professional competence. Physicians should observe all laws, uphold the dignity and honor of the profession and accept its self-imposed disciplines. They should expose, without hesitation, illegal or unethical conduct of fellow members of the profession.

##### *Section 5*

A physician may choose whom he will serve. In an emergency, however, he should render service to the best of his ability. Having undertaken the care of a patient, he may not neglect him; and unless he has been discharged he may discontinue his services only after giving adequate notice. He should not solicit patients.

##### *Section 6*

A physician should not dispose of his services under terms or conditions which tend to interfere with or impair the free and complete exercise of his medical judgment and skill or tend to cause a deterioration of the quality of medical care.

##### *Section 7*

In the practice of medicine a physician should limit the source of his professional income to medical services actually rendered by him, or under his supervision, to his patients. His fee should be commensurate with the services rendered and the patient's ability to pay. He should neither pay nor receive a commission for referral

\* *Opinions and Reports of the Judicial Council*. Chicago: American Medical Association iii-vii (1969). Reprinted by permission.

of patients. Drugs, remedies or appliances may be dispensed or supplied by the physician provided it is in the best interests of the patient.

#### Section 8

A physician should seek consultation upon request; in doubtful or difficult cases; or whenever it appears that the quality of medical service may be enhanced thereby.

#### Section 9

A physician may not reveal the confidences entrusted to him in the course of medical attendance, or the deficiencies he may observe in the character of patients, unless he is required to do so by law or unless it becomes necessary in order to protect the welfare of the individual or of the community.

#### Section 10

The honored ideals of the medical profession imply that the responsibilities of the physician extend not only to the individual, but also to society where these responsibilities deserve his interest and participation in activities which have the purpose of improving both the health and the well-being of the individual and the community.

### NOTE

#### AMERICAN MEDICAL ASSOCIATION OPINIONS OF THE JUDICIAL COUNCIL RELATING TO THE PRINCIPLES OF MEDICAL ETHICS\*

\* \* \*

#### Section 1—Opinion 5. Substitution of Surgeon without Patient's Knowledge or Consent

\* \* \*

The surgeon's obligation to the patient requires him to perform the surgical operation: (1) within the scope of authority granted him by the consent to the operation; (2) in accordance with the terms of the contractual relationship; (3) with complete disclosure of all facts relevant to the need and the performance of the operation; and (4) to utilize his best skill in performing the operation.

\* \* \*

\* *Opinions and Reports of the Judicial Council*. Chicago: American Medical Association 5, 9, 55 (1969). Reprinted by permission.

#### Section 2—Opinion 2. Experimentation: New Drugs or Procedures [1946]<sup>[†]</sup>

In order to conform to the ethics of the American Medical Association, three requirements must be satisfied in connection with the use of experimental drugs or procedures:

(1) the voluntary consent of the person on whom the experiment is to be performed should be obtained;

(2) the danger of each experiment must be previously investigated by animal experimentation; and

(3) the experiment must be performed under proper medical protection and management.

\* \* \*

#### Section 9—Opinion 2. Prognosis

The physician should neither exaggerate nor minimize the gravity of a patient's condition. He should assure himself that the patient, his relatives or his responsible friends have such knowledge of the patient's condition as will serve the best interests of the patient and the family.

d.

#### American Psychological Association Ethical Standards of Psychologists\*

The psychologist believes in the dignity and worth of the individual human being. He is committed to increasing man's understanding of himself and others. While pursuing this endeavor, he protects the welfare of any person who may seek his service or of any subject, human or animal, that may be the object of his study. He does not use his professional position or relationships, nor does he knowingly permit his own services to be used by others, for purposes inconsistent with these values. While demanding for himself freedom of inquiry and communication, he accepts the responsibility this freedom confers: for competence where he claims it, for objectivity in the report of his findings, and for consideration of the best interests of his colleagues and of society.

[†] The opinion of the Judicial Council on the "Ethical Guidelines for Clinical Investigation" (1966) appears at pp. 845-846 *infra*.

\* 18 *American Psychologist* 56-60 (1963). Reprinted by permission.

*Specific Principles*

**Principle 1. Responsibility.** The psychologist,<sup>2</sup> committed to increasing man's understanding of man, places high value on objectivity and integrity, and maintains the highest standards in the services he offers.

a. As a scientist, the psychologist believes that society will be best served when he investigates where his judgment indicates investigation is needed; he plans his research in such a way as to minimize the possibility that his findings will be misleading; and he publishes full reports of his work, never discarding without explanation data which may modify the interpretation of results.

\* \* \*

**Principle 6. Confidentiality.** Safeguarding information about an individual that has been obtained by the psychologist in the course of his teaching, practice, or investigation is a primary obligation of the psychologist. Such information is not communicated to others unless certain important conditions are met.

a. Information received in confidence is revealed only after most careful deliberation and when there is clear and imminent danger to an individual or to society, and then only to appropriate professional workers or public authorities.

\* \* \*

e. Only after explicit permission has been granted is the identity of research subjects published. When data have been published without permission for identification, the psychologist assumes responsibility for adequately disguising their sources.

f. The psychologist makes provisions for the maintenance of confidentiality in the preservation and ultimate disposition of confidential records.

\* \* \*

**Principle 16. Research Precautions.** The psychologist assumes obligations for the welfare of his research subjects, both animal and human.

a. Only when a problem is of scientific significance and it is not practicable to investigate it in any other way is the psychologist justifi-

fied in exposing research subjects, whether children or adults, to physical or emotional stress as part of an investigation.

b. When a reasonable possibility of injurious aftereffects exists, research is conducted only when the subjects or their responsible agents are fully informed of this possibility and agree to participate nevertheless.

c. The psychologist seriously considers the possibility of harmful aftereffects and avoids them, or removes them as soon as permitted by the design of the experiment.

d. A psychologist using animals in research adheres to the provisions of the Rules Regarding Animals, drawn up by the Committee on Precautions and Standards in Animal Experimentation, and adopted by the American Psychological Association.

e. Investigations of human subjects using experimental drugs (for example: hallucinogenic, psychotomimetic, psychedelic, or similar substances) should be conducted only in such settings as clinics, hospitals, or research facilities maintaining appropriate safeguards for the subjects.

\* \* \*

## NOTES

## NOTE 1.

HENRY K. BEECHER  
TENTATIVE STATEMENT OUTLINING THE  
PHILOSOPHY AND ETHICAL PRINCIPLES  
GOVERNING THE CONDUCT OF RESEARCH ON  
HUMAN BEINGS AT THE HARVARD  
MEDICAL SCHOOL\*

\* \* \*

The breaches of ethical conduct which have come to the personal attention of the writer are owing to ignorance or thoughtlessness. They were not of willful or unscrupulous origin. Basic considerations in human experimentation at Harvard are the same as they are everywhere: protection of the subject, protection of the investigator and protection of research and the institutions involved, and the sound development of medicine. These all require a levelheaded approach to experimentation in man. It is everywhere recognized that man is the final essential test site—the animal of necessity, so to speak, when it comes to the evaluation of new drugs and new procedures.

\* \* \*

<sup>2</sup>A student of psychology who assumes the role of psychologist shall be considered a psychologist for the purpose of this code of ethics.

\* Unpublished manuscript (undated). Printed by permission of the author who retains all rights.



The inescapable responsibility for determining what investigations may be done on a particular patient must rest with the investigator or physician concerned, bearing in mind that present-day specialization in medicine and complexity of procedures proposed or undertaken are frequently beyond the grasp of the subjects involved.

All of the so-called codes as guides to human experimentation emphasize the necessity that the experimenter be well trained and adequate as a scientist to undertake the study proposed. Medical research, when it involves treatment of any physical procedures beyond the simplest, requires that the investigator or his close associate be a qualified physician. No other profession gives such prerogatives and no other profession, probably, presents such a generally high level of unselfishness and compassion in directly caring for the sick or in planning procedures for the future. Of the qualities of the investigators, unselfishness is the most important for subject and project alike. Imagination, objectivity, and the power to generalize soundly are all essential. In the forefront of the qualities which lead to protection of subject and patient in investigation is a deep sense of responsibility on the part of the investigator, coupled with unselfishness and a keen and well-trained intelligence.

## NOTE 2.

WALTER MODELL

COMMENT ON "SOME FALLACIES AND ERRORS"\*

\* \* \*

... I suspect that every species of ostrich selects its own kind of sand to stick its head into. Here is how I do it. I bury my head in my notions of the status of medicine in modern Western culture and of the nature of the license given the physician by society (and as you will recall, medicine has a place close to religion in virtually all cultures). I then choose to believe this covers both categories of consent, the straightforward as well as for the experiment which does not benefit the subject.

It is my feeling that Western civilization accepts medicine as an experimental discipline and that, in expecting it to continue to grow, to be a progressive science, as against the attitudes of the medicine man whose incantations and

medicaments were not expected to nor designed for change, an acceptance of the trial of the new and the search for the better is implicit. I also think that when society confers the degree of physician on a man it instructs him to experiment on his fellow. I think that when a patient goes to a modern physician for treatment, regardless of whether he consciously consents to it, he is also unconsciously presenting himself for the purpose of experimentation.

There is really nothing new in this. Modern society accepts this all the time. It is the nature of life in our time. When you become a passenger on a new airplane, you automatically become the subject of an experiment in modern travel. When you purchase your ticket you give consent; when society licenses the pilot, it instructs him to experiment with your life at stake. In this case, no one seems to worry about the ethical or philosophic implications. Nor do I see much difference in the ethics of experimentation between an old doctor trying a new drug on a patient or a new doctor trying an old one.

It is this kind of thinking that makes me feel that there is nothing that I, as an ostrich, can do to shelter or protect the end of me that is so openly exposed to attack. I am fully conscious of my responsibilities as well as the apparent arrogance of my attitude, but I also wonder whether I am not much more honest and even more humble about what I do than a practitioner who prescribes pills all day long thinking that he is not doing experiments and that he is not taking risks with his patients' lives without their explicit instruction....

## NOTE 3.

WALSH McDERMOTT

OPENING COMMENTS ON THE CHANGING  
MORES OF BIOMEDICAL RESEARCH\*

When the needs of society come in head-on conflict with the rights of an individual, someone has to play God. We can avoid this responsibility so long as the power to decide the particular case-in-point is clearly vested in someone else, for example, a duly elected governmental official. But in clinical investigation, the power to determine this issue of "the individual versus society" is clearly vested in the physician. Both the power itself and, above all, *our awareness that we are wielding it* are increasing every day and can be expected to increase much further.

\* 3 *Clinical Pharmacology and Therapeutics* 145, 146 (1962). Reprinted by permission.

\* 67 *Annals of Internal Medicine*, Supplement 7: 39-42 (1967). Reprinted by permission.



It is this inescapable *awareness* that we are wielding power that has us so deeply troubled, for we are a generation nurtured on the slogan "the end does not justify the means" in matters concerning the individual and his society. Yet as a society we enforce the social good over the individual good across a whole spectrum of non-medical activities every day, and many of these activities ultimately affect the health or the life of an individual.

Traditionally in our Judeo-Christian culture we have handled this issue by one of two mechanisms. When, as in our racial problem for example, the conflict contains no built-in contradiction, we publicly and officially subscribe to a set of ideals. We can work privately and publicly toward the attainment of these ideals, and with their attainment would come the solution of the problem. This mechanism works when the forces in conflict are intrinsically reconcilable even though the reconciliation might take many decades or a century. But we use another mechanism when the conflict is head on, when the group interest and the individual interest are basically irreconcilable.

In circumstances like these, such as the decision to impose capital punishment or the selection of only a minority of our young men to become soldiers, the issue is decided by a judgment that is arbitrary as it affects the individual. In short, we play God. When we take away an individual's life or liberty by one of these arbitrary judgments we try to depersonalize the process by spreading responsibility for the decision throughout a framework of legal institutions. Thus, it is usually a jury, not a judge, that determines the death penalty; a local draft board, not a bureaucrat, that decides who goes to Vietnam. This second type of mechanism works only because there is widespread public acceptance that society has rights too, and that it is preferable that the power to enforce these rights over the rights of the individual be institutionalized.

I submit that the core of this ethical issue as it arises in clinical investigation lies in this second category—the one wherein, to ensure the rights of society, an arbitrary judgment must be made against an individual.

This is not to say that all ethical problems in clinical investigation fall into the irreconcilable category. On the contrary, in numerical terms most of them probably do not.

Without question, a considerable portion of the lapses in fully protecting individual rights

in clinical investigation can be avoided by more careful and open attention to the subject and by our ingenuity in developing new practices to attain some of the same old ends. This will prove quite costly in financial terms, but what is being accomplished in this way is very much to the good and is to be strongly encouraged. But there remains that hard core of the problem: the kind of situation in which it clearly seems to be in the best interests of society that the information be obtained. It can be obtained only from studies on certain already unlucky individuals, and no convincing case can be made that they can expect much in the way of benefits except those accruing to them as members of society.

Clearly there are three questions here: (1) From where does society get its rights or interest that makes it imperative to perform biomedical studies on an individual? (2) how is the individual subject selected? and (3) how are the social priorities decided?

The social priorities are easy; any small group of certified medical statesmen can settle them in an afternoon. As we all know, however, it is the other two questions that are so thorny.

Without too deep reflection it seems to me that society's actually having a right here is a relatively new phenomenon that is chiefly derived from the demonstration that knowledge gained by studies in a few humans can show us how to operate programs of great practical benefit to the group. Until the late nineteenth century, as I understand it, most human experimentation expanded knowledge but did not increase the power to control disease. The physicians of that day thus had no problem in maintaining the double ethical charge still preserved in the Helsinki Declaration: to "safeguard the health of the people," on the one hand, and to make the health of "my patient" the first consideration, on the other hand. But starting, I suppose, with the yellow fever studies in Havana, we have seen large social payoffs from certain experiments in humans, and there is no reason to doubt that the process could continue. It is by this demonstration, analogous to the great "invention of invention" of Newton's era, that medicine has given to society the case for its rights in the continuation of clinical investigation. Once this demonstration was made, we could no longer maintain, in strict honesty, that in the study of disease the interests of the individual are invariably paramount.

Yet we are temperamentally incapable of

leaving it at that. Our reflex action here is to try to imitate what we do when the same conflict arises in irreconcilable form elsewhere in our society. That is to say, we are willing to concede that some judgments must be arbitrary, but we attempt to clothe them with institutional forms so that at least the judgments are not made solely by one person. We will play God, but we would like to do it by group effort.

I am deeply convinced that such efforts provide no real solution because our culture has not yet faced up to the irreconcilable nature of the conflict at the heart of this particular issue. And until it does so, there exists no recognized consensus or article in the "social contract," if you will, to provide that base on which any law or regulation must rest if it is to be viable.

Conventional juridical procedures including the traditional jury system are too slow to fit the urgent nature of many clinical decisions. [B]y the terms of our culture, as may be seen in the Declaration of Helsinki, no matter who the investigator takes into partnership when he acts, he acts alone.

What can we do to solve this agonizing dilemma? Obviously we cannot convene a constitutional convention of the Judeo-Christian culture and add a few amendments to it. Yet, in a figurative sense, until we can do something very much like that, I believe deeply that the problem, at its roots, is unsolvable and that we must continue to live with it.

To be sure, by careful attention we can cut down the number of instances in which the problem presents itself to us in its starkest form. But there is no escape from the fact that, if the future good of society is to be served, there will be times when the clinical investigator must make an arbitrary judgment with respect to an individual. The necessity for such arbitrary judgments has had *tacit* social recognition and approval for some time. Because the approval was tacit, however, there was an imbalance of actions and words, in effect, a hypocrisy, that marvelous human invention by which we are enabled to adapt to problems judged to be not yet ripe for solution. By this hypocrisy society had its future medical interests fully protected. At the same time the attitude could be maintained that in medical matters, *as contrasted with those in many other walks of life*, the sole public interest was in the inviolability of the individual.

Now, most unfortunately, these essentially harmless hypocrisies of our culture have been codified. For the Helsinki Declaration . . . if

. . . followed to the letter . . . would produce the curious situation in which the only stated public interest is that of the individual. The future interest of society and its sometime conflict with the interest of the individual, in effect, are ignored. I believe it has been most unwise to try to extend the principle of "a government of laws and not men" into areas of such great ethical subtlety as clinical investigation.

When in our cultural evolution it has not yet been possible to develop an institutional framework for a particular kind of arbitrary decision that may affect an individual, there is only one basis on which to proceed, and that is on the basis of trust. My position may sound paternalistic, as indeed it is. Making arbitrary decisions concerning an individual in conflicts as yet unsolved by our society is one of the major responsibilities of a parent.

Society may not have given us a clear blueprint for clinical investigation, but it has long since given us immense trust to handle moral dilemmas of other sorts, including many in which, in effect, we have to play God. Thus, the moral dilemma of clinical investigation is not something new; what is new about the problem is its rapid increase in size. This rapid increase in size is no help to us now, but it may hasten the day, still far off, when in medical investigations we can institutionalize this making of arbitrary decisions between an individual and his society.

In the meantime we can do no more than carry on under the mantle of the trust we now possess. To continue to receive that trust we must be ever conscious that the issue of the individual vis-à-vis society is always *there*, and we can try our best to create an environment of awareness of it on our clinical services. For once a moral dilemma has become clearly recognized; whenever each person acts within that dilemma, his act can be seen for what it is, and the extent to which he has seemed to act with acceptable propriety can be judged.

But the hard core of our moral dilemma will not yield to the approaches of "Declarations" or "Regulations"; for as things stand today such statements must completely ignore the fact that society, too, has rights in human experimentation. Somehow, somewhere, in this question of human experimentation, as in so many other aspects of our society, we will have to learn how to institutionalize "playing God" while still maintaining the key elements of a free society.

\* \* \*

## NOTE 4.

HOWARD S. BECKER  
AGAINST THE CODE OF ETHICS\*

Sociologists face moral problems in every sphere of their lives. Most of these problems are shared with all men, many only with those who teach and do research, and relatively few with social scientists alone. To be of any use, a Code of Ethics for the American Sociological Association must deal successfully with the moral problems generic to social science. But these are precisely the problems dealt with least adequately by the draft code of ethics.

In dealing with general academic problems, the draft code can, for example, assert without equivocation that a professor should not appear as principal author of an article based on his student's dissertation. But in areas most characteristic of social science, the draft code cannot be as forthright. It cannot, for example, say that undercover research roles are not justified: it must equivocate by adding "unless they are clearly the only feasible means for reaching important scientific goals." Such equivocation only moves the problem back to a consideration of whether the scientific goals are important enough or truly cannot be reached in any other way, and helps us little.

The code is equivocal or unenlighteningly vague in dealing with most of the problems distinct to social science. This is so in spite of the intelligence and energy of the Committee on Ethics. It is so because there is no consensus about such problems; many sociologists would disagree with any more forthright statement in either direction.

The moral issues of our work, however, are important and deserve attention. I believe that they should be debated freely and fully rather than be obscured behind over-general "principles." I therefore wish to recommend that instead of adopting a code of ethics, the Association officially sponsor a symposium on the ethical problems characteristic of sociology.

Subsequently, the Association should undertake to publish the symposium, not as official doctrine, but as official recognition that ethical problems exist and that there are a number of ways of interpreting and coping with them. This would substantially advance our understanding of the complex moral issues of our craft.

\* \* \*

\* 29 *American Sociological Review* 409-410 (1964). Reprinted by permission.

## NOTE 5.

JAY KATZ  
THE EDUCATION OF  
THE PHYSICIAN-INVESTIGATOR\*

. . . The problems confronting modern medicine are the result not so much of the increase in research activities, but of the awareness that research and therapy, pursuit of knowledge and treatment, are not separate but intertwined. Therefore, the focus on experimentation and the concomitant emphasis on "informed consent" created new difficulties not merely because of the requirement to inform patient-subjects about proposed research, but also because consent raised troublesome questions about what *all* patients should be told about medical interventions.

It is a task of medical education to teach students how to deal with these emerging problems. Yet we have not explored in a systematic fashion whether education can serve medicine well as a method of control. I have often wondered how and by whom the Nazi concentration-camp experiments would have been conducted had the physician's responsibilities toward his patient-subject been exposed to careful scrutiny in the medical education of the nineteenth and early-twentieth centuries. The Nazi studies, despite opinions to the contrary, had their antecedents. There are, for example, reports of French and German experiments in which cancer tissue surgically removed from one diseased breast was transplanted into the apparently healthy other breast or experiments with variola vaccine performed on children in Sweden about which the experimenter commented that "perhaps I should have first experimented upon animals, but calves—most suitable for these purposes—were difficult to obtain because of their cost and their keep."

In my own medical education I have never forgotten Conrad Wesselhoeft's brilliant lecture on "The Care of the Patient." I turned over in my mind again and again one story he told—probably because its "lesson" did not satisfy me, though I was not aware of it then. He related one of his first experiences as a house officer. A patient with appendicitis had come to the emergency room and Dr. Wesselhoeft, once having made the diagnosis, recommended immedi-

\* 98 *Daedalus* 480, 484-487 (1969). Reprinted by permission of Daedalus, Journal of the American Academy of Arts and Sciences, Boston, Massachusetts.

ate surgery. The patient pleaded an important business engagement and asked that the operation be postponed for a few hours. Dr. Wesselhoeft reluctantly agreed and eventually the operation was successfully performed by him. That evening, at dinner, he related the story and noticed that his father, an eminent physician of his day, looked increasingly disturbed and did not say a word beyond sternly telling him: "Conrad, I want to see you in my study after dinner." There he chided his son severely for having taken too lightly his medical responsibilities by giving his patient leave for a few hours. Young Dr. Wesselhoeft felt that he had learned an important lesson that he wished to communicate to us. But what was the lesson? As I remember, he did not answer this question because it seemed so obvious.

I believe that I now know what troubled me then. Traditionally the concept of medical responsibility has been defined as responsibility for the patient's well-being. While such a definition could encompass, within the context of a medical relationship, concern for the patient's total functioning—physical, psychological, social, economic, spiritual—it is often limited to physical aspects. In order to exercise this more limited responsibility, patients must be carefully diagnosed, given the best treatment for their condition, and not be abandoned. Many students learn well to fulfill these obligations.<sup>20</sup> But there are other aspects to medical responsibility, and the controversy about what to disclose to patient-subjects in investigative settings has put one of them into sharper perspective—namely, the dialogue that should be pursued with the patient about treatment or no treatment or available alternative treatments in light of the risks, benefits, and prognosis as well as the totality of the

patient's life situation. Put another way, so long as medical responsibility primarily addressed itself to dispensing physical benefits, it was easier to view the physician as the sole decision-maker. Once physical benefits are placed in the web of the patient's total situation, the patient may have to be given a greater role in the decision-making process.

Not only does modern medicine command extensive therapeutic options—each with its known and unknown risks and benefits—but almost daily new therapeutic possibilities are introduced that have not yet met the test of time. Moreover, physicians are much more critical than they used to be about the efficacy of their therapeutic interventions. These considerations raise questions about the extent to which patients should participate in decisions affecting their health. To return briefly to Dr. Wesselhoeft's father, why did he seem so convinced that his son had not behaved with the utmost sense of professional responsibility?

Since medical experimentation is generally conducted with patients, the emphasis on "informed consent" raises questions about the nature of the dialogue between physicians and patients with respect to all interventions. This component of professional responsibility has not received the systematic attention it deserves. In medical commentary on professional responsibility, considerable agreement exists on the extent and limits of permissible physical interventions, but there is less consensus on or little systematic exploration into the dialogue that should take place between physicians and their patients. The Hippocratic Oath is silent on this point; it merely states that "I will follow that system of regimen which according to my ability and judgment, I consider for the benefit of my patients, and abstain from whatever is deleteri-

<sup>20</sup> Becker and his associates, on the basis of detailed observations of an entire medical school class, have described the development of the "responsibility perspective." They state that "basically the term [responsibility] refers to the archetypal feature of medical practice: the physician who holds his patient's fate in his hands and on whom the patient's life and death may depend." While concern with his "fate" could include the patient's entire functioning, the authors note that "two areas of activity seem most involved with questions of medical responsibility. The first consists . . . of arriving at a correct diagnosis on the basis of a thorough and accurate examination. . . . The second activity consists of performing diagnostic and therapeutic procedures containing some element of danger to the patient." The accompanying interview material sug-

gests that in these two activities concern for physical well-being is of primary or exclusive importance and that this aspect of medical responsibility is presented well and in great detail. In contrast, other aspects seem to be neglected. I found only one reference to the problem of disclosure. A student asked what to tell patients "who would die very shortly of an inoperable tumor. . . . The staff member gave a long and complex answer, pointing out that frequently patients figure it out for themselves, or on the other hand, didn't want to know anything about it. In either case the physician had no decision to make about whether to tell or not." See H. S. Becker, B. Geer, E. C. Hughes, and A. L. Strauss, *Boys in White* (Chicago: University of Chicago Press, 1961).

ous and mischievous." Dr. Thomas Percival, whose book *Medical Ethics* influenced profoundly the subsequent codifications of medical ethics in England and the United States, only commented once and in a very restricted fashion on the discourse between physicians and patients:

A physician should not be forward to make gloomy prognostications, because they savor of empiricism, by magnifying the importance of his services in the treatment or care of disease. But he should not fail, on proper occasions, to give to the friends of the patient, timely notice of danger, when it really occurs, and even to the patient himself, *if absolutely necessary* [emphasis supplied].

Nowhere in his detailed book, which carries the subtitle "A Code of Institutes and Precepts, Adapted to the Professional Conduct of Physicians and Surgeons," does Percival discuss the obligations of physicians to inform patients about the nature, purposes, and risks of therapeutic interventions. Nor does he comment on when a patient's consent should be obtained prior to a therapeutic intervention. Such omissions suggest

that Percival did not consider this issue important. And subsequent codes and commentaries by the American Medical Association have similarly treated this problem, if at all, briefly and without any elaboration.

These considerations put in question statements by commentators that experimental subjects are best safeguarded by the ethical training which investigators have received in their prior education as physicians. If disclosure and consent are posited as important problems for medical research, these commentators forgot to realize that physicians could not draw on systematic prior training. In an article on "Ethics and Clinical Research," Henry Beecher concluded that while "it is absolutely essential to strive" for informed consent, a "more reliable safeguard [in experimentation is] provided by the presence of an intelligent, informed, conscientious, compassionate, responsible investigator." One cannot quarrel with such a prescription, but has medicine trained physicians to analyze the complex and conflicting issues that medical decisionmaking often entails?

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## CHAPTER SIX

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# What Consequences to Subjects Should Affect the Authority of the Investigator?

This chapter examines those research conditions which give rise to tension between the actions of the investigator and the rights and interests of the subject. Since a primary concern in research is the protection of subjects against harm, our first analytic task is to specify the categories of harm which can result from experimental interventions. We have identified three major interests of the subject which, if interfered with, may cause him harm: (1) self-determination and privacy, (2) psychological integrity, and (3) physical integrity.

Though the categories of harm overlap, we treat them separately to highlight the distinctive problems which arise from each. For example, interferences with a subject's psychological or physical integrity may also violate his primary right to privacy and self-determination. Thus, experiments which employ deception may not only cause psychological harm but may also invade the subject's right to privacy and self-determination if he suffers unanticipated psychological exposure. Furthermore, we give special attention to interferences with self-determination because they are frequently overlooked in analyses of harm which have emphasized actual physical or psychological injury and neglected the impact of undisclosed manipulation. Accordingly, we include in the section on interferences with self-determination and privacy those interactions in which the subjects remain partially or completely uninformed about the nature, the impact, or even the existence of the investigation.

The remaining studies emphasize interferences with the subjects' psychological and physical integrity. Though in most of these instances the subjects were also unaware of some or all of the conditions inherent in the investigation, this is not an inevitable concomitant of these categories.

Clearly, a central task is to determine what actually constitutes harm. It has been argued that the minor risks or inconveniences to which some subjects are exposed do not rise to the level of harm and that in the absence of "direct personal injury" to subjects an investigator should be permitted to proceed on his own initiative. Alternatively it has been suggested that even if an experiment involves substantial risks, the investigator is still best situated to weigh those risks against the benefits which the research may provide. In any event, whatever the authority of the investigator, criteria for identifying the nature and extent of harm need to be articulated.

Once categories of harm have been identified, we turn to other elements of experimental objectives and design which may bear on extending or limiting the investigator's authority. We present ten such factors. Some focus on the investigator, such as his values, his attitude toward and method of selecting subjects, and the limitations on his ability to predict the consequences of his work. Others are addressed to the subject's role in the process: whether he is made aware of his participation in an experiment and of the risks and manipulation involved, as well as whether the experiment benefits him. Finally, we consider the impact of the pursuit of knowledge and other professional and societal interests on the investigator's authority. The relevance of these factors must be appraised separately for each category of harm and evaluated in terms of their bearing on the decisions that have to be made at each stage—formulation, administration, and review—in the human experimentation process.

Commentators on research with human beings disagree about the extent to which decisions about experimentation can be left to the discretion of investigators. The materials in this and the succeeding chapter delineate the investigator's values and specify those factors which he is best, and least, able to evaluate alone. Accordingly, they provide a basis for deciding when others, besides the investigator, ought to make decisions about human experimentation.

In examining these materials consider the following questions:

1. Do the categories of harm—interferences with self-determination, privacy, psychological integrity, and physical integrity—draw meaningful distinctions, and do they suggest different consequences for the authority of the investigator?
2. For each category of harm, whenever relevant, how should the authority of the investigator be affected by:
  - (a) The degree of harm—*e.g.*, slight, moderate or severe; reversible or irreversible?
  - (b) The type of harm—*e.g.*, to body, emotions, conduct, character, personal and societal values, dignity, anonymity, or reputation?
  - (c) The explanation of harm—*e.g.*, disclosure or nondisclosure about being a participant or about specific aspects of the design?
  - (d) The balancing of harm—*e.g.*, against benefits to subject, science, or society?



(e) The recipients of harm—*e.g.*, subjects who are considered worthy, inferior, normal, or deviant?

(f) The awareness of harm—*e.g.*, the ability to know, predict, and comprehend consequences?

3. For each element in the previous question, what difference does it make if the subject accepts the harm?

## A. What Constitutes Harm?

### 1.

#### Interferences with Self-Determination and Privacy

##### a. Unawareness about Being a Participant in Research\*

[i]

*Laud Humphreys*  
*Tearoom Trade—*

*Impersonal Sex in Public Places†*

The methods employed in this study of men who engage in restroom sex are the outgrowth of three ethical assumptions: First, I do not believe the social scientist should ever ignore or avoid an area of research simply because it is difficult or socially sensitive. Second, he should approach any aspect of human behavior with those means that least distort the observed phenomena. Third, he must protect respondents from harm—regardless of what such protection may cost the researcher.

Because the majority of arrests on homosexual charges in the United States result from encounters in public restrooms, I felt this form of sexual behavior to provide a legitimate, even essential, topic for sociological investigation. In our society the social control forces, not the criminologist, determine what the latter shall study.

Following this decision, the question is one of choosing research methods which permit the

investigator to achieve maximum fidelity to the world he is studying. I believe ethnographic methods are the only truly empirical ones for the social scientist. When human behavior is being examined, systematic observation is essential; so I had to become a participant-observer of furtive, felonious acts.

Fortunately, the very fear and suspicion of tearoom participants produces a mechanism that makes such observation possible: a third man (generally one who obtains voyeuristic pleasure from his duties) serves as a lookout, moving back and forth from door to windows. Such a "watchqueen," as he is labeled in the homosexual argot, coughs when a police car stops nearby or when a stranger approaches. He nods affirmatively when he recognizes a man entering as being a "regular." Having been taught the watchqueen role by a cooperating respondent, I played that part faithfully while observing hundreds of acts of fellatio.

\* \* \*

Although primarily interested in the stigmatized behavior, I also wanted to know about the men who take such risks for a few moments of impersonal sex. . . .

\* \* \*

How could I approach these covert deviants for interviews? By passing as deviant, I had observed their sexual behavior without disturbing it. Now, I was faced with interviewing these men (often in the presence of their wives) without destroying them.

\* \* \*

To overcome the danger of having a subject recognize me as a watchqueen, I changed my hair style, attire and automobile. At the risk of losing more transient respondents, I waited a

\* For examples from medicine, see The Jewish Chronic Disease Hospital Case, pp. 9-65 *supra* and H. K. Beecher: Ethics and Clinical Research, pp. 307-310 *supra*.

† 7 *Trans-action* 15 (January 1970). Reprinted by permission.

year between the sample gathering and the interviews, during which time I took notes on their homes and neighborhoods and acquired data on them from the city and county directories.

\* \* \*

This study, then, results from a confluence of strategies: systematic, first-hand observation, in-depth interviews with available respondents, the use of archival data, and structured interviews of a representative sample and a matched control group. At each level of research, I applied those measures which provided maximum protection for research subjects and the truest measurement of persons and behavior observed.

### NOTES

#### NOTE 1.

NICHOLAS VON HOFFMAN  
SOCIOLOGICAL SNOOPERS\*

We're so preoccupied with defending our privacy against insurance investigators, dope sleuths, counterespionage men, divorce detectives and credit checkers, that we overlook the social scientists behind the hunting blinds who're also peeping into what we thought were our most private and secret lives. But they are there, studying us, taking notes, getting to know us, as indifferent as everybody else to the feeling that to be a complete human involves having an aspect of ourselves that's unknown.

If there was any doubt about there being somebody who wants to know about anything any other human being might be doing it is cancelled out in the latest issue of *Trans-action*, a popular but respected sociological monthly. The lead article, entitled "Impersonal Sex in Public Places," is a resumé of a study done about the nature and pattern of homosexual activities in men's rooms. Laud Humphreys, the author, is an Episcopal priest, a duly pee-aich-deed sociologist, holding the rank of assistant professor at Southern Illinois University.

\* \* \*

Most of the people Humphreys observed and took notes on had no idea what he was doing or that they, in disguised form, would be showing up in print at some time in the future. Of

all the men he studied only a dozen were ever told what his real purpose was, yet as a sociologist he had to learn about the backgrounds and vital facts of the other tearoom visitors he'd seen. To do this Humphreys noted their license numbers and by tracing their cars learned their identities. He then allowed time to pass, disguised himself and visited these men under the color of doing a different, more innocuous door-to-door survey.

\* \* \*

Humphreys said that he did everything possible to make sure the names of the men whose secrets he knew would never get out: "I kept only one copy of the master list of names and that was in a safe deposit box. I did all the transcribing of taped interviews myself and changed all identifying marks and signs. In one instance, I allowed myself to be arrested rather than let the police know what I was doing and the kind of information I had."

Even so, it remains true that he collected information that could be used for blackmail, extortion, and the worst kind of mischief without the knowledge of the people involved. *Trans-action* defends the ethics of Humphreys' methodology on the basis of purity of motive and the argument that he was doing it for a good cause, that is getting needed, reliable information about a difficult and painful social problem.

Everybody who goes snooping around and spying on people can be said to have good motives. The people whom Sen. Sam Ervin is fighting, the ones who want to give the police the right to smash down your door without announcing who they are if they think you have pot in your house, believe they are well motivated. They think they are preventing young people from destroying themselves. J. Edgar Hoover unquestionably believes he's protecting the country against subversion when he orders your telephone tapped. Those who may want to overthrow the government are just as well motivated by their lights. Since everybody can be said to be equally well motivated, it's impossible to form a judgment on what people do by assessing their intentions.

To this Laud Humphreys replies that his methods were less objectionable than getting his data by working through the police: "You do walk a really perilous tightrope in regard to ethical matters in studies like this, but, unless someone will walk it, the only source of information will be the police department, and that's danger-

\* *The Washington Post* B1, col. 1; B9, col. 5 (January 30, 1970). Reprinted by permission. © 1970, The Washington Post.

ous for a society. The methods I used were the least intrusive possible. Oh, I could have hidden in the ceiling as the police do, but then I would have been an accomplice in what they were doing."

\* \* \*

Incontestably such information is useful to parents, teen-agers themselves, to policemen, legislators and many others, but it was done by invading some people's privacy. This newspaper could probably learn a lot of things that the public has a right and need to know if its reporters were to use disguises and the gimmickry of modern, transistorized, domestic espionage, but there is a policy against it. No information is valuable enough to obtain by nipping away at personal liberty, and that is true no matter who's doing the gnawing, John Mitchell and the conservatives over at the Justice Department or Laud Humphreys and the liberals over at the Sociology Department.

#### NOTE 2.

IRVING LOUIS HOROWITZ AND LEE RAINWATER  
JOURNALISTIC MORALIZERS\*

Columnist Nicholas Von Hoffman's quarrel with Laud Humphreys' "Impersonal Sex in Public Places" starkly raises an issue that has grown almost imperceptibly over the last few years, and now threatens to create in the next decade a tame sociology to replace the fairly robust one that developed during the sixties. . . .

\* \* \*

Sociologists have tended to assume that well-intentioned people fully accept the desirability of demystification of human life and culture. In the age of Aquarius, however, perhaps such a view will be recognized as naive.

"They are there, studying us, taking notes, getting to know us, as indifferent as everybody else to the feeling that to be a complete human involves having an aspect of ourselves that's unknown." Von Hoffman seems to mean this to be a statement about the right to privacy in a legal sense, but it really represents a denial of the ability of people to understand themselves and each other in an existential sense. This denial masks a fear, not that intimate details of our lives will be revealed to *others*, but rather that

we may get to know *ourselves* better and have to confront what up to now we did not know about ourselves. Just as psychoanalysis was a scientific revolution as threatening to traditional conceptions as those of Galileo and Kepler had been, it may well be that the sociologist's budding ability to say something about the how's and why's of men's relationships to each other is deeply threatening not only to the established institutions in society, but also in a more personal way to all members of society.

\* \* \*

Von Hoffman recognizes that his most appealing charge has to do with privacy, and so he makes much of the fact that Humphreys' collected information that could be used for "blackmail, extortion, and the worst kind of mischief without the knowledge of the people involved."

Here his double standard is most glaringly apparent. Journalists routinely, day in, day out, collect information that could be used for "blackmail, extortion, and the worst kind of mischief without the knowledge of the people involved." But von Hoffman knows that the purpose of their work is none of those things, and so long as their information is collected from public sources, I assume he wouldn't attack them. Yet he nowhere compares the things sociologists do with the things his fellow journalists do. Instead, he couples Humphreys' "snooping around," "spying on people" with similarly "well-motivated" invaders of privacy as J. Edgar Hoover and John Mitchell.

To say the least, the comparison is invidious; the two kinds of enterprises are fundamentally different. No police group seeks to acquire information about people with any other goal than that of, in some way, prosecuting them. Policemen collect data, openly or under cover, in order to put someone in jail. Whatever it is, the sociological enterprise is not that. Sociologists are not interested in directly affecting the lives of the particular people they study. They are interested in those individuals only as representatives of some larger aggregate—in Humphreys' case, all participants in the tearoom action. Therefore, in almost all sociological research, the necessity to preserve the anonymity of the respondent is not an onerous one, because no purpose at all would be served by identifying the respondents.

\* \* \*

\* 7 *Trans-action* 5-8 (May 1970). Reprinted by permission.

Von Hoffman's points are: that in studying the sexual behavior of men in restrooms, Humphreys violated their rights to intimacy and privacy; that the homosexuals were and remain unaware of the true purpose of Humphreys' presence as a lookout; and that in the follow-up questionnaire the researcher further disguised himself and the true nature of his inquiry. For von Hoffman the point of principle is this: that although Humphreys' intent may have been above reproach and that in point of fact his purposes are antithetical to those of the police and other public officials, he nonetheless in his own way chipped away at the essential rights of individuals in conducting his investigations. Therefore, the ends, the goals, however noble and favorable to the plight of sexual deviants, do not justify the use of any means that further undermine personal liberties. Let us respond to these propositions as directly as possible.

First, the question of the invasion of privacy has several dimensions. We have already noted the public rather than the private nature of park restrooms. It further has to be appreciated that all participants in sexual activities in restrooms run the constant risk that they have among them people who have ulterior purposes. The vocabulary of motives is surely not limited or circumscribed by one man doing research but is as rich and as varied as the number of participants themselves. The fact that in this instance there was a scientific rather than a sexual or criminal "ulterior motive" does not necessarily make it more hideous or more subject to criticism, but perhaps less so.

Second, the question of disguising "the true nature" and purpose of this piece of research has to be put into some perspective. To begin with, let us assume that the research was worth doing in the first place. We know almost nothing about impersonal sex in public places, and the fact that we know so little has in no small way contributed to the fact that the cops feel that *they* know all that needs to be known about the matter. Who, then, is going to gather this countervailing knowledge? . . . Moreover, to assume that the investigator must share all of his knowledge with those being investigated also assumes a common universe of discourse very rarely found in any kind of research, much less the kind involving sexual deviance. Furthermore, the conduct of Humphreys' follow-up inquiries had to be performed with tact and with skill precisely because he discovered that so many of the people in his survey were married men

and family men. Indeed, one of the great merits of Humphreys' research is that it reveals clearly etched class, ethnic, political and occupational characteristics of sexual participants never before properly understood. Had he not conducted the follow-up interviews, we would once again be thrown back on simpleminded, psychological explanations that are truly more voyeuristic than analytic, or on the policeman's kind of knowledge. It is the sociological dimensions of sexuality in public places that make this a truly scientific breakthrough.

To take on the ethic of full disclosure at the point of follow-up interviews was impossible given the purposes of the research. If Humphreys had told his respondents that he knew they were tearoom participants, most of them would have cooperated. But in gaining their cooperation in this way he would have had to reveal that he knew of their behavior. This he could not responsibly do, because he could not control the potentially destructive impact of that knowledge. . . . Therefore, the posture of Humphreys toward those interviewed must be viewed as humane and considerate.

But what von Hoffman is arguing is that this research ought not to have been done, that Humphreys should have laid aside his obligation to society as a sociologist and taken more seriously his obligation to society as a citizen. Von Hoffman maintains that the researcher's intentions—the pursuit of truth, the creation of countervailing knowledge, the demystification of shadowy areas of human experience—are immaterial.

\* \* \*

The only interesting issue raised by von Hoffman is one that he cannot, being a moralizer, do justice to. It is whether the work one does is good, and whether the good it does outweighs the bad. "No information," he writes, "is valuable enough to obtain by nipping away at personal liberty. . . ." It remains to be proven that Humphreys did in fact nip away at anyone's liberty; so far we have only von Hoffman's assertions that he did and Humphreys' assurance that he did not. But no amount of self-righteous dogmatizing can still the uneasy and troublesome thought that what we have here is not a conflict between nasty snoopers and the right to privacy, but a conflict between two goods: the right to privacy and the right to know.

What is required is a distinction between the responsibilities of social scientists to seek

and to obtain greater knowledge and the responsibilities of the legal system to seek and obtain maximum security for the private rights of private citizens. . . .

It is certainly not that sociologists should deliberately violate any laws of the land, only that they should leave to the courtrooms and to the legislatures just what interpretation of these laws governing the protection of private citizens is to be made. . . . The really tough moral problem is that the idea of an inviolable right of privacy may move counter to the belief that society is obligated to secure the other rights and welfare of its citizenry. Indeed one might say that this is a key contradiction in the contemporary position of the liberal: he wants to protect the rights of private citizens, but at the same time he wants to develop a welfare system that could hardly function without at least some knowledge about these citizens.

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#### NOTE 3.

PAVESICH v. NEW ENGLAND LIFE INSURANCE CO.  
122 GA. 190, 193-198, 50 S.E. 68, 69-71 (1905)

COBB, J. . . . The question . . . to be determined is whether an individual has a right of privacy which he can enforce, and which the courts will protect against invasion. It is to be conceded that prior to 1890 every adjudicated case, both in this country and in England, which might be said to have involved a right of privacy, was not based upon the existence of such right, but was founded upon a supposed right of property, or a breach of trust or confidence, or the like, and that therefore a claim to a right of privacy, independent of a property or contractual right, or some right of a similar nature, had, up to that time, never been recognized in terms in any decision. . . . The individual surrenders to society many rights and privileges which he would be free to exercise in a state of nature, in exchange for the benefits which he receives as a member of society. But he is not presumed to surrender all those rights, and the public has no more right, without his consent, to invade the domain of those rights which it is necessarily to be presumed he has reserved, than he has to violate the valid regulations of the organized government under which he lives. The right of privacy has its foundation in the instincts of nature. It is recognized intuitively, consciousness being the witness that can be called to establish its existence. Any person whose intellect is in a normal

condition recognizes at once that as to each individual member of society there are matters private, and there are matters public so far as the individual is concerned. Each individual as instinctively resents any encroachment by the public upon his rights which are of a private nature as he does the withdrawal of those of his rights which are of a public nature. A right of privacy in matters purely private is therefore derived from the natural law. . . . It is one of those rights referred to by some law writers as "absolute"—"such as would belong to their persons merely in a state of nature, and which every man is entitled to enjoy, whether out of society or in it."—Blackstone 123.

. . . An individual has a right to enjoy life in any way that may be most agreeable and pleasant to him, according to his temperament and nature, provided that in such enjoyment he does not invade the rights of his neighbor, or violate public law or policy. The right of personal security is not fully accorded by allowing an individual to go through life in possession of all of his members, and his body unmarred; nor is his right to personal liberty fully accorded by merely allowing him to remain out of jail, or free from other physical restraints. The liberty which he derives from natural law, and which is recognized by municipal law, embraces far more than freedom from physical restraint. The term *liberty* is not to be so dwarfed, "but is deemed to embrace the right of a man to be free in the enjoyment of the faculties with which he has been endowed by his Creator, subject only to such restraints as are necessary for the common welfare. . . ."

\* \* \*

[A]ncient law recognized that a person had a legal right "to be let alone," so long as he was not interfering with the rights of other individuals or of the public. This idea has been carried into the common law, and appears from time to time in various places. . . . "Eavesdroppers, or such as listen under walls or windows or the eaves of a house to hearken after discourse, and thereupon to frame slanderous and mischievous tales," were a nuisance at common law, and indictable, and were required, in the discretion of the court, to find sureties for their good behavior. (4 Blackstone 168.) The offense consisted in lingering about dwelling houses and other places where persons meet for private intercourse, and listening to what is said, and then tattling it abroad. . . . Instances might be multi-

plied where the common law has both tacitly and expressly recognized the right of an individual to repose and privacy. . . .

\* \* \*

NOTE 4.

EDWARD A. SHILS  
SOCIAL INQUIRY AND THE AUTONOMY  
OF THE INDIVIDUAL\*

\* \* \*

The respect for privacy rests on the appreciation of human dignity, with its high evaluation of individual self-determination, free from the bonds of prejudice, passion, and superstition. In this, the respect for human dignity and individuality shares an historical comradeship with the freedom of scientific inquiry, which is equally precious to modern liberalism. The tension between these values, so essential to each other in so many profoundly important ways, is one of the antinomies of modern liberalism. The ethical problems with which we are dealing . . . arise from the confrontation of autonomy and privacy by a free intellectual curiosity, enriched by a modern awareness of the depth and complexity of the forces that work in us and implemented by the devices of a passionate effort to transform this awareness into scientific knowledge.

\* \* \*

[ii]

Mortimer A. Sullivan, Jr., Stuart A. Queen,  
and Ralph C. Patrick, Jr.

*Participant Observation as Employed in the  
Study of a Military Training Program*†

Until recently the Air Force included in its research and development planning an extensive social science program. This program, itself part of a larger and more elaborate organization devoted to the Air Force's personnel and training requirements, utilized in its studies classical experimental design, polling, the interview, and, occasionally, observation and the ethnographic or survey approach. There existed, however, certain aspects of the Air Force training situa-

tion which apparently could not adequately be understood through the use of these techniques. In particular, certain officers wished to gain a better notion of how basic and technical training were lived, understood, and felt by new airmen. Hence, after a year of preliminary study, a plan was drawn up and approved for the utilization of a participant observer.<sup>1</sup>

The general purpose of the study was to gain insight into the motivations and attitudes of personnel (in training) as reflected in both their military and social behavior. Through such insight into airmen's own views and feelings it was hoped to find leads to new ways of reducing disciplinary problems (particularly AWOL), failures in the course of training, poor performance thereafter, and non-re-enlistment. . . .

To accomplish this purpose it was decided that a research officer should "enlist" as a basic trainee. He would be a fullfledged member of the group under study, his identity, mission, and role as a researcher unknown to every one (except the investigators), even to his own commanding officer. This then became one of the few cases of real participant observation.

There were literally thousands of problems to overcome, not only in deciding how the study would be conducted, but also in determining how the participant-observer would be guided in his work, the things to be looked for or recorded if observed, the form reports should take, and how the data would be used after the study was completed. . . .

\* \* \*

The preliminary arrangements for the "enlistment" of the observer and the recording and transporting of data were well taken care of by high-ranking Air Force personnel. The provost marshal of the command for which the study was undertaken worked closely with those primarily concerned in providing the needed support and information, and the Air Force's so-

\* Daniel Lerner, ed.: *The Human Meaning of the Social Science*. New York: Meridian Books, Inc. 114, 120-121 (1959). © 1959 by the World Publishing Company. Reprinted by permission.

† 23 *American Sociological Review* 660-667 (1958). Reprinted by permission.

<sup>1</sup> Participant observation is defined by Florence R. Kluckhohn as "... conscious and systematic sharing, insofar as circumstances permit, in the life-activities and, on occasion, in the interests and affects of a group of persons. Its purpose is to obtain data about behavior through direct contact and in terms of specific situations in which the distortion that results from the investigator's being an outside agent is reduced to a minimum." "The Participant-Observer Technique in Small Communities," 46 *American Journal of Sociology* (November 1940), p. 331.

cial science agency which guided the study made available a capable member of its organization, a civilian sociologist, to oversee and coordinate the research.

\* \* \*

In addition to the other team members, the provost marshal, and the Air Force's civilian sociologist, there were many individuals who contributed to the study. After the participant-observer left the South and arrived at the technical training base, he was told that an additional person had been informed of his presence and of his mission, a young chaplain who had had enlisted service in World War II and whose primary duty, in addition to ministerial responsibilities, was counseling newly arrived trainees. The chaplain contributed to the investigation not only through his familiarity with the training situation, but also by his personal interest in the problems of the observer. While the observer came to rely heavily upon the team for professional guidance whenever they met, he also depended upon the chaplain as his sole contact between meetings with the other team members.

The creation of a "new personality" for the observer was of some importance to the study. It would have been entirely possible for him to have "enlisted" and undergone training without disguising his name, age, or education. On the other hand, it appeared advantageous to provide the observer with an identity through which he might achieve a maximum of rapport with other trainees—most of whom, it was known, were under twenty years old and few of whom had any college education. . . .

For nine months before the beginning of the field study itself, the observer was coached in the ways of the adolescent subculture. A young airman was told the requirements of the study and given the job of creating a "new personality" for the observer. Dress, speech, and mannerism, as well as interests, attitudes, and general appearance were "corrected" by the observer's enthusiastic coach. . . . So successful was the airman's tutoring that when the time for "enlistment" arrived, the recruiting sergeant (who did not know of the study) suggested that the observer not be accepted by the Air Force because by all appearances he was a juvenile delinquent. . . .

\* \* \*

In deliberately cultivating a second self the research observer was engaged in something su-

perficially like intelligence work or espionage. But there was a very important difference in goal for, in this case, it was a general understanding of a significant subculture, the processes of its development and transmission to new recruits, and its effect on the official training program. It was not the indictment of anybody or the immediate change of anyone's behavior. In fact, the data were so safeguarded that they could not lead to disciplinary action against any of the men under study. Neither was the objective a general indictment or defense of the Air Force. It was simply to gather a body of previously unavailable information and to interpret in it a way that might be helpful both to the military and to social scientists.

\* \* \*

[T]he most interesting phenomenon to the participant-observer was the ease with which he was able to carry out his role with the other trainees. The men not only accepted him and his cover story, but identified many aspects of his past as being similar to their own lives. The observer shared the sorrows and hopes of the other trainees and felt compelled to do his best out of loyalty to them. When the others learned of "tricks" by which to pass inspections or to give the appearance of doing a job which they had actually not done, the observer joined in and suffered no guilt for doing what he, as an officer, knew was "wrong." The observer is convinced that his complete integration into the trainees' subculture was essential for understanding and conveying the attitudes and problems which he reported. However, he also attaches importance to the professional guidance given by the other team members and the counsel and reassurance which they and the chaplain offered.

\* \* \*

. . . The method of participant observation was adopted in this case only after responsible Air Force personnel believed they had obtained about all they could from general observation, questionnaires, and formal interviews. In addition, they suspected that airmen, like other human beings, could and did maintain "false fronts," often deceiving officers, researchers, and perhaps themselves. Here seemed to be a new approach that might probe beneath the surface in a revealing way. . . .

\* \* \*



## NOTES

## NOTE 1.

JULIUS A. ROTH

## DANGEROUS AND DIFFICULT ENTERPRISE?\*

The article on participant observation in a military program is remarkable to me in a number of respects.

For one thing, the authors make it sound as if the undercover type of participant observation is extremely rare. Yet, without any careful search, I can think of a number of studies of this type in recent years. There is Mann's article on the marine radioman, Caudill's study of the psychiatric patient's role, the book *Why Prophecy Fails* by Festinger, Riecken, and Schachter. A number of dissertations on occupational groups at the University of Chicago are based entirely or largely on information collected by students who worked at the job they studied without revealing their professional interest in their fellow workers. . . . Some of these people took their jobs mainly to make money to pay tuition and living expenses, and then decided to use the opportunity to collect data for a study of an occupation. But the priority of their other motives does not detract from the value of their observations. In my own current study of the social psychology of the treatment of tuberculosis, I have collected my most valuable data while a tuberculosis hospital patient and later as a tuberculosis hospital attendant, in each case keeping daily notes on my observations and experience without the knowledge of the persons [being observed].

In the second place, Sullivan, Queen, and Patrick make such secret observation sound extraordinarily difficult. Note the elaborate nine-month preparation of their observer. To cite again my own experience, I made no preparation and took no training for the observational roles I played. For example, my vocabulary and English usage were much better than those of my fellow patients and, later on, of my fellow attendants and even of the nurses who supervised us. Yet I made no attempt to disguise my speech or modify my vocabulary and I had no difficulty mixing comfortably with my temporary colleagues. The several University of Chicago sociologists I know who have engaged in such "undercover research" never mentioned making any special preparation for their participant role

and, with one exception, had no difficulty carrying out their study.

I am quite sure that the observer airman could readily have stepped into his role despite some deviations from the "average" without any noticeable effect on his observations. I believe these researchers have entirely too narrow a conception of people's tolerance of somewhat deviant behavior.

It is my opinion that for studying the dynamics of complex social interaction there is no substitute for participation in the activities of the group (or groups) in which one is interested. I am afraid that the . . . article is more likely to scare off social scientists who have had no experience with this approach than it is to win more recruits. I want to say to such people: It's not nearly as difficult as they make it sound. Give it a try yourself.

## NOTE 2.

HENRY W. RIECKEN

## THE UNIDENTIFIED INTERVIEWER\*

[A] major problem for the participant-observers in this study† was to . . . avoid exerting influence on the beliefs and actions of the members. We wished especially to avoid doing or saying anything that would affect the extent of proselyting; but we also wanted to avoid increasing or decreasing the conviction and the commitment of the members.

From our very first contact with the chief figures it was apparent that a study could not be conducted openly. The leaders had not yet adopted a policy of secrecy and exclusion, but they were at that time neither seeking publicity nor recruiting converts. Rather, their attitude can be best described as one of passive accept-

\* 62 *American Journal of Sociology* 210-212 (1956). Reprinted by permission.

† This study concerned an apocalyptic group which "had gathered around a middle-aged housewife in a suburb of an American city. She believed that, through 'automatic writing,' she had received a number of messages from beings dwelling in outer space, forecasting the destruction of the earth by flood on a certain date. She made known this prediction among her acquaintances, and a number of people began calling on her regularly to be instructed in the 'lessons' from outer space and to discuss the possibilities of salvation. Most 'members' were adult men and women, of between about twenty to about fifty-five years of age, of middle socio-economic status and well educated, all but two having at least attended college." *Ibid.* at 210.

\* 24 *American Sociological Review* 398 (1959). Reprinted by permission.



ance of individuals who came to call and seemed to be interested in the messages from outer space. Our observers were welcomed politely, and their questions were answered, for the most part, fully, but they were not proselytized vigorously or enlisted to spread the word. . . .

. . . In gathering the data . . . the observers tried to be non-directive, sympathetic listeners—passive participants who were inquisitive and eager to learn whatever others might want to tell them. But such a role was not without its difficulties.

In the first place, the passive-member role greatly hampered inquiry. . . . Second, while the attitude we strove for was easy enough for an observer to take during his first few contacts, it became increasingly difficult to maintain as he began to be seen as a "regular." Non-directive inquiry about others, while revealing little about one's own feelings or actions, is appropriate enough behavior for a newcomer, but, if prolonged, it tends to cast doubt on either the intelligence or the motives of interrogator. . . .

Nearly every conversation he had with a member about his conviction, commitment, or proselytizing presented the observer with an unsought opportunity to influence the other; for it is difficult, outside the interviewer's role, to inquire of an individual how he feels about a matter without having him return the question. . . . The pressure on observers to take part in the process of mutual support and confirmation was ever present and often strong.

. . . The observers were forced . . . to present the appearance of agreement with the major beliefs of the group. While they avoided taking strong stands on these issues and never voluntarily or spontaneously spoke up to reinforce conviction, their general air of acceptance as well as their mere presence and interest in the affairs of the group undoubtedly had some strengthening effect on the conviction of the others. The goal of avoiding influence completely, proved unrealistic, for, in order to remain members and yet gather the necessary data, the observers had to offer some support to the members' convictions. And this, while indeed minimal, must have had some effect.

\* \* \*

It is hard to estimate the effect of this apparent commitment by the observers. On the one hand, it probably reinforced members' convictions and confidence that they had been right in making whatever commitments they had made;

on the other hand, the observers' commitments may have made those of other members seem either more or less important. The perceived amount of the observers' commitment probably ranged from moderate to slight: there were at least two members whose commitment was less than that of any observer and at least four or five who exceeded that of any observer. For the latter, the lesser commitment of the observers probably made their own seem greater and more binding, whereas the former probably perceived their commitment to be even slighter in contrast to the observers'. In short, for most of the group the observers' investment in group activity was supportive, but the amount of support varied.

\* \* \*

[F]rom our experience, it seems likely that observers cannot avoid exercising *some* influence on behavior and beliefs. The conflict in roles and its attendant consequences seem to be inherent in the process of doing a study such as this, although it may be possible to devise better ways of handling the conflict and of further reducing observer effect. . . .

[iii]

Norman Lefstein

*Experimental Research in the Law—Ethical and Practical Considerations\**

\* \* \*

In the juvenile courts of two metropolitan communities a study of attorneys and their effects on the attitudes and behavior of boys accused of delinquency is being made. Initially, plans for this program—termed the lawyer project—were limited primarily to the opening of special legal aid offices to serve the two juvenile courts involved. Before the project commenced, however, an extensive program to evaluate counsel's effects—not originally planned—was developed.

An experimental method with random assignment to groups, specifically the "Solomon Four-Group Design," is employed in the study. Accordingly, in one of two treatment (lawyer) groups, boys are interviewed prior to an attorney's entry into the case; in one of two control (no lawyer) groups, boys are interviewed prior to their appearance in court. In all four groups the boys are interviewed at a designated interval

\* Unpublished manuscript 1-3, 6-13 (1967). Printed by permission of the author who retains all rights.

after their court appearance. The interviews, which are based upon a structured questionnaire, are administered by a trained staff of field workers. The major content areas of the questionnaire include a boy's knowledge and attitude toward the law and legal process, his readiness to accept or reject court imposed sanctions, and his attitudes toward various court participants. Representation for boys in the treatment (lawyer) group is provided by one of the three full-time staff attorneys associated with the project's legal office.

The research activities are totally separated from operation of the lawyer project office. For example, the interviewers who operate out of their own headquarters have never been informed of the relationship between the lawyer project and their interviewing, thus minimizing the risk of their communicating to the boys that the questions asked are related directly to attorney representation in juvenile court. The lawyers do not know, nor will they be informed until the project's completion, of the precise content of the interview. Also, the lawyers are not told which of the boys they represent have been interviewed prior to the court hearing.

The families of boys in the lawyer group are specifically offered the services of a project attorney. They may, however, refuse the representation tendered and appear in juvenile court without an attorney or with an attorney of their choosing. The lawyer project, therefore, is experimental insofar as it "offers" to a randomly selected group of juveniles the opportunity to be represented by the project's attorneys. The control group consists of an equal number of randomly designated youths who are not offered the services of a project attorney, but who are free to obtain representation through the normal legal services available in the project cities. While these procedures detract from the project's experimental rigor, they tend to obviate a critical objection which otherwise certainly would be leveled, i.e., that the program forces some boys, who may not want an attorney, to accept representation, and deprives of counsel others who may wish to have a lawyer's services.

Implementation of the research design required a series of difficult administrative decisions, each of which was deemed essential to insure validity of the controlled experiment. Moreover, several of the decisions, much like the original choice of an experimental method, raised practical as well as ethical questions. . . .

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*Representation Limited to Experimental Cases.* In order to insure external validity, the project lawyer offices have been made to resemble those of typical legal aid programs. The project lawyers, just like other attorneys furnishing legal aid in juvenile courts, become involved in delinquency cases after a court complaint has been filed. In one important respect, however, the lawyer project had to differ from typical legal services programs. This difference, though not affecting external validity, further serves to illustrate how the experimental design has influenced the action program. Normally legal aid attorneys will accept appointments to cases assigned by the court and will provide representation at the request of indigent persons referred for legal services. This practice could not be allowed with the lawyer project, for it would seriously have threatened operation of the research by requiring the attorneys to spend too much of their time working on non-experimental cases. Accordingly, the juvenile courts agreed not to appoint the project's lawyers to indigent cases in which counsel was requested. Similarly, referral cases are not accepted; persons seeking legal representation are directed to other legal services in the community or to the juvenile court.

*Form Letter and Field Representative.* Implementation of the research design required permission from the juvenile courts to allow daily inspection of all new delinquency filings, for random assignment to treatment and control groups is made possible with this information. Permission also was obtained for a form letter to be mailed to the parents of boys assigned to the treatment group informing them that they might have a project lawyer represent their son. The letter states that the project is authorized to provide representation without cost, and encourages parents to phone the project office to arrange an appointment with one of the staff attorneys. When the form letter fails to bring a response, a field representative employed by the project, whose function also was approved by the juvenile courts, visits the parents to determine whether they will accept legal representation for their child. . . .

Beyond complexities of administering experimental research programs in legal areas, there are broader, more difficult questions, such as the ethical propriety of doling out different treatment to random subjects without their consent, and failing to furnish complete information about the experiment to persons connected with

it. In the lawyer project, there are several instances during the processing of cases in which information is concealed from subjects of the experiment—indeed must not be revealed if the study is to have validity. For example, when the field representative contacts the child and his parents, he informs them that they may be represented by a project attorney; no mention is made of how the boy's name was obtained from juvenile court, that the effects of lawyer representation on the youth will be studied, and that a comparable control group of boys also is being studied. Similarly, when the lawyer speaks with the youth and his parents, the overall purposes of the project are not revealed. Although the attorneys know of the interviewing and its relationship to their representation, they are instructed, as is the field representative, not to reveal the existence of this relationship to their clients. Finally, the parents and child are never apprised of the various hypotheses which the research is designed to test. In light of these circumstances, it may be argued that subjects of this experiment are unable to make an informed, intelligent decision on whether they wish representation by a project attorney.

Incomplete disclosure of information during administration of a research study obviously is not peculiar to the experimental method; on the other hand, only an experimental design involves random assignment to groups without the knowledge and consent of those being studied. In articles dealing with use of this research method in the law, various justifications and principles have been advanced in support of random assignment, which, although persuasive for some experiments, are not altogether satisfactory when applied to others.

It is argued, for example, that with random assignment each person, "before the lot is drawn, has an initial, equal chance to become a member of either group." Consequently, random assignment is less arbitrary, therefore presumably fairer, than any other conceivable dividing line. Theoretically this appears to be unassailable. But is the proposition necessarily true in practice for all experiments? Are there not legal innovations for which some persons would readily qualify absent an experimental design, but, with an experimental design imposed, their chances of qualifying for the innovation are severely reduced? Professor Norval Morris, writing about programs to test the effects of early release from prison, pointed out this potential occurrence. Without an experimental design, a particular in-

mate's chances of qualifying for an early release from prison project, based upon his record and ability to impress the parole board, might have been excellent, perhaps even an 80 percent to 90 percent chance. As a member of the sample population for the experiment, his chances of release would be reduced to 50 percent.

Another argument by which random assignment is justified goes like this: given the presence of a legal innovation, an experimental design involves not the basic issue of differential treatment itself but merely a shift in an existing and tolerated line of differentiation so that it is of use in evaluation; in other words, since any innovation in the law involves discrimination, it might as well be planned in such a way as to maximize our knowledge of its effects. But this argument, besides overlooking that the differential treatment may not correspond with the desires of those in the experiment, disregards the fact that the demands of the research design may contribute to altering the *degree* of differential treatment. To illustrate, with the lawyer project it would be possible for the lawyers to handle substantially more cases than just those which can be randomly assigned. As noted earlier, however, cases of persons referred to the project office are not taken, due to the potential interference of this practice with the research design. Although persons seeking representation by a project attorney are referred to other legal aid or to the juvenile court, it is not certain that they always obtain a lawyer. In both of the juvenile courts in which the project operates, requests for the appointment of counsel are discouraged, and occasionally indigent persons seeking counsel are pressured into proceeding without a lawyer, despite a constitutional right to have counsel appointed. In light of these circumstances, it is conceivable that the lawyer project directly alters, because of its experimental design, the extent of differential treatment; that is to say, absent the lawyer project's experimental design, greater numbers of persons probably would receive "lawyer treatment."

The Manhattan Bail Study conducted in New York City several years ago, which was designed to test the effects of releasing defendants prior to trial on their personal recognizance, provides a clearer illustration of how an experimental design may alter the degree of differential treatment. During its first year, random assignment to treatment and control groups was maintained. Prisoners were interviewed shortly after arrest and their references in the commu-

nity screened; eligibility for release on recognizance was determined according to certain established criteria. Then, based upon random assignment, from among those found to be eligible, a treatment group was recommended to the court for release, whereas those in a control group, despite their eligibility, were not recommended. The results were significant: 60 percent of those in the treatment group were released by the court on their recognizance in contrast to only 14 percent in the control group. Clearly the extent of differential treatment was heightened as a direct consequence of the experimental research design, for one can reasonably infer that if the legal innovation—recommendation for release on personal recognizance—had been applied to those in the control group, a larger number of persons would have obtained their liberty.

To minimize possible discrimination with experimental designs, Professor Morris urges that a principle of "less severity" be adopted, i.e., no person in an experiment should receive more severe treatment than would commonly be accorded—some simply should receive "less severe" treatment. [I]n the lawyer project, the hypothesis has been that having an attorney will lead to a more favorable result and hence "less severe" treatment. But what if the boys represented by project attorneys receive substantially harsher sentences than those without counsel? Or suppose that the youths represented by the project's lawyers do receive more lenient dispositions than those without counsel, but, as a group, those who have had attorneys consistently get into additional difficulty, and ultimately are incarcerated far longer than those in the control group. In circumstances such as these, the principle of "less severe" treatment appears to lose much of its meaning.

\* \* \*

[iv]

Philip R. A. May and A. H. Tuma  
*The Effect of Psychotherapy and Stelazine  
on Length of Hospital Stay, Release Rate,  
and Supplemental Treatment of  
Schizophrenic Patients\**

It is unlikely that any one treatment by itself will be universally effective—in every way, for all types of patients, in everyone's hands, in

every type of setting. Since large numbers of patients are treated in public mental hospitals, it was believed important to study the relative effectiveness of treatment modalities in common use for schizophrenic patients under the sort of conditions that might be expected in such hospitals within the reasonably foreseeable future.

There is some difference of opinion about the effectiveness of individual psychotherapy, either alone or in combination with ataraxic drugs, in the treatment of schizophrenic patients: this has particular significance since a large part of public hospital time and effort is devoted to the treatment of such patients. It is important to the therapist and to the administrator to have knowledge of the outcome of psychotherapeutic treatment in a hospital setting, for this will have a direct bearing on the selection of patients for psychotherapy and on the assignment of skilled professional time to a variety of functions in the total treatment program. There is still considerable controversy as to whether tranquilizing drugs facilitate or inhibit the therapeutic process when given as an adjunct to psychotherapy. Other arguments center around the "sufficiency" of a chemical treatment for an illness that is thought to have its etiology, at least in part, in psychosocial processes. It is the writers' belief that such controversies will undoubtedly continue until scientific evidence can replace the process of self-fulfilling and wish-fulfilling prophecy.

Selected first-admission schizophrenic patients with no previous treatment were assigned by a random method to four treatment groups in a factorial design—individual psychotherapy; individual psychotherapy plus tranquilizing drugs; tranquilizing drugs alone; and a control group that did not receive any of the specific treatments stated above. Evaluation was carried out before and after treatment, using multiple independent criteria.

The patient sample consists of 40 male and 40 female first-admission schizophrenic patients between 18 and 40 years of age, selected from consecutive admissions to Camarillo State Hospital. . . .

\* \* \*

All patients, regardless of their specific treatment, were housed in one male and one female research ward with similar staffing pattern, ward program and general milieu. The latter included routine nursing care, sedation, hydrotherapy, occupational, industrial and recre-

\* 139 *Journal of Nervous and Mental Disease* 362-369 (1964). © 1964, The Williams & Wilkins Co., Baltimore, Maryland. Reprinted by permission.

ational therapies, ward meetings and social case work.

The patients were treated by physicians who had had less than five years psychiatric experience and who were in residency training or had completed it. This corresponds with the level of experience of physicians who may reasonably be expected to be found to treat patients in state hospitals. Each patient's treatment was supervised by a psychiatric consultant who believed in the particular treatment to which the patient was assigned and who was responsible for discussing with the patient's doctor the timing, management, dosage, process and duration of treatment as well as tactics and operational details.

Patients had psychotherapy for an average of not less than two hours a week, supervised by a psychoanalyst experienced in the treatment of schizophrenic patients, with one hour of supervision for each patient every two weeks.

With ataraxic drugs, the aim was to give whatever type or dose the experienced supervisor judged most likely to be effective for a particular patient—to study treatment under "battle-field conditions," and not to conduct a study of one particular drug. . . .

Treatment was given up to a maximum of one year, until *either* the patient was released *or* had been in the hospital for at least six months and the supervisor and the therapist agreed that treatment had been given a good trial and that further treatment with that particular method was unlikely to succeed. A six- to twelve-month treatment period would seem to be more realistic than the shorter periods often reported in many research studies. It is more congruent with the natural history of the disease and therefore may increase the possibility of differentiating clinically significant differences in response to treatment. It also permits meaningful use and interpretation of two ultimate objective criteria of outcome—length of hospital stay and release rate.

\* \* \*

This report presents the outcome of treatment in terms of the following four measures: release rate, length of stay in hospital for those successfully released, amount of supplemental treatment required, and estimated change on the Menninger Health-sickness Scale. Ratings for this latter measure were provided by a team of two psychoanalysts who had no direct connection with the patients' treatment.

An important criterion of treatment effectiveness is the release rate. [M]ore patients were released when treated with "Drugs plus psychotherapy" or with "Drugs" than "Psychotherapy" or "Controls" (release rates—95 and 90 per cent vs. 70 and 65 per cent respectively), the overall difference being statistically significant at the .05 level.

The combined "Control" and "Psychotherapy" groups were compared with the combined "Drug" and "Psychotherapy plus drug" groups to test the difference in release rates between the groups receiving drug and those not receiving it, regardless of any other considerations. For this comparison the difference is significant at the .01 level. . . .

Another criterion of the usefulness of a treatment is the length of hospital stay for patients released. The mean stay (in days) for each of the four groups is as follows: Psychotherapy, 191.00; Control, 187.15; Drug, 151.00; Drug plus psychotherapy, 126.32. . . . [D]rug treatment has the only significant effect in reducing length of stay. Neither psychotherapy alone nor its interaction with drug treatment had significant influence on the variation in length of stay.

In considering the effectiveness of a treatment, it is reasonable to consider how much supplemental care the patient has to be given in addition to the experimental treatment. Since sedatives and hydrotherapy were available for all patients on all treatments, the frequency and duration of their use were recorded and the groups were compared on this basis. . . . [T]he most supplemental treatment was given to patients in the control group, the least to those who received drugs alone. Psychotherapy, with or without drugs, falls between the extremes.

\* \* \*

Perhaps one of the most critical considerations from the viewpoint of the clinician is the amount of improvement in clinical status. . . . The greatest differences lie between the "Control" group on the one hand and the "Drug," "Psychotherapy plus drug" and "Psychotherapy" groups in this descending order. There seems to be virtually no difference between "Drug alone" and "Psychotherapy plus drug." Again, when we look for the source of this variation in the amount of clinical change we find it to be the drug treatment. Psychotherapy had virtually no effect on the amount of change, while the interaction between psychotherapy and drug was significant at the .20 level, suggesting a possible

advantage in the combination of these two therapeutic agents.

#### Discussion

\* \* \*

It is not a simple matter to carry out a controlled experimental study of seriously ill schizophrenic patients treated for six to twelve months, and particularly to maintain a control group with no specific treatment for this length of time. This particular study of 80 patients took some three years to complete, not including the design and pilot stages. Under the circumstances, a study of 80 patients is a formidable affair—yet from a statistical point of view, the numbers are small: for example, they are sufficient only to be 50 per cent certain of detecting a shift from 70 per cent to 95 per cent improvement rate, with statistical tests at the .05 level. The reporting of relatively high percentage levels of significance seems appropriate in clinical research and especially in preliminary reports dealing with small numbers, where it is important to be sure of uncovering a true difference when it *does* exist. For example, use of a .20 level of significance gives odds of one in five that spurious differences or relationships will be reported as true: however, in severe and potentially ruinous disorders such as schizophrenia, the physician, the patient and his family may willingly take the risk that there is one chance in five that a given treatment will lead to results that are in reality no different than "milieu" treatment alone.

The figures for psychotherapy and drugs illustrate particularly well one of the difficulties of research in this area. If "Drug alone" produces a release rate of 90 per cent, and if the addition of psychotherapy produces a real increase of release rate to 95 per cent, it would be necessary to have 188 cases in each group to be even 50 per cent certain of detecting the above difference as significant at the .05 level.

In the present study, with a small sample size, none of the differences between the "Drug alone" and "Drug plus psychotherapy" groups were statistically significant although, for improvement on the Menninger Health-sickness Rating Scale, there was an interaction effect significant at the .20 level. Clearly, definitive answers in this particular area must await replication with a larger sample at some future time.

However, the figures do not support the contention that psychotherapy keeps patients hospitalized longer. It may be that a particular

treatment has an effect on the doctor who gives it, so that, for example, hospital stay *seems* longer to the psychotherapist; or it may be that psychotherapists keep some patients in longer and release others earlier.

On the other hand, the use of drugs, with or without psychotherapy, seems to reduce hospital stay; in the present study, the shortest mean stay was associated with the use of Stelazine and individual psychotherapy combined.

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[P]rolongation of hospital stay is seldom advisable. From the point of view of psychotherapy, the patient who is not psychologically minded is unlikely to benefit psychotherapeutically from a prolonged hospital stay. If more prolonged therapy is indicated, the therapist will usually wish to change to outpatient care as soon as possible. A prolonged regressive experience may promote chronicity; for patients with little financial reserve, it may be ruinous.

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#### Threats to Anonymity and Reputation

[i]

*Lee Rainwater and David J. Pittman*  
*Ethical Problems in Studying a Politically*  
*Sensitive and Deviant Community\**

\* \* \*

The Pruitt-Igoc Housing projects were planned in the early 1950's and first occupied in 1954. Originally the plan was to build two segregated projects, Pruitt for Negroes, and Igoc, across the street, for whites. This plan was ruled unconstitutional, however, and after a short period of integration the project became all-Negro. . . .

By 1959 the project had become a community scandal both because of certain unattractive design features (for example, the elevators stop only on the fourth, seventh and tenth floors) and as a result of the wide publicity given to crimes (rape, murder, robbery) and accidents (people fell down elevator shafts and children fell out of windows in the project). In response to the steady unfavorable publicity and a grand jury investigation of the project, former Mayor Tucker of St. Louis appointed in 1960 a committee on public housing and social services that

\* 14 *Social Problems* 357-366 (1967). Reprinted by permission.

included representatives of business, labor, and the general public as well as the various private and public agencies whose services and facilities were available to public housing residents. The committee directed its primary attention to Pruitt-Igoe, "because it had been much in the public eye and because the tangle of needs and services, present and potential, could be grappled with in the smaller area first." By February, 1961, the committee had presented both its findings of fact and its recommendations.

About the same time in Washington the Federal government's concern with urban problems was quickening. The President's Committee on Juvenile Delinquency and Youth Crime, which had been the main instrumentality of Federal interest, had been supplemented by a Joint Task Force of the Public Housing Administration and the Department of Health, Education, and Welfare, which came to be most centrally concerned with "Community Planning for Concerted Services in Public Housing." Concerted services meant that special efforts would be made in selected demonstration areas to maximize the input of social services and to maximize the coordination of these services. . . .

It was planned that accompanying the concerted services program would be a research project to study the community and to evaluate the effectiveness of the concerted services. . . .

\* \* \*

[A]n ethical aspect of the public interest in our study arose not in relations with outsiders but within our own group. Some of the fifteen faculty and graduate student researchers expressed concern early in the study over the effect a really penetrating analysis of the style of life of poor Negroes might have on the public dialogue about race relations and poverty. That is, if one describes in full and honest detail behavior which the public will regard as immoral, degraded, deviant, and criminal, will not the effect be to damage the very people we hope our study will eventually help? We have heard such views offered by others, by eminent social scientists in universities and in government. The question is generally phrased something along the following line, "How do you know that the constructive effect of our research will outweigh the damage to the reputations of the people we study? Our science isn't that good yet. Maybe all that will happen is that we will strengthen prejudices and provide rationalizations for bigotry."

This is a knotty issue, and one which per-

haps can only be resolved by an act of faith. If you believe that in the long run truth makes men freer and more autonomous, then you are willing to run the risk that some people will use the facts you turn up and the interpretations you make to fight a rear guard action. If you don't believe this, if you believe instead that truth may or may not free men depending on the situation, even in the long run, then perhaps it is better to avoid these kinds of research subjects. We say perhaps it is better, because it seems to us that a watered-down set of findings would violate other ethical standards, and would have little chance of providing practical guides to action; thus it would hardly be worth the expenditure of one's time and someone else's money.

At the level of strategy, however, this concern for the effect of findings on public issues sensitizes one to the question of how research results will be interpreted by others, and to his responsibility to anticipate probable misuses, and from this anticipation attempt to counteract the possibility of misuse. That is, though we do not feel a researcher must avoid telling the truth because it may hurt a group (problems of confidentiality aside) we do believe that he must take this possibility into account in presenting his findings and make every reasonable effort to deny weapons to potential misusers.

For example, several years ago one of us published a study analyzing the problems of motivation and marital role difficulty that lead lower-class women to be poor family planners.<sup>3</sup> The study was commissioned by the Planned Parenthood Federation of America in hope of learning how to operate their clinics more effectively. The findings indicated that most lower-class women could not sustain the kinds of habits required to practice contraception effectively with the then existing methods (this was before the introduction of the "pill" and the intrauterine device). During the two years after the study appeared, there was considerable agitation in several cities and states to establish family planning services in public health and welfare facilities. In the course of the controversy that ensued several officials who opposed the establishment of family planning services in this way quoted the study to support their contention that lower-class women really did not want help in

<sup>3</sup> Lee Rainwater, *And the Poor Get Children: Sex, Contraception, and Family Planning in the Working Class*, Chicago: Quadrangle Books, Inc. 1960.



limiting their families. In examining what he had written, the author realized that he had not taken this possibility into account at all, although knowing the strong feelings people have about family planning and contraception he should have known better. He had not made crystal clear that there was no question but that these lower-class women wanted fewer children, even though they needed a good deal of help in realizing that desire. In his desire to get at the problems that created difficulties in Planned Parenthood's clinic organization, he had not sufficiently emphasized the wishes that lower-class women have for some kind of really effective help in family limitation.

Another example of the misuse of social science findings can be found in controversies dealing with problems of integration and segregation. Much recent research on the problems of slums and ghettos has emphasized the destructive effects of lower-class family and neighborhood systems. The authors of these studies clearly hope that by understanding the dynamics of slum living it will be possible to develop programs that do not fail as present housing, welfare, and retraining programs seem to be failing. However, since these researchers deal with the internal dynamics of the ghetto, their findings can prove quite attractive to individuals and institutions that seek to perpetuate containment of Negroes in segregated areas. Recently Rainwater testified for the National Association for the Advancement of Colored People in a *de facto* segregation suit against the Cincinnati School Board. In direct testimony he said some very elementary things about the destructive effects of going to a ghetto school and thus contributed to the plaintiff's case that *de facto* segregation, as much as legal segregation, affects negatively the "hearts and minds" of Negro children and therefore violates the due process clause of the Fourteenth Amendment. However, on cross-examination he found himself exposed to a fairly sophisticated line of questioning that sought to have him "admit" that the low achievement scores and high drop-out rate of Negro children in ghetto schools have nothing to do with the fact that these schools are "racially imbalanced" but rather is due to the home and neighborhood environment. The Board of Education obviously felt that it had a defense against the plaintiff's charges which was validated by social science research on Negro slum family and neighborhood behavior. This is apparently a popular defense by boards of education in several parts

of the country. They seek to substantiate the view that ghetto schools do not damage Negro children but that the damage is instead done by the family and the neighborhood over which the schools have no control. Indeed, we have heard of one other social scientist expert witness in a *de facto* segregation case who was cross-examined for five days along exactly this line. In that case the court was not impressed and found for the plaintiffs, but given the jaundiced eyes with which courts view social science testimony anyway, it seems likely that more often the effect of such "conflicting" findings will be beneficial to the case of those who wish to maintain a segregated school system.

It probably would have been difficult to anticipate such a sophisticated misuse of research findings before the fact. After all, that families may damage their children says nothing about whether schools do or do not also damage the same children. But once we know that such misuse is being made of the products of our discipline, perhaps we have a responsibility to try to do something about it, much as psychologists have done with the misuses of intelligence test data on racial groups. Perhaps we need some kind of "intelligence service" which appraises us of this kind of misuse so that in our subsequent writings we can make it less easy for people to misuse the findings and also so that as a group we can make efforts to counteract this kind of misinterpretation.

More generally, it seems evident that as sociology is more and more accepted as of relevance to the important issues the country must cope with, what sociologists have to say will be increasingly fateful in the lives of individuals and groups. It behooves us then, not only to study significant problems and report our findings accurately, but also to be sensitive to the way these findings are used, particularly to whether or not they are used in ways that seem illegitimate, given the findings. In this respect sociologists will come increasingly to have the same kinds of problems that historians, political scientists, economists, and psychologists have had for some time.

\* \* \*

The traditions of our field emphasize anonymity as necessary and desirable in research. We generally think of ourselves as studying social behavior about which our informants are protective. They may be protective because their deepest interests are involved, or for reasons that are



less vital, but it is traditional to honor our subjects' wish that what we learn about them not be communicated to a larger public in any way that will affect their interests or identify them. Confidentiality is deemed technically necessary, and once it is offered we are ethically bound to honor our promise.

However, there are some situations for which the offer of confidentiality may be both unnecessary and technically a bad choice. In some situations the applicability of research findings to applied goals will be rendered almost impossible if true confidentiality is maintained. And in some other situations it may be impossible to communicate the findings once the informants have been told that what we see and hear will be kept confidential.

It seems to us that we should rethink our automatic assumption that we offer to maintain the privacy of our informants. The question of whether or not to make such an offer demands a conscious and thoughtful decision that is made in the light of needs and goals of a particular research. Let us offer a couple of examples of situations in which confidentiality has not been offered and then suggest a principle which underlies these examples.

In our Pruitt-Igoe research, we have not made promises of confidentiality to anyone in the Housing Authority management. We have not done so since we feel that were the information we collect from them regarded as confidential it would not be possible to publish a sensible report of our findings. This applies both to individual functionaries in the Housing Authority and to the Authority as an organization. We cannot possibly conceal the identity of this particular project when we publish our results. We must be free to identify the organization and its various constituent units. Similarly we could not possibly discuss the role of the executive director of the Housing Authority or of the project managers without their being identified as particular persons. While in the end we might adopt some cover of pseudonyms it would be more to avoid becoming enmeshed in questions of personality than to prevent the identification of the actual persons involved. Thus, while we feel that no useful purpose would be served by not concealing the identities of the tenants in the project and of the low-level employees, at the higher reaches of the organization our presumably useful purpose can be served only by openness about the identities of the organization and the top level executives involved.

These persons know that it is possible that our study will have unpleasant repercussions on them, but they also are used to being exposed to the light of publicity.

\* \* \*

The decision not to promise confidentiality makes explicit our claim to a *right* to study social behavior in certain situations. Obviously we do not claim the right to study all kinds of behavior in non-confidential ways and to make public our findings, but we do and should study certain kinds of behavior in this way. As an initial formulation, we suggest that sociologists have the right (and perhaps also the obligation) to study publicly accountable behavior. By publicly accountable behavior we do not simply mean the behavior of public officials (though there the case is clearest) but also the behavior of any individual as he goes about performing public or secondary roles for which he is socially accountable—this would include businessmen, college teachers, physicians, etc.; in short, all people as they carry out jobs for which they are in some sense publicly accountable. One of the functions of our discipline, along with those of political science, history, economics, journalism, and intellectual pursuits generally, is to further public accountability in a society whose complexity makes it easier for people to avoid their responsibilities.

We would suggest that, in principle, anyone is publicly accountable for the actions which it is his duty to perform. Most of the time, however, since sociologists are not muckrakers, it is not necessary or desirable to single out individuals or even clearly identifiable small groups. In such situations one may reasonably use confidentiality as an inducement to cooperation. In other situations, however, this is clearly unwarranted. If one wishes to study the functioning of courts, or of a mayor's office, or of General Motors, or of unions, it is perhaps better to put up with the difficulties of only doing what one can do without promising to keep information confidential. Since publicly accountable individuals often recognize the fact of their accountability and the useful purposes that might be served by sociologists studying them, one can often gain a good deal of cooperation without the promise of confidentiality.

We are suggesting that sociologists in this respect have the same rights that journalists have. Our understanding of the social process may be such that we do not use this right in the

same way as journalists, because we are not interested in momentary sensations but in developing an understanding of the persisting tendencies of social systems, large or small.

[ii]

*Diane Bauer*

*Maryland Tests for Criminal Potential\**

With the approval of both state and Federal agencies, some 15,000 Maryland boys are being submitted to blood tests at the risk of being labeled for life as potential criminals. The testing, to be administered in a majority of cases without legal consent of parents, has been attacked by lawyers, doctors and civil liberties groups.

The search for boys with an XYY chromosome pattern, which some scientists theorize may be linked with violent criminal behavior, is being conducted by Johns Hopkins University with Federal funds. The study will include two main test groups over the next three years—6,000 boys confined to Maryland's juvenile jails . . . and 7,500 from a group of largely underprivileged Negro families enrolled in a free Johns Hopkins medical program.

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Both Maryland Children's Center and Waxter [Children's Center], court officials acknowledge, detain some children who are not accused of crimes, but are "dependent and neglected."

\* \* \*

Results of the tests on the 6,000 boys in state institutions will be turned over to juvenile correctional agencies by the staff conducting the study.

Dr. Digamber Borgeonkar, who heads the three-year project under a \$300,000 grant from the National Institutes of Health's Center of Crime and Delinquency, said he plans to give the parents test results only if requested and then only "as much as they can understand."

\* \* \*

Parents of 7,500 East Baltimore boys, 95 per cent from underprivileged Negro families enrolled in a free child care program at Johns Hopkins, are not being asked for a signed legal consent for their children's participation in the study, Dr. James Hudson said.

Dr. Hudson, project director of the Com-

prehensive Child Care Program, said Johns Hopkins does not have a "blanket permission" from the parents for test studies. He said blood is "drawn routinely to check for anemia," and by using the same samples for the chromosome test the problem of asking parents for legal permission to look for the XYY factor in the youngsters' blood is avoided.

\* \* \*

The results of the blood tests—which lawyers say would "label boys for life with the criminal stigma of a yet unproven scientific theory," according to Robert C. Hilson, director of Juvenile Services, "will probably be passed on to the courts for whatever use they can make of it."

Maryland juvenile court probation officers will be used to persuade resisting parents to sign a permission to take a blood sample, Dr. Borgeonkar said, with the single explanation on the document "for examination by the staff of the Division of Medical Genetics" of Johns Hopkins.

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Neither the [preliminary] letter nor the permission form [see NOTE 1.] tell parents anything about the theories relating XYY chromosome patterns to criminal violence which sparked the study, the ACLU complains.

Lawyers originally saw the XYY defect as a basis for a plea of insanity for clients charged with violent crimes.

Maryland courts have twice in the past year rejected the XYY diagnosis as a basis for an insanity plea. Both a local circuit court and the state Court of Special Appeals refused Ray Millard, convicted of armed robbery in Prince Georges County, permission to use his XYY chromosome pattern to prove his legal insanity.

[1] In Australia a murder suspect diagnosed as XYY was last year found not guilty by reason of insanity.

"What can be used by the defense can also be used for the prosecution," said a leading Montgomery County lawyer who represents juveniles in Maryland courts.

"With the state co-operating in such a study, a juvenile could be sent away for life to a state hospital or Patuxent," he warned, "not because he did anything serious, but just because some guy in a laboratory thinks he has one chromosome too many."

Timothy Crofton, assistant director of Edgewood, a private psychiatric treatment center,

\* *Washington Daily News* 7, cols. 1-3 (January 22, 1970). Reprinted by permission.

said he assisted Dr. Borgaonkar in obtaining the NIH grant and will act as "administrative coordinator" for the project, which will include the 500 emotionally disturbed boys under his care during the next three years.

Parents of these boys will not be asked for signed legal permissions for this specific test because a blanket permission is signed when each child is admitted. Mr. Crofton said, however, that he is "sure all parents are aware of the study."

Mr. Crofton, a former Montgomery County probation officer, said "my assignment is to try to get the co-operation of various probation officers to get parents to sign up when their child is committed to an institution."

\* \* \*

A baby's sex is determined by whether it receives an X or a Y chromosome from its father. Normal men receive an X from their mothers and a Y from their fathers. Women get one X from each parent. The usual number of chromosomes is 46.

This study is chiefly concerned with who receives an extra Y. Some geneticists believe that boys born with XYY chromosomes get a double dose of masculinity that makes them prone to violent, aggressive behavior.

Genetic scientists don't agree on the significance of chromosome abnormalities. A California doctor believes an abnormal number of chromosomes may result in a high potential for committing rape or other sex crimes. Other scientists say chromosome abnormalities are not as uncommon as previously believed and are of concern only in unusual cases.

Dr. Borgaonkar believes XYY males are more impulsive than aggressive, but may react more violently than others under certain stresses. He says earlier studies reported that the majority of XYY's have low IQs and are tall, acned and aggressive, but only the height factor has been proven so far.

## NOTES

### NOTE 1.

JOHNS HOPKINS HOSPITAL  
CONSENT FORM AND COVER LETTER (1969)\*

Your son, who is presently at \_\_\_\_\_, has been included in a special diagnostic genetic

\* Reprinted by permission of Dr. Digamber S. Borgaonkar, The Johns Hopkins University School of Medicine, Baltimore, Maryland.

(chromosome) study. This test is being conducted with the administrative approval of the Director, Department of Juvenile Services, State of Maryland, Baltimore, Maryland. We plan to test 6,000 boys, in a period of three years, from all the state institutions.

Please sign the enclosed form and mail it in the enclosed stamped, addressed envelope at your earliest convenience.

\* \* \*

Date: \_\_\_\_\_

BOY'S NAME: \_\_\_\_\_

As his parents and/or legal guardian, I give permission for a blood sample to be drawn from the above named youngster for examination by the staff of the Division of Medical Genetics of this institution.

It is understood that no drugs or medication will be administered; that the results of the examination will be made known to me upon request; and that there will be no charge for this service.

\_\_\_\_\_  
Signature of parent(s) or legal guardian

### NOTE 2.

DIANE BAUER  
XYY TESTS STOP\*

Dr. Saleem Shah, chief of the National Institute of Mental Health's Center for Studies of Crime and Delinquency, said yesterday that as a result of articles in *The Washington Daily News* he has suspended a blood test search for the rare XYY chromosome pattern among some 14,000 Maryland juveniles.

\* \* \*

A law suit, which names Dr. Shah as one of nine defendants, filed in Montgomery County Circuit Court last week charged that the study violates the civil liberties of the youths.

It accuses NIMH and Johns Hopkins of neglecting to obtain proper legal consent to the testing from parents, failing to inform parents of the legal consequences of the testing and destroying the confidentiality of the doctor-patient relationship by turning over the results to state juvenile courts.

"It is clear," said Dr. Shah, "that the procedures for getting informed consent (for the blood tests) have to be revised and made stricter.

\* *Washington Daily News* 5, cols. 1-3 (Feb. 13, 1970). Reprinted by permission.

Until that is clarified the taking of blood samples has been stopped."

Dr. Shah said that he could not say whether or not the study had violated the constitutional rights of the boys as charged by civil liberties groups and some experts in medical and legal ethics, because Johns Hopkins has not completed a review of the procedures which are being used by the project director, Dr. Digamber Borgeonkar.

Dr. Shah refused to show reporters the written procedures outlining the methods the researchers agreed to use in order to safeguard the boys' rights and which Johns Hopkins is required by law to file with NIMH.

A spokesman for Dr. Shah, James Helsing, said that NIMH is not required to provide the information to the public even under the new federal public information law because it "constitutes trade secrets."

\* \* \*

#### NOTE 3.

##### DIANE BAUER CRIMINAL-PRONE TESTS RESUMED\*

The Government has quietly resumed testing the blood of underprivileged Maryland boys in an effort to find and label potential trouble-makers by the make-up of their chromosomes. . . .

The Health, Education and Welfare Department is permitting the blood testing to be touted to impoverished black parents as a free \$100 medical bargain, apparently in order to obtain parents' signatures on a new consent form that was revised after it had been sharply criticized.

Rep. Cornelius E. Gallagher, D-N.J., who recently scheduled hearings on a similar—and abortive—proposal by a Dr. Arnold Hutschnecker to psychologically test the nation's 6-year olds for incipient anti-social behavior, now plans a full-scale investigation of the XYY chromosome blood testing being funded by HEW's National Institute of Mental Health (NIMH). . . .

\* \* \*

Project director Borgeonkar refused to reveal the percentage of Negroes included in the testing, but the population of Maryland juvenile institutions is about 75 percent black. Of the non-incarcerated children in the project, 95 percent are boys from black ghettos.

A new consent form was drafted by the ACLU after *The Daily News* revealed that Johns Hopkins could produce no written record that it had either sought or received permission from the parents of the underprivileged boys included in the study. The ACLU also charged that the consent form being used for institutionalized youths was inadequate and misleading.

HEW standards require that "informed consent" be obtained before any project funded by tax money can use humans as research subjects. In a letter to Sen. Ervin, Dr. Roger Egeberg, assistant HEW secretary for health and scientific affairs, said that, as required by law, "this project had been reviewed by the committee on clinical investigation of the Johns Hopkins University school of medicine."

But Dr. Gordon Walker, chairman of the university's committee on clinical investigation, said his committee had never held a meeting to discuss the original XYY proposal funded by NIMH. However, the proposal was circulated to the members individually for approval.

\* \* \*

The form does not point out that Maryland has no confidentiality law for doctors and that therefore the test results could be introduced in court. . . .

The consent form does assure parents that test results will be confidential, but unlike some standard consent forms used in Maryland jails, no official signs the XYY form to bind the state or the Baltimore hospital to observe the agreement.

\* \* \*

While Johns Hopkins and NIMH acknowledged that they suspended XYY testing in view of complaints about failure to inform the boys and their families of its nature and possible legal consequences, project officials said they had no plans to inform boys already tested that their rights may have been violated or that a new consent form is now in use.

Blood from the institutionalized boys is being drawn by students—"psychology majors"—without medical supervision, Mr. Ventura [legal counsel for Johns Hopkins] said. He refused to say if they had any other qualifications for taking blood by hypodermic needles from the boys. Project director Borgeonkar is not a physician.

"We feel the detailed information of the type you're looking for has no legitimate public interest," Mr. Ventura told *The News*.

\* \* \*

\* *Washington Daily News* 1, col. 4 (May 4, 1970). Reprinted by permission.

## NOTE 4.

JOHNS HOPKINS HOSPITAL  
 CONSENT FORM FOR CHROMOSOME STUDY OF  
 INSTITUTIONALIZED JUVENILE DELINQUENTS  
 (1970)\*

This is to ask your permission to include your son (name) in a research study of chromosomes. Chromosomes are things in a person's body that determine such factors as sex and color of eyes. The purpose of the study is to locate and compare boys with unusual chromosome patterns and to learn whether unusual chromosome patterns are related to such factors as a person's physical and mental development and behavior problems (including a tendency to violate the law).

Over a period of approximately three years, we hope to study all of the State's 6,000 institutionalized juvenile delinquents, all of the 500 boys at the Edgemoor Center, as well as 7,500 other boys.

If you elect to permit your son to participate, a member of the Johns Hopkins Hospital staff will draw about 2 cc's (about a thimbleful) of blood from him. *No drugs or medication will be given.* Further studies will be done in certain cases, but your son will be included only after we have obtained your further consent.

The chromosome study ordinarily costs about \$100 and, once performed in a lifetime, need ordinarily never be done again. However, no fees will be charged to you. You, and only you, will be told of the medically useful results of the study and we will explain the significance of this information to you.

This study has been cleared by the Maryland Departments of Health and Juvenile Services. However, the results will be used only by our medical researchers for scientific study and *will not be disclosed to any other person or agency.*

Because this program involves important medical research, we would very much appreciate your cooperation. However, *this program is purely voluntary; you are not required to sign this authorization.* If you do not, no tests will be run on your son. If you desire to authorize your son's participation, please sign below and mail this letter in the enclosed, stamped, addressed envelope at your earliest convenience. If your son is over 14, we will request him to

sign this voluntary consent form at the institution. A copy is enclosed for your records. If you have any questions, please call us collect at the above number—or call Mr. Timothy Crofton at Adelphi, Md.—Area Code 301-434-3206, evenings and weekends.

☐ I agree

to permit my son to participate  
in this study.

☐ I do not agree

\_\_\_\_\_  
 Signature of parent(s)/legal guardian

## NOTE 5.

ROBERT C. COWEN  
 BIOLOGISTS DEBUNK CRIMINAL GENE\*

London.—Geneticists feel embarrassed by the so-called "criminal" chromosome.

It is a human genetic factor that recently, and falsely, was thought to make its possessor criminally inclined.

This notion was introduced in American courts as an indication of criminal "insanity." It has been used, in Britain at least, as reason for abortion of unborn children.

All of this was the result of an erroneous conclusion drawn from bad statistics.

Now geneticists are concerned lest people again be improperly stigmatized as "abnormal" on equally hazy genetic grounds as medical centers build up files on the genetic background of adults and children.

This is one of the dangers cited at a meeting here on the social implications of biology, a meeting convened by the British Society for Social Responsibility in Science.

\* \* \*

Studies made over the past five years at certain penal and mental institutions indicated what seemed an abnormally high percentage of males with the XYY chromosome set among inmates. This was taken as evidence that the extra Y inclined its possessors toward aggressive criminality.

As experts pointed out at the meeting, geneticists now consider such a conclusion scientific rubbish. It was drawn without any knowledge of or reference to the proportion of XYY males in

\* Reprinted by permission of Dr. Digamber S. Borgaonkar, The Johns Hopkins University School of Medicine, Baltimore, Maryland.

\* *The Christian Science Monitor* 1-2 (Dec. 2, 1970). Reprinted by permission. © 1970 The Christian Science Publishing Society. All rights reserved.

the "normal" population. It was drawn with no knowledge whatsoever of how the extra Y chromosome actually does manifest itself in bodily structure or human behavior. Finding this out would take massive research, which has scarcely begun to be tackled.

Yet invalid as the concept of the "criminal" chromosome may have been, it had begun to be used publicly as a criterion for judging people. This, said Prof. Geoffrey Beale of Edinburgh University, has shocked geneticists. They just had not been aware of what the social consequences could be when they released socially sensitive research information.

While valid as research, the XYY studies were no basis for social decisions. Scientists, he said, must exercise more control over how much information is released to the public.

\* \* \*

[iii]

William Foote Whyte  
*Freedom and Responsibility in Research—  
The "Springdale" Case\**

A small upstate New York village has now been immortalized in anthropological literature under the name of "Springdale". . .

\* \* \*

[Prof. Arthur] Vidich spent two and a half years living in "Springdale" as field director of a Cornell project carried out in the Department of Child Development and Family Relations. The project was directed by Urie Bronfenbrenner, a social psychologist. As a result of this research experience, Vidich published several articles, but the official report in book form regarding the project did not materialize during his tenure at Cornell and is only getting into print at this writing. Some time after he left Cornell, Vidich began work on a book of his own, in collaboration with Joseph Bensman, who had had no previous association with the project.

The Vidich manuscript gave rise to considerable controversy between the author and the Springdale project director. . .

The points of controversy were essentially these:

1. Should individuals be identified in the book?
2. If individuals were identified, what—if

anything—should be done to avoid damage to them?

\* \* \*

Before Vidich came onto the scene, Springdale people had been assured, when their collaboration was sought, that no individuals would be identified in printed reports. While all of the Vidich characters are given fictitious names, they can easily be identified within Springdale. The author argues that, when there is only one mayor and a small number of village and town officials and school board members, it is impossible to discuss the dynamics of the community without identifying individuals. He further argues that what he has reported in the book is "public knowledge" within Springdale. Even if this be true, is there a difference between "public knowledge" which circulates from mouth to mouth in the village and the same stories which appear in print?

In addition to his objections regarding the anonymity pledge, Bronfenbrenner claimed that certain individuals were described in ways which could be damaging to them. On this he submitted a long bill of particulars. One example:

One member of invisible government, in agreement with the principal's educational policy, has remarked that "He's a little too inhuman—has never got into anything in the town. He's good for Springdale until he gets things straightened out. Then we'll have to get rid of him."

\* \* \*

The Springdale experience also raises a general problem regarding the relations of a staff member to the project director in a team project, especially when there is a long period between the initiation of the study and the publication of major research reports. The junior member of such a staff must naturally think about establishing his own professional reputation, which he can do primarily through publication. An article or two will help, but a book would help even more. Is he to be a co-author on a book which represents a major report of the study? In that case, he may have to wait some time for the appearance of the book, and, in the meantime, he has little in the way of credentials to offer as he seeks new teaching and research jobs. . .

\* \* \*

We will let the author have the next-to-last word on the controversy. Replying in the *Ithaca*

\* 17 *Human Organization* 1-2 (Summer 1958). Reprinted by permission.

*Journal* to a statement made by Bronfenbrenner, Vidich writes:

Strictly speaking, I take the position that in the interests of the pursuit of scientific truth, no one, including research organizations, has a right to lay claims of ownership of research data.

That is a violation of the entire spirit of disinterested research.

Asked whether he was aware that there would be a reaction in Springdale, Vidich replied:

I was aware that there would be a reaction in the town when the book was published. While writing the book, however, it did not occur to us to anticipate what these reactions might be, nor did it occur to us to use such anticipations of reactions as a basis for selecting the data or carrying out the analysis.

One can't gear social science writing to the expected reactions of any audience, and, if one does, the writing quickly degenerates into dishonesty, all objectivity in the sense that one can speak of objectivity in the social sciences is lost.

We do not have any firm answers to the various problems raised by this case, but we are quite convinced that the Vidich answer will not serve. He seems to take the position that he has a responsibility only to science. Has the researcher no responsibility to the people whom he studies? We are not prepared to state what the nature of this responsibility should be, but we find it strange indeed to hear a researcher argue that he assumes no responsibility at all.

\* \* \*

#### NOTES

##### NOTE 1.

ARTHUR VIDICH AND JOSEPH BENSMAN  
FREEDOM AND RESPONSIBILITY IN RESEARCH\*

We are pleased to be invited to join in the discussion of the issues which the Editor opened up in the editorial. . . .

\* \* \*

We feel, however, that his phrasing of the issues was too narrow, in that it was limited to the social and public relations problems of social science investigation. It failed to consider any of the problems related to the purposes of inquiry and to the scientific problems which social in-

quiry presumes to state and solve. For example, his editorial gave attention exclusively to the social scientist's responsibilities to the community and the research organization, and to his personal problems, such as career aspirations, rewards, publications, and the gaining of publicity. While all of these things are important as far as the organization of the discipline is concerned, they are irrelevant; progress in a science is somehow related to important substantive problems and issues and the activities which lead to progress in the solution of the problems posed. This he altogether failed to bring up in his discussion.

\* \* \*

The particular fates of Vidich, Bensman, the project, the department, Cornell University, Springdale, etc. are of much less significance than the problems which the editorial raises for the future of scientific investigation in western society. Not that the Springdale example presents a new problem; on the contrary, negative reactions by organizations, individuals, and interest groups have been characteristic for the Lynds' study of Middletown, West's study of Plainville, Warner's study of Yankee City, Selznick's study of the T.V.A., Hunter's study of Community Power, and Whyte's study of Street Corner Society. In the latter case, Doc still suffers from the recognition he received in the book.

Historically, this problem has not appeared, or has appeared to a much lesser extent, in the anthropology of non-western society. This is because primitive populations have been less concerned, aware, and vocal in their response to the anthropological description of their societies. The life history, studies of native politics and organizations, etc., all invade the native's "privacy," subject his inner life to exposure, and strip him of the magic on which his existence rests. Because it was possible to do this with native society, sociologists and anthropologists have learned a great deal about social life which they could apply to western society. Now that so many primitives have become westernized and are aware of the implications of anthropological research, they, too, resent the invasion of privacy and descriptions of the inner structure of their society.

There is an interesting parallel between the license taken by anthropologists and that taken by sociologists who have studied crime, minority groups, caste groups, factory workers, prostitutes, psychopathic personalities, hoboes, taxidancers, beggars, marginal workers, slum dwell-

\* 17 *Human Organization* 2-5 (Winter 1958-59). Reprinted by permission.



ers, and other voiceless, powerless, unrespected, and disreputable groups. Negative reaction to community and organizational research is only heard when results describe articulate, powerful, and respected individuals and organizations. We believe there would have been no objection to our study if it had been limited solely to the shack people.

We think all of the community and organizational studies mentioned above made important contributions. The problem is: *At what price should a contribution be made?*

One of the principal ideas of our book is that the public atmosphere of an organization or a community tends to be optimistic, positive, and geared to the public relations image of the community or the organization. The public mentality veils the dynamics and functional determinants of the group being studied. Any attempt in social analysis at presenting other than public relations rends the veil and must necessarily cause resentment. Moreover, any organization tends to represent a balance of divergent interests held in some kind of equilibrium by the power status of the parties involved. A simple description of these factors, no matter how stated, will offend some of the groups in question.

The only way to avoid such problems is not to deal with articulate groups who will publicly resist the attention which research gives to them, or to deal with the problems in such a way that they are inoffensive. Research of this type becomes banal, irrespective of its technical and methodological virtuosity. We think this has always been the case and that the Springdale example presents nothing new.

\* \* \*

If, however, as in our work, fundamental issues which are related to the basic problems of social science are raised, one cannot predict in advance the embarrassment which research may cause, including the embarrassment to oneself. If the social scientist wants to raise these kinds of issues, he has to risk the possibility of getting into these kinds of troubles. We foresaw this, as the research progressed, and there is no easy solution to the problem.

We think the social scientist can only answer the problem for himself, by asking himself what kind of research he wants to do. If he wants to do practical research which is important to some sponsoring body, he must accept the ethic of responsibility and give up the illusion of independent inquiry. If he wishes to do serious re-

search on problems which are not practical (as practicality is now defined in modern society) he must almost certainly conclude that he must work outside the framework of large research organizations, large institutional grants, or research-servicing organizations. The choice he makes must then be a personal one and, in each case, he can preserve the ethical system he has selected.

\* \* \*

#### NOTE 2.

ARTHUR J. VIDICH AND JOSEPH BENSMAN  
THE SPRINGDALE CASE—ACADEMIC  
BUREAUCRATS AND SENSITIVE TOWNSPEOPLE\*

\* \* \*

After three years of contact with the community, the members of the project and "The Project" as an official organization had established many personal and official contacts, commitments, friendships, and confidences. This was inevitable simply because of the duration and closeness of the contact. The problem was how these personal and official relations relate to scientific reporting. In the Springdale case the project director took the position that certain materials were questionable from the point of view of ethics and possible injury to persons.

I have just finished reading the manuscript of you and Bensman and, in response to your request, am giving concrete examples of material which, though it may represent public knowledge, is, in our judgment, highly questionable from the point of view of professional ethics and possible injury to the person involved. Since there are many instances of this kind, I shall confine myself to a few outstanding examples.

1. There are many references to the enmity between Flint and Lee. . . . Since, as you yourself have emphasized, these two persons will be immediately recognizable to anyone familiar with the community, assertions that Flint "has been excluded from town politics by Lee" who harbors "resentment" against him are fairly strong accusations. Moreover, the discussion of their personal antagonism is not really central to your analysis of the way in which the community operates and hence you would not lose much by omitting mention of the matter.

2. The whole discussion of Peabody, the school principal, and his relation to the community could,

\* Arthur J. Vidich, Joseph Bensman, and Maurice Stein, eds.: *Reflections on Community Studies*. New York: John Wiley and Sons, Inc. 313, 331-335, 338-341, 347-349 (1964). Reprinted by permission.



if it remained in its present form, do a good deal of harm and arouse justifiable resentment. For example, consider the possible impact on him and others of reading the following direct quotation attributed "to a prominent member of invisible government": "He's a little too inhuman—has never gone into anything in the town. He's good for Springdale until he gets things straightened out. Then we will have to get rid of him." Potentially equally damaging are the statements quoted from the observers' report, but these, along with all excerpts from the project files, would of course no longer appear in the manuscript.

3. In pointing out that the Polish community is controlled by political leaders through intermediaries who are willing to do their bidding in exchange for acceptance, is it necessary to point the finger so visibly at Kinserna? You do this very pointedly . . . you go so far as to assert that the upshot of his activities is "to get the Poles to accept measures and policies which are disadvantageous to them."

4. Personality descriptions of the ministers are likewise conspicuously on the *ad hominem* side. For example, you refer to one as "awkward, condescending, and not of the people" and to another as a "cantankerous troublemaker." Also, I wonder whether the description of the Episcopalian minister as trained in "one of the 'radical' Eastern seminaries" is not subject to misinterpretation by Springdale readers despite your use of quotations around the word radical. Given upstate New York's climate of opinion, such a statement may have some unfortunate consequences for the man concerned.

5. The clearly uncomplimentary remarks about Grainger . . . have especial importance for not only is he likely to read them himself, even though he is no longer living in the community, but they are also likely to be read by his colleagues and superiors in the Extension Service. It would be particularly unfair and unfortunate, especially in view of Grainger's whole-hearted cooperation with the project, if any statement made by you jeopardized Grainger's professional future. As the manuscript now stands, such a possibility is by no means out of the question. . . .<sup>16</sup>

The issue here is not the specific items of censorship, but rather the assumption of protective attitudes toward specific community members on the basis of personal attractiveness, entangling commitments, respondent's earlier cooperativeness, and other nonresearch considerations. As a result of personal, social, and organizational commitments, the project finds itself in the position of writing its findings with an eye to other than research or theoretical interests and issues.

As a final step in viewing the community as a reference group, the project decided that:

. . . Before any manuscripts are shown to outside representatives, such as publishers or their agents, we will ask one or two persons within the college and possibly in Springdale to read the manuscripts from the point of view of public relations. Although the final responsibility for deciding what we publish will rest with the project staff, the reactions of such readers would receive serious consideration and we would probably rewrite and omit in accordance with their recommendations. . . .<sup>17</sup>

In this instance, to avoid personal responsibility for the project's research reporting, selected nonresearch respondents would be invited to pass on manuscripts purely as a way of avoiding bad public relations, so that aspects of community life that may be theoretically relevant can be censored by local individuals on nonresearch grounds. Moreover, the local individuals to be selected would be specifically those who constituted the project's dominant reference group in the town, namely, the town's official leaders and spokesmen who represented most forcefully the stereotyped image of the positive-minded community which the project has absorbed as its own image of the town.

The identification of the project and its personnel with the town's interests and with the feelings and sentiments of individuals and groups being studied leads to a subtle adaptation of the research to the problems of the community even though those problems are not the problems of the research. In an extreme instance this policy would lead to no point of view except the point of view of the community. . . .

\* \* \*

At certain stages the community may become a more important reference group for the project than is the scientific community to which the research is ostensibly addressed. In Springdale, for example, the study of constructive activities in the community gradually came to include the ideology that the project and its members assume constructive attitudes toward Springdale in all phases of work including community relations, field work, participation, analysis of data, and reporting of scientific results.

\* \* \*

The town itself came to its own defense in

<sup>16</sup> Personal communication from project director, July 1956.

<sup>17</sup> Personal letter from project director, January 1956.

reviews of the book which appeared in Springdale and neighboring towns. For example, the *Times* in the county seat:<sup>24</sup>

*The Small Town in Mass Society*—[Springdale]  
Says It Isn't So

*Small Town in Mass Society*, by Arthur J. Vidich and Joseph Bensam (Princeton University Press, \$6.00)

An accurate review of this book should be from the viewpoint of a professional sociologist since it is intended as a textbook for the social sciences.

Lacking that point of view, our interest in the book stems from the fact that it is written by a former resident of [Springdale] and concerns itself with "class, power and religion" in [Springdale], called Springdale in the book.

Mr. Vidich is currently about as popular in [Springdale] as the author of *Peyton Place* is in her small town and for the same reason—both authors violate what Vidich calls the etiquette of gossip.

During the three years he lived here, Vidich was engaged in a research project, "Cornell Studies in Social Growth" sponsored by the New York State College of Home Economics and with the aid of funds from the National Institute of Mental Health, United States Public Health Service and the Social Science Research Council.

He then proceeded to use portions of the survey material, making Cornell very unhappy, added a considerable amount of misinformation and gossip and drew certain conclusions based on the three sources.

The Cornell survey material is fairly accurate and pertains to economics and population trends. The misinformation indicates that Vidich is something less than a scientist and has either deliberately distorted facts to prove his personal conclusions or has failed to inquire into basic facts. For example, he states that the railroad running through the village has not made a stop there in years: this misstatement seems immaterial except that he uses it to bolster his conclusion that local business is at a standstill.

He discusses the failure of ecumenicalism in [Springdale], stating that Episcopal and Congregational churches failed to merge because of the opposition of powerful members of the older generation who were fearful of losing the traditions of their churches. Actually, no merger was ever contemplated

and the temporary arrangement of sharing one minister ceased when his superior decided he was being overworked.

The inference is that [Springdale] is living in the past, unable to accept new ideas of mass society, and, further, that it is run by certain individuals.

The theme of control runs throughout the book. [Springdale] citizens will be amazed to discover that practically every phase of daily living is subject to the whims of one man and his cohorts. They run local government, including the school, decide church policies and influence the economic life of the community.

No attempt is made to disguise the individuals who may be readily identified by anyone having any knowledge of [Springdale]. In this field, Vidich seems to have resorted to pure gossip as his source of material.

The author is shocked by the fact people settle their differences in private rather than resorting to public argument; economy in government becomes "the psychology of scarcity"; he arrives at the conclusion people work fantastically hard to avoid coming to terms with themselves.

He finally sums up the whole picture by proclaiming that the entire population is disenchanted, has surrendered all aspirations and illusions. But, says he, [Springdalers] are too stupid to realize they are frustrated. To a certain extent (they) live a full and not wholly unenjoyable life. "Because they do not recognize their defeat, they are not defeated."

"Life consists in making an adjustment that is as satisfactory as possible within a world which is not often tractable to basic wishes and desires."

It should not have taken 314 pages of repetition and technical language to discover that life, as so defined, is not a problem peculiar to a small town.—CC<sup>25</sup>

The reactions of some of the people in the community were recorded in one part of a three-part feature story about the book which was carried by the *Ithaca Journal*. The varied reactions indicate that the town's response was not monolithic, and, moreover, that not all persons had equally absorbed the public relations.

#### *Book's Sales Spiral in Subject Village*

Here is the last of three articles about a book and its effect on the town about which it was written.

By Donald Greet

For a book that costs \$6 and is "slow" to read, *Small Town* proved to be a best-seller in [Springdale].

Elmer G. Kilpatrick, proprietor of a main street store, sold more than two dozen copies. He says only *Peyton Place* in a half-dollar paperback did better.

<sup>25</sup> *Oswego Times*, January 31, 1958.

<sup>24</sup> We were not able to secure a copy of the issue of the *Springdale Courier* which carried the review of the book. Though we wrote to the publishers asking for copies and enclosed funds to cover costs, no one ever replied to our request. Our relations with Cornell have been so strained that we have not asked to see their files on the matter. Reviews appeared in other regional papers like the *Ithaca Journal*, but we have made no effort to collect these.

Mrs. Mary Lou Van Scoy, librarian at the village library (which does not have *Peyton Place*), says two copies "have been on the move since we got it."

One copy, she says, "got bitten up by a dog."

There is evidence, then, that a good many people in [Springdale] have read the book and a good many more have been treated to certain salient passages by their friends.

Ask a waitress in the local restaurant if she is acquainted with *Small Town* and she will say, "Oh, yes, that book."

The three persons who felt the chief impact of the book are called in its pages Sam Lee, Howard Jones and John Flint.

Villagers know these men respectively as C. Arthur Beebe, C. Paul Ward and Winston S. Ives. Beebe is the retired head of the [Springdale] *Courier*. Ward is a partner in Ward & Van Scoy Feed Mills and Ives is an attorney.

All three have been and are active in local politics. The book refers to the threesome as the "invisible government," a term that has provoked both merriment and anger in [Springdale].

All three proved real enough to give their impression of *Small Town*. Says Beebe: "People have talked over every situation in the book. They have not felt generally that the book was fair."

"It was not as objective as it was supposed to have been. It was only one man's opinion. He (author Vidich) was judging a small community by big city standards. We felt it was sneaky."

Ward comments: "The whole thing is based on gossip and is not a true study. He (Vidich) didn't find it out by any bona fide investigation."

"The book could just as well have been written from New York (City). It was not a scientific study, which is what it purports to be."

Attorney Ives is somewhat more generous:

"Two-thirds of the book is probably alright but he (Vidich) got into his biggest difficulty with personalities and in dealing with certain recent events."

"My principal objection to the book is that there are unfortunately a number of factual inaccuracies which in some cases create a distinctly misleading impression."

"Another objection is that the book suggests 'invisible government' had no motive but control. In my experience and to my knowledge leaders have been motivated to do what they thought best for the community."

Others in town added their comments. The Rev. V. F. Cline, minister of the Baptist Church for 14 years, said: "It (the book) has caused a suspicion between individuals and groups."

Funeral director Myron Miller puts it succinctly: "Much ado about nothing."

Off-the-cuff statements, not intended for quoted publication, indicate that some portions of the book struck pretty close to home and gave [Springdalers] the chance to see themselves as others see them.

Said one observer: "The book did more to allay apathy in [Springdale] than anything in a long time."

Perhaps it is just coincidence, but interest in a village election this spring shot up from the usual two dozen voters to 178.

The village's two fire companies, needled in the book for pursuing their separate ways over the years, recently joined forces.

One thing is certain: Walk into [Springdale] and mention *Small Town* and you won't get away without a reaction. Those reactions range from horse-laughs, to polite smiles to the angry bristle of a porcupine.<sup>26</sup>

\* \* \*

[T]here are at least three different criteria by which the fundamental values in research can be evaluated:

1. By the ethic of scientific inquiry—the pursuit of knowledge for the sake of knowledge regardless of its consequences.

2. By the ethic of bureaucratic inquiry. . . .

3. And by the ethic of Christian human relations—for the sake of helping or at least not hurting others.

Every organizational structure imposes its own set of ethics on the individuals who work in it. This is largely because ethics have largely come to be work rules. Knowing that bureaucratic research is here to stay means also that bureaucratic ethics are here to stay, and that, furthermore, they will be elaborated in formal codes as part of the bureaucratic rules. All current trends in bureaucratic research point in the direction of ethical and professional codes which try to specify codes of research conduct that will be consistent with the exigencies of the bureaucratic method of research.

\* \* \*

However, the ethic of independent and disinterested research with regard only for the creation of new theories and the discovery of new facts is much older than the modern bureaucratic ethic. At some point almost everyone is willing to accept the ancient Greek ideal of personal integrity, especially after an individual scholar produces valuable and useful results.

\* \* \*

The work of the individual scholar, no matter where he is located, and no matter how he is financed, organized, constrained, or aided, is perhaps the sole source of creativity. The successful placing of limitations on individual scholar

<sup>26</sup> *Ithaca Journal* June 13, 1958.

arship under the guise of "ethics," work rules, institutional responsibility, or higher considerations forces a society to live off the intellectual capital of its independent thinkers.

## 2.

### Interferences with Psychological Integrity

- a. **M. M. Berkun, H. M. Bialek, R. P. Kern,  
and K. Yagi**  
**Experimental Studies of  
Psychological Stress in Man\***

Degradation of behavior in combat has always occupied the attention of commanders. Behavior in battle may also maintain the prebattle level of proficiency, or surpass it even to the point of heroism; but this does not present a problem as does behavior that has visibly deteriorated under the stress of combat.

This apparently well-documented degradation of behavior in combat was one of the principal problems presented for research when the United States Army first entered into contract with the George Washington University for the establishment of the Human Resources Research Office (HumRRO). Research Task FIGHTER was organized within HumRRO to study the causes of behavioral degradation under psychological stress and to recommend personnel management and training procedures to the Army for reducing the severity of this problem.

\* \* \*

The stratagem of inducing a threat by communicating information, rather than by presenting only the stimuli for primitive fears, can be used to create for *S* a situation in which he is expected or required to do something which is measurable and which is relevant to the threat he perceives.

As an illustration, consider a hypothetical experiment in which *S* is told to drive an ambulance to an isolated site to bring a critically injured man to the hospital. The ambulance stalls on the way out and the driver must get it going again without assistance. Now, the ambulance and its "gimmicked" motor are perceptual supports—merely props. The essential element in the stress is information given the driver verbally.

There is a specific task: to trouble-shoot the engine and correct the defect which has been planted in it by the experimenter. This task is meaningful in terms of the emergency with which *S* must deal—and the emergency dominates his motivation. By being thus "embedded" in the situation, the task does not expose the deception to *S*, nor does his performance of it depend exclusively upon his motivation to please himself or to please his examiner with a high test score. Further, the measure is taken during the existence of the emergency, so that performance can be observed during as well as after the stress.

\* \* \*

Five proposed stressor situations have been tested and compared with appropriate control situations on each of the above measures. . . .

\* \* \*

#### *Ss and Equipment*

*Ss* were simply passengers aboard an apparently stricken plane which was being forced to "ditch" or crash-land, and the performance required of them consisted of filling out two forms which appeared reasonable for the situation. The first, called the Emergency Data Form, was a description of, and instructions for, disposition of the individual's personal possessions in case of death. It was an example of deliberately bad human engineering, consisting of complicated directions on 23 categories of items. The second form, Official Data on Emergency Instructions, was an achievement test with 12 multiple-choice items testing retention of airborne-emergency instructions which all *Ss* had been required to read, ostensibly as standard operating procedure before the flight. All materials developed for this purpose were pretested with a group of *Ss* comparable to those for whom they were intended.

A twin-engine DC-3 military aircraft, capable of carrying 12 passengers plus crew, was used. It was equipped with an earphone intercom and a reading table for each passenger. Sixty-six men, aged 18–24, in their first 8 weeks of Army Basic Training, who had demonstrated ordinary reading knowledge of English, were randomly selected and assigned to one of three groups—an Experimental group, a Flying Control group, and a Grounded Control group. The Flying Control group was taken up for a flight but was not exposed to a simulated emergency. The Grounded Controls were given the same measures as were the other *Ss*, but did not fly at all.

\* 76 *Psychological Monographs* (No. 15) 1–8 (1962). Reprinted by permission.

### Procedure

The experiment was conducted on 2 successive days. On the morning of the first day half of the Controls flew, and in the afternoon half of the Experimentals flew. On the second day the sequence was reversed for the remaining flying Ss. The Grounded Control group was split into four subgroups, one tested each morning and one each afternoon. The time schedule for administration of the various tests was kept constant for all groups.

One group of 10 Ss at a time was taken to the airport, purportedly to participate in a study of the effects of altitude on psychomotor performance. One experimenter supposedly conducting this study, and another experimenter disguised as a steward accompanied Ss. Ss were informed that their urine was to be collected after the flight; its chemical analysis to be "correlated" with their psychological test performance. To permit collection of only the urine secreted during and immediately after the flight, these Ss were required to void their bladders ½ hour before take-off.

Prior to boarding, an airport representative gave all personnel, including the experimenters, a folder containing the emergency instructions to be studied for 10 minutes. As standard operating procedure aboard military aircraft, all personnel, including the plane crew, donned life preservers and parachute harnesses under supervision of the aircraft commander.

Once aloft, at 5,000 feet Ss completed one irrelevant test and then waited for the plane to reach a higher altitude. In the case of the Experimental group flights, the aircraft lurched while changing altitude. Ss saw that one propeller had stopped turning and heard about other malfunctions over the intercom; they were then informed directly that there was an emergency. A simulated pilot-to-tower conversation was provided to the Ss over their earphones to support the deception. As the aircraft passed within sight of the airfield, Ss could see fire trucks and ambulances on the airstrip in apparent expectation of a crash landing.

After several minutes the pilot ordered the plane steward to prepare for ditching in the nearby ocean, since the landing gear would not function properly. This was a prearranged signal for the "steward" to administer the questionnaires both to Ss and to the overt experimenter, to make it appear to Ss that this experimenter was in the same emergency situation in which they found themselves. The Emergency Data

Form was given first, on a straightforward basis, because requesting information of this sort would seem plausible under the circumstances. The Emergency Instructions test was given under the pretext that this would furnish proof to insurance companies that emergency precautions had been properly followed. These papers were supposedly to be put in a waterproof container and jettisoned before the aircraft came down on the ocean.

While Ss worked on both of these pencil-and-paper tests, the pilot-to-tower communication was cut out from their earphones to permit them to work without distraction. At no time was there any intercommunication among Ss since both the aircraft noise and the seating arrangement made any contact impossible. After a specified time period, the aircraft made a safe landing at the airport. Ss were taken to a nearby classroom where, after individually responding to the SSS (Table 1), they were thoroughly informed as to the true nature and purpose of the experiment.

About 1 hour later, a group interview was arranged to further probe the Ss for their reaction to the situation, to determine if any had seen through the deception, and to calm any fears which might remain about the seriousness of the situation. Urines were collected at 3 hours after landing.

The Flying Control group was exposed to the same flight procedure, except that no emergency was simulated. At the proper time, both the overt experimenter and Ss were given the measures by the steward, ostensibly as routine operating procedure aboard military aircraft.

The Grounded Control Ss did not fly at all; they were told that the purpose of the experiment was to develop forms and questionnaires for use by the military. Ss in this group were to fill out what they thought were trial versions of these forms as part of the developmental process.

Before any of the data were processed, five experimental Ss were eliminated because it was determined that they had not been deceived. During the interviews one S reported that he saw figures written on an airsickness bag used as scratch paper by a previous S. Another accidentally overheard a genuine conversation on the intercom between the pilot and the flight engineer of the aircraft. The other three had had considerable flight experience and were able, cognitively, to determine that the emergency was not authentic.

The group interview with the Experimental

Ss served generally to confirm the expectations of the experimenters that the situation was perceived as real, that various degrees of anxiety had been aroused in Ss about the possibility of death or injury, and that there were various degrees of self-confidence about ability to survive a possible ditching. The anxiety was determined to have disappeared by the time of the interview.

\* \* \*

*Three Cognitive Stresses Embedded into a Military Exercise*

\* \* \*

The three situations comprising this experiment have this in common: *S* is led to believe that he is in immediate danger of losing his life or of being seriously injured, and the task required of all Ss relevant to the predicament is the same. The situations differ only in the events contrived to cause the "emergency": accidental nuclear radiation in the area, a sudden forest fire in the area, or misdirected incoming artillery shells. These three events can be ordered along the dimension of amount of *perceptual support* (this term refers to the saliency and/or intensity of environmental cues confirming the cognitive elements of the stressor).

The situation with the least perceptual support is identified as Chemical, Biological, and Radiological (CBR) warfare. *S* is stationed alone at an isolated outpost and is told to report to the Command Post by radio the presence of any aircraft overhead. He later hears over his radio that an accident with radioactive material has resulted in dangerous fallout over his area. He is led to believe that the accident occurred during the exercise but is definitely not an intentional part of it. Immediate rescue is possible for him only if he can report his location over his radio transmitter, which has quite suddenly failed. The failure of his transmitter is, to his knowledge, coincidental with the accidental radiation hazard. The maneuver in which he was participating is canceled because of the accident and all activity now is concerned solely with the evacuation of personnel from the affected area. The only perceptual confirmation available at the position is an instrument which presumably (but not actually) measures the amount of nuclear radiation in the area.

The setting for the second situation is the same, except that the "accident" is a forest fire surrounding *S*'s outpost. For perceptual support, *S* is enveloped in artificial smoke generated about

300 yards away. This cue is more obvious than the radiation dosimeter in the CBR situation. Again, his failing radio thwarts his rescue.

In the third situation a series of explosions simulates a barrage of artillery shells coming in and bursting near *S*. These explosions substantiate reports which *S* hears on his radio to the effect that some artillery shells appear to be hitting outside the designated target area. The explosions constitute the most salient of the perceptual supports used in these three situations. As in the other situations, *S*'s transmitter—his key to rapid rescue—inexplicably fails, though he continues to receive messages.

The Control group, to be contrasted with each of the above three Experimental groups, needs radio communication in order to request future rations and water. This was an important incentive for their radio repair work but since their immediate needs had been met, it clearly was not at the same level of intensity as were those of the Experimental treatments.

\* \* \*

b.

W. E. Glover, A. D. M. Greenfield, and  
R. G. Shanks

**The Contribution Made by Adrenaline to the Vasodilation in the Human Forearm during Emotional Stress\***

\* \* \*

. . . In the present experiments DCI [dichloroisopropylnor-adrenaline] has been used to define the contribution of adrenaline to the changes in blood flow in the forearm in response to emotional stress.

The experiments were carried out on six healthy young men. The subject, wearing normal indoor clothing, lay on a couch in a laboratory, the temperature in which was maintained constant in the range 20–22°C. . . A needle was inserted in the left brachial artery, and through it saline was infused at a rate of 4 ml./min by means of a mechanically driven syringe. In three experiments adrenaline hydrochloride was infused intra-arterially for 3–4 min., and in four experiments was infused into an antecubital vein for 5 min. Ascorbic acid was added to the perfusates as a preservative. DCI was infused intra-arterially for periods up to 30 min.

"Emotional stress" was produced by lead-

\* 164 *Journal of Physiology* 422, 423, 428 (1962). Reprinted by permission.

ing the subject to believe that the wrong dose of a drug had been infused into his arm and that he was in considerable peril.\* After 3–10 min. the subject was reassured and the purpose of the experiment was explained. Such observations can be made only once on each subject, and this limited the number of experiments we could make.

\* \* \*

. . . The results . . . agree with the conclusion of Blair *et al.* that the humoral contribution to the increase in forearm blood flow during emotional stress may vary from subject to subject, and the conclusion of Barcroft *et al.* that both humoral and nervous factors are concerned. It would be interesting to know whether the same subject always produced the same type of response to varying emotional stimuli. Unfortunately, it is possible to hoax the subject on only one occasion.

C.

**Allen E. Bergin**  
**The Effect of Dissonant Persuasive**  
**Communications upon Changes in a**  
**Self-Referring Attitude†**

\* \* \*

The purposes of the present study were (a) to test whether conclusions regarding the role of credibility and discrepancy in attitude change research remain valid when "interpretations" about self-referring attitudes are involved, and (b) to test predictions derived from dissonance theory‡ concerning the effects of extremely dis-

crepant communications upon attitude change under conditions of high issue involvement and under varying levels of communicator credibility.

Specifically, it was predicted that (a) Ss receiving a communication from a source of high credibility would change their self-ratings in the direction of the communication significantly more than would Ss receiving a similar communication from a low-credibility source, (b) the amount of change in the high-credibility condition would increase monotonically as discrepancy increased, and (c) that the difference in amount of change between the low- and high-credibility conditions would increase as discrepancy increased.

*Method*

To test the hypotheses attempts were made to change the Ss' conceptions of their own masculinity or femininity by manipulating the discrepancy and credibility of a communication about those characteristics. Ss first rated themselves on masculinity-femininity following which they received a communication on this subject at one of three discrepancy levels from either a high- or low-credibility source. Ss subsequently made a second self-rating of masculinity-femininity and the difference between the two ratings was taken as the index of attitude change.

Ss were 60 freshmen and sophomores enrolled in an introductory psychology course at Stanford. They were distributed randomly among the six experimental conditions provided by the

\* "[T]he words 'considerable peril' were perhaps wrongly chosen, and have caused some people to form an opinion of the experiments which differs from what actually happened. The aim of the experiments was physiological, not pathological, and was to reproduce in the laboratory a state of alarm of a quality that is frequently experienced in normal life—for example when driving, and a pedestrian emerges from behind a parked vehicle, or on missing an important connection for a vital appointment. One of my colleagues who was present on all occasions makes the important point that in the 'acting,' subjects were not led to think, for example, that there was any danger to life. We should obviously have taken more space to describe the methods more clearly." [Letter from A.D.M. Greenfield, May 1, 1971.]

† 30 *Journal of Personality* 423, 425–427, 434, 436 (1962). Reprinted by permission.

‡ Festinger's theory of cognitive dissonance (1957) provides a theoretical basis for organizing

the attitude-change variables with which we are principally concerned. The theory assumes that individuals strive to maintain consistency among their cognitions and that the existence of nonfitting cognitive elements produces tension which a person tries to reduce. A dissonance-producing situation common to both persuasion and interpretation is one in which a communicator presents a view contrary to the one held by the communicatee. In this event, the person is confronted with a need to reduce the dissonance produced by the presence of two contrary cognitions. A prediction of how he will choose to reduce the resultant state of dissonance will be in part a function of the credibility of the communicator, in part of the degree of discrepancy between the communicator and communicatee's positions, and in part of personal involvement with the communication content. Although these factors have been manipulated experimentally in studies of communication and persuasion, they have not been applied to the problem of changing attitudes about oneself which is a major purpose of interpretation.



three degrees of communication discrepancy and the two levels of communicator credibility. Males and females were distributed approximately evenly among the conditions in order to equalize any effects of sex differences in responses to the communication.

In the high-credibility condition *Ss* reported individually to the Psychiatry Department of the Stanford Medical Center where the *E* assumed the role of director of a personality assessment project. To further establish his credibility, *Ss* were sent to *E* by a receptionist, and the experimental room was furnished with elaborate equipment, a couch, an impressive array of medical and psychological volumes, and a large portrait of Freud. In the first session *Ss* were informed that the study was concerned with peoples' accuracy in evaluating their own personalities. The procedure was described as one in which *S* would rate himself on some scales after which he would take a battery of diagnostic tests. The degree of agreement between his self-description and that yielded by the objective measures would then be determined.

After rating himself, *S* was administered an elaborate battery of tests including cards from the Rorschach and TAT, the *Fe* scale from the California Personality Inventory (CPI), the Draw-A-Person test, a word-association test, and a specially devised figures-perception test concerning descriptions of male, female, and neutral figures. During this portion of the procedure *Ss* were hooked up to two disguised electroshock-therapy sets which were connected by means of false wiring to the *S's* chest and the palm of the right hand and in turn to a kymograph which reeled off (previously recorded) readings of *S's* responses to the test stimuli. *Ss* were informed that these instruments yielded physiological measures of their underlying emotional reactions which were extremely valid indicators of a person's underlying personality predispositions. The session was also tape recorded to further impress the *Ss* with the rigor of the procedures.

In a second session within the same week *Ss* were shown a Rorschach Psychogram and a set of Leary Interpersonal Diagrams which were purported to represent an analysis of their test responses. It was indicated that these diagrams summarized all of the test material and provided a set of discrepancy scores that could be translated back to the self-rating forms. After emphasizing the validity of the psychological evaluation, *E* described to *S* the (predetermined) discrepancy (moderate, high, or extreme) be-

tween the *S's* self-rating on masculinity-femininity and the rating supposedly derived from the test analysis.

\* \* \*

In the low-credibility conditions *Ss* reported for the first session to a decrepit room in the basement of the Education Building where they simply filled out a set of self-ratings.

At the beginning of the second session *Ss* were introduced to another person, purportedly another subject with whom *S* would participate in the experiment. The "other" subject was actually *E's* confederate, a high school freshman. The *Ss* were told that the experiment was concerned with two different problems. The first, a study of interpersonal perception, required each of them to rate their partner on the personality dimensions they had rated themselves on several days earlier. The confederate then pretended to rate *S* on the dimensions; however, the actual ratings had been made by *E* in advance of the session according to the discrepancy condition to which *S* was assigned. When the confederate finished he was asked to transfer these ratings onto *S's* original self-ratings so that a direct comparison could be made. During this time *S* was kept busy answering the CPI. *S* was then shown his own self-rating in comparison to that made of him by the confederate. The discrepancy was discussed in the course of which *E* implied that high school students may not be the best judges of the traits of others.

\* \* \*

Four rating scales were devised covering personality traits of concern to college students. These were: dominance-submission, masculinity-femininity, hostility, and independence-dependence. The masculinity-femininity scale was chosen as the dimension on which the discrepant communication was to be made, since the *Ss* were assumed to have special concern for their masculine or feminine image and, therefore, high involvement with communications on this topic. The masculinity-femininity scale consisted of seven defined points and six intermediate points, making a 13-point scale ranging from "completely feminine" to "slightly more masculine than feminine" for females and from "completely masculine" to "slightly more feminine than masculine" for males. The other three scales were used to provide a context for the critical scale and thus to disguise the persuasive intent of the relevant communication.



The three discrepancy levels (moderate, high, extreme) used were defined in terms of latitudes of acceptance and rejection. . . . In this experiment, a communication of moderate discrepancy was one within *S*'s latitude of acceptance and at least two points away from his most acceptable rating. A high discrepancy was within *S*'s latitude of rejection and at least four points away from his most acceptable rating. An extreme discrepancy was at the point *S* had rated as most contrary to his own view of himself and was at least six points away from his most acceptable rating. The communication of these ratings was accomplished by marking *S*'s self-rating in red at the predetermined discrepancy level so that the comparison was obvious. . . .

\* \* \*

To prevent distorted perceptions of the message in the direction of one's previous beliefs the communications were made as unambiguous as possible. The communicator's rating was shown as a distinct red mark on the self-rating scale with the distance away from *S*'s own rating clearly shown. His attention was explicitly focused on this difference by *E*'s instructions. . . .

\* \* \*

#### Discussion

The results are consistent with the predictions derived from cognitive dissonance theory. They suggest that amount of dissonance increases monotonically with amount of discrepancy and when attitude change is the primary means available to the *S* for reducing dissonance, attitude change will also increase monotonically. On the other hand, when involvement and dissonance are high and credibility is low, other means—such as discrediting the communicator—become the primary dissonance-reducing response and amount of change is minimized.

\* \* \*

It could be argued that the changes in self-ratings simply represent compliance with the wishes of a prestigious figure rather than a re-orientation in the way *S*s view themselves. It appears that such compliance effects were minimized as is evidenced by the following: (a) the emotional reactions of *S*s (grasping, turning redfaced, poring over the rating scale) in the high-credibility condition indicated that they were more serious about the discrepancies and what they implied than if they intended merely

to comply, (b) the avoidance by the *E* of giving the impression that he wanted *S*s to change their self-ratings, and (c) the fact that in the post-experimental interview when *S*s were asked whether they had changed their self-ratings, a sizable proportion (35 per cent) had changed significantly without being aware of it. They reported that the second rating was their own view and that it had not changed since the time of the original ratings. This was quite startling in light of the fact that many of these people had shifted several points in the direction of the communication. Apparently they were committed to the new rating, not simply publicly espousing it while privately rejecting it.

\* \* \*

#### NOTES

##### NOTE 1.

HERBERT C. KELMAN  
HUMAN USE OF HUMAN SUBJECTS—THE  
PROBLEM OF DECEPTION IN SOCIAL  
PSYCHOLOGICAL EXPERIMENTS\*

\* \* \*

Ethical problems of a rather obvious nature arise in the experiments in which deception has potentially harmful consequences for the subject. Take, for example, the brilliant experiment by Mulder and Stermerding on the effects of threat on attraction to the group and need for strong leadership. In this study—one of the very rare examples of an experiment conducted in a natural setting—independent food merchants in a number of Dutch towns were brought together for group meetings in the course of which they were informed that a large organization was planning to open up a series of supermarkets in the Netherlands. In the High Threat condition, subjects were told that there was a high probability that their town would be selected as a site for such markets and that the advent of these markets would cause a considerable drop in their business. On the advice of the executives of the shopkeepers' organizations, who had helped to arrange the group meetings, the investigators did not reveal the experimental manipulations to their subjects. I have been worried about these Dutch merchants ever since I heard about this study for the first time. Did some of them go out of business in anticipation of the heavy competition? Do some of them have an

\* 67 *Psychological Bulletin* 1, 3-4 (1967). © 1967, American Psychological Association. Reprinted by permission.

anxiety reaction every time they see a bulldozer? Chances are that they soon forgot about this threat (unless, of course, supermarkets actually did move into town) and that it became just one of the many little moments of anxiety that must occur in every shopkeeper's life. Do we have a right, however, to add to life's little anxieties and to risk the possibility of more extensive anxiety purely for the purposes of our experiments, particularly since deception deprives the subject of the opportunity to choose whether or not he wishes to expose himself to the risks that might be entailed?

\* \* \*

#### NOTE 2.

##### JULIUS SEEMAN DECEPTION IN PSYCHOLOGICAL RESEARCH\*

\* \* \*

With respect to the incidence of deception, it may be useful to note the frequency with which deception appears in the published literature and to determine whether any long-term trends are evident. For this purpose, the total published literature in several journals was analyzed for the years 1948 and 1963. Journals were chosen to reflect different fields within psychology.

[J]ournals emphasizing "experimental" and "clinical" areas had a relatively low incidence of deception studies in comparison with "personality and social" areas. The latter areas also showed a distinct rise in the use of deception. When the figures for the *Journal of Personality* and the *Journal of Abnormal and Social Psychology* are combined, the mean for 1948 is 18.47 percent and the mean for 1963 is 38.17 percent. It seems safe to conclude that to some degree deception has come to be the method of choice in this area of research.

\* \* \*

d.

##### Stanley Milgram Some Conditions of Obedience and Disobedience to Authority†

The situation in which one agent commands another to hurt a third turns up time and again as a significant theme in human relations. . . .

. . . We describe an experimental program,

\* 24 *American Psychologist* 1025 (1969). Reprinted by permission.

† 18 *Human Relations* 57-75 (1965). Reprinted by permission.

recently concluded at Yale University, in which a particular expression of this conflict is studied by experimental means.

In its most general form the problem may be defined thus: if X tells Y to hurt Z, under what conditions will Y carry out the command of X and under what conditions will he refuse. In the more limited form possible in laboratory research, the question becomes: if an experimenter tells a subject to hurt another person, under what conditions will the subject go along with this instruction, and under what conditions will he refuse to obey. The laboratory problem is not so much a dilution of the general statement as one concrete expression of the many particular forms this question may assume.

One aim of the research was to study behavior in a strong situation of deep consequence to the participants, for the psychological forces operative in powerful and lifelike forms of the conflict may not be brought into play under diluted conditions.

This approach meant, first, that we had a special obligation to protect the welfare and dignity of the persons who took part in the study; subjects were, of necessity, placed in a difficult predicament, and steps had to be taken to ensure their well-being before they were discharged from the laboratory. Toward this end, a careful, post-experimental treatment was devised and has been carried through for subjects in all conditions.<sup>3</sup>

<sup>3</sup> It consisted of an extended discussion with the experimenter and, of equal importance, a friendly reconciliation with the victim. It is made clear that the victim did not receive painful electric shocks. After the completion of the experimental series, subjects were sent a detailed report of the results and full purposes of the experimental program. A formal assessment of this procedure points to its overall effectiveness. Of the subjects, 83.7 per cent indicated that they were glad to have taken part in the study; 15.1 per cent reported neutral feelings; and 1.3 per cent stated that they were sorry to have participated. A large number of subjects spontaneously requested that they be used in further experimentation. Four-fifths of the subjects felt that more experiments of this sort should be carried out, and 74 per cent indicated that they had learned something of personal importance as a result of being in the study. Furthermore, a university psychiatrist, experienced in outpatient treatment, interviewed a sample of experimental subjects with the aim of uncovering possible injurious effects resulting from participation. No such effects were in evidence. Indeed, subjects typically felt that their participation was instructive and enriching. . . .

*Terminology.* If Y follows the command of X we shall say that he has obeyed X; if he fails to carry out the command of X, we shall say that he has disobeyed X. The terms *to obey* and *to disobey*, as used here, refer to the subject's overt action only, and carry no implication for the motive or experiential states accompanying the action.

\* \* \*

A subject who complies with the entire series of experimental commands will be termed an *obedient* subject; one who at any point in the command series defies the experimenter will be called a *disobedient* or *defiant* subject. As used in this report, the terms refer only to the subject's performance in the experiment, and do not necessarily imply a general personality disposition to submit to or reject authority.

*Subject Population.* The subjects used in all experimental conditions were male adults, residing in the greater New Haven and Bridgeport areas, aged 20 to 50 years, and engaged in a wide variety of occupations. Each experimental condition described in this report employed 40 fresh subjects and was carefully balanced for age and occupational types. The occupational composition for each experiment was: workers, skilled and unskilled: 40 per cent; white collar, sales, business: 40 per cent; professionals: 20 per cent. . . .

*The General Laboratory Procedure.* The focus of the study concerns the amount of electric shock a subject is willing to administer to another person when ordered by an experimenter to give the "victim" increasingly more severe punishment. The act of administering shock is set in the context of a learning experiment, ostensibly designed to study the effect of punishment on memory. Aside from the experimenter, one naive subject and one accomplice perform in each session. On arrival each subject is paid \$4.50. After a general talk by the experimenter, telling how little scientists know about the effect of punishment on memory, subjects are informed that one member of the pair will serve as teacher and one as learner. A rigged drawing is held so that the naive subject is always the teacher, and the accomplice becomes the learner. The learner is taken to an adjacent room and strapped into an "electric chair."

The naive subject is told that it is his task to teach the learner a list of paired associates, to test him on the list, and to administer punishment whenever the learner errs in the test. Punishment

takes the form of electric shock, delivered to the learner by means of a shock generator controlled by the naive subject. The teacher is instructed to increase the intensity of electric shock one step on the generator on each error. The learner, according to plan, provides many wrong answers, so that before long the naive subject must give him the strongest shock on the generator. Increases in shock level are met by increasingly insistent demands from the learner that the experiment be stopped because of the growing discomfort to him. However, in clear terms the experimenter orders the teacher to continue with the procedure in disregard of the learner's protests. *Thus, the naive subject must resolve a conflict between two mutually incompatible demands from the social field. He may continue to follow the orders of the experimenter and shock the learner with increasing severity, or he may refuse to follow the orders of the experimenter and heed the learner's pleas. The experimenter's authority operates not in a free field, but against ever-mounting countervailing pressures from the person being punished.*

A few technical points need to be considered before the findings of the study are described. For the purpose of delivering shock, a simulated shock generator is used, with 30 clearly marked voltage levels that range from 15 to 450 volts (and each activated by one of 30 individual lever switches set in a horizontal line). The naive subject is given a sample shock of 45 volts to convince him of the authenticity of the instrument. The generator bears verbal designations that range from "Slight Shock" to "Danger: Severe Shock."

The responses of the victim are standardized on tape, and each protest is coordinated to a particular voltage level on the shock generator. Starting with 75 volts the learner begins to grunt and moan. At 150 volts he demands to be let out of the experiment. At 180 volts he cries out that he can no longer stand the pain. At 300 volts he refuses to provide any more answers to the memory test, insisting that he is no longer a participant in the experiment and must be freed. In response to this last tactic, the experimenter instructs the naive subject to treat the absence of an answer as equivalent to a wrong answer, and to follow the usual shock procedure. The experimenter reinforces his demand with the statement: "You have no other choice, you must go on!" (This imperative is used whenever the naive subject tries to break off the experiment.) If the subject refuses to

give the next higher level of shock, the experiment is considered at an end. A quantitative value is assigned to the subject's performance based on the maximum intensity shock he administered before breaking off. Thus any subject's score may range from zero (for a subject unwilling to administer the first shock level) to 30 (for a subject who proceeds to the highest voltage level on the board). For any particular subject and for any particular experimental condition the degree to which participants have followed the experimenter's orders may be specified with a numerical value, corresponding to the metric on the shock generator.

\* \* \*

*Pilot Studies.* Pilot studies for the present research were completed in the winter of 1960; they differed from the regular experiments in a few details: for one, the victim was placed behind a silvered glass, with the light balance on the glass such that the victim could be dimly perceived by the subject.

Though essentially qualitative in treatment, these studies pointed to several significant features of the experimental situation. At first no vocal feedback was used from the victim. It was thought that the verbal and voltage designations on the control panel would create sufficient pressure to curtail the subject's obedience. However, this was not the case. In the absence of protests from the learner, virtually all subjects, once commanded, went blithely to the end of the board, seemingly indifferent to the verbal designations ("Extreme Shock" and "Danger, Severe Shock"). This deprived us of an adequate basis for scaling obedient tendencies. A force had to be introduced that would strengthen the subject's resistance to the experimenter's commands, and reveal individual differences in terms of a distribution of break-off points.

This force took the form of protests from the victim. Initially, mild protests were used, but proved inadequate. Subsequently, more vehement protests were inserted into the experimental procedure. To our consternation, even the strongest protests from the victim did not prevent all subjects from administering the harshest punishment ordered by the experimenter; but the protests did lower the mean maximum shock somewhat and created some spread in the subject's performance; therefore, the victim's cries were standardized on tape and incorporated into the regular experimental procedure.

*The situation did more than highlight the*

*technical difficulties of finding a workable experimental procedure: it indicated that subjects would obey authority to a greater extent than we had supposed. It also pointed to the importance of feedback from the victim in controlling the subject's behavior.*

One further aspect of the pilot study was that subjects frequently averted their eyes from the person they were shocking, often turning their heads in an awkward and conspicuous manner. One subject explained: "I didn't want to see the consequences of what I had done." Observers wrote:

... subjects showed a reluctance to look at the victim, whom they could see through the glass in front of them. When this fact was brought to their attention they indicated that it caused them discomfort to see the victim in agony. We note, however, that although the subject refuses to look at the victim, he continues to administer shocks.

This suggested that the salience of the victim may have, in some degree, regulated the subject's performance. If, in obeying the experimenter, the subject found it necessary to avoid scrutiny of the victim, would the converse be true? If the victim were rendered increasingly more salient to the subject, would obedience diminish? The first set of regular experiments was designed to answer this question.

*Immediacy of the Victim.* This series consisted of four experimental conditions. In each condition the victim was brought "psychologically" closer to the subject giving him shocks.

In the first condition (Remote Feedback) the victim was placed in another room and could not be heard or seen by the subject, except that, at 300 volts, he pounded on the wall in protest. After 315 volts he no longer answered or was heard from.

The second condition (Voice Feedback) was identical to the first except that voice protests were introduced. As in the first condition the victim was placed in an adjacent room, but his complaints could be heard clearly through a door left slightly ajar, and through the walls of the laboratory.<sup>6</sup>

<sup>6</sup> It is difficult to convey on the printed page the full tenor of the victim's responses, for we have no adequate notation for vocal intensity, timing, and general qualities of delivery. Yet these features are crucial to producing the effect of an increasingly severe reaction to mounting voltage levels. (They can be communicated fully only by sending interested parties the recorded tapes.) In general terms,

The third experimental condition (Proximity) was similar to the second, except that the victim was now placed in the same room as the subject, and 1½ feet from him. Thus he was visible as well as audible, and voice cues were provided.

The fourth, and final, condition of this series (Touch-Proximity) was identical to the third, with this exception: the victim received a shock only when his hand rested on a shockplate. At the 150-volt level the victim again demanded to be let free and, in this condition, refused to place his hand on the shockplate. The experimenter ordered the naive subject to force the victim's hand onto the plate. Thus obedience in this condition required that the subject have physical contact with the victim in order to give him punishment beyond the 150-volt level.

Forty adult subjects were studied in each condition. The data revealed that obedience was significantly reduced as the victim was rendered more immediate to the subject. . . .

Expressed in terms of the proportion of obedient to defiant subjects, the findings are that 34 per cent of the subjects defied the experimenter in the Remote condition, 37.5 per cent in Voice Feedback, 60 per cent in Proximity, and 70 per cent in Touch-Proximity.

How are we to account for this effect? A first conjecture might be that as the victim was brought closer the subject became more aware of the intensity of his suffering and regulated his behavior accordingly. This makes sense, but

however, the victim indicates no discomfort until the 75-volt shock is administered, at which time there is a light grunt in response to the punishment. Similar reactions follow the 90- and 105-volt shocks, and at 120 volts the victim shouts to the experimenter that the shocks are becoming painful. Painful groans are heard on administration of the 135-volt shock, and at 150 volts the victim cries out, "Experimenter, get me out of here! I won't be in the experiment any more! I refuse to go on!" Cries of this type continue with generally rising intensity, so that at 180 volts the victim cries out, "I can't stand the pain," and by 270 volts his response to the shock is definitely an agonized scream. Throughout, he insists that he be let out of the experiment. At 300 volts the victim shouts in desperation that he will no longer provide answers to the memory test; and at 315 volts, after a violent scream, he reaffirms with vehemence that he is no longer a participant. From this point on, he provides no answers, but shrieks in agony whenever a shock is administered; this continues through 450 volts. Of course, many subjects will have broken off before this point.

our evidence does not support the interpretation. There are no consistent differences in the attributed level of pain across the four conditions (i.e. the amount of pain experienced by the victim as estimated by the subject and expressed on a 14-point scale). But it is easy to speculate about alternative mechanisms:

*Empathic cues.* In the Remote and to a lesser extent the Voice Feedback condition, the victim's suffering possesses an abstract, remote quality for the subject. He is aware, but only in a conceptual sense, that his actions cause pain to another person; the fact is apprehended, but not felt. The phenomenon is common enough. The bombardier can reasonably suppose that his weapons will inflict suffering and death, yet this knowledge is divested of affect, and does not move him to a felt, emotional response to the suffering resulting from his actions. . . .

*Denial and narrowing of the cognitive field.* The Remote condition allows a narrowing of the cognitive field so that the victim is put out of mind. The subject no longer considers the act of depressing a lever relevant to moral judgment, for it is no longer associated with the victim's suffering. When the victim is close it is more difficult to exclude him phenomenologically. He necessarily intrudes on the subject's awareness since he is continuously visible. . . .

*Reciprocal fields.* If in the Proximity condition the subject is in an improved position to observe the victim, the reverse is also true. The actions of the subject now come under proximal scrutiny by the victim. Possibly, it is easier to harm a person when he is unable to observe our actions than when he can see what we are doing. His surveillance of the action directed against him may give rise to shame, or guilt, which may then serve to curtail the action. . . .

\* \* \*

*Closeness of Authority.* If the spatial relationship of the subject and victim is relevant to the degree of obedience, would not the relationship of subject to experimenter also play a part?

There are reasons to feel that, on arrival, the subject is oriented primarily to the experimenter rather than to the victim. He has come to the laboratory to fit into the structure that the experimenter—not the victim—would provide. He has come less to understand his behavior than to reveal that behavior to a competent scientist, and he is willing to display himself as the scientist's purposes require. Most subjects seem quite concerned about the appearance they are making before the experimenter, and one could argue that this preoccupation in a relatively new and strange setting makes the subject somewhat in-

sensitive to the triadic nature of the social situation. In other words, the subject is so concerned about the show he is putting on for the experimenter that influences from other parts of the social field do not receive as much weight as they ordinarily would. This overdetermined orientation to the experimenter would account for the relative insensitivity of the subject to the victim, and would also lead us to believe that alterations in the relationship between subject and experimenter would have important consequences for obedience.

In a series of experiments we varied the physical closeness and degree of surveillance of the experimenter. In one condition the experimenter sat just a few feet away from the subject. In a second condition, after giving initial instructions, the experimenter left the laboratory and gave his orders by telephone; in still a third condition the experimenter was never seen, providing instructions by means of a tape recording activated when the subjects entered the laboratory.

Obedience dropped sharply as the experimenter was physically removed from the laboratory. The number of obedient subjects in the first condition (Experimenter Present) was almost three times as great as in the second, where the experimenter gave his orders by telephone. Twenty-six subjects were fully obedient in the first condition, and only 9 in the second. . . .

Moreover, when the experimenter was absent, subjects displayed an interesting form of behavior that had not occurred under his surveillance. Though continuing with the experiment, several subjects administered lower shocks than were required and never informed the experimenter of their deviation from the correct procedure. (Unknown to the subjects, shock levels were automatically recorded by an Esterline-Angus event recorder wired directly into the shock generator; the instrument provided us with an objective record of the subjects' performance.) Indeed, in telephone conversations some subjects specifically assured the experimenter that they were raising the shock level according to instruction, whereas in fact they were repeatedly using the lowest shock on the board. This form of behavior is particularly interesting; although these subjects acted in a way that clearly undermined the avowed purposes of the experiment, they found it easier to handle the conflict in this manner than to precipitate an open break with authority.

\* \* \*

*Tensions.* The description of final scores does not fully convey the character of the subjects' performance, and it would be useful to interrupt our reporting of quantitative relationships to remark on the subjects' general reaction to the situation.

One might suppose that a subject would simply break off or continue as his conscience and temperament dictated. Yet this is very far from what happened. There were powerful reactions of tension and emotional strain in a substantial proportion of the participants. Persons were observed to sweat, tremble, stutter, bite their lips, and groan as they found themselves increasingly implicated in the experimental conflict. Some sense of the subjects' mood can be gotten from a transcript of their remarks at break-off point. Here are the remarks of two defiant subjects:

Is he banging? . . . Is he hurt out there? Well, I don't want to be responsible for anything that happens to him. No, I can't go on with it. I don't know whether he's alright or not. I mean he may have a heart condition or something; I wouldn't feel right doing it. He's banging for help. He's evidently in trouble or he wouldn't bang. . . . I don't see any sense to this—to knocking him out or something. I just can't see it.

(Subject refuses to administer shocks.)

The man is banging. He wants to get out. . . . I'm sorry. If the man wants to get out I'd just as soon stop. . . . I don't want to administer any more [extremely excited]. . . . I will gladly refund the check, but I will not harm this man. . . . I'm sorry, I will not give him any more if he obviously wants to get out. There is no money in the world that will make me hurt another individual.

And here is a transcript from an obedient subject. He began the experiment calmly, but became increasingly tense as the experiment proceeded. After administering the 150-volt shock, he began to address the experimenter in agitated tones:

(150 volts delivered.) You want me to keep going? (165 volts delivered.) That guy is hollering in there. There's a lot of them here. He's liable to have a heart condition. You want me to go?

(180 volts delivered.) He can't stand it! I'm not going to kill that man in there! You hear him hollering? He's hollering. He can't stand it. What if something happens to him? . . . I'm not going to get that man sick in there. He's hollering in there. You know what I mean? I mean I refuse to take responsibility. He's getting hurt in there. He's in there hollering. Too many left here. Geez, if he gets them wrong.

There's too many of them left. I mean who is going to take responsibility if anything happens to that gentleman?

[*The experimenter accepts responsibility.*]

All right.

(195 volts delivered.) You see he's hollering. Hear that. Gee, I don't know. [*The experimenter says: "The experiment requires that you go on."*—I know it does, sir, but I mean—huh—he don't know what he's in for. He's up to 195 volts.

(210 volts delivered.)

(225 volts delivered.)

(240 volts delivered.) Aw, no! You mean I've got to keep going up with the scale? No sir. I'm not going to kill that man! I'm not going to give him 450 volts! [*The experimenter says: "The experiment requires that you go on."*—I know it does, but that man is hollering in there, sir. . . .

Despite his numerous, agitated objections, which were constant accompaniments to his actions, the subject unfailingly obeyed the experimenter, proceeding to the highest shock level on the generator. He displayed a curious dissociation between word and action. Although at the verbal level he had resolved not to go on, his actions were fully in accord with the experimenter's commands. This subject did not want to shock the victim, and he found it an extremely disagreeable task, but he was unable to invent a response that would free him from *E's* authority. Many subjects cannot find the specific verbal formula that would enable them to reject the role assigned to them by the experimenter. Perhaps our culture does not provide adequate models for disobedience.

One puzzling sign of tension was the regular occurrence of nervous laughing fits. In the first four conditions 71 of the 160 subjects showed definite signs of nervous laughter and smiling. The laughter seemed entirely out of place, even bizarre. Full-blown, uncontrollable seizures were observed for 15 of these subjects. On one occasion we observed a seizure so violently convulsive that it was necessary to call a halt to the experiment. In the post-experimental interviews subjects took pains to point out that they were not sadistic types and that the laughter did not mean they enjoyed shocking the victim.

In the interview following the experiment subjects were asked to indicate on a 14-point scale just how nervous or tense they felt at the point of maximum tension. The scale ranged from "Not at all tense and nervous" to "Extremely tense and nervous." Self-reports of this sort are of limited precision, and at best provide only a rough indication of the subject's emo-

tional response. Still, taking the reports for what they are worth, it can be seen that the distribution of responses spans the entire range of the scale, with the majority of subjects concentrated at the center and upper extreme. A further breakdown showed that obedient subjects reported themselves as having been slightly more tense and nervous than the defiant subjects at the point of maximum tension.

\* \* \*

*Background authority.* . . . The effectiveness of the experimenter's commands may depend in an important way on the larger institutional context in which they are issued. The experiments described thus far were conducted at Yale University, an organization which most subjects regarded with respect and sometimes awe. In post-experimental interviews several participants remarked that the locale and sponsorship of the study gave them confidence in the integrity, competence, and benign purposes of the personnel; many indicated that they would not have shocked the learner if the experiments had been done elsewhere.

This issue of background authority seemed to us important for an interpretation of the results that had been obtained thus far; moreover it is highly relevant to any comprehensive theory of human obedience. Consider, for example, how closely our compliance with the imperatives of others is tied to particular institutions and locales in our day-to-day activities. On request, we expose our throats to a man with a razor blade in the barber shop, but would not do so in a shoe store; in the latter setting we willingly follow the clerk's request to stand in our stocking feet, but resist the command in a bank. In the laboratory of a great university, subjects may comply with a set of commands that would be resisted if given elsewhere. *One must always question the relationship of obedience to a person's sense of the context in which he is operating.*

To explore the problem we moved our apparatus to an office building in industrial Bridgeport and replicated experimental conditions, without any visible tie to the university.

Bridgeport subjects were invited to the experiment through a mail circular similar to the one used in the Yale study, with appropriate changes in letterhead, etc. As in the earlier study, subjects were paid \$4.50 for coming to the laboratory. The same age and occupational distributions used at Yale, and the identical personnel, were employed.



The purpose in relocating in Bridgeport was to assure a complete dissociation from Yale, and in this regard we were fully successful. On the surface, the study appeared to be conducted by RESEARCH ASSOCIATES OF BRIDGEPORT, an organization of unknown character (the title had been concocted exclusively for use in this study).

The experiments were conducted in a three-room office suite in a somewhat run-down commercial building located in the downtown shopping area. The laboratory was sparsely furnished, though clean, and marginally respectable in appearance. When subjects inquired about professional affiliations, they were informed only that we were a private firm conducting research for industry.

Some subjects displayed skepticism concerning the motives of the Bridgeport experimenter. One gentleman gave us a written account of the thoughts he experienced at the control board:

... Should I quit this damn test? Maybe he passed out? What dopes we were not to check up on this deal. How do we know that these guys are legit? No furniture, bare walls, no telephone. We could of called the police up or the Better Business Bureau. I learned a lesson tonight. How do I know that Mr. Williams [the experimenter] is telling the truth. ... I wish I knew how many volts a person could take before lapsing into unconsciousness. ...

Another subject stated:

I questioned on my arrival my own judgment [about coming]. I had doubts as to the legitimacy of the operation and the consequences of participation. I felt it was a heartless way to conduct memory or learning processes on human beings and certainly dangerous without the presence of a medical doctor.

There was no noticeable reduction in tension for the Bridgeport subjects. And the subjects' estimation of the amount of pain felt by the victim was slightly, though not significantly, higher than in the Yale study.

A failure to obtain complete obedience in Bridgeport would indicate that the extreme compliance found in New Haven subjects was tied closely to the background authority of Yale University; if a large proportion of the subjects remained fully obedient, very different conclusions would be called for.

As it turned out, the level of obedience in Bridgeport, although somewhat reduced, was not significantly lower than that obtained at Yale. A large proportion of the Bridgeport subjects were fully obedient to the experimenter's commands (48 per cent of the Bridgeport subjects delivered

the maximum shock vs. 65 per cent in the corresponding conditions at Yale).

\* \* \*

*Levels of Obedience and Defiance.* One general finding that merits attention is the high level of obedience manifested in the experimental situation. Subjects often expressed deep disapproval of shocking a man in the face of his objections, and others denounced it as senseless and stupid. Yet many subjects complied even while they protested. The proportion of obedient subjects greatly exceeded the expectations of the experimenter and his colleagues. At the outset, we had conjectured that subjects would not, in general, go above the level of "Strong Shock." In practice, many subjects were willing to administer the most extreme shocks available when commanded by the experimenter. For some subjects the experiment provides an occasion for aggressive release. And for others it demonstrates the extent to which obedient dispositions are deeply ingrained, and are engaged irrespective of their consequences for others. Yet this is not the whole story. Somehow, the subject becomes implicated in a situation from which he cannot disengage himself.

The departure of the experimental results from intelligent expectation, to some extent, has been formalized. The procedure was to describe the experimental situations in concrete detail to a group of competent persons, and to ask them to predict the performance of 100 hypothetical subjects. For purposes of indicating the distribution of break-off points judges were provided with a diagram of the shock generator, and recorded their predictions before being informed of the actual results. Judges typically underestimated the amount of obedience demonstrated by subjects.

... The psychiatrists predicted that most subjects would not go beyond the tenth shock level (150 volts; at this point the victim makes his first explicit demand to be freed). They further predicted that by the twentieth shock level (300 volts; the victim refuses to answer) 3.73 per cent of the subjects would still be obedient; and that only a little over one-tenth of one per cent of the subjects would administer the highest shock on the board. But, as the graph indicates, the obtained behavior was very different. Sixty-two per cent of the subjects obeyed the experimenter's commands fully. Between expectation and occurrence there is a whopping discrepancy.



\* \* \*

Many people, not knowing much about the experiment, claim that subjects who go to the end of the board are sadistic. Nothing could be more foolish as an overall characterization of these persons. It is like saying that a person thrown into a swift-flowing stream is necessarily a fast swimmer, or that he has great stamina because he moves so rapidly relative to the bank. The context of action must always be considered. The individual, upon entering the laboratory, becomes integrated into a situation that carries its own momentum. The subject's problem then is how to become disengaged from a situation which is moving in an altogether ugly direction.

The fact that disengagement is so difficult testifies to the potency of the forces that keep the subject at the control board. Are these forces to be conceptualized as individual motives and expressed in the language of personality dynamics, or are they to be seen as the effects of social structure and pressures arising from the situational field?

A full understanding of the subject's action will, I feel, require that both perspectives be adopted. The person brings to the laboratory enduring dispositions toward authority and aggression, and at the same time he becomes enmeshed in a social structure that is no less an objective fact of the case. From the standpoint of personality theory one may ask: What mechanisms of personality enable a person to transfer responsibility to authority? What are the motives underlying obedient and disobedient performance? Does orientation to authority lead to a short-circuiting of the shame-guilt system? What cognitive and emotional defenses are brought into play in the case of obedient and defiant subjects?

\* \* \*

*Postscript.* Almost a thousand adults were individually studied in the obedience research, and there were many specific conclusions regarding the variables that control obedience and disobedience to authority. . . .

\* \* \*

What is the limit of such obedience? At many points we attempted to establish a boundary. Cries from the victim were inserted; not good enough. The victim claimed heart trouble; subjects still shocked him on command. The victim pleaded that he be let free, and his answers no longer registered on the signal box; subjects

continued to shock him. At the outset we had not conceived that such drastic procedures would be needed to generate disobedience, and each step was added only as the ineffectiveness of the earlier techniques became clear. The final effort to establish a limit was the Touch-Proximity condition. But the very first subject in this condition subdued the victim on command, and proceeded to the highest shock level. A quarter of the subjects in this condition performed similarly.

The results, as seen and felt in the laboratory, are to this author disturbing. They raise the possibility that human nature, or—more specifically—the kind of character produced in American democratic society, cannot be counted on to insulate its citizens from brutality and inhumane treatment at the direction of malevolent authority. A substantial proportion of people do what they are told to do, irrespective of the content of the act and without limitations of conscience, so long as they perceive that the command comes from a legitimate authority. If in this study an anonymous experimenter could successfully command adults to subdue a fifty-year-old man, and force on him painful electric shocks against his protests, one can only wonder what government, with its vastly greater authority and prestige, can command of its subjects. There is, of course, the extremely important question of whether malevolent political institutions could or would arise in American society. The present research contributes nothing to this issue.

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**Bernard Bressler, Albert J. Silverman,  
Sanford I. Cohen, and Barry Shmavonian  
Research in Human Subjects and the  
Artificial Traumatic Neurosis—  
Where Does Our Responsibility Lie?\***

The authors are engaged in a research project at Duke University dealing with isolation and sensory deprivation which uses human subjects selected at random. These subjects evidenced no gross psychopathology. The experiment is, on the surface, a comparatively simple one. Without previous instruction the subject is placed in a small, soundproof, completely darkened chamber for a period of two hours, at the end of which he is interviewed in the chamber to obtain his immediate impressions and reactions.

\* 116 *American Journal of Psychiatry* 522-526 (1959). Copyright 1959, the American Psychiatric Association. Reprinted by permission.

Following this, he is seen outside the chamber by another interviewer, who questions him further on the cues picked up in the initial interview (which is taped). Then the subject fills out a comparatively simple written questionnaire regarding his impression of the experiment. On the day following the experiment he meets with a third interviewer for evaluation of his memory of his reaction to the experiment and also, generally, to determine how he has handled the total experience.

Reviewing our experience with one subject, a seemingly well adjusted, bright young woman obtaining her Ph.D. in psychology, we were confronted with an important problem which deserves considerable discussion and sober thought. A brief description of her progress during and following the experiment will indicate the exact nature of our concern.

It was quite apparent from the initial interview inside the chamber that the subject was experiencing a great deal of anxiety, and as she described it,

"My heart is beating quite fast." She described the feeling of "trying to get some kind of anchor; some kind of bearing," since all of her visual cues were gone. She reported periods of uneasiness, reassuring herself that she didn't have claustrophobia, and a headache which lasted for a short time. It was readily apparent (as is typical for almost all subjects) that she had no idea of the time elapsed despite her intellectual realization that she hadn't been in the chamber very long. When asked for a specific time estimate of her isolation she replied "Oh, hours." Her initial appearance was that of a perplexed, frightened, uncomfortable and anxious person. Her thinking process was quite slowed, and there was a great change in her verbalization and general speech patterns. As the interviewer entered the chamber her first impression was that he looked much larger than his actual size, and it was at this moment that she began to feel anxiety. Also she produced several bizarre fantasies: one about a submarine and another, derived from a science fiction story, about people locked in a room where they were slowly frozen to death. She required roughly an hour and a half to recover her normal intellectual functioning since, as she stated, "I was bewildered."

Following the isolation experience the subject described her feelings as "being shaken up inside." She said that she had a tremendous impulse to find someone she could trust and tell him "all about it." On her arrival home she was very hungry, something quite unusual for her, since she rarely eats at night. She played a few records and fixed something to eat although "it wasn't real hunger, just something to do." Finally, after great difficulty she went to sleep, awakening in the morning to find that the sheets had all been kicked away from the bed.

In the interview the next day she further elaborated some of her isolation fantasies. For example, she had the thought that "Maybe there was only two or three hours' worth of oxygen in the room." She identified herself with sailors in a submarine, a submarine which was obviously in danger. She recalled skipping from one dangerous situation to another and stated that she had concentrated on such situations. One of the dangers was "that the experimenter walked into the room, and an enemy closed the door behind him. Of course, there were no locks. He couldn't get out, and the fellow froze to death." At times she had some near paranoid ideas, *i.e.*, thinking that the room was deliberately overheated, that her headache was due to deliberate oxygen deprivation, *etc.* At one point she became very threatened and thought "Well, I'll play a trick on you. If I go to sleep, you'll get me out." She said she had tried sleeping, and actually it was at this moment that the experiment was interrupted, and an interviewer entered.

At first, "You know I couldn't answer properly. I had a feeling I had nothing to say. I felt slowed down. Then there was that tremendous thumping of my heart." She said that at one point she reviewed her past and present life but had no thoughts about the future. Although she had a lot of love fantasies, she was unable to describe them to the interviewer who had teased her in the past. She thought about the opera "Aida," in which Radames was put into a vault to die. Her reaction to the total isolation experiment was: "There was something fascinating about it. It's not entertaining like going to a movie, but it's something fascinating, it's like having gone through the war. I'll never regret having gone through the war and having had my experiences despite the fact that they were pretty uncomfortable. There is something about it that just fascinates me."

Some time elapsed during which we had no further contact with the subject. However, it was subsequently decided to retest this subject (and others) in order to further evaluate the material under different circumstances. The experiment was repeated exactly as before, except that this time she was told in detail exactly what would happen.

One of the most remarkable effects of this second isolation experience on the subject was:

"I said to myself now I can really get my fantasies going. In other words I didn't start thinking of the present as I did before; this time I tended to wander more towards the past and towards the future, towards trying to solve problems. You know it seemed a lot longer this time, it was much harder for me to stay awake, and I was much less alert."

Later she indicated that she was sure she fell asleep, despite her intention to stay awake and watch her fantasies, and although she had taken a nap earlier to avoid being sleepy during the experiment. She also noticed a difference in her thought proc-

esses. "Well, they were sort of level, then going up and down, up and down, like a slow wave. This is unlike before when there were lots of things which kept me alert." Further, she essentially described a tremendous amount of passivity and massive denial, which she grudgingly admitted were related to "having to avoid something." The subject indicated that she was quite surprised at her total reaction.

Her previous experiences in the chamber had been so vivid that she assumed that by "letting herself go" they would be even more vivid, yet actually they weren't. Another important factor of the isolation experience was that many of her thoughts were directly concerned with or immediately related to her childhood, particularly of a period of summer vacation. At one time she referred to the underwater submarine scene which she described earlier; note this was associated with a story she read at age 9 or 10 in which a heroic diver died after a very exciting episode.

Throughout the second experiment the subject experienced periods of boredom which puzzled her since they seemed to be completely out of control. Usually associated with the boredom were feelings of wanting to get out and a return to some of her projective thinking, for example, "I thought that the boys were holding me in here longer than they did before," (despite the fact that she had been carefully reassured that she would remain in the chamber only two hours).

She had many feelings of being in a dangerous situation, despite the reassurance that she had been through this before and that "nothing had happened." As this material was elaborated her ninth and tenth years of childhood became most important. During this time she had spent a vacation in the country, following which she had left her home in Europe to come to the United States. Associated with this move was much unhappiness since "I was perfectly happy. I mean I couldn't see why . . . the fact that . . . my friends were doing this. They said the United States was a place where we could live much freer than we were, yet I felt perfectly free."

It is to be noted that this woman was in a period of transition; she was leaving her present position, a very protective training program, to go on her own. This leaving was directly reminiscent by association of leaving her homeland in childhood.

One other interesting incident occurred during the second interview. At one time the subject suddenly felt as if the room were tilted and noticed that her head was tilted, too. Try as she might she could not right herself. Not until she forcefully "straightened" her head with her hands did the room resume its normal position.

In the interview on the day following the experiment the subject greatly elaborated the material reported in the previous interview. She described her experience as "a kind of numbness. In a sense all my feelings were sort of on even keel." This is a sec-

ondary distortion. Despite her massive denial, she did experience moments of anxiety. She said in reply to a question that it was "as if I weren't responding to anything." This statement again exposes her attempted repression and—when this was unsuccessful—massive denial.

She reported an interesting dream in which "people were bunched together, people from my high school days and people that I knew (in her home country) as a little girl, and the people I knew here. . . . I was sort of surprised. I thought to myself what on earth are all these people doing here, I haven't seen them for years."

The dream and her subsequent elaborations were associated to her memory of the vacation in the country before she came to the United States. Her feelings about the separation trauma were very much like her feelings about being separated from Duke. She said further about the experience in the chamber "I said I'd come back again. I didn't enjoy the experience but it was so weird; it was fascinating." (We never explained her excessive use of the word fascination.)

Following this interview it was necessary to have another session because of a very peculiar symptom she developed.

She seemed to have lost sense of direction and was confused whether to take a left or right turn as she was leaving a room. This symptom was clearly related to her tremendous indecision over "where to go," i.e., the separation in the present was, in turn, related to the feelings of separation from her country as a child which had been stirred up by the isolation experiment.

Another subject reported immediately following the experiment that he had directed his thinking during the period of isolation. He said that thinking of specific events made him more comfortable and that he found these thoughts reassuring since the darkness and silence were disquieting.

The subject, a medical student, mentioned that one of the things he had reviewed was an interview with a young patient the day before, and he implied that thinking of this kept his mind from other thoughts. The initial interviewer believed the subject to be more upset by the experience than he described, but there was no direct evidence thereof. Associations during the interview were only followed in terms of the immediate experimental situation for fear that allowing the subject to associate to past events might either (a) enhance any disorganization which the subject already felt or (b) allow the subject to escape into the past and "forget" the present provocative events. Furthermore it had been decided that extensive exploration should not be attempted during the first interview if the subject appeared to be confused, disoriented or extremely uncomfortable. When the subject was interviewed on the following day, he

was considerably more comfortable although he still expressed much embarrassment and discomfort in regard to the previous day's experience. To reassure him the interviewer spoke of the many unusual thoughts subjects had during this experiment and pointed out that sometimes discussing these feelings and ideas allowed the subject to look at them realistically and feel more comfortable about them. At this point the subject remarked that not only had he been thinking about his young male patient, but that there were some sexual connotations. Furthermore, the previous interview conducted in the chamber just after the completion of the experiment was associated with sexual feelings he experienced in regard to the interviewer. He described feeling panic at this time and feeling panic at the previous thoughts about the patient but he had felt unable to report it. Certainly the experience acted as a traumatic event for this subject by bringing to the conscious level latent homosexual feelings and thoughts.

There is some evidence that these feelings have remained conscious. For several days thereafter and indeed, until the present time, this subject is unable to look directly at the interviewer and apparently is embarrassed when they chance to meet.

A female subject requested release from the chamber, and, when this was not done rapidly enough, she located the emergency door and walked out of the chamber a half hour before the official termination of the experiment.

In the interview which followed, the subject stated that she was beginning to feel restless, and described a certain amount of anxiety and fear although she did not consider the experience overwhelming. All of the interviewers noted that the subject's usual vivaciousness was replaced by an affective state which varied from flatness to irritability and petulance. She seemed extremely suspicious, exhibited poor judgment in the things she had expressed and the manner in which she expressed them. When she was first seen, she appeared quite confused and had the appearance of sleepwalking. This state lasted for several days, and a full month passed before she was her usual vivacious self.

During the interview on the following day the subject related that she had always been afraid of darkness and that this was particularly true for the past year since the death of her father; she insists that each night one or two lights be left on in the house, because she and her mother "are alone and some prowler might enter the house." She then described how she had attempted to resist this thought by thinking of pleasant things while she was in isolation. As the thought that someone might be "prowling on the outside" and that he might break into the chamber came closer to awareness, the subject's anxiety and restlessness increased until she had to leave the chamber despite strong inhibitions based

on her desire to help and to please the experimenters.

### Discussion

It seems reasonably clear that the processing of these subjects in an experiment which was, on the surface, comparatively innocuous, acted as a traumatic event. This result is even more significant in view of the fact that the subjects were carefully chosen as individuals with "strong egos." Although intelligence and education are not necessarily the attributes of a strong ego, these factors plus our knowledge of the subjects had made us reasonably secure in our evaluations of them.

By definition the amount of psychic energy which one's ego is unable to master within a reasonable span of time is designated as a trauma; ordinarily there are protective barriers (*i.e.*, the defenses) against the outcropping of such stimuli (*i.e.*, id impulses). It is also known that there are numerous instances in which the ability of the defenses erected by the ego to maintain usual or normal functioning is diminished. We believe this to be particularly true in the sensory deprivation experiment we have conducted in which we have introduced a *temporary abrogation of usual ego functioning*. In other words we deliberately (or artificially) deprive the ego of many of the usual activities and resources which help to keep its defenses intact. In our experiment the paralysis of voluntary motion and withdrawal of many of the usual sensory stimuli which guide the ego produce a strange, potentially dangerous situation for the ego. Under these circumstances, if the protective defense barriers break down, the ego loses its ability to react with purposeful reactions, and anxiety, in one form or another, will appear. Although the ego opposes the emergence of an id impulse that is equated with some dangerous situation by producing anxiety, it is equally possible, as indicated above, that the ego, unaware, is helpless or temporarily overwhelmed in the face of some conflict and this, in turn, produces anxiety. Thus we believe that the artificial abrogation of ego functioning markedly weakens the ability of the ego to utilize counteracthexis so that repressed and conflictual material tends to emerge into consciousness. This is particularly true for those conflicts which might otherwise be handled slowly or more realistically but which can no longer be put off by a paralyzed and weakened ego.

Subsequently it is our feeling that the experiment produced a temporary artificial traumatic neurosis. Although the problems indicated were

naturally latent, we believe they would not have emerged with the same intensity in ordinary circumstances.

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### 3.

#### Interferences with Physical Integrity

**Renée C. Fox**  
**Experiment Perilous\***

\* \* \*

All physicians are confronted with problems of uncertainty. Some of these result from their own incomplete or imperfect mastery of available medical knowledge and skills; others derive from limitations in current medical knowledge; and still others grow out of difficulties in distinguishing between personal ignorance or ineptitude and the limitations of medical science.

In a sense, the physicians of the Metabolic Group can be thought of as specialists in problems of uncertainty—particularly those uncertainties related to limits of present medical knowledge. As clinical investigators, it was their special role to work on the periphery of what is medically known: to concern themselves with ill-understood basic mechanisms underlying the normal and abnormal functioning of the human body, and with unresolved problems in the diagnosis, treatment, and prognosis of human disease. Chiefly by means of experimentation, their task was to devise, explore, and appraise new ideas, methods, procedures, and drugs that might possibly contribute to medical knowledge, skill, and clinical prowess.

Because they worked "close to the growing edge of things" in the capacity of researchers, the physicians of the Metabolic Group were confronted with uncertainties of the medically unknown in a variety of forms. These included uncertainties regarding fundamental biochemical and physiological mechanisms underlying the phenomena and conditions they studied; uncertainties connected with the experimental compounds and procedures with which they worked—their basic properties and potential clinical effects; methodological uncertainties, related to the laboratory techniques they were developing;

and finally, clinical uncertainties that were non-experimental in nature, which had to do with the diagnosis, treatment, and prognosis of their patients' illnesses.

As research pioneers or trail-blazers, who often worked outside the terrain of well-established medical knowledge, the physicians of the Metabolic Group were perhaps more continuously and immediately exposed to problems of uncertainty that result from limits of the field than were many of their nonresearch colleagues. The advances in knowledge and skill which their work effected helped to clarify and occasionally even dispel some of these uncertainties. But at the same time, as in all research, these gains in knowledge frequently uncovered new problems of uncertainty to be explored.

Things multiply. You solve one problem, and you're faced with two others. Things you didn't know once become obvious. But then other things you didn't even know existed arise. . . .

As we shall see, "chance" factors, which the physicians of the Metabolic Group did not necessarily plan or anticipate, played a considerable role in bringing various uncertainties to the attention of these physicians, and in determining the direction of their experimental work. The greater part of their research, of course, consisted of rationally-organized experimental attempts to learn more about various problems of uncertainty, or, hopefully, to resolve them. For the purpose of experimentation, one might say that the physicians of the Metabolic Group intentionally sought out problems of uncertainty, and, to some extent, deliberately induced them. In this sense, they were actively responsible for creating some of the uncertainties with which they worked. As we shall see, this affected the way that they felt about the consequences of these uncertainties for their research subjects, who were also their patients.

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[T]he major share of the Metabolic Group's investigative and clinical activities centered around problems related to the normal and abnormal functioning of the adrenal glands. A considerable number of their studies involved estimating the metabolic activity of newly synthesized adrenal steroids and related compounds that had been tested on animals and had shown promising evidence of being useful as therapeutic agents. . . . The Group "hoped that through long-term studies extensive correlations might be

\* Glencoe, Ill.: The Free Press 28-33, 43-60 (1959). © The Free Press, Inc., 1959. Reprinted by permission of The Macmillan Company.

made between modifications of steroid structures and modifications of their biological activity in man." However, in spite of the intensive investigative work they devoted to this problem and some empirical success in the treatment of various clinical syndromes with these adrenocortical hormones, the physicians of the Metabolic Group were "still largely in the dark as to their exact nature and basic mechanisms of action."

The Metabolic Group received the steroid preparations which they assayed directly from the laboratories of the pharmaceutical firms that manufactured them. Since methods for synthesizing steroid compounds are still "far from fool-proof," these preparations sometimes contained impurities that could adversely affect patients who received them. However, the Group had no sure way of detecting these impurities before administering the steroids to patients; nor could they always explain the reactions that such impure compounds evoked. "We have had five reactions of the type you had to Lot No. 2179," a member of the Group wrote to a patient,

but we must admit that we are not certain why this particular lot has caused these reactions. It is essentially no different from previous lots in any way we have so far been able to determine, except for the obvious important fact that no reactions have occurred with other batches. . . .

The laboratory methods which the group had developed for investigating the nature and quantities of steroids secreted by the adrenal cortex were still characterized by a considerable degree of error, and were not yet refined enough to separate steroids originating from the adrenal cortex from those originating from other tissues. . . .

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The fact that the medical sciences have not yet developed a "broad, over-all theory of drug action" also contributed to the many elements of uncertainty which characterized the Group's investigative work with steroids.

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Partly as a consequence, many of the experiments conceived by the Group were highly empirical in nature: "trial-and-error shots in the dark" of which the outcome was very uncertain and unpredictable. . . .

\* \* \*

The fact that the Group conducted most of its research on patients—individuals who were af-

flicted with some form of illness—rather than on healthy subjects, added to the elements of uncertainty which so often complicated the course of their experiments, or "clouded" their interpretation. . . .

\* \* \*

Gradually, out of experimental "casting in the dark," the Metabolic Group (and other research units like it) have been able to advance understanding of the adrenal gland and its secretions, develop programs for the treatment of patients suffering from disorder of adrenal function, and modify certain generalized disease processes of nonadrenal origin by manipulation of the type and quantity of adrenal steroid secretion, or by surgical removal of adrenal tissue. But "the problem is never finally solved; the last word is never said." For, as we have indicated, the advance in knowledge and clinical efficacy which the Group's research brought about led to the detection and, in a sense, to the creation of problems of uncertainty not previously encountered, recognized, or explored:

Before cortisone, the treatment of diabetes and Addison's disease combined was frightful—just hopeless. Jim Hayes, for example, was in the hospital fifty-five times—most of those admissions because he was hypoglycemic. But cortisone has really changed this whole disease picture. In the last two or three years, he hasn't had to come in at all. . . . Now the trouble is that when Addisonians *do* arrive, they come in with "crisis *sine* crisis." According to the textbook definitions, they're not actually in crisis. It's a different sort of picture. How would you classify Ed Murray's symptoms, for example? And how do you treat them? Well, in the past three or four years, we've had only one case come in with the classic symptoms. All the rest arrive they way he did. . . . What's more, Addisonians live longer now. So they are developing neoplastic and degenerative diseases. Last year, for instance, we had three hypertensive Addisonians; and we can expect to have more diabetic Addisonians as time goes on. . . .

[I]n collaboration with the surgical service, the Metabolic Group was also engaged in studying the effects of several types of experimental surgery on patients: the effects of bilateral complete adrenalectomy on patients with severe advanced hypertensive vascular disease, reactivated cancer of the prostate, and hyperadrenalism; the effects of transplanting a healthy kidney into the bodies of patients with terminal renal disease; and to a lesser extent, the effects of cardiac surgery on patients with serious heart disease.

Were such procedures "justified as an experi-

mental approach in man"? Could they be "carried out with reasonable safety" in patients? How should patients be prepared for such surgery? What kind of anesthesia ought to be chosen? What kind of surgical approach ought to be used? How should patients be maintained postoperatively? What kinds of operative complications would be forthcoming? What therapeutic benefits, if any, could be derived from these procedures?

In the words of the Surgeon-in-Chief of the hospital, this kind of research "could not have been undertaken by the faint of heart." For the only way that such questions could be answered and problems resolved was by actually performing these procedures on a certain number of patients and studying their results. Medical ethics required the Group to try this radical experimental surgery on patients who were beyond help by conventional means, before attempting to carry it out on patients less gravely ill. The magnitude of the uncertainties with which the Group was faced in undertaking these procedures on patients this seriously ill is very great.

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The fact that these physicians were frequently limited in the extent to which they could improve their patients' clinical status had implications for another constellation of problems with which the Metabolic Group was faced. These were the problems of meeting both their responsibilities as clinicians and their commitments to medical research to the fullest extent possible; ascertaining the limits of each set of obligations; and preventing or reconciling some of the conflicts that existed between them.

In many cases, the clinical and research activities of the Metabolic Group implemented one another. Some of the tests and procedures the Group asked patients to undergo, or the drugs they gave them primarily in order to diagnose or treat their diseases, also provided the Group with basic or general information about such conditions which was relevant to research in which they were engaged. In such instances (providing that things went well), the members of the Group simultaneously fulfilled their obligations as clinicians to further their patients' welfare and their responsibilities as investigators to advance medical knowledge.

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However, there were also many occasions on which the procedures or drugs which the Group administered to patients did not benefit

the persons subjected to them, or proved to have negative consequences for them. Situations of this sort were attributable to a number of factors. To begin with, as we know, the patients under the care of the Metabolic Group were likely to have serious, relatively ill-understood, or complicated diseases. Partly as a consequence, subjecting them to some of the procedures and agents that were used to diagnose or treat their disorders entailed a considerable amount of risk, some of which the Metabolic Group could calculate in advance, and some of which they could not. . . .

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The Metabolic Group was also engaged in a considerable amount of research which they undertook primarily to advance general medical knowledge, and only secondarily or incidentally because they thought it might be helpful to patients who consented to act as their subjects. The members of the Group "hoped" that the patients who participated in these experiments might gain some clinical benefit from doing so, and they were pleased when this happened. But to the limited extent that medical ethics allowed them to do so, they subordinated their clinical desire to serve the immediate interests of the particular patients involved in such experiments, and gave priority to the more long-range, impersonal research task of acquiring information that might be of general value to medical science.

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The physicians of the Metabolic Group were deeply committed to . . . [the principles of the Nuremberg Code] and conscientiously tried to live up to them in the research they carried out on patients. However, like most norms, the "basic principles of human experimentation" are formulated on such an abstract level that they only provide general guides to actual behavior. Partly as a consequence, the physicians of the Metabolic Group often found it difficult to judge whether or not a particular experiment in which they were engaged "kept within the bounds" delineated by these principles.

This was especially true of the experiments they conducted primarily to advance medical knowledge. The justification for this kind of research did not lie in its potential immediate value for the patients who acted as subjects. Rather, it was premised on the more remote, general, uncertain probability that its "anticipated results . . . their humanitarian importance . . . for the good



of society" and the chance of achieving them—would exceed the immediate amount of "suffering" and "risk" the experiment might entail. The criteria on which physicians ought to form such a calculus are not specified by the rules of conduct for clinical research. Thus, without many established or "clean-cut" bases of judgment to guide them, the physicians of the Metabolic Group were constantly faced with the problem of trying to decide whether the particular experiments they were conducting fell within the limits of their rights as investigators, or whether they were overstepping those rights by subjecting the patients involved to more inconvenience and danger than the possible significance of those experiments for the "advancement of health, science, and human welfare" seemed to warrant.

In addition, as we know, the many uncertainties connected with experimentation and the clinical status of the patients who served as subjects made it hard to predict what the actual results of carrying out a procedure or administering a drug would prove to be. For this reason, the physicians of the Metabolic Group sometimes found themselves in situations where their experiments resulted in more inconvenience or suffering for their patient-subjects than they anticipated, intended, or desired.

Accounts of . . . experiments conducted by the Metabolic Group follow. The primary purpose of these experiments was to obtain scientific information. In this respect, and a number of other ways, they are characteristic of many studies conducted by the Group. Therefore, they suggest or illustrate some of the concrete problems the Metabolic Group faced in trying to determine the boundaries of their responsibilities as physicians to protect and further the welfare of their patients, and their rights as investigators to subject them to a certain amount of discomfort and risk, so as to strike a "proper balance" between these two potentially incompatible aspects of the role of clinical investigator.

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Another characteristic these experiments had in common is that they all involved the use of a number of procedures which imposed varying degrees of risk upon the patients who underwent them. Most of these procedures were accepted clinical methods for establishing a diagnosis. In this context, risks that accompany these procedures are generally established as "tolerable"—that is, within the confines of ethical medical practice. However, in the research cases

we are considering, these methods were not being used to diagnose patients' conditions in order to help them more effectively. Rather, they were being employed for general investigative purposes. . . . These were inquiries from which the patients acting as subjects could not expect to receive immediate clinical benefit, if any at all. Before they undertook such experiments the Group had to decide whether procedures which had "acquired respectability . . . in a diagnostic setting" could also be justifiably used "in an investigative setting" where the interests of patients were not being directly or immediately served. In these cases, the physicians of the Metabolic Group felt that they were. It was their considered judgment that given the cogency of the research problems they wished to explore, and the fact that the patients who acted as subjects for these experiments were ill with diseases which they could only help symptomatically, the discomforts and risks involved were not excessive.

The problems that the Group sometimes faced in deciding whether it was clinically wise and morally just to subject patients to this kind of investigation are well demonstrated by another experiment they conducted. This is one which had undesirable consequences for the patient, despite the fact that before they initiated the experiment the Group had carefully discussed the risks it might entail, and decided that they were not too great.

Mr. Max Gold, the patient involved, was afflicted with a rarely seen combination of disorders: Addison's disease and mitral stenosis. For the physicians of the Metabolic Group his condition was a "significant experiment of nature." It offered them an extraordinary opportunity to study the metabolic processes underlying a concurrence of disorders which they themselves had neither the moral right nor technical ability to induce. Mr. Gold was also the first patient with both these conditions to have undergone the procedure for surgical correction of mitral stenosis known as mitral valvuloplasty. This enhanced his potential importance as a research subject and the Metabolic Group's interest in studying the patterns of his metabolic processes.

It is a well-established fact that patients with Addison's disease generally have a defect in their ability to excrete administered water. For this reason, one of the phenomena the Group wished to observe was whether this would be equally characteristic of Mr. Gold, or whether because of his heart condition and the surgery



he had recently undergone to correct it, his pattern of excretion would be different. For this purpose, the Group considered using what is known as the Robinson-Kepler-Power "water test." This is a simple screening examination; one of the clinical procedures which helps physicians to rule in the possibility that a patient has Addison's disease, or to rule it out. The test lasts for eighteen-and-a-half hours, during which time the patient is not allowed to eat any food or drink any fluids. He is then given a large amount of water to drink over a short period of time. Throughout the test all his urine is collected and its volume and specific gravity are measured. Whereas in normal subjects a prompt diuresis follows ingestion of the water, in patients with Addison's disease the diuresis is greatly delayed. The test is regarded as safe for patients with possible Addison's disease, as well as those who prove not to have it.

Before they finally decided to subject Mr. Gold to the water test, the Group tried to estimate the degree of stress the procedure might impose on him, and discussed the question of whether it was greater than what he ought to be asked to undergo for investigative rather than diagnostic purposes. The major reason for which various members of the Group felt some hesitancy about conducting the test was that it would entail withholding, while the test was in progress, the desoxycorticosterone acetate (DOCA) and cortisone therapy the patient was receiving daily. The possibility was raised that in his postoperative state the patient might "react more radically" than is usually the case to the withdrawal of medication, fasting, and the administration of a large volume of water under these circumstances. However, the members of the Group were somewhat more inclined to believe that because of the therapy he had received Mr. Gold was "well enough stocked" with DOCA and cortisone to sustain the stress of having them withheld for eighteen hours and undergoing the procedure. On the strength of this conviction and because they felt that Mr. Gold "might be the last case of Addison's disease and mitral stenosis that [they] would ever see," the Group decided to proceed with the experiment.

As already indicated, the optimism of the Group regarding the ability of Mr. Gold to withstand the procedure was not upheld. Due to factors they did not anticipate, could not control, and even in retrospect did not understand very well, the patient reacted to the test in a seriously adverse way. Thus, despite the caution with

which they proceeded, the effect of the experiment on the patient's clinical status conflicted sharply with what the Group had predicted, and with their desire as his personal physicians to protect him from harm. At the Group's Evening Rounds at the end of the day on which Mr. Gold underwent this test, his untoward reaction to it was described by one physician in the following way:

Well, I guess just about everybody knows that Mickey had a water test today. And out of the blue, he got very hot and dizzy. The room started to spin around, he said. His eyes began to twitch and his vision got blurred. . . . His pressure was still 100 over 80 when I took it; but his pulse was feeble and thready. Those symptoms lasted for about a half hour. Right now he's getting saline, desoxy, and Compound F. That should be enough. . . . And, of course, it's no longer possible to consider keeping him on a constant diet. We've got no choice but to treat this man. So our experiment ends right here. We might just as well put him on an all-therapeutic program.

The experiments described contain at least two other practices, at once characteristic of the Group's research and essential to it, that produced many of the situations in which they experienced conflict between their obligations to advance knowledge and their responsibility to promote the welfare of their patient-subjects. These were the practices of administering new drugs to patients and of withholding drugs from them, primarily for investigative purposes.

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In their experiment with Mr. Ray Woodham, on the other hand, what the Group did to implement research interfered with the welfare of a patient in another characteristic way. For the purposes of metabolic study, the Group temporarily deprived the patient of cortisone: a drug which they had good reason to believe would help him excrete the many pounds of excessive water with which his body was swollen as a result of kidney disease. In fact, some of the Group's experiments were conducted primarily for the purpose of determining how long a patient could tolerate not receiving a needed drug, and exactly what the nature of his reaction would be to having it withheld. For example, the Group carried out the following experiment on Mr. Michael Terhune, a patient who had undergone total bilateral adrenalectomy for carcinoma of the prostate, and thus was not capable of producing his own cortisone:

1/7: Mike is at present undergoing DOCA withdrawal for the purpose of demonstrating sodium chloride diuresis. No attempt will be made to prolong this withdrawal; adequate demonstration of the electrolyte defect is all that is desired. Following this, he will resume DOCA therapy, and will then be gradually tapered off cortisone for the purpose of following 17-ketosteroids, FS, and androgenic excretions. In view of the fact that we were never able to withdraw Mr. Carr's cortisone longer than 2½ days without impending crisis, it will be important to determine the length of time Mike can tolerate this procedure. . . .

1/26: On 1/14 we began the slow tapering of Mr. Terhune's cortisone dose from 50 mgms. daily p.o. to 0. This is the fourth day off cortisone. The dose of DOCA has been maintained at 5 mgms. i.m. daily throughout this period. On or about 1/14 he began to complain that the pain in his back and legs was more severe. This has not been particularly progressive. There is no doubt that he has been frightened during the four days off cortisone. On several occasions he has raised the spectre of death. It has been difficult to reassure him that he is in no danger. This evening he complained of more marked weakness and excessive perspiration. He has noted some stuffiness in his nose for two days, and since last night, a dry cough.  $\tau$  99 degrees (M), BP, 130/80, Pulse 72 min., strong. He is perspiring excessively about the forehead and neck. No sign of respiratory infection. We will however, watch him carefully. If there is any rise in temperature, we will have to restart his cortisone.

1/28: 100 mgms. cortisone p.o. at 11:25 A.M.

As this experiment, and also the studies conducted on Ray Woodham and Max Gold demonstrate, the conflict between their responsibilities as clinicians and as investigators which the physicians of the Metabolic Group faced was a "true dilemma." On the one hand, their using or withholding a procedure or drug for the purpose of experimentation often put them in the position where they were not serving the welfare of the patients who were subjects, or might even be jeopardizing it. On the other hand, what they did to benefit these patients or to protect them from harm often curtailed or impaired their research. Thus, when Mr. Gold reacted adversely to the water test, for example, in the words of one physician, the Group had "no choice but to treat this man," which in turn, meant the "end of the experiment." Similarly, the negative clinical effects of . . . withdrawing cortisone from Mr. Terhune made it necessary to terminate these experiments despite the research value that the Group could have derived from prolonging them. "No matter which way you slice it," a member of the Group explained,

being a clinical investigator has its problems. A lot

of the research you do is of no benefit to patients, and there's a real possibility that you can do them harm. So, in order to do research you've got to close your eyes to some extent, or at least, take calculated risks with the patients on whom you run the experiments. . . . Still, you almost never attain the ideal research. . . . You rarely get to the basis of the problem you're investigating, because it's touch and go all along the way with these patients. Their care and welfare have to be taken into consideration. . . . So, you usually end up by compromising your research goals and standards. . . .

These conflicts between experimenting on patients and caring for them with which the physicians of the Metabolic Group were confronted were in some ways closely connected with another of their problems as clinical investigators: that of finding patients who would make appropriate subjects for their research, and of motivating them to serve in this capacity.

From the point of view of the Metabolic Group, the ideal research patient would have had the following characteristics. His medical condition would either be directly relevant to the research interests of the Group, or so unusual that it represented an opportunity to study phenomena rarely seen. He would be healthy enough to withstand the inconvenience and stresses of experimentation for long enough periods of time in order to enable the Group to realize high standards of accuracy and evidence in their research, and also to benefit him clinically through the procedures or drugs to which they subjected him. On the other hand, his condition would be sufficiently serious to justify asking him to undergo the degree of discomfort and risk that their experiments entailed, and to accept other than established methods of diagnosis and treatment. Furthermore, such an ideal research patient would not only be willing to submit to the conditions of the experiment, but he would be highly enough motivated to do so for a relatively long time. In short, the ideal research patient would have been one whose physical condition and attitudes allowed the physicians of the Metabolic Group to fulfill both their responsibilities as clinicians and investigators to an optimal degree.

Needless to say, the physicians of the Metabolic Group rarely, if ever, encountered a patient who met all these requirements. For example, the patients whose conditions were severe enough to justify subjecting them to an experimental procedure as radical as a total adrenalectomy, and to motivate them to consent to undergo it, were too ill to endure certain procedures that would have enabled the Metabolic Group to realize high standards of experimental

precision, or to derive very much therapeutic value from it. On the other hand, because patients with Addison's disease had received so much clinical benefit from advances in the diagnosis and treatment of their condition which the research of the Group had helped to make possible, they did not present as many "interesting" possibilities for study as they once had, it was no longer justifiable to expose them to the stresses of experimentation on the grounds that there were no effective established means for helping them, and the patients themselves were not as willing as they formerly were to undergo the tedium and rigors of experimentation.

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In sum, the problem for the Group was to find a sufficient number of patients with physical conditions which were suited to their research needs and interests and on whom it seemed morally justifiable to conduct the experiments they had in mind—and, if possible, to motivate those patients to serve as research subjects without violating their ethical responsibility to make known to the prospective subject all inconveniences and hazards the experiment might entail, or imposing any element of force, fraud, deceit, duress, overreaching, or other ulterior form of constraint or coercion upon them in order to obtain their consent to act as subjects.

Listening in on what the physicians of the Metabolic Group had to say one evening about the total bilateral adrenalectomy and the patients with hypertensive cardiovascular disease on whom they had carried out this experimental procedure, we hear many of the problems with which they were faced reviewed in this connection, and learn something about the way they were affected by them. The discussion that follows took place during the meeting which the Group customarily held at the end of each day. "Evening Rounds," as these meetings were called, were held in the Group's conference room, which rather appropriately was located half-way between the laboratory and the Metabolic Ward. The Group gathered around the long table in this room to discuss the clinical and research status of various patients for whom they were jointly responsible, and to make any decisions regarding them that were currently necessary. At this particular meeting, the Group were having a difficult time trying to decide what they ought to do for a seventeen-year-old patient with severe hypertension of unknown origin.

DR. D.: There's one thing I want to do before Bob Baum is discharged, if he can tolerate it, and

that's a histamine test. . . . The thing we ought to consider in following him is doing another retrogram on his left kidney some time in the future. Because if we definitely find bilateral kidney disease that way, then at least we've taken care of one specter. . . . He doesn't seem to be a good candidate for an exploratory now. So, what I'd do is send him out, plan to follow him closely, and have him come back in a few months. . . . I think our enthusiasm for doing an exploratory on this boy has waned considerably. . . .

DR. R.: Actually, I'm not quite sure why we've changed.

DR. D.: I'm not either.

DR. R.: Because we really haven't found out a helluva lot that would make us change our minds. . . .

DR. P.: I think there will be a great deal of feeling over exploring this boy, because he's young, and because we can't be sure that he doesn't have a pheo[chromocytoma]. . . . Then there'll probably be a great beating of the drums over sympathectomy and adrenalectomy. . . .

DR. D.: As I see it, the only reason for discharging him now is for psychological reasons. . . .

DR. P.: I agree. The real reason we're sending him out is because he needs more diagnostic tests of a traumatic nature.

DR. G.: It's also a matter, too, of not having any definitive treatment to offer him, isn't it?

DR. D.: We don't want to adrenalectomize him.

DR. G.: Why not?

DR. D.: I'm interested in doing some adrenalectomies, but not on him. . . . Do you mean to tell me we're going to start taking every young hypertensive case we find and do an adrenalectomy on them? Let me go on record right here and now, once and for all, about my position on adrenalectomies. I think now—and I've thought from the very first—that the adrenalectomy is a hopeless procedure—not one bit justifiable. There is no evidence whatsoever so far as I'm concerned that it is a bona fide therapeutic procedure. However, once committed to a program, there is the necessity for doing adrenalectomies on a few carefully selected cases for evaluative purposes. If we're going to make a definitive evaluation of the role of the adrenal cortex in hypertension, we've got to select a group of patients who don't have such serious complications as the patients we've done adrenalectomy on up till now. . . . But I don't want to do it on this patient.

DR. G.: It's just about time you went back to clinical medicine, Jim.

DR. D.: You're perfectly right. Because this group has been puttering around with adrenalectomy for two years now. It seems to me that once and for all the technique deserves some sort of real trial. This is the one place where that can and should be done. So, I'm willing to try it on ten carefully selected cases.

DR. G.: Quite apart from therapeutic considerations, then.

DR. D.: Right.

DR. R.: I think the patients who have survived adrenalectomy are doing very well. (*Group laughter*) No, I mean it. I'm serious. Howard Beech, for instance, was just about as bad as this patient is. . . .

DR. D.: Oh, was he? Well, the fact of the matter is, he was much, much better than he is. He was far and away the earliest case of hypertension we did. . . . You can list Will as a good result—I'll take that. You can list Mr. Hemming, but he falls in a different category entirely. . . . Mr. Ardsley's pressure is 230 over 160. . . . So, it's been a tremendous triumph!

DR. C.: Well, I'm not enthusiastic about the procedure at all. But in Abe Samuelson's case, I really think it's done something for him.

DR. D.: Sure, it's done something for him. But he's become a real invalid as a result.

DR. H.: A vegetable. . . .

DR. D.: He's been sitting in the house for 400 consecutive days now, not moving a muscle, having his family wait on him hand and foot, and doing

nothing but listening to the radio and watching TV all day. Even without the operation, on *that* regime his pressure would have come down!

DR. R.: So it's been a therapeutic success, but not a psychological one. (*Laughter*)

DR. D.: What you're doing, Bob, is evaluating these people as good merely on the basis of their not being dead, because so many of the others have died.

DR. C.: Bill Pappas is a good result.

DR. R.: And Walter Cousins was. . . .

DR. D.: Except he's now dead.

DR. R.: Well, you can't live forever! (*Group laughter*)

DR. P.: Well, we took a gamble, and maybe we lost.

DR. D.: I don't know. . . .

DR. C.: Now that we've gotten off *that* depressing subject, let's talk about the Cushing's disease cases. . . .

\* \* \*

## B.

### In Determining the Investigator's Authority, What Is the Relevance of:

#### 1.

##### Choice of and Attitude toward Subjects?

###### a.

##### Lawrence W. Shaw and Thomas C. Chalmers Ethics in Cooperative Clinical Trials\*

\* \* \*

A well-designed scientific trial of a new therapeutic agent provides for the careful comparison of the new agent with some prior therapy, or with no specific therapy if such comparison is ethically warranted. The setting up of a valid comparison between the two modes of therapy involves, in the usual case, the random assignment of the eligible patients to the two modes of therapy. Our thesis is that the use of this sound scientific approach in the search for knowledge has been, and remains, at a low level because of unfortunate and unfounded prejudices concerning the ethical propriety of randomization as a technique of the decision-making process in the practice of medicine. In our view, the random allocation of patients in a scientific clinical trial is more ethical than the customary procedure, that of trying out a new therapy in an unscientific manner by relying on clinical impression and comparison with past ex-

perience. This conclusion follows from application of the fundamental principle that the physician-investigator's primary responsibility is to his patient. The sequence of thought is:

1. If the clinician knows, or he has good reason to believe, that a new therapy (A) is better than another therapy (B), he cannot participate in a comparative trial of Therapy A versus Therapy B. Ethically, the clinician is obligated to give Therapy A to each new patient with a need for one of these therapies.

2. If the physician (or his peers) has genuine doubt as to which therapy is better, he should give each patient an equal chance to receive one or the other therapy. The physician must fully recognize that the new therapy might be worse than the old. Each new patient must have a fair chance of receiving either the new and, hopefully, better therapy or the limited benefits of the old therapy.

If these principles are accepted, the physician must follow the course that if he *knows* which therapy is better, he does not conduct a trial in the human domain. If the physician does not know which therapy is better, he should randomize, and thus he can readily participate in a scientific trial. In fact, this latter methodology might be a more ethical way of practicing medicine than the routine prescription of a medication that has never been established as more beneficial than harmful. If this point of view were more widely appreciated, the literature would carry more reports of sound scientific trials and

\* 169 *Annals of the New York Academy of Sciences* 487-488 (1970). Reprinted by permission.

fewer reports of inconclusive trials of new therapies.

\* \* \*

## NOTES

### NOTE 1.

THOMAS C. CHALMERS

#### THE ETHICS OF RANDOMIZATION AS A DECISION MAKING TECHNIQUE AND THE PROBLEM OF INFORMED CONSENT\*

\* \* \*

Let me illustrate the value of randomization by presenting the data in some detail on a surgical operation for a vascular disorder in a serious disease, namely portacaval shunt surgery in patients with cirrhosis. It is now almost 20 years since the operation was introduced as a means of preventing fatal hemorrhage from esophageal varices, and 51 papers have been written in the English literature on the experiences of physicians who have treated more than 10 patients. Forty-five of these have concluded that the operation is effective, and only six have called it ineffective. Only four of the 51 studies were well controlled, and three of these found the operation ineffective, and one showed only slight enthusiasm. Yet, the operation is assumed to be effective by almost every physician in America and Britain.

Widespread performance of portacaval shunt surgery in patients with cirrhosis and esophageal varices has been primarily based on two observations—that patients who have survived the operation rarely bleed again from their varices, and they seem to live longer than a group of cirrhotics compiled as an example of the best so-called natural history of the disease. There have been a few dissenters to the view that all patients who have bled from varices should be shunted if they seem to be in good enough shape to survive surgery. However, opinions have been about equally divided with regard to the efficacy of shunts in patients whose varices have not yet bled. This equal division of opinion among both surgeons and internists made it seem to be entirely ethical to conduct a controlled trial of shunt surgery as a prophylactic

procedure. Three groups have now conducted reasonably well-controlled trials: The Boston Inter-Hospital Liver Group, Dr. H. O. Conn of New Haven, and a group of collaborating Veterans Administration Hospitals under the chairmanship of Dr. F. C. Jackson. Combined survival data from all three of these groups indicate that the operation actually shortens rather than prolongs life. This is true when the decision whether to operate or not is made at random, rather than according to the clinical condition of the patient. The explanation for the discrepancy between the data gathered in the controlled studies and those of other series lies in a comparison of the survival of patients selected for surgery in the Boston Inter-Hospital Liver Group studies, and those with esophageal varices who had not bled but who were not selected for surgery. The 50 percent survival time of the selected patients is five years and of the unselected patients with the same criteria for the diagnosis of esophageal varices is less than one year. But the operation had no significant influence on survival in the selected group. Even if one subtracts from the so-called control group all of the patients who died within three months after the diagnosis of their varices, because it was unlikely that they might have been candidates for surgery in that time, the difference still is striking after one year. In other words, the best way to survive if you have cirrhosis of the liver and esophageal varices that have not yet bled is to be selected for a portacaval shunt, and not have the operation performed.

Because of the negative findings in the controlled studies of prophylactic shunt surgery and the lack of any controlled trial of surgery in patients who have already bled, it is now considered entirely ethical to carry out the study in the latter group, and this is being done by the Boston Inter-Hospital Liver Group and by the Veterans Administration Group.

These data have been presented in some detail to illustrate the tremendous importance of the selection factor in the evaluation of therapies which are inherently dangerous themselves and for which patients in good condition are likely to be selected, i.e., the selected patients might very well live a long time because of their good condition and not because of the treatment, and it is even conceivable that the treatment might shorten their lives. This information can only be obtained by a controlled trial in which the decision as to whether to operate or not is made solely at random, and the patients are followed

\* Report of Fourteenth Conference of Cardiovascular Training Grant Program Directors, National Heart Institute, Bethesda, Md.: U.S. Department of Health, Education, and Welfare 89-90 (1967). Reprinted by permission.

in such a way that the major therapy is the main variable.

\* \* \*

NOTE 2.

THOMAS C. CHALMERS  
WHEN SHOULD RANDOMISATION BEGIN?\*

\* \* \*

The series of articles on the anticoagulant effect of purified fraction of Malayan pit viper venom may be a milestone in the development of successful therapy for patients with thromboembolic disease, but it also raises a critical question as to the scientific and ethical requirements of new-drug trials in man. How early in the development of new drugs should the process of randomisation be introduced into the therapeutic trial? I am firmly convinced that the first patient to receive a new agent should be randomised. . . .

\* \* \*

The standard argument against early randomisation is that an improper trial would result if the drug has not previously been explored in selected patients to determine the proper dose and to decide whether or not a randomised trial is ethical. Is it proper for patients to be selected arbitrarily for earliest trials of a new agent when it is even more likely at that time that they might do better if they received standard therapy?

\* \* \*

NOTE 3.

PAUL FREUND  
ETHICAL PROBLEMS IN HUMAN  
EXPERIMENTATION†

\* \* \*

. . . A juvenile court judge of rather progressive and scientific mind decided to try an experiment regarding sentencing. There were two institutions available: one an ordinary prison; and the other a minimum-security institution where the offenders would return in the evening after being allowed to go out into the town and work during the day. In sentencing to one or the other place of detention, this judge was not in-

clined to weigh the merits of the individual offenders or appraise the likelihood of their benefiting from one rather than the other form of treatment. What he did was to pair off the offenders coming before him in the best way he could, in point of age, ethnic grouping, intelligence quotient, family background and so on; one of the pair would be sent to one place of correction and the other to the other, and the plan was that at intervals careful observations would be made so that in the future there would be some basis for judging the kind of offender who can be expected to profit or not to profit from one or another type of treatment. Perhaps I should not be disturbed by this, but I am troubled because it seems to be an unprincipled way of taking liberties with the liberty of young offenders—unprincipled, that is, except from the standpoint of experimental design. I would be a little easier in my mind if this experiment were thought to be of benefit to the particular offenders who are being sentenced rather than to their successors or their children. If, for example, the risk of recidivism were such that these very offenders might be back before the court, and the data drawn from the experiments might be helpful in their own subsequent cases, a stronger argument could be made for this kind of randomized procedure. . . .

\* \* \*

NOTE 4.

DONALD T. CAMPBELL AND  
ALBERT ERLEBACHER  
HOW REGRESSION ARTIFACTS IN QUASI-  
EXPERIMENTAL EVALUATIONS CAN MISTAKENLY  
MAKE COMPENSATORY EDUCATION LOOK  
HARMFUL\*

There are problems . . . with randomization experiments. Randomization at the invitational level avoids the disappointment problem generated by randomly allocating eager applicants to the control condition. But it exaggerates the always present problem of experimental mortality inasmuch as not all those randomly invited accept the treatment. Using only those invited who accept as the experimental group produces a selection bias with favorable pseudo-effects. . . .

\* \* \*

\* 1 *The Lancet* 858 (1968). Reprinted by permission.

† 273 *New England Journal of Medicine* 687, 689 (1965). Reprinted by permission.

\* J. Hellmuth, ed.: *Compensatory Education: A National Debate*, Vol. III of *The Disadvantaged Child*. New York: Brunner-Mazel 22-25 (1970). Reprinted by permission.

Social ameliorative changes which are applied or made available to everyone do not readily permit the creation of control groups. . . . But those expensive remediations which are in short supply and which cannot be given everyone provide settings in which true experiments are readily possible. Once the decision makers in government and applied research are educated to their importance, they can become the standard evaluational procedure.

On the one hand, we must not create a political climate which demands that no ameliorative efforts be made unless they can be evaluated. There will be many things obviously worth doing which cannot be experimentally evaluated, and which should still be done. The shift to new math is an example. By making math achievement tests inappropriate it undermined the only convenient benchmark for its own evaluation. College education is another example, a boon for which we have almost no interpretable experimental or quasi-experimental evidence. (Since college education is given to those who need it least, the regression artifacts are biased to make it look effective.) We applied social methodologists should be alert to recognize such cases and not assume that every new program must be and can be evaluated.

On the other hand, where we can experiment and where the social costs of such experimentation are outweighed by the social value of reality testing, we should hold out for the least biased, most informative procedures.

There exist in administrators, researchers, legislators, and the general public "ethical" reluctances to random assignment. These center around a feeling that the control group is being deprived of a precious medicine it badly needs. But if it be recognized that the supposed boon is in fact in short supply, then it can be seen that the experiment has not increased the number so deprived, but has instead reassigned some of that deprivation so that the ethical value of knowing may be realized. Is randomization as the mode of such reassignment ethically defensible? It might represent an ethical cost (one nonetheless probably worth paying) if all the children in the nation had been rank ordered on need, and those most needy given the compensatory education up to the budgetary and staff limits of the program. But instead, the contrast is with a very haphazard and partially arbitrary process which contains unjust inversions of order of need far more extensive than a randomization experi-

ment involving a few thousand children would entail. These unjust deprivations are normally not forced to our attention, and so do not trouble our ethical sensitivities as does the deprivation of the control group. But there is no genuine ethical contrast here.

Within randomization, there are some designs and stances that may ease any residual ethical burden. For example, the randomization could be limited to the boundary zone, at the least needy edge of those to be treated, the most needy edge of the untreated. For this narrow band of children, all considered as essentially tied at the cutting point on a coarse grained eligibility score, random assignment to treatment and nontreatment could be justified as a tie-breaking process. We would learn about the effects of the program only for a narrow band of talent. We would wonder about its effectiveness for the most disadvantaged. But this would be better than nothing, and better than quasi-experimental information.

The funds set aside for evaluation are funds taken away from treatment. This cost-benefit trade-off decision has already been made when quasi-experimental evaluation has been budgeted, or when funds are committed to any form of budgeting and accounting. Taking these evaluational funds, one could use nine tenths of them for providing experimental expansions of compensatory instruction, one tenth for measurement of effects on the small experimental and control samples thus created. Here the ethical focus could be on the lucky boon given to the experimentals. Since evaluation money would be used to expand treatment, the controls would not be deprived. . . .

b.

### **William A. Nolen The Making of a Surgeon\***

\* \* \*

We weren't very "long" on research at Bellevue. Most city hospitals don't have the money that it takes to do basic, or laboratory, surgical research. The taxpayers, who foot the bill for municipal hospitals, would be up in arms if someone slipped in a request for an extra million

\* New York: Random House 140, 143-146 (1970). Copyright © 1968, 1970 by William A. Nolen, M.D. Reprinted by permission of Random House, Inc.



for a complete dog lab with all the personnel and equipment needed.

On our division we didn't do any basic research at all, and that was fine with me. Some of my friends had chosen to train in research-oriented hospital centers without realizing what they were getting into, and most of them hated what they were doing.

\* \* \*

I was glad that we weren't forced to do research, unless we were so inclined, but there was one drawback to this attitude. When we finished our training there were certain hospitals, like those under Dr. Ramsey's direction, where it was virtually impossible to obtain an appointment to the staff unless you had a bibliography as long as your arm. I had no desire to practice in that sort of institution but Al Johnson did. . . .

Al was determined to get the appointment and asked Dr. Stevens to intervene in his behalf. "What it boils down to, Al, is this," Dr. Stevens told him after talking with Dr. Ramsey. "If you're willing to spend some time doing research, and if you can come up with two or three papers that are publishable, Dr. Ramsey will reconsider your application. It's another year or so out of your life, but if you want the job you'll have to make the sacrifice. I'll try to arrange a grant from some foundation, and you can do your work here at Bellevue if you can find a suitable project."

My reaction to this proposition would have been an immediate and emphatic "No"; there were plenty of other hospitals as good as the one Dr. Ramsey headed. The appointment certainly wouldn't have been worth a year of my life. But Al felt differently. He accepted the deal, and after a few weeks of mulling it over, decided he'd devote his research time to solving the problem of leg ulcers. He couldn't have chosen a problem better suited to investigation at Bellevue, since there were always half a dozen such patients in the ward.

Al went to work. Healing was the problem. Leg ulcers are a result of poor circulation, infection and injury. They may, and frequently do, extend from the ankle halfway up the leg. In our patients, with their poor hygiene and substandard living conditions, it was virtually impossible to get these sores to heal without several weeks in bed and, eventually, a skin graft. Even when we finally did get them healed, it was 2 to 1 that within six months the patient would be back with a recurrence.

At the time when Al embarked on his research project there were reports in the surgical journals of an enzyme, a chemical substance, that had been used successfully to expedite healing in dogs, but the work hadn't yet been done on humans. After reading all the literature on the subject, Al decided to try the drug on some of our patients.

Jack Lesperance agreed to admit six leg-ulcer patients from the outpatient department for Al's purposes. We brought them in on a Monday morning. There was no problem from the legal point of view. These gentlemen would have sold their mothers into slavery just to spend two nights in a warm hospital away from the Bowery. They signed the releases authorizing Al's research without a moment's hesitation.

All day long Al could be seen running back and forth from the intern's lab to the small room into which we had jammed the six beds for his vagrants. He had to complete some base-line (preliminary) blood studies before he could give them the intravenous solutions which he hoped would hasten the healing of their leg ulcers. It was five-thirty in the evening before he really had things under way and all six of his patients had fluids running into their arms.

Everything went smoothly for the first fifteen minutes. He might as well have been giving them sugar water for all the effect it had. Then one of them, Russ Peters, an old friend of ours, called Al over. "Say, Doc," he said, "I feel kind of hot. Could I be getting a fever?"

"I wouldn't think so," Al answered, "but let me check."

Russ's temperature was 103; by the time Al had finished checking the others, all five of them were perspiring profusely and shouting for help. Al was beside himself. He ran out onto the ward looking for an intern and bumped into our entire house staff. We were just escorting Dr. Stevens onto the ward for his Monday-night rounds.

"Can we help you, Al?" Dr. Stevens asked.

Al couldn't stand still. "I'd just like to borrow an intern or two, if I may, Dr. Stevens," he answered. "I need a little help."

"Certainly, Al. Who would you like to send, Jack?"

Jack gave him two interns. Al all but ran off with them down the hall. He wrapped the patients in alcohol-soaked sheets, threw ice water on them, turned fans on each one. All during rounds, each time Dr. Stevens looked up he could see Al or one of his co-workers running through the hall with ice or some other cooling



medium. "Al's certainly taking a vigorous approach to his research project, isn't he?" Dr. Stevens commented.

It was a near disaster. Even though he had shut off the I.V.'s as soon as temperatures started to climb, every patient hit 106 and five of them went temporarily off their rocker from the fever. The new chemical just wasn't ready for human use. . . .

## NOTES

### NOTE 1.

EDWARD A. SHILS  
SOCIAL INQUIRY AND THE AUTONOMY  
OF THE INDIVIDUAL\*

\* \* \*

Where, as in the United Kingdom, the direct approach of modern social science was developed in the study of living aborigines or of the lower social and economic classes of one's own society, serious issues did not arise. In the first place, these inquiries did not enter very deeply into the private sphere of their subjects; they confined themselves largely to external economic matters and to publicly observable actions. There was a restraint on curiosity, deriving from the puritanical ethos of the culture from whence the investigators came. There was, furthermore, no obvious problem in intruding on the privacy of savages or workingmen, particularly those at or near the poverty level, because, at bottom, the investigators did not feel that they shared membership in a common moral community with the persons investigated. They possessed no secrets which were sacred to the investigators; they possessed no secrets whose penetration could be expected to arouse discomfiture among the investigators or the circles in which they moved. The situation was little different in the United States. The first large-scale inquiries based on interviewing dealt with slum dwellers, Negroes, immigrants, juveniles on the margin of delinquency, persons with dubious moral standards, *et al.*—people regarded as not possessing the sensibilities which demand privacy or the moral dignity which requires its respect. Moreover, the investigators were inhibited in their curiosity by the wider culture and by the traditions of their discipline.

\* \* \*

\* Daniel Lerner, ed.: *The Human Meaning of the Social Sciences*. New York: Meridian Books, Inc. 114, 116–117 (1959). © 1959 by the World Publishing Company. Reprinted by permission.

### NOTE 2.

LOUIS LASAGNA  
SPECIAL SUBJECTS IN  
HUMAN EXPERIMENTATION\*

\* \* \*

I am concerned . . . about the use of students as volunteers in experiments conducted by their academic superiors. If a student is of age to give consent, there would seem to be no special ethical problem about volunteering in *general*. But if a student in a classroom is asked to volunteer by his instructor, there is at least the implied threat of loss of affection (and decreased academic grade) if the student fails to volunteer, which takes the situation into a nasty area of restricted choice. Furthermore, it has been the practice in some institutions actually to give extra credits for such participation, a procedure that raises the issue of infringement of the rights of those who do not volunteer or are not chosen after volunteering. Should the non-volunteers at least be allowed another means (non-experimental) of earning extra credits equal in amount to those earned by the volunteers, so as not to be academically disadvantaged? Perhaps, but the problems involved in being "fair" to all parties concerned suggest that it may be simpler, as well as more ethical, for professors to avoid soliciting volunteers from student groups whose academic standing or future employment may be in their hands. It is not enough to say that a professor will not be swayed in his marking or writing of reference letters by whether a student has volunteered; the *belief* that he will do so is enough to act as a troublesome influence on both the volunteer and the non-volunteer.

\* \* \*

### c.

Joseph H. Fichter and William L. Kolb  
Ethical Limitations on  
Sociological Reporting†

\* \* \*

Within the area of contemporary material a distinction can be drawn between studies of primitive societies and civilized communities. It is to be supposed that the details of social life

\* 98 *Daedalus* 449, 456–457 (1969). Reprinted by permission of Daedalus, Journal of the American Academy of Arts and Sciences, Boston, Massachusetts.

† 18 *American Sociological Review* 544, 546–550 (1953). Reprinted by permission.

among the Samoans were not reported to these people, and if any reputations suffered from such study it was only among non-Samoans. There have been instances, however, of anthropologists' reports getting back to American Indian tribes, causing some dissension and suspicion among the members of the tribe. In either case the sociologist must consider these people as the subjects of human rights, even though the prospect of moral damage may not be great.

In studying contemporary communities the problem of reporting varies according to whether the data concerned are sacred or non-sacred. The analysis of behavior patterns which involve high traditional values (like religion, family and sex, ethnic and group loyalties) should, of course, be as objective as possible, but an effort should be made to avoid needless and callous affront to the people who hold such values and such an effort requires special attention and care. In non-sacred areas (such as economic and political activities, housing and recreational problems) there can be greater freedom of reporting.

\* \* \*

... At the core of the Western value system is a belief in the basic dignity and worth of the human being. This belief is based on different assumptions according to the particular stream of tradition in which one locates it: the Fatherhood of God, natural law, universal human needs and aspirations, or human reason. Whatever the base, the belief implies that men are bound to one another in a moral community. Membership in this community requires that the individual's rights to privacy, secrecy, and reputation be respected, even though the human beings studied may not be members of the sociologist's own society.

The belief also implies that a man or group can renounce membership in the moral community by choosing modes of action which violate these basic values of dignity and worth. In mid-century it seems probable that men like Hitler and Stalin, organized groups like "Murder Incorporated," the Ku Klux Klan, and some others, have placed themselves outside the moral community and have surrendered the protection of its norms. Thus the social scientist need have no qualms about reporting in full detail the activities of such groups and people. Although this norm has never been explicitly formulated, it has guided a great deal of the research and reporting in social science.

Yet the decision of the sociologist to place particular persons or groups outside the moral community involves great responsibility, and he must be careful that his criteria of judgment permit tolerance, compassion, and wisdom. This is especially the case when he studies "unpopular" racial, religious and political groups, prostitutes, homosexuals, drug addicts, and the psychologically ill, the poor and powerless. It is hardly questionable that these people remain members of the moral community and hence retain their rights of privacy, respect, and secrecy. The needs of the society may require a limitation of their rights by the courts or by the social scientist in his reporting, but basic rights can be limited only to the extent that they *must* be limited. Beyond that point such people must be treated in the same way as other members of the moral community.

The recognition of basic human rights which accompany membership in the moral community is an important means by which social scientists can avoid the dangers of the use of purely subjective criteria. Within the consensus of the Western tradition it is objectively true that there are moral evils and modes of action which place the perpetrator outside this community. We must know as much as possible about such people and the scientist need have little inhibition in the report he provides about them. All other persons and groups, no matter how personally distasteful to the scientists, seem to require the respect of their fellow-members in the moral community.

#### NOTE

HOWARD S. BECKER  
PROBLEMS IN THE PUBLICATION OF  
FIELD STUDIES\*

\* \* \*

Fichter and Kolb seem to assume that, except for Hitler, Stalin, and others who are not members of our moral community, there is no irreconcilable conflict between the researcher and those he studies. In some cases he will clearly harm people and will refrain from publication; in others no harm can be done and publication is not problematic. The vast majority of cases will fall between and, as men of good will,

\* Arthur J. Vidich, Joseph Bensman, and Maurice R. Stein, eds.: *Reflections on Community Studies*. New York: John Wiley and Sons, Inc. 267, 272-274 (1964). Reprinted by permission.

the researcher and those he studies will be able to find some common ground for decision.

But this analysis can be true only when there is some consensus about norms and some community of interest between the two parties. In my view that consensus and community of interest do not exist for the sociologist and those he studies.

The impossibility of achieving consensus, and hence the necessity of conflict, stems in part from the difference between the characteristic approach of the social scientist and that of the layman to the analysis of social life. Everett Hughes has often pointed out that the sociological view of the world—abstract, relativistic, generalizing—necessarily deflates people's view of themselves and their organizations. Sociological analysis has this effect whether it consists of a detailed description of informal behavior or an abstract discussion of theoretical categories. The members of a church, for instance, may be no happier to learn that their behavior exhibits the influence of "pattern variables" than to read a description of their everyday behavior which shows that it differs radically from what they profess on Sunday morning in church. In either case something precious to them is treated as merely an instance of a class.

Consensus cannot be achieved also because organizations and communities are internally differentiated and the interests of subgroups differ. The scientific report that pleases one faction and serves its interests will offend another faction by attacking its interests. Even to say that factions exist may upset the faction in control. What upsets management may be welcomed by the lower ranks, who hope the report will improve their position. Since one cannot achieve consensus with all factions simultaneously, the problem is not to avoid harming people but rather to decide which people to harm.

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d.

#### David McK. Rioch Concluding Remarks\*

\* \* \*

It is not inconceivable that as our methods improve and as with increased information we

can more precisely select relevant problems, certain patients with incurable diseases and short life expectancy might be willing to volunteer as subjects for collaborative studies utilizing implanted electrodes. Considering these problems I have necessarily done some thinking about my own attitudes. . . . It has seemed to me that when I come to retire and, as it were, only have use for my brain in getting around the yard, but will not be responsible for decisions significantly affecting other people's lives, I might quite reasonably approach an experimental neurosurgeon in whose work and scientific orientation I had confidence and say, "Let us do an experiment together, as there are a number of things both you and I would like to find out." Now, I find myself hesitating in discussing this subject as it has many nuances and conflicting factors. I am aware that there is a good deal of psychiatric opinion to the effect that people who volunteer as subjects for experiments are conceivably more crazy than people who don't. However, I also recognize that within the next few years I might want to ask a patient to volunteer for certain observations, and I would not want to be in the position of asking another person to do something which I would not do. It is clear that I could not be certain as to what cultural role I would be assuming, what my "motivation" would be, in becoming an experimental subject. . . .

. . . Although many people are "horrified" by experiments on human subjects, I find myself not infrequently somewhat more concerned about the effect of these experiments on the experimenters. The doing of experiments tends to shut out from the awareness of the experimenter most of those aspects of relationship with the patient that are really valuable in human interchange. That is, when one begins to look at the patient or the subject as part of an experimental operation one ceases to respond; the patient can no longer evoke from the observer those involved and intimate behaviors by which as *humans* we live. Such attitudes tend to feed upon themselves and become more general, and the experimenter becomes shut off from his subject and is no longer a physician. I sometimes think that much of our study of patients is directed toward coming closer to them as human beings, but we find it dangerous and shield our need with scientific methods.

\* \* \*

\* E. R. Ramey and D. O'Doherty, eds.: *Electrical Studies of the Unanesthetized Brain*. New York: Paul B. Hoeber 411-412 (1960). Reprinted by permission of Harper & Row, Publishers.

## NOTE

HERBERT C. KELMAN  
HUMAN USE OF HUMAN SUBJECTS—  
THE PROBLEM OF DECEPTION IN SOCIAL  
PSYCHOLOGICAL EXPERIMENTS\*

\* \* \*

In our other interhuman relationships, most of us would never think of doing the kinds of things that we do to our subjects—exposing others to lies and tricks, deliberately misleading them about the purposes of the interaction or withholding pertinent information, making promises or giving assurances that we intend to disregard. We would view such behavior as a violation of the respect to which all fellow humans are entitled and of the whole basis of our relationship with them. Yet we seem to forget that the experimenter-subject relationship—whatever else it is—is a *real* interhuman relationship, in which we have responsibility toward the subject as another human being whose dignity we must preserve. The discontinuity between the experimenter's behavior in everyday life and his behavior in the laboratory is so marked that one wonders why there has been so little concern with this problem, and what mechanisms have allowed us to ignore it to such an extent. I am reminded, in this connection, of the intriguing phenomenon of the "holiness of sin," which characterizes certain messianic movements as well as other movements of the true-believer variety. Behavior that would normally be unacceptable actually takes on an aura of virtue in such movements through a redefinition of the situation in which the behavior takes place and thus of the context for evaluating it. A similar mechanism seems to be involved in our attitude toward the psychological experiment. We tend to regard it as a situation that is not quite real, that can be isolated from the rest of life like a play performed on stage, and to which, therefore, the usual criteria for ethical interpersonal conduct become irrelevant. Behavior is judged entirely in the context of the experiment's scientific contribution and, in this context, deception—which is normally unacceptable—can indeed be seen as a positive good.

\* \* \*

\* 67 *Psychological Bulletin* 1, 5 (1967). © 1967, American Psychological Association. Reprinted by permission.

## 2.

## Awareness of Participation?

a.

Kai T. Erikson  
A Comment on Disguised Observation  
in Sociology\*

\* \* \*

"[P]ersonal morality" and "professional ethics" are not the same thing. Personal morality has something to do with the way an individual conducts himself across the range of his human contacts; it is not local to a particular group of persons or to a particular set of occupational interests. Professional ethics, on the other hand, refer to the way a group of associates define their special responsibility to one another and to the rest of the social order in which they work. In this sense, professional ethics often deal with issues that are practical in their application and limited in their scope: they are the terms of a covenant among people gathered together into a given occupational group. For instance, it may or may not be ethical for an espionage agent or a journalist to represent himself as someone he is not in the course of gathering information, but it certainly does not follow that the conduct of a sociologist should be judged in the same terms; for the sociologist has a different relationship to the rest of the community, operates under a different warrant, and has a different set of professional and scientific interests to protect. In this sense, the ethics governing a particular discipline are in many ways local to the transactions that discipline has with the larger world.

\* \* \*

... It may seem a little cranky to insist that disguised observation constitutes an ugly invasion of privacy and is, on that ground alone, objectionable. But it is a matter of cold calculation to point out that this particular research strategy can injure people in ways we can neither anticipate in advance nor compensate for afterward. For one thing, the sheer act of entering a human transaction on the basis of deliberate fraud may be painful to the people who are thereby misled; and even if that were not the case, there are countless ways in which a stranger who pretends to be something else can disturb

\* 14 *Social Problems* 366, 367-373 (1967). Reprinted by permission.

others by failing to understand the conditions of intimacy that prevail in the group he has tried to invade. Nor does it matter very much how sympathetic the observer is toward the persons whose lives he is studying: the fact of the matter is that he does not *know* which of his actions are apt to hurt other people, and it is highly presumptuous of him to act as if he does—particularly when, as is ordinarily the case, he has elected to wear a disguise exactly because he is entering a social sphere so far from his own experience.

So the sheer act of wearing disguises in someone else's world may cause discomfort, no matter what we later write in our reports; and this possibility raises two questions. The first, of course, is whether we have the right to inflict pain at all when we are aware of these risks and the subjects of the study are not. The second, however, is perhaps more important from the narrow point of view of the profession itself: so long as we suspect that a method we use has at least *some* potential for harming others, we are in the extremely awkward position of having to weigh the scientific and social benefits of that procedure against its possible cost in human discomfort, and this is a difficult business under the best of circumstances. If we happen to harm people who have agreed to act as subjects, we can at least argue that they knew something of the risks involved and were willing to contribute to that vague program called the "advance of knowledge." But when we do so with people who have expressed no readiness to participate in our researches (indeed, people who would presumably have refused if asked directly), we are in very much the same ethical position as a physician who carries out medical experiments on human subjects without their consent. The only conceivable argument in favor of such experimentation is that the knowledge derived from it is worth the discomfort it may cause. And the difficulties here are that we do not know how to measure the value of the work we do or the methods we employ in this way, and, moreover, that we might be doing an extraordinary disservice to the idea of detached scholarship if we tried. Sociologists cannot protect their freedom of inquiry if they owe the rest of the community (not to mention themselves) an accounting for the distress they may have inadvertently im-

posed on people who have not volunteered to take that risk.

\* \* \*

In one of the most sensible pieces written on the subject, Julius Roth has reminded us that all social research is disguised in one respect or another and that the range of ethical questions which bear on the issue must be visualized as falling on a continuum. Thus, it is all very well for someone to argue that deliberate disguises are improper for sociologists, but it is quite another matter for him to specify what varieties of research activity fall within the range of that principle. Every ethical statement seems to lose its crisp authority the moment it is carried over into marginal situations where the conditions governing research are not so clearly stipulated. For instance, some of the richest material in the social sciences has been gathered by sociologists who were true participants in the group under study but who did not announce to other members that they were employing this opportunity to collect research data. Sociologists live careers in which they occasionally become patients, occasionally take jobs as steel workers or taxi drivers, and frequently find themselves in social settings where their trained eye begins to look for data even though their presence in the situation was not engineered for that purpose. It would be absurd, then, to insist as a point of ethics that sociologists should always introduce themselves as investigators everywhere they go and should inform every person who figures in their thinking exactly what their research is all about.

But I do think we can find a place to begin. If disguised observation sits somewhere on a continuum and is not easily defined, this only suggests that we will have to seek further for a relevant ethic and recognize that any line we draw on that continuum will be a little artificial. What I propose, then, at least as a beginning, is the following: first, that it is unethical for a sociologist to *deliberately misrepresent* his identity for the purpose of entering a private domain *to which he is not otherwise eligible*; and second, that it is unethical for a sociologist to *deliberately misrepresent* the character of the research in which he is engaged. . . .

\* \* \*

## NOTE

LAUD HUMPHREYS  
TEAROOM TRADE—IMPERSONAL SEX  
IN PUBLIC PLACES\*

\* \* \*

At the conclusion of his article, Erikson proposes two rules regarding misrepresentation of the researcher's identity and purposes:

It is unethical for a sociologist to *deliberately misrepresent* his identity for the purpose of entering a private domain to which he is not otherwise eligible.

It is unethical for a sociologist to *deliberately misrepresent* the character of the research in which he is engaged.

Since one's identity within the interaction membrane of the tearoom is represented only in terms of the participant role he assumes, there was no misrepresentation of my part as an observer: I was indeed a "voyeur," though in the sociological and not the sexual sense. My role was primarily that of watchqueen, and that role I played well and faithfully. In that setting, then, I misrepresented my identity no more than anyone else. Furthermore, my activities were intended to gain entrance not to "a private domain" but to a public restroom. The only sign on its door said "Men," which makes me quite eligible for entering. It should be clear, then, that I have not violated Erikson's first canon. Although passing as deviant to avoid disrupting the behavior I wished to observe, I did not do so to achieve copresence in a private domain.

The second rule may be applied to the reactive part of my research, when I interviewed persons I had observed in the tearooms under the pretext of a social health survey. Here it should be noted that all interviews were in fact made as part of a larger social health survey, and abstracted data from my interviews are already in use in that study. The problem then may be viewed in two ways: First, I gave less than full representation of what I was doing, though without giving false representation. I wore only one of two possible hats, rather than going in disguise. Second, I made multiple use of my data. Is it unethical to use data that someone has gathered for purposes one of which is unknown to the respondent? With the employment of proper security precautions, I think such multiple use is quite ethical; it is frequently employed by

anyone using such data banks as the records of the Bureau of Census.

\* \* \*

b.

Fred Davis  
Comment on "Initial Interaction of  
Newcomers in Alcoholics Anonymous"\*

\* \* \*

In their article "Initial Interaction of Newcomers in Alcoholics Anonymous," John Lofland and Robert Lejeune report on an "experiment [which] consisted in sending six male agents (*sic*; graduate students in sociology) to A.A. open meetings where they posed as alcoholic newcomers". . . .

There is little need to dwell on the more narrowly professional issues occasioned by research strategies of this genre (i.e., those political ones having to do with the power and repute of sociologists to command access to persons and organizations in furtherance of scholarly objectives). Suffice it to say that the leaders and members of no corporate group, especially one imbued with a reformistic spirit of mission, can be reasonably expected to view such acts of premeditated deception with, to understate the case, indifference. . . .

Beyond these . . . considerations however, there looms the more cogent issue of the character and extent of the sociologist's license to exempt himself from the expectations, common reciprocities and *modus operandi* of the persons and organizations to which he attaches himself in his role of participant-observer. I can only raise again the same kinds of disturbing, yet ever relevant, questions that many have raised before me. Is such license complete or partial? Enduring on all occasions, or terminal according to time, place and circumstances? Contingent when studying "good" causes and institutions, but uninhibited when studying "bad" ones? Equally applicable in whatever degree to the powerful and powerless alike or, as a matter of expedience, of differential applicability? (A colleague has ventured the disquieting allegation that while sociologists are as a rule scrupulous in setting forth their research auspices and purposes when making first-hand studies of such powerful groups as the military, labor unions and liberal professions, they tend to be a good deal less conscientious on

\* Chicago: Aldine Publishing Co. 172 (1970). Reprinted by permission.

\* 8 *Social Problems* 364-365 (1961). Reprinted by permission.

this score when studying such powerless groups and aggregates as isolated religious cults, deviants of various kinds and anonymous respondents at every twenty-third household.) . . .

\* \* \*

Last, there is what some may treat as only a sentimental objection, but one which despite its elusiveness, I feel, comes closest to the heart of the matter. That is, in field situations in which the sociologist (or anthropologist) openly represents himself to his subjects for what he is (i.e., a person whose interest in them is professional rather than personal) he unavoidably, and properly I would hold, invites unto himself the classic dilemma of compromising involvement in the lives of others. Filling him with gossip, advice, invitations to dinner and solicitations of opinion, they devilishly make it evident that whereas he may regard himself as the *tabula rasa* incarnate upon whom the mysteries of the group are to be writ, they can only see him as someone less detached and less sublime. There then follows for many a fieldworker the unsettling recognition that, within very broad limits, it is precisely when his subjects palpably relate to him in his "out-of-research role" self (or "presentation," depending on one's disassociative bent) that the *raison d'être* for his "in-role" self is most nearly realized; they are more themselves, they tell and "give away" more, they supply connections and insights which he would otherwise have never grasped. (One is tempted to conceive of this moral paradox as the sociologist's original sin, although happily the benign interpositions of area sampling, pre-coded questionnaires and paid interviewers now spare more and more of us from suffering its pangs.)

It is in large measure due to this ineluctable transmutation of role postures in field situations that, when he later reports, the sociologist often experiences a certain guilt, a sense of having betrayed, a stench of disreputability about himself; these, despite the covers, pseudonyms and deletions with which he clothes his subjects. (Or, have I alone heard such "confessions" from fellow sociologists?) In an almost Durkheimian sense, I would hold that it is just and fitting that he be made to squirm so, because in having exploited his non-scientific self (either deliberately or unwittingly) for ends other than those immediately apprehended by his subjects he has in some significant sense violated the collective conscience of the community, if not that of the profession.

Now, the resort to calculated and whole-cloth deception of the type discussed here does not of course escape the final terms of this dilemma which may unalterably be our lot. It does, however, escape the intermediate ones: the discovery that *in vivo* the participant research role becomes something, both, more and less than itself; the conscious opening up of self to the possibility of rebuttal, disaffection, divided loyalties, compromising attachments and difficult disclosures; the price of engagement as opposed to that of mere doing. And, it is ultimately in this sense that such actions strike me as less than human, and hence unworthy of a discipline which, whatever else it represents itself as, also calls itself by that name.

\* \* \*

#### NOTE

JULIUS A. ROTH  
COMMENTS ON "SECRET OBSERVATION"

\* \* \*

All research is secret in some ways and to some degree—we never tell the subjects "everything." We can escape secrecy more or less completely only by making the subjects participants in the research effort, and this process, if carried far enough, means that there would be no more "subjects." So long as there exists a separation of role between the researchers and those researched upon, the gathering of information will inevitably have some hidden aspects even if one is an openly declared observer. The following are at least some of the reasons for this:

1. The researcher usually does not know everything he is looking for himself when he first starts out and structures his study to some extent as he goes along. Some of the things he finds of interest to study as the research goes on are things which the subjects might have objected to if they had been told about it in the beginning.

2. In many types of study of social behavior, the researcher does not want the subjects' behavior influenced by his knowledge of what the observer is interested in.

3. Even if the subjects of a study are given as precise and detailed an explanation of the purpose and procedure of the study as the investigator is able to give them, the subjects will not understand all the terms of the research in the same way that the investigator does. The terms

\* 9 *Social Problems* 283-284 (1961). Reprinted by permission.



used have different connotations to them, their experiential contexts differ, and their conceptions of the goals of the study are likely to be different. Therefore, even in those cases where the researcher has made a deliberate effort to explain to his subjects just what he is going to do, he will frequently find them acting surprised when he actually goes ahead and does it.

When a psychologist gives a subject a TAT and tells the subject that he is simply telling stories, is this "secret research"? Or still further removed, when you give a prospective employee what looks like an application form and then do a personality analysis of his responses, is this or is this not equivalent to posing as a fake participant?

When we are observing a crowd welcoming a hero, it is obviously absurd to say that we should warn everybody in the crowd that a sociologist is interpreting their behavior. The same can probably be said if we observe the behavior of the passengers we ride on the bus with every day. But suppose we are systematically observing the behavior of fellow workers in a shop or an office? Or the members of one's own family?

Does the manner in which one comes to be a secret observer affect the morality of the situation? Is it moral if one gets a job in a factory to earn tuition and then takes advantage of the opportunity to carry out a sociological study, but immoral to deliberately plant oneself in the factory for the express purpose of observing one's fellow workers? If the outcome is the same—e.g., if the manner in which the observations are used are the same—I, for one, see no moral difference in these two situations, but I find some of my colleagues do not agree with this position.

If the possibility of disrespect for an organization or group is at issue, we are faced with the question of just when a collection of people becomes a self-identifiable group that may have considered itself being researched on. Would this mean that groups which are consciously organized deserve more consideration than those which are not? As observers must we be careful of how we deal with hospital nurses, but be more free in how we deal with patients who are unorganized and are not likely to read our reports? Might it perhaps be considered proper to keep secret notes on the behavior of truck drivers with whom one hitches a ride or with whom one works, but not upon the members of the Teamsters Union as an organization?

\* \* \*

The point of all these illustrations is that social science research cannot be divided into the "secret" and the "non-secret." The question is rather how much secrecy shall there be with which people in which circumstances? Or, to state the question in a more positive (in more researchable) manner: When we are carrying out a piece of social science research involving the behavior of other people, what do we tell whom under what circumstances? Posing the question in this manner puts us in the same boat with physicians, social workers, prostitutes, policemen, and others who must deal with information which is sometimes delicate, threatening, and highly confidential. We are then in a position to draw upon our own knowledge of these other groups and the way in which they handle information to carry out their work and to draw analogies between those professions and our own.

\* \* \*

C.

### Edward A. Shils Social Inquiry and the Autonomy of the Individual\*

\* \* \*

Experimentation often involves manipulation, although it need not always do so. Manipulative experimentation involves the exercise of influence for an end which is not fully shared between experimenter and the experimental subject. Such experimentation is not a relation between equals; it is a relationship in which power is exercised, at best within a framework of consent and mutual good will.

Now authority is exercised throughout society, and most of us regard it as reasonable to accept within its proper limits. It is exercised by legislators, physicians, priests, teachers, and civil servants. In all these relationships, the end striven for by the person exercising authority is not perceived as clearly or shared equally by the person over whom it is exercised. That is in the nature of authority, and its inevitability renders it acceptable, even though it should be recognized that it often falls very far short of the highest ethical standards of liberal individualism. But apart from its inevitability, we regard it as proper by virtue of the common commitment to membership in the civil community. Even within

\* Daniel Lerner, ed.: *The Human Meaning of the Social Sciences*. New York: Meridian Books, Inc. 114, 142-144 (1959). © 1959 by the World Publishing Company. Reprinted by permission.



this commitment, however, which is by no means entirely voluntary, there are limitations. When the exercise of civil authority shades off into manipulation, i.e., when the ends of the instigated action become more and more opaque to the person over whom it is exercised, and the opacity is a deliberate creation of the ruler, the bonds of obligation loosen. The authority of the experimenter has none of the claims of the civil authority; it is more like a contractual relationship, with the limitation on the right to contract away one's will or dignity or to serve unforeseen purposes of the experimenter. The less the experimental subject appreciates or desires the ends sought by the experimenter and the less intelligible to him are the means used for eliciting his obedience, the more problematic it becomes ethically. As consensus becomes attenuated, manipulation increases. This is the kind of power exercised in the operation of a sociological experiment.

The subject of an experiment will practically never know as much about the experiment and its meaning as the designer of the experiment, and if he did, it might prejudice the desired outcome. The problem that remains, therefore, given this irreducible trace of the ethically problematic in social and psychological experimentation, is whether it is kept down to a minimum in its pernicious effects. Here, on the whole, I think the record of social science experimentation is quite unblemished. Its purity, however, can be partly associated with its scientific inconsequentiality. If it studied more important variables which touch more deeply and lastingly on the life, conduct, and outlook of the subjects, it might perhaps have acquired more scientific substance, but it would have done so at a much greater ethical risk.

Recently, a group of Cornell University anthropologists came into control over a Peruvian *hacienda* of two thousand persons on whom they are using their authority to institute large-scale and long-range changes. They are undertaking to rule the lives of men and women without the legitimacy that any government, even a tyranny, possesses, but with the legitimacy of a large landowner. If we assume that they introduce no measures except what they think beneficial for their subjects, then they are a benevolent despotism. If, as seems to be the case, they attempt to establish democracy there, to raise the standard of living, to increase education and civic responsibility, their position is little different from that of the conventional liberal reforming land-

lord, except that they are also trying to observe precisely the results of their efforts. They have two claims to justification—one, the enhancement of welfare, and the other, the increase in their knowledge of how the changes came about—but as far as I can gather, no measure is instituted for exclusively cognitive purposes. Although I have not seen any detailed reports on this unprecedented undertaking in which the Peruvian government has rented two thousand of its citizens to a foreign landlord, the dominant impression I receive is that the Cornell group is trying to apply its already available knowledge to the practical task of improving the life of a hitherto impoverished and suppressed group, increasing their self-respect, their desire and capacity for self-government, their productivity, their understanding and skill. It should also be noted that in a formal sense, there is apparently no experimentation.

In trying to arrive at an assessment of sociological experimentation, the Cornell-Peru project is important because it reminds us that, whatever their attitude toward the larger social order and the institutions of authority which play important parts in regulating it, social scientists do not seek the gratifications of aggression in their relations with their experimental subjects. Unlike the experiments of the physicians who worked on Jews, Poles, and other "inferior races" in the German concentration camps, no deprivations are being knowingly inflicted on the Peruvian peasants. Social scientists, whatever their other imperfections, are usually not sadists.

### 3.

#### Disclosure of Risks?

a.

#### Authorization and Release of Responsibility for the Investigational Use of LSD-25 and Psilocybin (1964)\*

I, the undersigned, hereby authorize [the] chief investigator, to administer to me the drugs LSD-25 and psilocybin, as well as concentration procedures, psychological examinations and tests. I hereby consent to the administration of the aforesaid drugs and to all psychological tests, examinations, procedures, and experiments involving their use as Doctor [A] deems neces-

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sary or advisable. I hereby authorize Doctor [A] to make use of all records, tests and personal data derived from these experiments and previous experiments for the purposes of his research and to quote, summarize or otherwise incorporate in research reports or publication all records, tests and personal data derived from these experiments and previous experiments providing that there is no disclosure of the identity of the undersigned. I fully understand that the safety and usefulness of examinations, tests, treatments or therapies involving the use of LSD-25 and psilocybin are now being investigated and that the manufacturers and distributors of said agents have supplied them for research and investigational purposes. I have read and understood and signed the attached description of the potential risks and consequences of undergoing these experiments, examinations and tests involving the use of LSD-25 and psilocybin, and of concentration procedures. I hereby personally assume all risks known and unknown and agree to hold Doctor [A] and his agents and employees free and harmless from any claims, demands or suits for damages for any injuries or complications whatsoever which may result from or arrive out of these experiments, examinations, tests and procedures involving the use of LSD-25, psilocybin and concentration procedures.

It is understood that no charge is being made for LSD-25 and psilocybin used in the course of the experiments, examinations or tests.

This release is freely given, and I affirm that I am not acting under fraud, duress or menace of any person whomsoever.

DATED: \_\_\_\_\_

SIGNED: \_\_\_\_\_

WITNESS: \_\_\_\_\_

WITNESS: \_\_\_\_\_

### NOTES

#### NOTE 1.

#### SUMMARY OF RISKS FROM EXPERIMENTAL ADMINISTRATION OF LSD-25 AND PSILOCYBIN (1964) \*

LSD-25 and psilocybin are now classified by the Federal Food and Drug Administration as experimental. This summary describing risks attendant upon the use of such agents in investigational examinations, tests, treatments and thera-

pies has been prepared in compliance with legislation requiring that prospective patients or experimental subjects be acquainted with these risks.

The primary risks from use of psychedelic agents arise from the possibility of emotional upset leading either to suicide or a psychotic reaction (nervous breakdown). The most extensive summary of these risks is contained in an article by Dr. Sidney Cohen, written in 1960 in the *Journal of Nervous and Mental Disease* (Vol. 130, pp. 30-40). The data presented was obtained from questionnaires sent to various investigators who had experience in administering these agents. Of the almost 5,000 cases reported involving over 25,000 ingestions, psychotic reactions lasting more than 48 hours were reported in 0.8 cases per 1,000 among experimental subjects, and 1.8 cases per 1,000 among patients undergoing therapy. No attempted suicides were reported among the experimental subjects and 1.2 cases per 1,000 of attempted suicides, and 0.4 cases per 1,000 of completed suicides were reported among patients undergoing therapy.

Safeguards have been instituted in order to eliminate these major types of reactions. However, the risk of suicide or psychotic reaction is present and must, therefore, be recognized.

There are no known deleterious physical effects from use of psychedelic agents in the dosage range which the subject will undergo in these experiments. Physical injuries which might occur include tongue biting and bruises and result from a patient's inability to handle the emotional effects of uncovering his own mind.

Some anxiety and emotional turmoil is to be expected from the administration of these agents. These effects are typical of those which occur whenever exploration of a person's psyche is undertaken.

Concentration procedure, as performed in the experiment, may evoke anxiety and emotional turmoil but there are otherwise no known deleterious physical or psychological effects to be expected from them. Aside from the individual effects of the drugs and concentration exercises, there are no known deleterious effects from the combining of LSD-25 and/or psilocybin with concentration procedures.

DATED: \_\_\_\_\_

SIGNED: \_\_\_\_\_

WITNESS: \_\_\_\_\_

WITNESS: \_\_\_\_\_

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## NOTE 2.

J. KENNETH BENSON AND JAMES O. SMITH  
THE HARVARD DRUG CONTROVERSY—  
A CASE STUDY OF SUBJECT MANIPULATION  
AND SOCIAL STRUCTURE\*

\* \* \*

... Perhaps the most frequent complaint lodged against the Leary-Alpert psilocybin research project concerns the alleged failure of the researchers to provide adequate safeguards for the health of their research subjects. Such a complaint was voiced by behavioral scientists at a meeting of the faculty and students of Harvard's Center for Research in Personality. Some public reactions to the research by individual members of the medical profession have included expressions of concern for the welfare of research subjects. During the controversy, some editorials (one written by Harvard's Farnsworth) appearing in medical journals warned of possible harm from hallucinogenic materials without naming the Harvard research project specifically.

The concern of critics seems to have been based on the following presumed dangers of exposure to psilocybin and other hallucinogenic drugs:

- (1) short-term psychosis-like experiences are reported by some subjects;
- (2) long-term mental disorders are precipitated in some cases;
- (3) suicide attempts occur in a few instances;
- (4) psychological dependence sometimes develops, even though physiological addiction apparently does not;
- (5) the use of other, more dangerous, drugs may be encouraged;
- (6) long-term changes in the personality of the subject may take place.

For their part, Leary and Alpert denied that the health of their research subjects was in jeopardy. They argued that the deleterious effects of the hallucinogens reported in earlier studies were accounted for by the variables of "set" and "setting." If research subjects expect a psychosis-mimicking experience and are exposed to the drugs in a clinic-like setting appropriate to psychosis, then a terrifying, potentially harmful experience will ensue, they contended. By contrast, exposure to the drugs in a friendly, relaxed, supportive atmosphere in which subjects have been

led to expect beneficial, insightful, educational experiences is productive of such experiences. In Leary's words,

Set and suggestive contexts account for ninety-nine per cent of the specific response to the drug. Thus, you cannot sensibly talk about the effects of psilocybin. It's always the set and suggestive context triggered off by the drug. A fascinating tension between these two factors—set and context—inevitably develops. If both are positive and holy, then a shatteringly sacred experience results. If both are negative then a hellish encounter ensues.

There is considerable agreement among psychopharmacologists that the suggestive context of the drug experience largely determines the psychological effects of the hallucinogens.

Leary and Alpert did not deny that personality changes were occurring among their research subjects. They argued, however, that the changes were for the better, providing the subject with a better understanding of himself and of others and enhancing his capacity for self-improvement, for creativity, and for love of others.

\* \* \*

## NOTE 3.

WILLIAM MCGLOTHLIN, SIDNEY COHEN,  
AND MARCELLA S. MCGLOTHLIN  
LONG-LASTING EFFECTS OF LSD ON NORMALS\*

This is a report of a study designed to measure personality, attitude, value, interest, and performance changes resulting from the administration of LSD to normals. Several investigators using LSD with humans in nontherapy experiments have observed that some of their subjects report various lasting effects attributable to the drug experience. In addition, the recent controversy over the nonmedical use of LSD has given rise to numerous claims and counterclaims in this regard. We have previously reported on a pilot study in which tests of anxiety, attitudes, and creativity were given to 15 subjects prior to, and one week following, a single 200 $\mu$ g LSD session. Some significant changes in the anxiety and attitude tests were observed, but none were found for the creativity measures.

The assessment of lasting effects of hallucinogens involves extradrug variables to a greater extent than do most drug studies. We are asking, in effect, whether a dramatic drug-induced experience—one which temporarily dissolves the primacy of habitual perceptions of self-image, environment, beliefs, and values—

\* G. Sjöberg, ed.: *Ethics, Politics and Social Research*. Cambridge, Mass.: Schenkman Publishing Co. 115, 119–120 (1967). Reprinted by permission.

\* 17 *Archives of General Psychiatry* 521–532 (1967). Reprinted by permission.

will have a lasting impact on the individual's personality. We would expect any such impact to be influenced by the person's prior personality, motivation, and expectation, and by the presence of suggestion and reinforcement prior, during, and after the drug experience. In the present study, the subjects volunteered for a paid experiment without prior knowledge of its nature. A large battery of psychological tests was administered prior to a series of three, 200  $\mu$ g LSD sessions, and again at intervals of two weeks and six months following the third session. The hypothesized postdrug personality changes include those most commonly reported in questionnaire evaluations: (1) lower anxiety; (2) attitude and value changes, primarily characterized by greater introspection, less defensiveness, aggression and rigidity, less materialism and competitiveness, and greater tolerance towards others; (3) increased creativity; and (4) enhanced interest and appreciation of music and art.

The subjects were US-born male graduate students who responded to an advertisement for experimental subjects to be paid at the rate of \$2 per hour. The Minnesota Multiphasic Inventory (MMPI) was administered for screening . . . A subsequent interview dealt, in part, with the subject's experience, knowledge, and attitude on LSD and other hallucinogens. During this interview, the subjects were told that the experiment involved the use of drugs and they might or might not receive LSD.

Of the 155 subjects tested and interviewed in December 1964, 12 per cent knew a considerable amount about LSD, 15 per cent had never heard of it and the remainder had only casual knowledge. . . .

\* \* \*

. . . Following the initial administration of the main test battery, each subject received a one-hour interview with the clinical psychologist who attended the drug session. The psychologist attempted to establish rapport with the subject, allay anxiety, assure him that he would be well cared for, and that no surprises, tests, or other demands would be introduced during the drug session. Special effort was made to convey the notion that, for maximum comfort, he should adopt an attitude of relaxing and "going with" the drug effect, *i.e.*, to passively observe the effect without trying to control or direct its course. Questions pertaining to safety of LSD were answered, but no mention was made of possible personality or other changes resulting from the experience. The experiment was double blind

during the preparation and until that point in the drug session at which there were sufficient symptoms to identify the drug given.

. . . Seventy-two subjects participated in the main experiment (mean age 24, range 21 to 35). There were three treatment groups, each with 24 subjects. The experimental group received 200  $\mu$ g LSD, one control group received 20 mg amphetamine (5 mg immediate, and 15 mg sustained release), and the other control group received 25  $\mu$ g LSD. . . .

\* \* \*

After the six-month follow-up testing was completed, a questionnaire was administered which dealt with the subject's own evaluation of the drug experiences and any lasting effects. In a summary evaluation, 14 of the 24 experimental subjects indicated that the drug sessions had produced some lasting effects. . . .

. . . At the six-month testing, 33 per cent of the 200  $\mu$ g LSD group subjectively reported lower anxiety and tension which they attributed to the drug experiences. The comparable percentages for the amphetamine and 25  $\mu$ g LSD groups were 13 and 9. . . .

\* \* \*

. . . The most frequently reported change in the experimental group on the six-month questionnaire was "a greater appreciation of music" (62 per cent). Forty-six per cent responded similarly with respect to art. These subjective evaluations were supported by certain behavioral changes. . . .

\* \* \*

. . . At the six-month testing, 25 per cent of the 200  $\mu$ g LSD group felt that the drug experience had resulted in enhanced creativity in their work, as compared to 9 per cent and 0 per cent for the amphetamine and 25  $\mu$ g LSD groups respectively. . . .

\* \* \*

. . . Of the 24 experimental subjects, the number reporting no effects, moderate, and pronounced lasting effects were 10, 10, and 4. The comparable results for the amphetamine and 25  $\mu$ g LSD groups were 20, 3, 0, and 23, 0, 0 respectively.

\* \* \*

The postdrug results for the personality, attitude, and value tests are generally consistent with the hypothesis, as well as the subjective reports of change, although the amounts of change

are typically quite small. There is some evidence of a more introspective and passive orientation accompanied by a less defensive attitude in the experimental group. The subjective reports of increase in aesthetic appreciation were supported by behavioral activities, but there was no evidence of enhanced performance on the art tests. Similarly, there was no tendency for improvement in the postdrug measures of creativity.

The findings relating personality variables to attitude toward, and response to, the taking of LSD are much more definite. As would be expected, persons who place strong emphasis on structure and control generally have no taste for the experience and tend to respond minimally if exposed. Those who respond intensely tend to prefer a more unstructured, spontaneous, inward-turning (though not socially introverted) life, and score somewhat higher on tests of aesthetic sensitivity and imaginativeness. They also tend to be less aggressive, less competitive, and less conforming.

The above results should be interpreted in the context of the population from which the subjects were drawn. They were graduate students committed to a well-defined goal, and were typically not motivated to take LSD, nor to alter their values or aspirations. They received the drug in a secure aesthetically pleasing setting, but without suggestions of possible lasting effect. Under these conditions, 58 percent of the experimental group subjectively reported some lasting effect after six months. However, attempts to measure these changes via psychological tests provided only minimal supportive evidence.

b.

**William P. Irvin**  
**"Now, Mrs. Blare, About the**  
**Complications . . ."**\*

Mrs. Blare entered, sat down and lit a cigarette. Then she said calmly: "Well, here I am, Doctor. My husband was a little upset until I told him you said it was just a fibroid tumor, just a simple hysterectomy, and there really wasn't much to it."

Dr. Jones winced. What his lawyer had told him made him sorry he'd used those words. "Now, Mrs. Blare, I didn't exactly mean it that way. You see. . ."

"Doctor, what is it? Do I have cancer?"

"No, still fibroids, Mrs. Blare. But a hysterectomy—well, frankly, there are some things that can go wrong. Not often, of course. But I should tell you of the possibilities."

"Oh, I'm not worried, Doctor. But if it'll make you feel better, go ahead and tell me."

"Well, after you're admitted to the hospital, they'll shave you. And occasionally they may nick the skin a little. . . . No, I realize that's not so bad. . . . Yes, I realize you're not the type to get upset over little things. . . . Well, then they'll draw your water. Sometimes this can cause a little inflammation of the bladder. . . . That's right—like you had with your last pregnancy. . . . Well, I know, it took four months, but usually we can cure it much faster. We'd use some of the newer drugs because they don't cause as many reactions. . . . A reaction? Well, you break out in a rash and itch and . . . That's right—like your cousin, John, after he got penicillin. . . . He died? Oh, I didn't know. Mrs. Blare, you're shaking ashes all over my rug."

"Next they'll draw some blood from your arm for tests. . . . Yes, I know you've had it done before. But sometimes you can get a virus infection that causes a little liver reaction. . . . Your friend's husband died, too? Well, most people get better. Of course, it takes years sometimes, and—well, anyway, it doesn't happen often. Mrs. Blare, you look pale. Here, take this pill. That's better."

"Now, at bedtime, they'll give you some drugs to help you rest. . . . Yes, I guess you could get a drug reaction from them, but usually. . . . No, I don't mean that would be your second drug reaction. I mean, you probably wouldn't have any reaction. . . . Yes, I know what I said about the bladder."

"You'll also get a little enema at bedtime. . . . Mrs. Blare what happened to your cousin in Omaha has nothing to do with this case. They won't punch a hole in *your* intestine. . . . Of course, I don't guarantee it. . . . Peritonitis? Well, yes, a hole in the intestine can cause it, but nobody will punch a hole in your intestine. . . . No, I wasn't aware that your brother is a lawyer."

"Well, let's see. Early the next day they'll take you to the operating room, which brings us to the anesthetic. Occasionally, it can cause a little problem. . . . Well, the heart might stop working. . . . Oh, yes, we can start it again. Usually. If we can get it going, it usually keeps on working O.K. Of course, if the brain has been damaged, the patient might not be too bright after surgery. . . . Yes, an idiot, you might say—but really, that doesn't happen often."

\* 40 *Medical Economics* 102-108 (July 29, 1963). Reprinted by permission. © 1963 by Medical Economics, Inc., a subsidiary of Litton Publications, Inc., Division of Litton Industries, Inc., Oradell, N.J.

"Next we open the abdomen and remove the uterus. Of course, once in a while—not often, you understand—but sometimes. . . Mrs. Blare, just because your grandmother said you were born under an unlucky star. . . Now stop shaking. Here, take another pill. . . No, it won't cause a drug reaction. . . I don't think.

"Now, in removing the uterus, we might—on very rare occasions, you understand—get into the bowel. . . I mean we might cut a small hole in the bowel. Sort of like the enema thing, yes. . . Well, what we do is just sew it up. . . Yes, peritonitis is possible.

"If all goes well, and we haven't nicked the ureter. . . Oh, the tube that goes to the Bladder. . . Well, it might cause a fistula and—let's talk about that later. . . Yes, your insurance would cover it.

"Now the uterus is out, and the incision is closed. . . No, we won't sew the bowel up too tight. I mean, we won't touch the bowel. . . Yes, I know what I said before. . . No, I'm not contradicting myself. Now please relax. . . After the surgery you'll be given some fluids through a needle in your vein. . . Well, yes, I guess so. That old virus and the liver again. . . Yes, you mentioned that he died.

"If the wound doesn't break open. . . Well, all your intestines would spill out. . . Oh sure, we'd put them back. . . No, that wouldn't cause idiocy.

"There's only one more thing. Of course, it doesn't happen often. We call it a staph infection. . . Oh, you've read about it in the papers? . . . They all died? But that was in a nursery. . . Well, yes, grownups can die from it, but we have drugs, and. . . Well, a drug reaction isn't usually as bad as a staph infection.

"To sum it all up, Mrs. Blare, a hysterectomy really isn't so simple. Now if you'll just sign this paper that says I've informed you of these little complica—Mrs. Blare! We're not through. Where are you going? Come back, Mrs. Blare!"

## NOTE

PAUL B. BEESON  
MORAL ISSUES IN CLINICAL RESEARCH\*

\* \* \*

. . . I think in regard to the conscience of the physician, etc., one of the eroding things that

does affect us is that physicians are accustomed to the fact that everything they do has a risk; every treatment has a potentiality for harming the patient; consequently the physician is constantly making a judgement whether a thing is more likely to help than to hinder. As a matter of fact we simply could not treat patients if we told them in advance every toxic effect of the treatment or diagnostic procedure we contemplated using in their case. We have to make that decision and we rely on the patient's trust and the fact that he cannot put himself in our place, and we make this decision. So we come to the business of clinical investigation and measure the risk; and the risk may be in our minds small, yet if we were to tell a person of all the possible things that could go wrong in the course of the experiment he probably would not wish to submit to it. This despite the fact, and I think this ought to be pointed out, that it is surprising how willing people are to submit to clinical experiments, to having tests made upon themselves, even when they realize these are tests which are made purely for knowledge and not with the idea of benefiting them directly.

\* \* \*

c.

## Ernst Prelinger

### Safeguarding the Subject's Emotional Well-Being in the Context of Personality Research\*

Consider the case of Dr. X who conducts research on the personality patterns and background factors of college students who have become political activists. Methodologically this research depends heavily on detailed, intensive, and fairly frequent interviews aimed at the subjects' past lives, family relations, ideologies, current experiences and activities, etc. The subject sample is obtained by approaching students who have become quite prominent and well-known activists on the campus and inviting their participation in the study. They are advised that they will be interviewed intensively and warned that the interviews may become upsetting to them. They are instructed, furthermore, that at any time they may terminate their participation in the research. They are also advised that psychotherapeutic referrals will be arranged in case of upset resulting from being in the study or in case that issues raised concerning their personality functioning stimulate their curiosity or interest

\* 36 *Yale Journal of Biology and Medicine* 455, 465 (1964). Reprinted by permission.

\* Unpublished Manuscript (1970). Printed by permission of the author who retains all rights.

in further, personal self-exploration. As far as is known, no potential subject initially refused participation in the study as a result of these warnings; this fact may at least be partly the result of difficulties in anticipating the future impact of intensive interviews. Some subjects seem to have experienced some anxiety upon being thus warned but in at least some instances such anxiety in itself was perceived by them as a challenge to be mastered.

As the study progressed, a substantial proportion of the subjects became sufficiently upset to require referral to a psychotherapist. In most instances their participation in the research was diluted, in some instances terminated at that point. Reasons for this turn of events seem to be several: (1) The subjects, being late adolescents, activist, and also having been sufficiently interested in themselves to have volunteered for research of this kind, may be partly self-selected from a pool of rapidly developing, little settled or consolidated personalities. (2) The interview procedure, which by its very nature was directed toward investigation and probing, may have been lacking in providing supports available in the usual psychotherapeutic situation. (3) There is some evidence that the researcher, despite his own technical qualifications, may have had insufficient awareness of the transferences and transference expectations which are aroused within the subjects while they are being studied. (In a number of instances the subjects became quite demanding of the investigator, asked to live in his household, requested substantial financial loans, etc., and they apparently were indulged in these respects to some extent).

A number of issues, quite aside from the quality of the research project in itself, may be raised: (1) Are there definable conditions and safeguards which justify the conduct of such research altogether? (2) How can one assess the subjects' degree of understanding of the risks they assume by participating in the research? (3) What are the implications of interruptions of the subjects' participation in the research, with the resulting lack of closure of issues raised, whether the interruption is brought about by the investigator or by the subject? (4) What should be the relations between participation in the research and the undertaking of psychotherapy? Relevant here are questions of whether simultaneous participation in both enterprises limits or distorts participation in each one of them. There are issues of the relation between the researcher and the therapist (a special issue concerns the proba-

bly necessary scrutiny, in the therapy, of the subjects' relation with the investigator). (5) Is it necessary to make the subject's willingness possibly to enter psychotherapy part of the initial agreement between researcher and subjects?

#### 4.

#### Disclosure of Manipulation?

a.

#### Kenneth Ring, Kenneth Wallston, and Michael Corey Mode of Debriefing as a Factor Affecting Subjective Reaction to a Milgram-Type Obedience Experiment—An Ethical Inquiry\*

\* \* \*

This study represents an attempt to assess some effects of the Milgram-type experiment in order to provide a substantive basis for evaluating it and other experiments that pose ethical problems. . . .

\* \* \*

[W]e have chosen to vary information that either clearly provides or fails to provide a subject with justification for her performance in the experiment. If, as we expect, offering a subject justification does significantly lower the level of emotional upset induced by participation in Milgram-type experiments, it would seem mandatory to exploit this device as long as we continue to carry out such experiments. . . .

*Subjects.* Fifty-seven female undergraduates enrolled in the introductory psychology course at the University of Connecticut served as subjects in this study. Most had not previously participated in a psychological experiment. All had volunteered, since participation in psychological experiments was not a requirement of the course; however, subjects did receive two points credit toward their final grade for having taken part.

*Procedure.* Although not an exact replication, the procedure followed in this study was designed to duplicate many of the features present in a Milgram-type obedience experiment.

\* \* \*

*First Debriefing.* In the debriefing of experimental subjects, the experimenter explained that the

\* 1 *Representative Research in Social Psychology* 67, 68-85 (1970). Reprinted by permission.



experiment involved a number of deceptions and proceeded to indicate them: (1) the experimenter was not really interested in the problem of how reinforcement works; (2) the other girl was actually an accomplice who (3) did not really receive any auditory stimulation; and (4) what the subject heard was in fact a prerecorded performance made by a drama student. The experimenter apologized for having had to so blatantly deceive the subject and hastened to justify the disguise of the experiment by giving a false though plausible account of its purposes.

The experimenter represented the experiment as a Milgram-type obedience study; specifically, he described it as an experiment dealing with "factors affecting conformity to an authority." After briefly elaborating this notion, he explained that not enough subjects had been run yet to enable him to give an account of how the results of the experiment were turning out; instead he offered to summarize some findings from (fictitious) related research concerned with personality differences between people who persist in the task and those who refuse to continue. Fortunately, subjects always expressed interest in hearing about them.

Subjects assigned to the *obedience justification (OJ)* condition were then told: In general, we find some rather clear-cut personality differences between what we call persistent subjects and those who quit before the experiment is over. Persistent subjects—those who continue in spite of the supposed reaction of the confederate—usually show higher ego-strength on personality inventories and seem better adjusted than those who quit. In other words, persistence in this experiment is usually, though of course not always, indicative of psychological well-being. We also have found that persisters more often seem to act in terms of long-range goals and are less often diverted from them by the pressures of the moment—of course, that's exactly how they behave in the experiment, so perhaps that's not so surprising a finding. Altogether then, persistence in this experiment may be considered a more desirable response than quitting before the experiment is over, though, obviously, we always get both kinds of people in these experiments.

Subjects in the *defiance justification (DJ)* condition, on the other hand, heard the experimenter say:

In general, we find some rather clear-cut personality differences between what we call defiant subjects and those who are fully obedient to the experimenter's instructions. Defiant subjects—those who before the experiment is officially over refuse to continue in spite of the experimenter's urging to do so—usually show higher ego-strength on personality inventories

and seem better adjusted than those who remain obedient. In other words, defiance in this experiment is usually, though of course not always, indicative of psychological well-being. We have also found that defiant subjects are in general less submissive to authority—of course, that's exactly how they behave in the experiment, so perhaps that's not so surprising a finding. Altogether, then, defiance in this experiment may be considered psychologically a more desirable response than complete obedience, though, obviously, we always get both kinds of people in these experiments.

It should be clear that we were attempting, through the use of these descriptions, to provide subjects with a basis for evaluating their behavior during the experiment: obedient subjects hearing the first account and defiant subjects hearing the second should feel, if they didn't already, that they behaved in a socially desirable fashion during the experiment and that their behavior is indicative of psychological well-being; defiant S's receiving the first description and obedient S's receiving the second, however, ought to feel they did not behave as they now wish they had and that they are less well-adjusted than people who performed in the opposite fashion during the experiment. It should perhaps be noted that providing a defiance justification evaluational set for obedient subjects represents an explicit effort to induce that cognitive state of affairs claimed by Baumrind and Kelman to characterize subjects after the experiment is over.

\* \* \*

In the debriefing session for the control group, subjects were led to believe that the experiment was "on the level"—that everything really was as it appeared to be. The experimenter terminated subjects in this *no debriefing (ND)* condition by saying:

Well, that's the experiment, Miss \_\_\_\_\_. Miss (confederate's name), unfortunately, didn't learn the concept, but of course not all our subjects do. She did seem to find some of the latter stimulation painful, but that does happen occasionally. Since I explained the purpose of this experiment at the outset, I don't have much to add here. I wonder whether I could answer any questions you might have?

All questions were answered to uphold the initial cover story given for the experiment. If the subject inquired after the welfare of the learner, the experimenter responded, somewhat blandly, "I'm sure she's all right."

The purpose of this condition was to permit a comparison of the emotional state of subjects who believed that they had actually inflicted



painful stimulation on the learner with that of subjects who had been disabused of that notion by the time an assessment of their emotional state was undertaken. In this way, we could determine the effects of a subjectively real performance in the experiment, uncontaminated by post-performance, experimenter-induced bases of evaluation.

Once this stage in the debriefing session had been reached for all subjects, the experimenter introduced a questionnaire designed to elicit the subject's reactions to the experiment. . . .

\* \* \*

Once the subject had finished the questionnaire and sealed it in the envelope given to her, the experimenter (who had been sitting in the opposite corner of the room to encourage freedom of response) asked the subject to get her things and accompany him upstairs. After giving her envelope to a departmental secretary, she was escorted to the office of the senior author, who gave her a full and truthful debriefing.

*Second Debriefing.* Following Milgram's procedure, the second debriefing was varied depending on the subject's reaction to the experiment (all subjects had been observed through the one-way glass by the senior author) and whether she had been assigned to an experimental or control condition. Whenever a subject appeared quite upset, the experimenter's initial efforts were directed toward calming her down by whatever means seemed most appropriate and likely to be effective.

\* \* \*

After the deceptions of the first debriefing had been indicated to the experimental subjects, the experimenter was able once again to follow a fairly standard sequence, description in varying detail depending on a subject's interest and mood, the background of the experiment, related research, and questions the present experiment was intended to answer. Subjects were encouraged to interrupt at any time to ask questions or comment.

Next (if they hadn't already been indicated) subjects were asked to express their reactions to the experiment and given a chance to "air their feelings."

The experimenter then endeavored to enlist the subject's cooperation in agreeing not to talk about the experiment with anyone until it was completed. He promised to (and did) send to each subject a written report of the major find-

ings of the experiment and, after thanking them again for their participation, mentioned that if they had any second thoughts about the experiment they should please return to his office to discuss them. (None ever did.)

Throughout this debriefing session, the experimenter sought to encourage subjects to feel they could speak freely and attempted to give a considerate and complete response to all comments and questions. The sessions themselves were of variable length, running from about 20 minutes to nearly an hour. No formal duration records were kept, but an approximate average for these debriefings would be 25 to 30 minutes.

*Experimental Design and Hypothesis.* . . . Of the 57 subjects in the experiment, 15 were eliminated from the analyses for the following reasons: five subjects defied the experimenter; six were suspicious in varying degrees concerning one or more aspects of the procedure; three indicated by their responses to the post-experimental form that they either did not grasp, believe, or remember the gist of what the first experimenter had told them concerning the desirability of obedience or defiance, and one subject was randomly eliminated from the *ND* condition to equilibrate *n*'s. Of the 42 subjects who remained, 14 were allocated to each condition; and in each condition, two experimenters ran exactly half the subjects.

Although in itself of only secondary interest, our hypothesis was that subjects in the *ND* condition would rate themselves as most upset, those in the *OJ* condition least upset, while *DJ* subjects would fall somewhere in between. The rationale underlying this hypothesis was simply that it would be somewhat comforting to know one was not really causing the learner to suffer pain and more comforting still to be given justification for one's experimentally demonstrated willingness to inflict what one thought was pain-eliciting stimuli. . . .

*Followup interviews.* Two to five weeks after they had participated in the experiment, 20 S's were selected (by a procedure neither random nor systematic) for followup interviews. The interviewer, who contacted subjects by phone, explained both over the phone and in the interview room that she was conducting an opinion survey concerning psychological experiments as part of the department's research assessment program. Subjects were told they had been chosen randomly and they should feel free to express their real opinions. They were told that the interview

would be recorded but that they would be identified by a number that would protect their anonymity.

\* \* \*

The subject was first asked to give a brief description of each experiment she had served in during the semester. If she had been only in the debriefing experiment, there was, of course, no problem in directing her attention to that experiment. If she had taken part in more than one, however, the interviewer, as if she herself had no preference, asked the subject to respond to the questions with the debriefing experiment in mind ("Suppose we start with, oh say, that concept-learning experiment you mentioned.").

### Results

*Behavior in the Experimental Setting.* [O]f the 57 subjects taking part in the experiment, 52 (91 percent) were fully obedient. . . .

While there was nearly complete ultimate conformity to the experimenter's authority, the manner of obedient subjects' response was more variable. Some subjects persevered in their task with little overt show of emotion; others appeared extremely distressed. . . .

\* \* \*

Of the five subjects classified as defiant, four simply refused to continue while one was so visibly upset that the experiment had to be halted.

\* \* \*

On the basis of subjects' self-assessments of their emotional states, it is clear that the experimental procedure did generate substantial negative affect. Although there was some variation among the first ten items, the usual pattern was that subjects in the *OJ* condition tended to be somewhat less bothered by their experiences than were subjects in the *DJ* and *ND* conditions. These data suggest that the mode of debriefing received by a subject does make a difference: a significant reduction in emotional tension can be achieved by providing a subject with justification for her behavior; if she is (explicitly) led to think badly of her actions, however, the residual emotional tension is as great as if she believes she has actually harmed someone.

These general trends should not obscure differential treatment effects on specific emotional states. For example, the direction as well as the strength of a subject's anger is a function of her

experimental treatment: *OJ* subjects tended not to be very angry either at themselves or at the experimenter; *DJ* subjects were angrier, but almost all their anger was focused on themselves; *ND* subjects were angrier still, but both at themselves and at the experimenter.

In view of these findings, perhaps the most intriguing data . . . deal with subjects' evaluations of the experiment as distinguished from their emotional reactions to it. As long as they were informed of the deceptions involved, their evaluation of the experiment was prevailingly and unmistakably positive. For the most part, these subjects indicated that they enjoyed the experiment, would be willing to participate in others like it, and found it an instructive and rewarding experience. Most notable of all, in our judgment, is that virtually none of these subjects resented being deceived, regretted being in the experiment, or thought that it involved anything unethical or should be discontinued.

It appears that even a cursory debriefing is sufficient to induce a marked discrepancy between how a subject reacts to an experiment and how she later comes to evaluate it. This discontinuity of affect and evaluation accords perfectly with Milgram's accounts which make clear that while many subjects are upset by the experimental procedure itself, they value the experiment and their participation. We fail to observe this pattern only for *ND* subjects whose evaluation of the experiment was consistently more negative; this is not surprising, of course, since at the time of their evaluation they still believe they had inflicted considerable pain on the learner.

*Behavior during the Second Debriefing.* Although many experimental subjects were greatly relieved by what they had learned from their first, partially false debriefing, some of them and most of the control group subjects were still quite agitated at the time they were given their second, truthful debriefing. Most of the subjects who arrived still feeling a little shaken were visibly calmer as soon as the real purposes of the experiment had been disclosed and usually evinced strong and sincere interest in learning more about the experiment. Most subjects responded remarkably well and with considerable good-humor on being informed of the deception-within-a-deception design of the experiment, and many displayed a lively sympathy with the objectives of the research. By the end of the debriefing, almost all subjects seemed emotionally intact and left with a clear understanding of what they had undergone that day and why.

Nevertheless, two cautionary remarks should be inserted here. The description given above is based entirely on the senior author's global impressions which may well be distorted both by the passage of time and his particular biases. No attempt was made to assess in any objective way a subject's emotional state after the second debriefing. In addition, as the foregoing account implied, there were a few who, even though they fully understood the purposes of the experiment and said that they were not resentful over what had happened, left the second debriefing still apparently somewhat upset, or at least not entirely free of their emotional tension. To determine the accuracy of the impressionistic observations made of the subjects during their second debriefing as well as to evaluate possible long-term negative aftereffects, we need to draw on the findings from the followup interviews.

*Results of Followup Interviews.* Subjects' responses to the interviewer's questions—many of which were the same as or similar to those that appeared on the post-experimental questionnaire—were in the main consistent with those they had expressed earlier: by and large, they regarded their experimental experience positively.

The modal subject indicated that (1) while she did not enjoy participating in the experiment at the time, (2) she did feel a lot better about it once it had been explained to her; (3) she was satisfied with the explanation she had received and (4) was grateful for it; (5) she felt she had learned something from the experiment, (6) did not regret being in it or (7) feel angry that she had been deceived; (8) she did not think any ethical standards were violated, or (9) that she was required to do anything she should not have been asked to do. . . .

Although the interview data that we collected are obviously not conclusive, we can say that they afford no evidence to indicate that there were any serious long-term aftereffects from this experiment. To affirm this, however, is not to deny that there were some significant negative consequences for varying numbers of subjects.

The most common of them was the view expressed by approximately half the interviewed subjects that they would now be more suspicious of psychological experiments and more wary about being deceived. In a number of instances, however, these subjects had subsequently been in other deception experiments which apparently served to reinforce their mistrust. Several subjects stated that they did not like the idea of de-

ception per se, especially the deception that was foisted during the first debriefing and implied that the deception was more disturbing than the requirements of the experimental task itself.

Seven subjects approximately one-third of those interviewed, did indicate that they had been bothered by what they had done during the experiment. Some of these subjects commented that they had been disappointed or angry with themselves afterward for their behavior—a residual emotional state that was not entirely dissipated by the second debriefing. Others mentioned self-doubts. Several suggested that while they themselves were not that deeply affected by their experimental experience, they knew certain other people who would be and that "they wouldn't have been able to take it." One subject had clearly the most negative reaction to the experiment; when asked whether she regretted having been in the study, she replied:

It made me rather upset . . . for at least the rest of the day, if not the rest of the week. I felt very sick. I mean, I knew I hadn't done anything really, you know, to involve anyone getting pain, but the fact that, you know, I thought at the very beginning that I might have caused somebody pain and this was just getting me sick and even if they told me at the end that I wasn't doing anything to any other person, it still, you know, I had gotten past the point of being able to rationalize giving someone else pain.

\* \* \*

### Discussion

Taken as a whole, the data from this study fail to substantiate the charge that there are likely to be widespread and persistent negative aftereffects from Milgram-type obedience experiments. . . .

There was, however, one objection to Milgram-type experiments on which we were able to collect some supporting evidence. It will be recalled that Baumrind claimed that such experiments were potentially harmful because they might affect a subject's ability to trust adult authorities in the future. Our followup interview data suggested that, in fact, many subjects were experiencing such difficulties, even though there was no necessary concomitant decrease in their interest in serving as subjects in psychological experiments.

\* \* \*

Although the findings of this experiment demonstrate that one kind of allegation against the Milgram-type obedience experiment—that

based on its effects on subjects—is almost entirely without empirical foundation, they do not of course rule out other criticisms of this variety of experimentation. Indeed, it would be possible to utilize some of the data from the present experiment to argue against such research. For example, one could contend that:

1. Since a substantial number of subjects are upset during the experiment (18 of the 42 subjects retained for the principal analyses rated themselves 75 or higher on the "upset" scale), that in itself should give us pause, regardless of the effects of subsequent debriefings.

2. It is beside the point that only a small minority of subjects seem to be adversely, even if not severely, affected by the experiment; we should not conduct any experiments giving rise to such effects, however slight they may be and regardless of the number of people involved.

3. Whatever the ethical implications of such experiments, they raise serious methodological problems; for example, increasing subjects' suspicions about other experiments they might later be in.

4. Even though subjects themselves may not regret having taken part in such experiments or think that they entail any ethical violations, it is clear that they have in fact been made to act in a degrading way and that we should not conduct experiments that induce this kind of behavior, whatever subjects' own interpretation of it may be.

\* \* \*

#### NOTE

PAUL ERRERA

STATEMENT BASED ON INTERVIEWS WITH  
FORTY "WORST CASES" IN THE MILGRAM  
OBEDIENCE EXPERIMENTS\*

An attempt was made to evaluate whether subjects involved in Dr. Milgram's "Obedience to Authority" experiment had any harmful reactions as a result of their experience. Single 50 minute interviews were held with a selected sample of the population—individual and group sessions, some twelve months after the termination of the study.

Not enclosed in this report is a description of the selection process, a description of the population seen as compared to the overall sample, and an accounting of those who did not keep their return appointments.

The subjects seen responded to our invitation with obvious interest. Most of them participated actively in the discussions. None were

found by this interviewer to show signs of having been harmed by their experience.

The largest number claimed to have enjoyed participating in the project. Some reported marked anxiety and 3 described extreme stress at the time of the experiment. The reported distress was mostly relieved within a few days and completely dispelled after receiving the letter Dr. Milgram sent to each participant describing the purpose of the project and some preliminary findings.

The subjects seem to have handled their experience in a variety of ways. Some presented the task as a routine one void of any stress. They were asked to do something; they did it to the best of their ability. Others described the concerns they had in proceeding with the experiment. They expressed anger at the professor or at the institution responsible for the experiment; they became annoyed at the student for not learning and held him responsible for what happened—a few offered to switch roles with him.

Each subject seemed to handle his task in a manner consistent with well-established patterns of behavior. No evidence was found of any traumatic reactions. Those who had been the angriest appeared to be chronically angry at their environment—ever ready to attack available authority figures.

A few accepted responsibility for their actions and described their distress when faced with their willingness to inflict pain on another human being. They felt that as a result of the experiment they had learned something valuable about themselves.

b.

**Elaine Walster, Ellen Bersheid,  
Darcy Abrahams, and Vera Aronson  
Effectiveness of Debriefing  
Following Deception Experiments\***

\* \* \*

Deception experiments differ so greatly from one another in the nature and degree of deception used that even the harshest critic of this technique would be hard pressed to state unequivocally that all deception has potentially harmful effects. There are, however, two frequently mentioned dangers of deception experiments, to which some experiments are more liable than others. First, some critics have voiced

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\* 6 *Journal of Personality and Social Psychology* 371, 372-380 (1967). Reprinted by permission.

their concern that lying to people may lead them to lose faith in their fellow human beings. Because scientists are ordinarily highly respected, the discovery that a scientist will lie might upset subjects even more than lies told by others. Secondly, and perhaps more importantly, it has been pointed out that some deception manipulations are emotionally disturbing to a subject, and that some disturbances might not be entirely amenable by debriefing. . . .

Under what conditions might we expect debriefing to fail? Suppose that a subject is told that his test results indicate that he is not very creative. In fact, the experimenter reports, few people tested have ever scored so low on creativity. Further suppose that this subject happened to be a budding poet who picked up his mail on his way to the experimental session and discovered the fourteenth publisher's rejection slip for his first serious effort. While pursuing his way to the experiment, the subject might quite naturally wonder if the series of rejections should be attributed to his lack of talent or to the possibility that the uncultured masses are not clamoring for sonnets about the Crimean War.

It seems quite possible that the experimenter's authoritative evaluation of the subject's creative talents would initiate in this particular subject some independent thinking during the course of the experiment. It is quite likely, for example, that this subject would try to reach some consistency between the content of the experimenter's report and his own original ideas about his level of creativity. To do this, he might well reexamine his self-concept and selectively recall past incidents, most notably the 14 rejection slips, which would agree with the experimenter's information. Memories of criticisms from friends and family, the recollection of some low grades in English composition, would, when interpreted in the light of the test results, strengthen his belief in his supposed low creativity. Consequently, at some point in the course of the experiment, the subject might decide that since his own cognitions augment the experimental evidence, what the experimenter said was true. He might even come to the conclusion that he himself had been imperceptive not to realize his lack of creativity before.

At the completion of the experiment, of course, the subject-poet would be informed that the negative evaluation he received was chosen at random, and it would be explained that it was just as likely that he could have received a neutral or favorable evaluation of his creative tal-

ents. As previously mentioned, there is ordinarily little reason for the subject not to accept completely the notion that he has been deceived, that he is not the unimaginative dullard he thought he was, and be none the worse for wear. It even seems quite probable that the subject might believe the debriefing message in its entirety, that is, that the creativity test was not genuine. In this case, however, his own supporting and freshly organized cognitions might remain. It is still true that he *has* gotten 14 consecutive rejection slips and that he *did* receive those low grades. Consequently, it is possible that, though the specific anxieties produced by the deception might be completely removed by the debriefing, his general opinion about himself might well be lowered, and his life turned upon a new course, because of the extra thinking the manipulation initiated.

When the deception happens to strike an area of deep concern and worry to the individual, when it is likely to initiate a train of thought which would not be altered by the revelation of the deception, it is possible that the damage done to a subject by the deception might be irreversible.

The authors will report in this paper an experiment which tests the hypothesis that it will be more difficult to successfully debrief (i.e., return to his preexperimental state) a subject who has received false information on some aspect of himself about which he is currently concerned, than it will be to debrief a subject who has received information which is irrelevant to his current concerns. While the preceding discussion focussed on the possible residual effects of receiving negative information, a parallel result could be expected to occur when the subject has received positive information. That is, it might also be more difficult to debrief someone who receives positive information in an area of current concern. This experiment, then, was designed to test for the existence of both positive and negative residual effects after debriefing.

\* \* \*

The 80 subjects who participated in this study were freshmen and sophomore women enrolled at the University of Minnesota. Subjects were recruited from an introductory psychology course and from the university library. All subjects had taken the MMPI as part of the freshman testing program.

In an initial contact, subjects agreed to participate in two separate experiments. They were

told that the first experiment would have an hour delay between the first and second part. Thus, a second experiment had been scheduled during this hour for their convenience. They were told they could either participate in this experiment or not as they chose. In fact, all subjects chose to participate in both experiments. In reality, of course, the two experiments were both parts of the same experiment.

The purpose of Experiment 1 was twofold: (a) to *measure* the subject's preexperimental concern about the kind of social impression she makes, and (b) to randomly assign the subject to an experimental group and *manipulate* her concern about the kind of social impression she makes. We wanted half of our subjects to be highly concerned and curious about their social abilities and the other half to be little concerned about these abilities.

\* \* \*

The purpose of Experiment 2 was to lead one-half of the unconcerned and one-half of the concerned subjects to believe for the duration of Experiment 2 that they possessed good social skills, and the remainder of the subjects to believe that they possessed poor social skills. . . .

\* \* \*

Subjects were debriefed at great length after all of these measures were collected. Since subjects viewed this experiment as an effort to find out the extent to which deception might or might not be harmful to them, virtually all of them indicated that they were happy to have participated.

\* \* \*

It was hypothesized that it would be more difficult to debrief the high-concern subjects than low-concern subjects. Thus, we expected high-concern subjects who were told they possessed good skills in Experiment 2 to overestimate their performance, even after debriefing, to a greater extent than would those low-concern subjects who were also told they possessed good social skills. Similarly, we expected high-concern subjects who were told they possessed poor social skills to underestimate their social skills, even after debriefing, to a greater extent than low-concern subjects.

\* \* \*

From [our data] it is clear that regardless of whether we deal with manipulated concern or selected concern, and regardless of whether we

consider self-estimates on the sociability index or on the interview performance index, high-concern subjects do *not* seem to be more difficult to debrief than low-concern subjects. Selected concern and type of deception do not interact in affecting subjects' estimates of their sociability . . . or of their interview performance . . . as we predicted they would. . . .

The data obtained from this experiment lead us to believe that our hypothesis is incorrect. This belief is further strengthened by . . . a very lengthy pilot study conducted by Abrahams (1967); the hypothesis of this paper was tested with only minor variations. (Ostensibly the Abrahams study was concerned with "problem solving ability" and "creativity" rather than with "interview performance" and "sociability.") The data from Abrahams' experiment also failed to provide any evidence that highly concerned subjects are more difficult to debrief than unconcerned subjects.

\* \* \*

Although we feel that our hypothesis is incorrect, this does not mean that we have concluded that debriefing is uniformly effective with all subjects. Our data indicate that subjects in the various conditions do exhibit lingering aftereffects of the deception, although they are not the effects we predicted.

Let us first consider the estimates subjects made of their own sociability immediately after having been debriefed. For all subjects, it appears that the personality report had an impact which lasted at least for a few moments beyond the occurrence of debriefing. Regardless of whether concern was manipulated or selected, and regardless of level of concern, subjects who were given favorable sociability reports rated themselves significantly higher on the sociability index than did those subjects who received an unfavorable personality report. . . . Since the type of sociability report a girl received was determined by chance, we must assume these differences are due to the fact that the subjects were not entirely disabused of the information they received in Experiment 2.

\* \* \*

In addition to the aftereffects previously discussed, there is one additional finding that seems worth commenting upon; though our manipulated-concern measure seemed to have little impact upon the success or failure of debriefing, there is some evidence that selected concern may be of importance.

From [our data] it is clear that a subject's initial degree of concern (with her social abilities) has a marked effect on her post-debriefing estimate of her interview performance.

When we consider only the data from *selected high-concern* subjects, we see that even after the passage of time, debriefing does not seem to be totally effective. Selected high-concern subjects who received good sociability reports in Experiment 2, even after being told these reports were false, estimated that they did better in the interview situation than did subjects who received poor sociability reports. *Selected low-concern* subjects who received good sociability reports estimated their interview performance very much as did high-concern subjects who received the good report. However, the interview performance estimates of the low-concern subjects who received a poor sociability report are markedly different from the estimates of comparable high-concern subjects. These low-concern subjects guessed they did *better* in the interview situation than did subjects in any other group. This finding is peculiar.

\* \* \*

If low concern does in fact reflect high self-esteem, perhaps we can find an explanation for these findings. The apparent failure of debriefing for high-concern subjects is disturbing, but comprehensible. What is peculiar is the high performance estimates made by low-concern subjects who were given low personality results. Perhaps this simply demonstrates that high self-esteem individuals are especially likely to reject unpleasant information about themselves. . . .

\* \* \*

We conclude two things from the preceding study:

1. The question of whether or not it is more difficult to successfully debrief concerned subjects than unconcerned subjects of information relevant to their current concerns remains unanswered. We can only say that two lengthy attempts to demonstrate this effect have been unsuccessful. Whether or not a stronger concern manipulation would produce the effect is, of course, a moot question, but we have been unable to produce evidence of even a slight tendency for subjects to behave in the predicted manner.

2. We have presented evidence that debriefing might not be as immediately effective as experimenters have hoped and assumed. This evidence is distressing for a number of reasons. First of all, it is disturbing that in the present experi-

ment and in the Abrahams experiment, even after a very lengthy and thorough debriefing (probably atypical in thoroughness), subjects still behaved to some extent as though the debriefing had not taken place. Subjects behaved in this manner even though they had voiced to the experimenter their understanding that the manipulation was false, their understanding of the true purpose of the experiment, and even though, by their manner and replies, the experimenter had been satisfied that they did indeed understand the nature of the deception.

Even more disturbing is the evidence that the aftereffects of debriefing might be complex, unpredictable, and may depend in part upon the personality traits of the subjects. The nature of the effect of personality traits in the present experiment was not totally explicable to us. Aftereffects in the Abrahams experiment were also present and somewhat inexplicable. The success of debriefing in that experiment was influenced by several significant interactions between sex of subject, sex of experimenter, and treatments. At the time the Abrahams experiment was run, we were willing to conclude that the significant interactions obtained were perhaps due to chance, to experimental error, to measurement error, and so on. The results of the present experiment, however, combined with the results of the Abrahams experiment, have aroused our suspicions and anxiety that there are often residual effects of debriefing, and that these effects appear to be complex and not easily interpreted. . . .

## NOTE

DIANA BAUMRIND

PRINCIPLES OF ETHICAL CONDUCT  
IN THE TREATMENT OF SUBJECTS\*

\* \* \*

My own belief . . . is that subjects are less adversely affected by physical pain or psychological stress than they are by experiences which result in loss of trust in themselves and the investigator, and by extension in the meaningfulness of life itself. College students, who are the most frequently used subject pool, are particularly susceptible to conditions which produce an experience of anomie. My secretary, when typing an earlier version of this paper, described to me an incident which illustrates the way in which

\* 26 *American Psychologist* 887, 888-889 (1971). © 1971, American Psychological Association. Reprinted by permission.



deception can contribute to the feeling of anomie in young people. She recounts the incident, which she remembers vividly although it occurred 8 years ago, as follows:

When I was 18, a sophomore in college, a psychologist from a nearby clinic came to my dormitory one evening and explained that he was looking for subjects for an experiment which involved simply telling stories about pictures which would be shown us. This sounded interesting, so I signed up. At the interview the same psychologist introduced me to a girl a few years my senior, who stayed bland and noncommittal throughout the time she interviewed me. She showed me a few pictures, and since they were extremely uninteresting I felt that the stories I was making up must be very poor. But she stopped at that point and told me that I was doing very well. I was gratified and said something to that effect before we went on to the rest of the pictures. Then I filled out a form about my reactions to the interview, the experimenter, etc., and she took it and left. After being alone for a few minutes I looked around the office and noticed a list of the last names of subjects, with "favorable" and "unfavorable" written alternately after each one. Shortly thereafter the male psychologist returned and said that, as I had guessed, what the interviewer had said had nothing to do with my performance. They were testing the effects of praise and dispraise on creative production, and he said so far they had discovered that dispraise had negative effects and praise seemed to have none at all. Since I expressed interest, he promised that the subjects would be given full results when they were tabulated (but we never heard from him).

My reaction to the experiment at the time was mixed. I assumed that the deception was necessary to get the proper reaction from me, and that since I had behaved unsuspiciously the results of the experiment were valid. However, I was embarrassed at having been manipulated into feeling pride at a non-achievement and gratification at praise I didn't deserve. Nevertheless, I felt that it was "right" that I was embarrassed, since I had always been taught a "pride goeth before a fall" philosophy of achievement. I had been an underachiever in school until just a few years previous to this experiment. Since in my early years in school I had alternated between being praised for doing well and being damned for doing *too* well, I had always been a poor judge of my own achievements and had no internal standards for evaluating my performance—although I knew I was very intelligent and felt that some sort of moral flaw kept me from doing as well as I might. At the time I was attending a very inferior college and felt (rightly) that my grades had nothing to do with how well I was really doing relative to my *ability*. This experiment confirmed my conviction that standards were completely arbitrary. Furthermore, for several years I had followed a pattern of achievement which it took me another 5 years to get free of: I would go along for quite a while doing well in classes, in-

terpersonal relations, etc. Then I would have a moment of hubris in which I was more self-confident or egotistical than it behooved me to be in that situation. At this point someone would cut me down to size; I would be totally devastated, and it would take me a long time to work myself up to my previous level of performance. The experiment had, in a lesser degree, the same effect upon me, and it may have served to confirm me in this pattern because the devastating blow was struck by a psychologist, whose competence to judge behavior I had never doubted before.

\* \* \*

## 5.

### Benefits to Subjects?

a.

#### Stanley Milgram Issues in the Study of Obedience— A Reply to Baumrind\*

\* \* \*

[T]here can be . . . an important positive side to participation [in experimentation]. Baumrind suggests that subjects derived no benefit from being in the obedience study, but this is false. By their statements and actions, subjects indicated that they had learned a good deal, and many felt gratified to have taken part in scientific research they considered to be of significance. A year after his participation one subject wrote:

This experiment has strengthened my belief that man should avoid harm to his fellow man even at the risk of violating authority.

Another stated:

To me, the experiment pointed up . . . the extent to which each individual should have or discover firm ground on which to base his decisions, no matter how trivial they appear to be. I think people should think more deeply about themselves and their relation to their world and to other people. If this experiment serves to jar people out of complacency, it will have served its end.

\* \* \*

A concern with human dignity is based on a respect for a man's potential to act morally. Baumrind feels that the experimenter *made* the subject shock the victim. This conception is alien to my view. The experimenter tells the subject to

\* 19 *American Psychologist* 848, 850-852 (1964). © 1964, American Psychological Association. Reprinted by permission.



do something. But between the command and the outcome there is a paramount force, the acting person who may obey or disobey. I started with the belief that every person who came to the laboratory was free to accept or to reject the dictates of authority. This view sustains a conception of human dignity insofar as it sees in each man a capacity for *choosing* his own behavior. And as it turned out, many subjects did, indeed, choose to reject the experimenter's commands, providing a powerful affirmation of human ideals.

\* \* \*

If there is a moral to be learned from the obedience study, it is that every man must be responsible for his own actions. This author accepts full responsibility for the design and execution of the study. Some people may feel it should not have been done. I disagree and accept the burden of their judgment.

Baumrind's judgment, someone has said, not only represents a personal conviction, but also reflects a cleavage in American psychology between those whose primary concern is with *helping* people and those who are interested mainly in *learning* about people. I see little value in perpetuating diverse forces in psychology when there is so much to learn from every side. A schism may exist, but it does not correspond to the true ideals of the discipline. The psychologist intent on healing knows that his power to help rests on knowledge; he is aware that a scientific grasp of all aspects of life is essential for his work, and is in itself a worthy human aspiration. At the same time, the laboratory psychologist senses his work will lead to human betterment, not only because enlightenment is more dignified than ignorance, but because new knowledge is pregnant with humane consequences.

#### NOTES

##### NOTE 1.

ALAN C. ELMS  
SOCIAL PSYCHOLOGY AND SOCIAL  
RELEVANCE\*

\* \* \*

Not only can one justify imposing certain unpleasant experiences on psychological research participants; one might even argue that the experience these people undergo can sometimes be a moral good in itself. As I've noted, Milgram

did not falsely attribute any despicable qualities to his volunteers, as has occurred in a few studies; it happens to be quite true that the obedient volunteers were willing to shock innocent human beings upon command and each volunteer proved this to himself. Should we instead leave people to their moral inertia, or their grave moral laxity, so as not to disturb their privacy? Who is willing to justify privacy on this basis? Who would have done so, with foreknowledge of the results, in pre-Nazi Germany? Do we not try to wake our friends, our students, our followers or leaders from moral sloth when it becomes apparent, and are we bound to use our weakest appeals when we do so? Who now condemns the Old Testament prophets for having tried to arouse people to the evil within themselves? Milgram doesn't claim prophetic stature, but his experiments may similarly awaken some of the people involved. It's true that these people didn't ask to be shown their sinful tendencies; but people rarely do. That's why ministers lure people with church social functions, why writers clothe their hard moral lessons in pretty words and stories, why concerned artists . . . blend morality and estheticism: because people prefer not to face the truth about themselves if they can avoid it. I have heard the other side of this argument, come to think of it: the argument that a certain group of people doesn't want to be educated, that maybe they'd prefer to remain in happy ignorance, and therefore should be left to their familiar pattern of life. Yassuh, massa.

The thrust of such arguments in Milgram's case is that he was simply too effective in bringing volunteers into dramatic confrontation with their own conflicting moral trends and their own weaknesses. We don't hear the same complaints about other psychological studies, or about most public speakers or writers or teachers or preachers, because they seldom move their audiences enough to make complaints worthwhile. Plenty of ministers, I am sure, would be ecstatic over the possibility of giving their congregations such a harrowing contact with their own immoral inclinations as Milgram has done, and would feel the process producing this experience to be truly heaven-sent. (In fact, one doctor of divinity who was a research volunteer asked Milgram afterwards whether he would put some of the good reverend's divinity students through the procedure, and let the good reverend in on the results. Milgram, feeling a bit of doubt as to the ethics of such a procedure, said no.)

\* \* \*

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## NOTE 2.

HERBERT C. KELMAN  
HUMAN USE OF HUMAN SUBJECTS—  
THE PROBLEM OF DECEPTION IN SOCIAL  
PSYCHOLOGICAL EXPERIMENTS\*

. . . If we do use deception, it is essential that we find ways of counteracting and minimizing its negative effects. Sensitizing the apprentice researcher to this necessity is at least as fundamental as any other part of research training.

In those experiments in which deception carries the potential of harmful effects (in the more usual sense of the term), there is an obvious requirement to build protections into every phase of the process. Subjects must be selected in a way that will exclude individuals who are especially vulnerable; the potentially harmful manipulation (such as the induction of stress) must be kept at a moderate level of intensity; the experimenter must be sensitive to danger signals in the reactions of his subjects and be prepared to deal with crises when they arise; and, at the conclusion of the session, the experimenter must take time not only to reassure the subject, but also to help him work through his feelings about the experience to whatever degree may be required. In general, the principle that a subject ought not to leave the laboratory with greater anxiety or lower self-esteem than he came with is a good one to follow. I would go beyond it to argue that the subject should in some positive way be enriched by the experience, that is, he should come away from it with the feeling that he has learned something, understood something, or grown in some way. This, of course, adds special importance to the kind of feedback that is given to the subject at the end of the experimental session.

\* \* \*

b.

**Commonwealth v. Wiseman**  
**350 Mass. 251, 249 N.E.2d 610 (1969)**

CUTTER, Justice.

This bill seeks, among other relief, to enjoin all showings of a film entitled "Titicut Follies," containing scenes at Massachusetts Correctional Institution at Bridgewater (Bridgewater), to which insane persons charged with

crime and defective delinquents may be committed. . . . Mr. Wiseman and Bridgewater Film Company, Inc. (BFC) appeal from an interlocutory decree, an order for a decree, and the final decree which enjoins showing the film "to any audience" and requires Mr. Wiseman and BFC to deliver up to the attorney general for destruction specified films, negatives, and sound tapes. The plaintiffs appeal from the final decree. . . .

\* \* \*

The film shows many inmates in situations which would be degrading to a person of normal mentality and sensitivity. Although to a casual observer most of the inmates portrayed make little or no specific individual impression, others are shown in close-up pictures. These inmates are sufficiently clearly exhibited (in some instances naked) to enable acquaintances to identify them. Many display distressing mental symptoms. There is a collective, indecent intrusion into the most private aspects of the lives of these unfortunate persons in the Commonwealth's custody.

\* \* \*

These considerations, taken with the failure of Mr. Wiseman to comply with the contractual condition that he obtain valid releases from all persons portrayed in the film, amply justify granting injunctive relief to the Commonwealth. The impracticality of affording relief to the inmates individually also supports granting this collective relief to the Commonwealth as *parens patriae*, in the interest of all the affected inmates. . . .

The defendants contend that no asserted interest of privacy may be protected from the publication of this film because the conditions at Bridgewater are matters of continuing public concern, as this court has recognized. . . .

Even an adequate presentation to the public of conditions at Bridgewater, however, would not necessitate the inclusion of some episodes shown in the film, nor would it justify . . . the depiction of identifiable inmates, who had not given valid written consents and releases, naked or in other embarrassing situations. We agree with the trial judge that Mr. Wiseman's wide ranging photography amounted to "abuse of the privilege he was given to make a film" and a serious failure to comply with conditions reasonably imposed upon him. Mr. Wiseman could hardly have fairly believed that officials, solicitous about

\* 67 *Psychological Bulletin* 1, 8 (1967). © 1967, American Psychological Association. Reprinted by permission.

obtaining consent and releases from all inmates portrayed, could have been expected to approve this type of film for general distribution.

\* \* \*

That injunctive relief may be granted against showing the film to the general public on a commercial basis does not mean that all showings of the film must be prevented. . . . [T]he film gives a striking picture of life at Bridgewater and of the problems affecting treatment at that or any similar institution. It is a film which would be instructive to legislators, judges, lawyers, sociologists, social workers, doctors, psychiatrists, students in these or related fields, and organizations dealing with the social problems of custodial care and mental infirmity. The public interest in having such persons informed about Bridgewater, in our opinion, outweighs any countervailing interests of the inmates and of the Commonwealth (as *parens patriae*) in anonymity and privacy.

The effect upon inmates of showing the film to persons with a serious interest in rehabilitation, and with potential capacity to be helpful, is likely to be very different from the effect of its exhibition merely to satisfy general public curiosity. There is possibility that showings to specialized audiences may be of benefit to the public interest, to the inmates themselves, and to the conduct of an important state institution. Because of the character of such audiences, the likelihood of humiliation, even of identifiable inmates, is greatly reduced. In any event the likelihood of harm seems to us less than the probability of benefits.

\* \* \*

C.

#### Jane E. Brody Daring Is Urged in Cancer Cases\*

The president of the American Cancer Society urged here today that scientists take more chances in applying experimental techniques to the treatment of human cancer.

Dr. Leonard W. Larson, noted that "when some of the present laboratory practices become clinical routine," the cancer death rate, now 800 Americans a day—can be expected to decline considerably.

\* \* \*

\* *The New York Times* 18, col. 3 (March 26, 1966). © 1966 by The New York Times Company. Reprinted by permission.

Dr. Larson emphasized that "we cannot be satisfied with the status quo in cancer therapy, which is saving one patient in three with the most obvious and easily cured cancers."

He expressed considerable impatience with the slow rate at which laboratory research methods were becoming clinically useful.

In an interview following his speech, Dr. Larson explained that the main reason for this time lag was scientists' fear of being accused of experimenting on humans.

Also contributing to the lag, he said, "is the reluctance of experimenters to take a chance. I realize there are dangers, I realize there are laws. But I think scientists should be permitted to try—say, a drug—which they feel has some chance of being effective and has a minimum of risk."

"Of course," he added, "this should only be done with the full knowledge and consent of the patient."

Dr. Larson, who is a former president of the American Medical Association, pointed out that taking "calculated risks" has characterized many of the greatest medical advances.

"In eradicating polio, we had to gamble that vaccines would not cause polio or cancer."

Early batches of polio vaccine were found to contain a virus that produced cancer in hamsters. The virus has since been eliminated from the vaccine.

"In removing greatly dreaded plagues from the list of lethal diseases, we had to take chances that sulfa drugs and antibiotics would not subject patients to deadly allergies and other diseases."

"Now," he noted, "important new knowledge has come from basic studies in the fields of hormones and immunity, and we should lose no time in applying it to human diseases like cancer."

An early effort along this line has resulted in spectacular disappearances of hopelessly advanced human skin cancer in a few patients at Roswell Park Memorial Institute in Buffalo, N.Y.

"There is reason for extreme caution in experimental medicine," Dr. Larson told the 50-odd science writers attending the seminar.

"But," he went on, "with the cure rates for leukemia at zero and for breast cancer at a complete standstill during recent decades, and with death rates for colon cancer rising and for lung cancer soaring, there is also reason for haste."

"We have a choice of living dangerously or dying early."

## 6.

## Pursuit of Knowledge?

a.

**Theodore Roszak**  
**The Making of a Counter-Culture\***

\* \* \*

[T]here exists no way whatever, on strictly scientific grounds, to invalidate *any* objective quest for knowledge, regardless of where it may lead or how it may proceed. The particular project may be unpalatable to the more squeamish among us—for “purely personal reasons”; but it does not thereby cease to be a legitimate exercise of objectivity. After all, knowledge is knowledge; and the more of it, the better. Just as Leigh-Mallory set out to climb Everest simply because it was *there*, so the scientific mind sets out to solve puzzles and unravel mysteries because it perceives them as being *there*. What further justification need there be?

Once an area of experience has been identified as an object of study or experimental interference, there is no rational way in which to deny the inquiring mind its right to know, without calling into question the entire scientific enterprise. In order to do so, one would have to invoke some notion of the “sacred” or “sacrosanct” to designate an area of life that must be closed to inquiry and manipulation. But since the entire career of the objective consciousness has been one long running battle against such suspiciously nebulous ideas, these concepts survive in our society only as part of an atavistic vocabulary. They are withered roses we come upon, crushed in the diaries of a prescientific age.

We are sadly deceived by the old cliché which mournfully tells us that morality has failed to “keep up with” technical progress (as if indeed morality were a “field of knowledge” in the charge of unidentified, but presumably rather incompetent, experts). The expansion of objective consciousness must, of necessity, be undertaken at the expense of moral sensibility. Science deracimates the experience of sacredness wherever it abides, and does so unapologetically, if not with fanatic fervor. And lacking a warm and lively sense of the sacred, there can be no ethical commitment that is anything more than superficial humanist rhetoric. We are left with, at best,

good intentions and well-meaning gestures that have no relationship to authoritative experience, and which therefore collapse into embarrassed confusion as soon as a more hard-headed, more objective inquirer comes along and asks, “But why not?” Having used the keen blade of scientific skepticism to clear our cultural ground of all irrational barriers to inquiry and manipulation, the objective consciousness is free to range in all directions. And it so does.

\* \* \*

b.

**Sigmund Freud**  
**Fragment of an Analysis of a Case of**  
**Hysteria (1905)\***

\* \* \*

[T]he presentation of my case histories remains a problem which is hard for me to solve. . . . If it is true that the causes of hysterical disorders are to be found in the intimacies of the patients’ psychosexual life, and that hysterical symptoms are the expression of their most secret and repressed wishes, then the complete elucidation of a case of hysteria is bound to involve the revelation of those intimacies and the betrayal of those secrets. It is certain that the patients would never have spoken if it had occurred to them that their admissions might possibly be put to scientific uses; and it is equally certain that to ask them themselves for leave to publish their case would be quite unavailing. In such circumstances persons of delicacy, as well as those who were merely timid, would give first place to the duty of medical discretion and would declare with regret that the matter was one upon which they could offer science no enlightenment. But in my opinion the physician has taken upon himself duties not only towards the individual patient but towards science as well; and his duties toward science mean ultimately nothing else than his duties towards the many other patients who are suffering or will some day suffer from the same disorder. Thus it becomes the physician’s duty to publish what he believes he knows of the causes and structure of hysteria, and it becomes a disgraceful piece of cowardice on his part to neglect doing so, as long as he can avoid caus-

\* New York: Doubleday and Co. Inc., 272–273 (1969). Copyright © 1968, 1969 by Theodore Roszak. Reprinted by permission.

\* James Strachey, ed.: *The Standard Edition of the Complete Psychological Works of Sigmund Freud*. London: The Hogarth Press and the Institute of Psycho-Analysis 7–9 (1953). Reprinted by permission of Sigmund Freud Copyrights Ltd., the Hogarth Press and Basic Books, Inc.

ing direct personal injury to the single patient concerned. I think I have taken every precaution to prevent my patient from suffering any such injury. I have picked out a person the scenes of whose life were laid not in Vienna but in a remote provincial town, and whose personal circumstances must therefore be practically unknown in Vienna. I have from the very beginning kept the fact of her being under my treatment such a careful secret that only one other physician—and one in whose discretion I have complete confidence—can be aware that the girl was a patient of mine. I have waited for four whole years since the end of the treatment and have postponed publication till hearing that a change has taken place in the patient's life of such a character as allows me to suppose that her own interest in the occurrences and psychological events which are to be related here may now have grown faint. Needless to say, I have allowed no name to stand which could put a non-medical reader upon the scent; and the publication of the case in a purely scientific and technical periodical should, further, afford a guarantee against unauthorized readers of this sort. I naturally cannot prevent the patient herself from being pained if her own case history should accidentally fall into her hands. But she will learn nothing from it that she does not already know; and she may ask herself who besides her could discover from it that she is the subject of this paper.

\* \* \*

## NOTES

## NOTE 1.

E. J. BLOUSTEIN  
PRIVACY AS AN ASPECT OF HUMAN DIGNITY\*

\* \* \*

An intrusion on our privacy threatens our liberty as individuals to do as we will, just as an assault, a battery or imprisonment of our person does. And just as we may regard these latter torts as offenses "to the reasonable sense of personal dignity," as offensive to our concept of individualism and the liberty it entails, so too should we regard privacy as a dignitary tort. Unlike many other torts, the harm caused is not one which may be repaired and the loss suffered is not one which may be made good by an award of damages. The injury is to our individuality, to our

dignity as individuals, and the legal remedy represents a social vindication of the human spirit thus threatened rather than a recompense for the loss suffered.

\* \* \*

To be sure, this identification of the interest served by the law of privacy does not of itself "solve" any privacy problems; it does not furnish a ready-made solution to any particular case of a claimed invasion of privacy. In the first place, not every threat to privacy is of sufficient moment to warrant the imposition of civil liability or to evoke any other form of legal redress. We all are, and of necessity must be, subject to some minimum scrutiny of our neighbors as a very condition of life in a civilized community. Thus, even having identified the interest invaded, we are left with the problem whether, in the particular instance, the intrusion was of such outrageous and unreasonable character as to be made actionable.

Secondly, even where a clear violation of privacy is made out, one must still face the question whether it is not privileged or excused by some countervailing public policy or social interest. The most obvious such conflicting value is the public interest in news and information which, of necessity, must sometimes run counter to the individual's interest in privacy. Again, identification of the nature of the privacy interest does not resolve the conflict of values, except insofar as it makes clear at least one of the elements which is to be weighed in the balance.

\* \* \*

## NOTE 2.

RESTATEMENT OF THE LAW, SECOND  
TORTS\*

§ 652B. *Intrusion upon Seclusion*

*One who intentionally intrudes, physically or otherwise, upon the solitude or seclusion of another, or his private affairs or concerns, is subject to liability to the other for invasion of his privacy, if the intrusion would be highly offensive to a reasonable man.*

*Comment:*

a. The form of invasion of privacy covered by this Section does not depend upon any pub-

\* 39 *New York University Law Review* 962, 1002-1004 (1964). Reprinted by permission.

\* Tentative Draft No. 13. Philadelphia: The American Law Institute 103-132 (1967). Copyright 1967. Reprinted by permission of the American Law Institute.

licity given to the person whose interest is invaded, or to his affairs. It consists solely of an intentional interference with his interest in solitude or seclusion, either as to his person or as to his private affairs or concerns, of a kind that would be highly offensive to a reasonable man.

b. The invasion may be by physical intrusion into a place in which the plaintiff has secluded himself, as where the defendant forces his way into the plaintiff's room in a hotel, or insists over the plaintiff's objection in entering his home. It may also be by the use of the defendant's senses, with or without mechanical aids, to oversee or overhear the plaintiff's private affairs, as by looking into his upstairs windows with binoculars, or tapping his telephone wires. It may be by some other form of investigation or examination into his private concerns, as by opening his private and personal mail, searching his safe or his wallet, examining his private bank account, or compelling him by a forged court order to permit an inspection of his personal documents.

\* \* \*

c. The defendant is subject to liability under the rule stated in this Section only when he has intruded into a private place, or has otherwise invaded a private seclusion which the plaintiff has thrown about his person or affairs. Thus there is no liability for the examination of a public record concerning the plaintiff, or of documents which the plaintiff is required to keep and make available for public inspection. Nor is there liability for observing him, or even taking his photograph while he is walking on the public highway, since he is not then in seclusion, and his appearance is public, and open to the public eye. Even in a public place, however, there may be some matters about the plaintiff, such as his underwear or lack of it, which are not exhibited to the public gaze; and there may still be invasion of privacy when there is intrusion upon such matters.

\* \* \*

d. There is likewise no liability unless the interference with the plaintiff's seclusion is a substantial one, of a kind which would be highly offensive to the ordinary reasonable man, as the result of conduct to which the reasonable man would strongly object. . . .

\* \* \*

#### § 652D. *Publicity Given to Private Life*

*One who gives publicity to matters concerning the private life of another, of a kind highly offensive to a reasonable man, is subject to liability to the other for invasion of his privacy.*

*Comment:*

\* \* \*

c. *Private life.* The rule stated in this Section applies only to publicity given to matters concerning the private, as distinguished from the public, life of the individual. There is no liability when the defendant merely gives further publicity to information about the plaintiff which is already public. Thus there is no liability for giving publicity to facts about the plaintiff's life which are matters of public record, such as the date of his birth, the fact of his marriage, his military record, the fact that he is admitted to the practice of medicine or is licensed to drive a taxicab, or the pleadings which he has filed in a lawsuit. On the other hand, if the record is one not open to public inspection, as in the case of income tax returns, it is not public, and there is an invasion of privacy when it is made so.

\* \* \*

Every individual has some phases of his life and his activities, and some facts about himself, which he does not expose to the public eye, but keeps entirely to himself, or at most reveals only to his family or to close personal friends. Sexual relations, for example, are normally entirely private matters, as are family quarrels, many unpleasant or disgraceful or humiliating illnesses, most intimate personal letters, most details of a man's life in his home, and some of his past history which he would rather forget. When these intimate details of his life are spread before the public gaze, in a manner highly offensive to the ordinary reasonable man, there is an actionable invasion of his privacy, unless, as stated in § 652F, there is a privilege to make the matter public because it is one of legitimate public interest.

\* \* \*

#### § 652F. *Privilege to Give Publicity to Matters of Public Interest*

(1) *One is privileged to give publicity to facts concerning another which would otherwise constitute an invasion of his privacy, to the extent that such publicity is given to [news or other] matters in which the public has a legitimate interest.*

(2) *The privilege stated in subsection (1) extends to false statements of such facts, unless they are made with knowledge of their falsity, or in reckless disregard whether they are true.*

\* \* \*

Comment:

a. The privilege stated in this Section rests primarily upon the traditional freedom of speech and of the press. . . .

\* \* \*

h. *Education and information.* The privilege to give publicity to matters of public interest is not limited to "news," in the sense of reports of current events. It extends also to the use of names, likenesses or facts in giving information to the public for purposes of education, amusement or enlightenment, where the public may reasonably be expected to have a legitimate interest in what is published.

\* \* \*

C.

**Joseph H. Fichter and William L. Kolb  
Ethical Limitations on Sociological  
Reporting\***

\* \* \*

As soon as the sociologist leaves the field of quantitative analysis and attempts to describe in conceptual terms the social relations in a small group or community, the problem of what to report becomes much greater. Even when the community is cloaked in anonymity, indirect identification is almost always possible, and there is likely to be a subtle and unintended violation of human rights. The threat becomes even greater when the sociologist adds to his description of the social relations in the group or community an interpretation of the motivation which supports these relations and other social behavior. Thus, where systematic sociological description and interpretation of motivation combine, the sociologist faces the gravest moral challenge, and particularly so where this mode of description and analysis is applied to a leading member of the group. The likelihood that such a person will be identified and his social behavior and personal reputation placed under scrutiny by his fellows on the basis of the research report is very great. Here, more than anywhere else, the sociologist

must take care not to needlessly injure another human being.

The problem of truth telling thus becomes a circumstantial one. This means that while telling the truth cannot *per se* be wrong or harmful, the ethical question of whether or not to include a certain objective fact always arises in relation to person and circumstances. Thus complete objectivity, or telling all the truth in all circumstances, is not necessarily a morally good act.

This is true for several reasons. The researcher is, of course, bound to secrecy where information has been given in confidence or where he has made promises of secrecy. At the same time, as a scientist, he will discover natural secrets, which by their seriousness demand silence on the part of the reporter. There is also the problem of detraction—the injury of another's reputation by revealing what is detrimental but true about him. If the harmful fact is already widely disseminated or if the subject is mistaken in the belief that the fact will result in the impairment of his reputation, the sociologist may not have any obligation to conceal the fact. Otherwise its revelation is a serious matter.

\* \* \*

. . . Some positivists seem to regard science only as a fascinating game played according to a set of rules. It is doubtful that the sociologist using this conception of science may ever legitimately overrule the rights of the people studied. The simple wish of the people to conceal certain aspects of their behavior must then be considered sufficient to bar the report of that behavior.

If one regards science as a search for truth as an end in itself, the demands of the objectivity of science will carry much weight in the decision to publish all pertinent data. Except in history, however, the truth for which the social scientist searches is nomothetic, not idiographic, truth. It may be necessary to base generalizations on certain idiographic items, but man has the entire span of his career on earth to discover and disclose such items. Certainly a particular item of current behavior turned up in a community study need not be used to support a generalization if such use inflicts injury on the people being investigated.

There is a third conception of pure science. Social scientists may believe that science is both a rigidly ruled game and a search for truth which is valuable for itself, but they usually also believe that science well developed and used by experts or disseminated among the people can

\* 18 *American Sociological Review* 544, 547-548 (1953). Reprinted by permission.



make for a better life. There is a sense of urgency about accomplishing this mission of pure science in the modern world. Thus, within this perspective, considerable pressure arises to ignore the rights of people who are scientifically studied. Despite this pressure it remains true that a willful disregard for the rights of persons and groups to their privacy, reputations, and secrets, will tend to destroy the very values which the scientist hopes his basic research can render more achievable.

Frequently the scientist makes a community or small group study not as a pure scientist but in one sense or another as an applied scientist. He may carry on the research for what he himself considers desirable practical ends; he may be employed by officials of the community or group or by those of the larger society; or he may be employed by some private group with a specific selfish or altruistic interest. In all three of these instances there is pressure to report all the significant findings even though injury may be done to the objects of the study. Nevertheless the sociologist must abide by the rule that he exercise every effort to determine whether or not the values to be implemented by the study, and the probability of being able to achieve them through the use of its findings, justify the harm done to the members of the community or group.

Preoccupation with applied science is frequently accompanied by the temptation to look for and publish data which will further the realization of what the researcher himself regards as the good society or community. He is likely to believe that all of his data must be revealed in all circumstances. It appears to us that a scientist of this persuasion is most in need of the virtues of tolerance, compassion, and love, because he is in danger of placing the considerations of the "good" society above all consideration of individual rights and injuries.

The hired scientist, moreover, cannot avoid responsibility for revealing data injurious to individuals and groups by pleading loyalty to community or nation or by indicating his contractual responsibilities to a private group. Loyalty to community or nation may require injury to individuals and groups, but in such cases the scientist shares whatever guilt is incurred with all other responsible agencies. In instances of purely contractual research the scientist must accept full responsibility, because loyalty to nation or community is not involved. He is free to refuse the job, and if the values of the employing group are wrong or do not justify the amount of injury

done the scientist must accept the moral responsibility.

\* \* \*

d.

**Howard S. Becker**

#### **Problems in the Publication of Field Studies\***

Unless the scientist deliberately restricts himself to research on the ideologies and beliefs of the people studied and does not touch on the behavior of the members of the community or organization, he must in some way deal with the disparity between reality and ideal, with the discrepancy between the number of crimes committed and the number of criminals apprehended. A study that purports to deal with social structure thus inevitably will reveal that the organization or community is not all it claims to be, not all it would like to be able to feel itself to be. A good study, therefore, will make somebody angry.

[A] good study of a community or organization must reflect the irreconcilable conflict between the interests of science and the interests of those studied, and thereby provoke a hostile reaction. Yet many studies conducted by competent scientists do not have this consequence. Under what circumstances will the report of a study fail to provoke conflict? Can such a failure be justified?

In the simplest case, the social scientist may be taken in by those he studies and be kept from seeing the things that would cause conflict were he to report them. . . .

\* \* \*

This is probably an uncommon occurrence. Few people social scientists study are sophisticated enough to anticipate or control what the researcher will see. More frequently, the social scientist takes himself in, "goes native," becomes identified with the ideology of the dominant faction in the organization or community and frames the questions to which his research provides answers so that no one will be hurt. He does not do this deliberately or with the intent to suppress scientific knowledge. Rather, he unwittingly chooses problems that are not likely to cause trouble or inconvenience to those he has found to be such pleasant associates. . . .

\* Arthur J. Vidich, Joseph Bensman, and Maurice R. Stein, eds.: *Reflections on Community Studies*. New York: John Wiley and Sons, Inc. 267, 275-276, 278-279, 283-284 (1964). Reprinted by permission.



\* \* \*

[E]ven if he is not deceived . . . the social scientist may deliberately decide to suppress conflict-provoking findings. He may suppress his findings because publication will violate a bargain he has made with those studied. If, for example, he has given the subjects of his study the right to excise offensive portions of his manuscript prior to publication in return for the privilege of making the study, he will feel bound to honor that agreement. Because of the far-reaching consequences such an agreement could have, most social scientists take care to specify, when reaching an agreement with an organization they want to study, that they have the final say as to what will be published, though they often grant representatives of the organization the right to review the manuscript and suggest changes.

The social scientist may also suppress his findings because of an ideological commitment to the maintenance of society as it is now constituted. Shils makes the following case.

Good arguments can be made against continuous publicity about public institutions. It could be claimed that extreme publicity not only breaks the confidentiality which enhances the imaginativeness and reflectiveness necessary for the effective working of institutions but also destroys the respect in which they should, at least tentatively, be held by the citizenry.

He believes that the first of these considerations is probably correct and thus constitutes a legitimate restriction on scientific inquiry, whereas the second, although not entirely groundless ethically, is so unlikely to occur as not to constitute a clear danger.

\* \* \*

Shils rests his case on the possibility that the publicity generated by research may interfere with the "effective working of institutions." When this occurs the scientist should restrict his inquiry. We can accept this argument only if we agree that the effective working of institutions as they are presently constituted is an overriding good. Shils, in his disdain for the "populistic" frame of mind that has informed much of American sociology (his way of characterizing the "easy-going irreverence toward authority" and the consequent tendency to social criticism among social scientists), is probably more ready to accept such a proposition than the majority of working social scientists. Furthermore, and I do not know that he would carry his argument so far, the right of public institutions to delude

themselves about the character of their actions and the consequences of those actions does not seem to me easily defended.

\* \* \*

In discussing the several facets of the problem, I have avoided stating any ethical canons. I have relied on those canons implicit in the scientific enterprise in suggesting that the scientist must strive for the freest possible conditions of reporting. Beyond that I have said only that it is a matter of individual conscience. In so restricting my remarks and in discussing the problem largely in technical terms, I have not meant to indicate that one need have no conscience at all, but only that it must remain a matter of individual judgment.

I ought properly, therefore, to express my own judgment. Briefly, it is that one should refrain from publishing items of fact or conclusions that are not necessary to one's argument or that would cause suffering out of proportion to the scientific gain of making them public. This judgment is of course ambiguous. When is something "necessary" to an argument? What is "suffering"? When is an amount of suffering "out of proportion"? Even though the statement as it stands cannot determine a clear line of action for any given situation, I think it does suggest a viable vantage point, an appropriate mood, from which decisions can be approached. In particular, it suggests on the one hand that the scientist must be able to give himself good reasons for including potentially harmful material, rather than including it simply because it is "interesting." On the other hand, it guards him against either an overly formal or an overly sentimental view of the harm those he studies may suffer, requiring that it be serious and substantial enough to warrant calling it "suffering." Finally, it insists that he know enough about the situation he has studied to know whether the suffering will in any sense be proportional to gains science may expect from publication of his findings.

\* \* \*

## NOTE

EARL H. BELL

FREEDOM AND RESPONSIBILITY IN RESEARCH\*

I read with great interest the editorial, "Freedom and Responsibility in Research: The

\* 18 *Human Organization* 49 (1959). Reprinted by permission.

Springdale Case." The problem relative to responsibility of authors to the community is one which always pushes itself into focus when I start writing a report. Personally, I have come to the conclusion that responsibility to the community does *not* conflict with responsibility to science. As a matter of fact, I have found frequently that attempting to state material coolly and objectively, rather than in terms of personalities and anecdotes, sharpens my understanding of sociological processes.

After writing the first draft of the Haskell County, Kansas Study, I took the manuscript to the community and went over it with my major informants. In many ways, this was the most productive part of the field work. It enabled the informants, for the first time, to understand what I was attempting to accomplish. This broader understanding brought to mind many things which they had not told me, largely because I did not have the knowledge of the culture and social system to formulate some significant questions. They also pointed out numerous errors of both fact and interpretation and thus saved me personal embarrassment and scientific error.

\* \* \*

e.  
**Oscar M. Ruebhausen and Orville G. Brim, Jr.**  
**Privacy and Behavioral Research\***

\* \* \*

[An] important safeguard for confidentiality can be provided through control techniques. For example, the identity of the respondent may be coded and separated from his response except for the code number. The code, in turn, may be made accessible only to a few of the most responsible officials, or perhaps, only on two signatures or by the use of double keys. Even as elementary a safeguard as a locked file can make for substantial improvement. Penalties within the profession may also be devised for any breach of the confidentiality which should be of the very essence of professionalism.

Another readily available step is the destruction of research data. At the very least, that part of the data which would identify any individual with any portion of it should be destroyed, and destroyed at the earliest moment it is possible to do so. . . . [B]ehavioral scientists have strong incentives to retain all original research data. Such data can provide information of a

longitudinal nature about the development of personality or organizations over time, the early childhood antecedents of career success, the degree of change in interest and attitude from one age to another, the effects of marriage upon personality characteristics and other fascinating problems. There are now great repositories of such data in the United States collected about individuals in schools, both secondary and college, and other institutional settings, which have been maintained because of this natural resistance of the research scientist to discard anything of such potential value. Nevertheless, the maintenance and use of this information for purposes other than that originally agreed to, and the threat to confidentiality inherent in its continued maintenance, strongly suggest that the proper course of the person or institution possessing such data is either to obtain the consent of the individual involved to its continued preservation, or to destroy the data, painful as the latter prospect may be.

It should be emphasized that neither the integrity of the scientist nor the technical safeguards of locks and codes can protect research data against a valid subpoena; such data are at present quite clearly subject to subpoena. In the last analysis, therefore . . . confidentiality can be assured only by destruction of the data. . . .

\* \* \*

Assuredly, one can visualize situations in which the release of research data for a use not initially contemplated would, because of the great public interest involved, be socially tolerable. . . .

\* \* \*

## 7.

### Values of Investigator?

a.

**Howard S. Becker**  
**Whose Side Are We On?\***

To have values or not to have values: the question is always with us. When sociologists undertake to study problems that have relevance to the world we live in, they find themselves caught in a crossfire. Some urge them not to take sides,

\* 65 *Columbia Law Review* 1184, 1205-1208 (1965). Reprinted by permission.

\* 14 *Social Problems* 239, 245-247 (1967). Presidential Address, delivered at the annual meeting of the Society for the Study of Social Problems. Reprinted by permission.

to be neutral and do research that is technically correct and value free. Others tell them their work is shallow and useless if it does not express a deep commitment to a value position.

This dilemma, which seems so painful to so many, actually does not exist, for one of its horns is imaginary. For it to exist, one would have to assume, as some apparently do, that it is indeed possible to do research that is uncontaminated by personal and political sympathies. I propose to argue that it is not possible and, therefore, that the question is not whether we should take sides, since we inevitably will, but rather whose side we are on.

\* \* \*

We must always look at the matter from someone's point of view. The scientist who proposes to understand society must, as Mead long ago pointed out, get into the situation enough to have a perspective on it. And it is likely that his perspective will be greatly affected by whatever positions are taken by any or all of the other participants in that varied situation. Even if his participation is limited to reading in the field, he will necessarily read the arguments of partisans of one or another side to a relationship and will thus be affected, at least, by having suggested to him what the relevant arguments and issues are. A student of medical sociology may decide that he will take neither the perspective of the patient nor the perspective of the physician, but he will necessarily take a perspective that impinges on the many questions that arise between physicians and patients; no matter what perspective he takes, his work either will take into account the attitude of subordinates, or it will not. If he fails to consider the questions they raise, he will be working on the side of the officials. If he does raise those questions seriously and does find, as he may, that there is some merit in them, he will then expose himself to the outrage of the officials and of all those sociologists who award them the top spot in the hierarchy of credibility. Almost all the topics that sociologists study, at least those that have some relation to the real world around us, are seen by society as morality plays and we shall find ourselves, willy-nilly, taking part in those plays on one side or the other.

\* \* \*

We can never avoid taking sides. So we are left with the question of whether taking sides means that some distortion is introduced into our work so great as to make it useless. Or, less drastically, whether some distortion is introduced

that must be taken into account before the results of our work can be used. I do not refer here to feeling that the picture given by the research is not "balanced," the indignation aroused by having a conventionally discredited definition of reality given priority or equality with what "everyone knows," for it is clear that we cannot avoid that. That is the problem of officials, spokesmen and interested parties, not ours. Our problem is to make sure that, whatever point of view we take, our research meets the standards of good scientific work, that our unavoidable sympathies do not render our results invalid.

We might distort our findings, because of our sympathy with one of the parties in the relationship we are studying, by misusing the tools and techniques of our discipline. We might introduce loaded questions into a questionnaire, or act in some way in a field situation such that people would be constrained to tell us only the kind of thing we are already in sympathy with. All of our research techniques are hedged about with precautionary measures designed to guard against these errors. Similarly, though more abstractly, everyone of our theories presumably contains a set of directives which exhaustively covers the field we are to study, specifying all the things we are to look at and take into account in our research. By using our theories and techniques impartially, we ought to be able to study all the things that need to be studied in such a way as to get all the facts we require, even though some of the questions that will be raised and some of the facts that will be produced run counter to our biases.

But the question may be precisely this. Given all our techniques of theoretical and technical control, how can we be sure that we will apply them impartially and across the board as they need to be applied? Our textbooks in methodology are no help here. They tell us how to guard against error, but they do not tell us how to make sure that we will use all the safeguards available to us. We can, for a start, try to avoid sentimentality. We are sentimental when we refuse, for whatever reason, to investigate some matter that should properly be regarded as problematic. We are sentimental, especially, when our reason is that we would prefer not to know what is going on, if to know would be to violate some sympathy whose existence we may not even be aware of. Whatever side we are on, we must use our techniques impartially enough that a belief to which we are especially sympathetic could be proved untrue. We must always inspect our work carefully enough to know whether our

techniques and theories are open enough to allow that possibility.

Let us consider, finally, what might seem a simple solution to the problems posed. If the difficulty is that we gain sympathy with underdogs by studying them, is it not also true that the superordinates in a hierarchical relationship usually have their own superordinates with whom they must contend? Is it not true that we might study those superordinates or subordinates, presenting their point of view on their relations with their superiors and thus gaining a deeper sympathy with them and avoiding the bias of one-sided identification with those below them? This is appealing, but deceptively so. For it only means that we will get into the same trouble with a new set of officials.

It is true, for instance, that the administrators of a prison are not free to do as they wish, not free to be responsive of the desires of inmates, for instance. If one talks to such an official, he will commonly tell us, in private, that of course the subordinates in the relationship have some right on their side, but that they fail to understand that his desire to do better is frustrated by his superiors or by the regulations they have established. Thus, if a prison administrator is angered because we take the complaints of his inmates seriously, we may feel that we can get around that and get a more balanced picture by interviewing him and his associates. If we do, we may then write a report which *his* superiors will respond to with cries of "bias." They, in their turn, will say that we have not presented a balanced picture, because we have not looked at *their* side of it. And we may worry that what they say is true.

The point is obvious. By pursuing this seemingly simple solution, we arrive at a problem of infinite regress. For everyone has someone standing above him who prevents him from doing things just as he likes. If we question the superiors of the prison administrator, a state department of corrections or prisons, they will complain of the governor and the legislature. And if we go to the governor and the legislature, they will complain of lobbyists, party machines, the public and the newspapers. There is no end to it and we can never have a "balanced picture" until we have studied all of society simultaneously. I do not propose to hold my breath until that happy day.

We can, I think, satisfy the demands of our science by always making clear the limits of what we have studied, marking the boundaries beyond which our findings cannot be safely applied. Not

just the conventional disclaimer, in which we warn that we have only studied a prison in New York or California and the findings may not hold in the other forty-nine states—which is not a useful procedure anyway, since the findings may very well hold if the conditions are the same elsewhere. I refer to a more sociological disclaimer in which we say, for instance, that we have studied the prison through the eyes of the inmates and not through the eyes of the guards or other involved parties. We warn people, thus, that our study tells us only how things look from that vantage point—what kinds of objects guards are in the prisoners' world—and does not attempt to explain why guards do what they do or to absolve the guards of what may seem, from the prisoners' side, morally unacceptable behavior. This will not protect us from accusations of bias, however, for the guards will still be outraged by the unbalanced picture. If we implicitly accept the conventional hierarchy of credibility, we will feel the sting in that accusation.

It is something of a solution to say that over the years each "one-sided" study will provoke further studies that gradually enlarge our grasp of all the relevant facets of an institution's operation. But that is a long-term solution, and not much help to the individual researcher who has to contend with the anger of officials who feel he has done them wrong, the criticism of those of his colleagues who think he is presenting a one-sided view, and his own worries.

What do we do in the meantime? I suppose the answers are more or less obvious. We take sides as our personal and political commitments dictate, use our theoretical and technical resources to avoid the distortions that might introduce into our work, limit our conclusions carefully, recognize the hierarchy of credibility for what it is, and field as best we can the accusations and doubts that will surely be our fate.

b.

**Alvin W. Gouldner  
The Sociologist as Partisan—  
Sociology and the Welfare State\***

\* \* \*

... I fear that the myth of a value-free social science is about to be supplanted by still another myth, and that the once glib acceptance of the value-free doctrine is about to be superseded by a new but no less glib rejection of it. ...

\* \* \*

\* 3 *The American Sociologist* 103, 113 (1968). Reprinted by permission.

[W]hen we talk about the bias or impartiality of a sociologist we are, in effect, talking about the sociologist as if he were a "judge." Now, rendering a judgment premises the existence of conflicting or contending parties; but it does not imply an intention to *mediate* the difficulties between them. The function of a judge is not to bring parties together but is, quite simply, to do justice. Doing justice does not mean, as does mediation or arbitration, that both the parties must each be given or denied a bit of what they sought. Justice does not mean logrolling or "splitting the difference." For the doing of justice may, indeed, give all the benefits to one party and impose all the costs upon another.

What makes a judgment possessed of justice is not the fact that it distributes costs and benefits equally between the parties but, rather, that the allocation of benefits and costs is made in conformity with some stated normative standard. Justice, in short, is that which is justified in terms of some value. The "impartiality" or objectivity of the judge is an imputation made when it is believed that he had made his decision primarily or solely in terms of some moral value. In one part, then, the objectivity of the judge requires his explication of the moral value in terms of which his judgment has been rendered. One reason why Becker's analysis founders on the problem of objectivity is precisely because it regards the sociologists' value commitment merely as an inescapable fact of nature, rather than viewing it as a necessary condition of his objectivity.

Insofar as the problem is seen as one of choosing up sides, rather than a working one's way through to a value commitment, I cannot see how it is ever possible for men to recognize that the side to which they are attached can be wrong. But men do not and need not always say, "my country right or wrong." Insofar as they are capable of distinguishing the side to which they are attached, from the *grounds* on which they are attached to it, they are, to that extent, capable of a significant objectivity.

It should again be clear, then, that I do not regard partisanship as incompatible with objectivity. The physician, after all, is not necessarily less objective because he has made a partisan commitment to his patient and against the germ. The physician's objectivity is in some measure vouchsafed because he has committed himself to a specific value: health. It is this commitment that constrains him to see and to say things about the patient's condition that neither may want to know.

But in saying that the explication of the so-

ciologist's value commitment is a necessary condition for his objectivity, we are saying little unless we recognize at the same time the grinding difficulties involved in this. For one, it is no easy thing to know what our own value commitments are. In an effort to seem frank and open, we all too easily pawn off a merely glib statement about our values without making any effort to be sure that these are the values to which we are actually committed. This is much of what happens when scientists conventionally assert that they believe only in "the truth." Secondly, a mere assertion of a value commitment is vainly ritualistic to the extent that the sociologist has no awareness of the way in which one of his commitments may conflict with or exclude another. For example, there is commonly some tension between a commitment to truth and a commitment to welfare. Third, we also have to recognize that the values in terms of which we may make our judgments may not necessarily be shared by the participants in the situations we have studied. Our objectivity, however, does not require us to share values with those we study, but only to apply the values that we claim are our own, however unpopular these may be. In other words, this form of objectivity requires that we be on guard against our own hypocrisy and our need to be loved. This creates a problem because the values we may actually hold may differ from those we feel that we must display in order to gain or maintain access to research sites.

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## 8.

### Limits of Prediction?

a.

#### Margaret Mead The Problem of an Unpredictable Future Position of an Individual Identified in a Research Project—1953–1968 A Melanesian Leader in Papua, New Guinea\*

In 1953, Theodore Schwartz and I made an extensive study of a political movement in the Admiralty Islands, I as part of a restudy and Dr. Schwartz as a specific study of the movement and its native leader Paliau. At that time, Paliau

\* 98 *Daedalus* 382–384 (1969). Reprinted by permission of *Daedalus*, Journal of the American Academy of Arts and Sciences, Boston, Massachusetts.

had become known in many parts of the world, especially in Australia and in Roman Catholic circles, as a leader who had transformed a cargo cult into a successful political movement. He was well known as a sergeant in the police force of the Mandated Territory of New Guinea. During World War II, he found himself behind the Japanese lines and took the responsibility for organizing the food and living arrangements for indentured laborers from other islands who had been left on New Britain when Rabaul fell to the Japanese. After the war, it was impossible to try as traitors Melanesians from the Mandated Territory of New Guinea who had worked under the Japanese administration: members of the Trust Territory that owed no allegiance to Australia. An attempt, however, was made to punish them as war criminals. Paliau maintained that he and others arraigned with him had been instructed by the Australians, when they evacuated Rabaul, to obey the Japanese. He successfully refuted the charges of being a war criminal, but his career in the police force was over. He returned to his own island of Balowan, and with many vicissitudes—a cargo cult, imprisonment for involvement in the cult under the heading of “spreading false rumors,” an attack on his life by the insane former husband of a new wife, a trip to Port Moresby for “indoctrination”—he maintained a hold on some five thousand people whom he had succeeded in uniting in his new movement. He was first permitted to become chairman of half of this population, a local Council compound. While we were there in 1953, the local government Council authority was extended to the entire five thousand people, out of an estimated twenty thousand Admiralty Island inhabitants. It seemed unlikely at that time that he would increase his political importance further. Many local government officials were hostile; his original dream of operating in a larger sphere, including the entire Bismark Archipelago, seemed completely incapable of realization.

In 1954, I wrote *New Lives for Old*, and in 1958, Theodore Schwartz completed the manuscript of *The Paliau Movement in the Admiralty Islands, 1946–1954*. In 1957, I discussed Paliau in one of the Terry Lectures at Yale as a man of unusual ability, comparable in quality to Churchill or Roosevelt, astonishing in a man who came from a small island group of only six hundred preliterate people. The Terry lectures were published in 1964 under the title of *Continuities in Cultural Evolution*.

In 1964, in the wake of the newly estab-

lished electoral proceedings in New Guinea, Paliau was elected a member to the new House of Assembly for the Admiralty Islands, in spite of active opposition. In September of that year, on my way back to the Admiralties, I was interviewed in Sydney, Australia, by a reporter for the *Pacific Island Monthly*, who had taken the trouble to look up *Continuities in Cultural Evolution*, a book of a type not usually presented in the popular and highly politicized periodical. As a consequence, *Pacific Island Monthly* published Paliau's picture with a full-page article entitled, “Paliau Compared to Roosevelt.” This article might have been a hazard to a man who continued to be the target of much local mission attack. But Dr. Schwartz and I had written our descriptions so that they could be read by anyone interested in the Paliau movement, and because Dr. Schwartz had painstakingly gone over the text with Paliau (who was now operating on a wider, much more politically important stage than when we had done our original writing), he could be proud of, rather than hurt, by the discussions that had been published.

In 1967, National Education Television made a film of anthropological work in Manus from 1928 to the present and included Paliau as a prominent figure. The filming ended in the autumn of 1967, and it was clear then that it would take approximately a year to complete the editing. The next election for the General Assembly was coming up in May–June of 1968. Had Paliau lost, it might have been said, then or later, by commentators on the responsibilities of social scientists working among emerging peoples, that the film had lost him the election and his chance to be a significant molder of the future of Papua-New Guinea. However he was re-elected!

This case is presented in illustration of the vicissitudes of social research on living persons whose future roles cannot, in the nature of the case, be predicted.

#### NOTE

JOSEPH H. FICHTER AND WILLIAM L. KOLB  
ETHICAL LIMITATIONS ON SOCIOLOGICAL  
REPORTING\*

\* \* \*

. . . Those instances in which the scientist can foretell with certitude that serious injury will be done to the objects of his study seem to be very few in number. It is also likely that the lar-

\* 18 *American Sociological Review* 544, 548–549 (1953). Reprinted by permission.

gest proportion of his data will be free of possibly injurious materials. It is the in-between area of probable injury that is most difficult to determine and yet which must be determined.

To know what the effect of exposing a group's secrets will be, to realize how seriously a person's reputation may be damaged, and to visualize the effects of violation of privacy presupposes knowledge on the part of the scientist which he may not have. This knowledge can be approached to the extent to which the scientist saturates himself in the social relations of the group which he studies. It probably cannot be achieved by the aloof scientist who simply culls the reports of those who have done the actual and basic data collecting.

Since there is a great difference between imaginary and objective derogation of reputation, the sociologist may tend to brush off the former as relevant and uncontrollable. Human decency, however, would seem to require that the scientist make an effort to inquire even into this possibility of psychological and subjective injury. The scientist cannot guard against all such contingencies and against the unexpected and unwarranted complaints of people, but he should do his human best to avoid them ahead of time and to be sympathetic to them if they come.

If the sociologist attempts to interpret the social behavior of the people he studies, he must assess the responsibility of the people for their own actions. False sentimentality must not result in the denial of the fact that a person must accept the consequences of the acts for which he is responsible. The scientist cannot erase the responsibilities, duties, and obligations, of the objects of his study. Yet, at the same time, he must recognize that the human being is never completely responsible for his actions, and that in many cases factors over which the person or group has no control may come close to completely determining certain acts. Since the assessment of responsibility will be contained in the research report, injury can be done if the assessment is not carefully made.

b.

**Diana Baumrind**  
**Some Thoughts on Ethics of Research—**  
**After Reading Milgram's "Behavioral Study**  
**of Obedience"**\*

[1] regard the emotional disturbance described by Milgram as potentially harmful be-

cause it could easily effect an alteration in the subject's self-image or ability to trust adult authorities in the future. It is potentially harmful to a subject to commit, in the course of an experiment, acts which he himself considers unworthy, particularly when he has been entrapped into committing such acts by an individual he has reason to trust. The subject's personal responsibility for his actions is not erased because the experimenter reveals to him the means which he used to stimulate these actions. The subject realizes that he would have hurt the victim if the current were on. The realization that he also made a fool of himself by accepting the experimental set results in additional loss of self-esteem. Moreover, the subject finds it difficult to express his anger outwardly after the experimenter in a self-acceptant but friendly manner reveals the hoax.

A fairly intense corrective interpersonal experience is indicated wherein the subject admits and accepts his responsibility for his own actions, and at the same time gives vent to his hurt and anger at being fooled. Perhaps an experience as distressing as the one described by Milgram can be integrated by the subject, provided that careful thought is given to the matter. The propriety of such experimentation is still in question even if such a reparational experience were forthcoming. Without it I would expect a naive, sensitive subject to remain deeply hurt and anxious for some time, and a sophisticated, cynical subject to become even more alienated and distrustful.

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### NOTE

STANLEY MILGRAM  
ISSUES IN THE STUDY OF OBEDIENCE—  
A REPLY TO BAUMRIND†

\* \* \*

. . . Baumrind confuses the unanticipated outcome of an experiment with its basic procedure. She writes, for example, as if the production of stress in our subjects was an intended and deliberate effect of the experimental manipulation. There are many laboratory procedures specifically designed to create stress . . . , but the obedience paradigm was not one of them. The extreme tension induced in some subjects was unexpected. Before conducting the experiment, the procedures were discussed with many colleagues, and none anticipated the reactions that subsequently took place. Foreknowledge of results can

\* 19 *American Psychologist* 421-423 (1964). Reprinted by permission.

† 19 *American Psychologist* 848-849 (1964). Reprinted by permission.



never be the invariable accompaniment of an experimental probe. Understanding grows because we examine situations in which the end is unknown. An investigator unwilling to accept this degree of risk must give up the idea of scientific inquiry.

Moreover, there was every reason to expect, prior to actual experimentation, that subjects would refuse to follow the experimenter's instructions beyond the point where the victim protested; many colleagues and psychiatrists were questioned on this point, and they virtually all felt this would be the case. Indeed, to initiate an experiment in which the critical measure hangs on disobedience, one must start with a belief in certain spontaneous resources in men that enable them to overcome pressure from authority.

It is true that after a reasonable number of subjects had been exposed to the procedures, it became evident that some would go to the end of the shock board, and some would experience stress. That point, it seems to me, is the first legitimate juncture at which one could even start to wonder whether or not to abandon the study. But momentary excitement is not the same as harm. As the experiment progressed there was no indication of injurious effects in the subjects; and as the subjects themselves strongly endorsed the experiment, the judgment I made was to continue the investigation.

Is not Baumrind's criticism based as much on the unanticipated findings as on the method? The findings were that some subjects performed in what appeared to be a shockingly immoral way. If, instead, every one of the subjects had broken off at "slight shock," or at the first sign of the learner's discomfort, the results would have been pleasant, and reassuring, and who would protest?

A most important aspect of the procedure occurred at the end of the experimental session. A careful post-experimental treatment was administered to all subjects. The exact content of the dehoax varied from condition to condition and with increasing experience on our part. At the very least all subjects were told that the victim had not received dangerous electric shocks. Each subject had a friendly reconciliation with the unharmed victim, and an extended discussion with the experimenter. The experiment was explained to the defiant subjects in a way that supported their decision to disobey the experimenter. Obedient subjects were assured of the fact that their behavior was entirely normal and that their

feelings of conflict or tension were shared by other participants. Subjects were told that they would receive a comprehensive report at the conclusion of the experimental series. In some instances, additional detailed and lengthy discussions of the experiments were also carried out with individual subjects.

When the experimental series was complete, subjects received a written report which presented details of the experimental procedure and results. Again their own part in the experiments was treated in a dignified way and their behavior in the experiment respected. All subjects received a follow-up questionnaire regarding their participation in the research, which again allowed expression of thoughts and feelings about their behavior.

The replies to the questionnaire confirmed my impression that participants felt positively toward the experiment. In its quantitative aspect . . . 84 percent of the subjects stated that they were glad to have been in the experiment; 15 percent indicated neutral feelings, and 1.3 percent indicated negative feelings. To be sure, such findings are to be interpreted cautiously, but they cannot be disregarded.

Further, four-fifths of the subjects felt that more experiments of this sort should be carried out, and 74 percent indicated that they had learned something of personal importance as a result of being in the study. . . .

c.

#### **Alexander D. Langmuir New Environmental Factor in Congenital Disease\***

This issue contains an original communication of great scientific importance and serious social implications. The highly significant correlation between the appearance of adenocarcinoma of the vagina in teen-age girls and young women, a very rare disease, and the ingestion of diethylstilbestrol by their mothers during the first trimester of pregnancy points to a new mechanism in the pathogenesis of congenital neoplastic disease and adds a new dimension to the whole matter of what drugs are safe or unsafe to administer to pregnant women.

The first indication of the existence of this effect was reported a year ago. A cluster of seven cases of adenocarcinoma of the vagina in young

\* 284 *New England Journal of Medicine* 912-913 (1971). Reprinted by permission.



females, 15 to 22 years of age, was recognized in the New England area during the period 1966 to 1969. This report created interest within gynecologic and oncologic circles, but was largely overlooked by epidemiologists. Such a concentration of such a rare disease in such a young age group points vividly to the existence of a specific causative factor somewhere in the immediate environment of these patients.

Now the original discoverers of the cluster have pointed to the most likely factor—namely, the administration of stilbestrol to the mothers of these patients early in pregnancy. They had been treated between 1946 and 1951, a period when stilbestrol was being used for the therapy of repeated or threatened abortion. A confirmed history of this association has been elicited in seven of the eight patients so far studied, but in none of 32 carefully matched controls.

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Although the authors have been conservative in reporting their observations as merely "an association," the epidemiologic evidence indicates a direct etiologic relation similar to other known congenital effects, such as maternal German measles as a cause of congenital rubella syndrome and thalidomide as a cause of phocomelia. The relation will be confirmed if additional cases associating this rare disease with maternal stilbestrol administration are reported.

The scientific implications of this discovery are evident. Presumably, the mechanism involves some derangement in the early development of the female urogenital tract at some critical phase, as yet unknown. The rare condition, vaginal adenosis, was present in five of the seven stilbestrol-associated cases and may well be an important stage in the evolution of the malignant process.

\* \* \*

If the findings of this one group of workers is confirmed by others, it will become evident that the use of stilbestrol in pregnancy or even in suspected pregnancy will be contraindicated. Furthermore, until the pathogenesis of the effect has been established, it seems prudent for physicians to use caution in prescribing estrogenic substances during pregnancy. Indeed, physicians must think more seriously before administering any drug to a pregnant woman.

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As the authors take care to point out, the extent and seriousness of the problem cannot

now be defined. The occurrence of the disease may not be limited to adolescence. Early lesions may be developing in prepubertal girls. Additional cases may continue to appear as the main cohort of exposed women, now in their early maturity, continue to experience normal cyclic endocrine stimuli through the menstrual cycle and pregnancy. Only when the data on the frequency of vaginal adenocarcinoma and adenosis in relation to maternal stilbestrol and other estrogenic therapy become available throughout the country, and, indeed, throughout the world, can the scope of the problem be measured.

In the meantime, physicians may be besieged by patients worried about the welfare of their daughters. At present writing they may be assured that the risk is extremely low. Although accurate data are not available on the number of women who received stilbestrol during pregnancy, reasonable estimates put the figure in the many thousands. Thus, the indicated risk to any individual girl is slight as has been emphasized by the authors. Nevertheless, it seems clear that all women, particularly girls entering menarche, who have persistent irregular bleeding should receive a careful intravaginal inspection to rule out possible neoplastic disease, not only of the uterus but also of the vagina.

## NOTE

JUDAH FOLKMAN  
TRANSPLACENTAL CARCINOGENESIS BY  
STILBESTROL\*

\* \* \*

The fact that in mothers exposed to stilbestrol, neoplasms of the breast or genital tract did not develop after the same latent period when tumors appeared in their daughters may be explained by the exquisite sensitivity of fetal and neonatal tissues to carcinogens. For example, in mice fed griseofulvin, hepatomas develop. On a total dosage basis, tumors will appear in the infant mouse with only 1/4000th of the dose required to produce the same tumor in the adult mouse.

\* \* \*

By avoidance of the prescription of stilbestrol to pregnant women, this unusual cancer may be prevented in the future. But more worrisome is stilbestrol residue in meat. Of 40,000,000

\* 285 *New England Journal of Medicine* 404-405 (1971). Reprinted by permission.

cattle slaughtered in this country each year, 30,000,000 have been fed stilbestrol to increase their weight. This practice began about 1954. The surveillance program of the Department of Agriculture reveals that the finding of residue is infrequent. Since the fetus is so much more vulnerable to minute doses of carcinogen, there is no way of judging the risk of stilbestrol residue that remains undetected by the current government assay method. Advocates of the federal agricultural policy who believe that the present surveillance system is satisfactory might argue that stilbestrol ingestion is not any more dangerous than the contraceptive pill or the high levels of estrogens appearing in maternal blood during pregnancy. The argument is weak; there is an essential difference. Stilbestrol is a synthetic non-steroidal estrogen known to be carcinogenic. Both maternal estrogen and the synthetic estrogens used in contraceptive pills are steroidal. Sweden and other countries have already banned the feeding of stilbestrol to cattle.

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d.

**A. Bradford Hill, John Marshall, and  
David A. Shaw  
A Controlled Clinical Trial of Long-Term  
Anticoagulant Therapy in Cerebrovascular  
Disease\***

\* \* \*

In cerebrovascular disease the effect of anticoagulant therapy upon a recently established lesion is not entirely predictable. Thus it has been suggested that anaemic infarcts may become haemorrhagic under the influence of anticoagulants, and experimentally induced infarction in animals has been shown to behave in this way. On the other hand [some investigators] have failed to observe this effect in their experiments, nor can we be certain that experimentally induced infarction necessarily parallels naturally occurring thrombotic occlusion. Apart from the particular case of cerebrovascular disease, the precise mode of action of anticoagulant drugs, and their influence on the course of occlusive vascular disease generally, are not fully understood, and consequently the definition of "effective anticoagulation" is debatable. Furthermore, the treatment carries a risk of haemorrhagic complications; the frequency of these varies in

different reported series, but their occurrence has always to be weighed against the possible benefit which the treatment may confer. The history of the use of anticoagulants in disease of the coronary arteries, which constitute a vascular system less complex than the cerebral arteries, bears out the reluctance of these drugs to yield to clinical appraisal.

In spite of all these foreseeable difficulties, there is a need in cerebrovascular disease for a trial of a form of therapy which theoretically might improve the outlook for the immense number of patients for whom, at present, we have so little to offer. In the presence of so many unpredictable factors the only satisfactory approach is by a strictly controlled clinical trial, in which the progress of patients receiving the treatment is compared with that of a similar group not so treated, but managed in the same way in all other respects over the same period of time. This we have endeavoured to do in our present study of the place of long-term anticoagulant therapy in the treatment of cerebrovascular disease.

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In 1958 Millikan, Siekert, and Whisnant reported their experience in a much larger group of patients, totalling 317, all of whom received anticoagulants. . . . Treatment appeared to confer striking benefit in stopping ischaemic episodes in the "insufficiency" groups and in reducing, in the "thrombotic" groups, the mortality anticipated on the basis of the authors' observations of untreated patients. . . .

\* \* \*

There is thus a considerable weight of evidence to support the use of anticoagulants in chronic cerebrovascular disease, yet none of the studies referred to fulfill the criteria required in a strictly controlled trial. In particular, the use of the patient as his own "control" is regarded as unsatisfactory in a disease in which we have so little knowledge of the natural history, and in which the course is so variable and unpredictable. Furthermore, most authors have confined their attention to certain sharply defined diagnostic categories of cerebrovascular disease, though the issue that confronts the general physician, to whom the vast majority of patients with cerebrovascular disease is referred, is a broader one.

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. . . For these reasons we have based our study on a broader plan, by including a wide

\* 29 *Quarterly Journal of Medicine* 597-609 (1960). Reprinted by permission of the Clarendon Press, Oxford.

range of cerebrovascular cases commonly encountered in general medical practice. We have attempted to answer the questions as to whether or not long-term anticoagulant therapy (1) increases the expectation of life, or (2) decreases the incidence of further cerebrovascular accidents, or (3) influences the functional capacity of patients with cerebrovascular disease in general, or in specific sub-groups thereof. In the present paper only the first two considerations will be discussed.

\* \* \*

The trial was stopped in its present form earlier than had been anticipated, because of the emergence of a disturbing picture. Non-fatal cerebrovascular accidents were distributed about equally to the two groups (5 to 4), and so were deaths due to unrelated causes (3 to 2). On the other hand, the haemorrhagic fatalities that might be due to the treatment were very unevenly divided (5 to 0). While this difference does not quite reach the 0.05 level of significance, it so closely approaches it as to pose a serious ethical problem. It may well be that long-term anticoagulant therapy in patients with cerebrovascular disease carries a significant hazard of cerebral haemorrhage.

\* \* \*

On the basis of some of the results already reported from the United States, when this trial was set up it might perhaps have been argued that it was unethical to withhold the treatment from half the patients. The wheel has turned so far that we feel it is unethical to proceed with the treated group without making the modification described. Although this result has emerged from the mortality figures, no conclusion can be drawn from the comparison of the non-fatal recurrence rates in the two groups. The low incidence of further non-fatal cerebrovascular accidents in the high-dosage group regarded in isolation might well give rise to the clinical impression that treatment had been beneficial. Yet comparison of the results with those in the low-dosage group clearly shows that this is not so. Indeed, the recurrence rate in the latter group is so low over the period of follow-up that any form of treatment would require to be almost one hundred per cent effective, and devoid of serious hazard, before it could claim to be of definite value. A longer period of follow-up, however, will be required before this point can be settled.

In conclusion, the present study strongly in-

dicates that the general use of anticoagulant therapy in patients with cerebrovascular disease, who are selected and managed along the lines adopted in this trial, is hazardous, because of the risk of cerebral haemorrhage. This risk is present even when the anticoagulant therapy is carefully controlled, and the prothrombin time maintained at a level which is generally accepted to be safe. It may be that certain restricted types of cerebrovascular disease gain benefit from anticoagulant therapy, but, in view of the many variable factors present in the condition, this can be ascertained only by properly designed and strictly controlled clinical trials.

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## 9.

### Interests of Profession?

a.

#### Herbert C. Kelman Human Use of Human Subjects— The Problem of Deception in Social Psychological Experiments\*

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The use of deception has become more and more extensive, and it is now a commonplace and almost standard feature of social psychological experiments. Deception has been turned into a game, often played with great skill and virtuosity. A considerable amount of the creativity and ingenuity of social psychologists is invested in the development of increasingly elaborate deception situations. . . .

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It is easy to view this problem with alarm, but it is much more difficult to formulate an unambiguous position on the problem. As a working experimental social psychologist, I cannot conceive the issue in absolutist terms. I am too well aware of the fact that there are good reasons for using deception in many experiments. There are many significant problems that probably cannot be investigated without the use of deception, at least not at the present level of development of our experimental methodology. Thus, we are always confronted with a conflict

\* 67 *Psychological Bulletin* 1-9 (1967). © 1967, American Psychological Association. Reprinted by permission.

of values. If we regard the acquisition of scientific knowledge about human behavior as a positive value, and if an experiment using deception constitutes a significant contribution to such knowledge which could not very well be achieved by other means, then we cannot unequivocally rule out this experiment. The question for us is not simply whether it does or does not use deception, but whether the amount and type of deception are justified by the significance of the study and the unavailability of alternative (that is, deception-free) procedures.

I have . . . special concern about second-order deceptions, for example, the procedure of letting a person believe that he is acting as experimenter or as the experimenter's accomplice when he is in fact serving as the subject. Such a procedure undermines the relationship between experimenter and subject even further than simple misinformation about the purposes of the experiment; deception does not merely take place *within* the experiment, but encompasses the whole definition of the relationship between the parties involved. Deception that takes place while the person is within the role of subject for which he has contracted can, to some degree, be isolated, but deception about the very nature of the contract itself is more likely to suffuse the experimenter-subject relationship as a whole and to remove the possibility of mutual trust. Thus, I would be inclined to take a more absolutist stand with regard to such second-order deceptions—but even here the issue turns out to be more complicated. I am stopped short when I think, for example, of the ingenious studies on experimenter bias by Rosenthal and his associates. These experiments employed second-order deception in that subjects were led to believe that they were the experimenters. Since these were experiments about experiments, however, it is very hard to conceive of any alternative procedures that the investigators might have used. There is no question in my mind that these are significant studies; they provide fundamental inputs to present efforts at reexamining the social psychology of the experiment. These studies, then, help to underline even further the point that we are confronted with a conflict of values that cannot be resolved by fiat.

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What concerns me most is not so much that deception is used, but precisely that it is used without question. It has now become standard operating procedure in the social psychologist's

laboratory. I sometimes feel that we are training a generation of students who do not know that there is any other way of doing experiments in our field. . . .

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A basic assumption in the use of deception is that a subject's awareness of the conditions that we are trying to create and of the phenomena that we wish to study would affect his behavior in such a way that we could not draw valid conclusions from it. For example, if we are interested in studying the effects of failure on conformity, we must create a situation in which the subjects actually feel that they have failed, and in which they can be kept unaware of our interest in observing conformity. In short, it is important to keep our subjects naive about the purposes of the experiment so that they can respond to the experimental inductions spontaneously.

How long, however, will it be possible for us to find naive subjects? Among college students, it is already very difficult. They may not know the exact purpose of the particular experiment in which they are participating, but at least they know, typically, that it is *not* what the experimenter says it is. Orne . . . pointed out that the use of deception "on the part of psychologists is so widely known in the college population that even if a psychologist is honest with the subject, more often than not he will be distrusted." As one subject pithily put it, "Psychologists always lie!" Orne added that "This bit of paranoia has some support in reality." There are, of course, other sources of human subjects that have not been tapped, and we could turn to them in our quest for naiveté. But even there it is only a matter of time. As word about psychological experiments gets around in whatever network we happen to be using, sophistication is bound to increase. I wonder, therefore, whether there is any future in the use of deception.

\* \* \*

For several reasons, however, the use of deception especially encourages the subject to dismiss the stated purposes of the experiment and to search for alternative interpretations of his own. First, the continued use of deception establishes the reputation of psychologists as people who cannot be believed. Thus, the desire "to penetrate the experimenter's inscrutability and discover the rationale of the experiment" . . . becomes especially strong. Generally, these ef-

forts are motivated by the subject's desire to meet the expectations of the experimenter and of the situation. They may also be motivated, however, as I have already mentioned, by a desire to outwit the experimenter and to beat him at his own game, in a spirit of genuine hostility or playful one-upmanship. Second, a situation involving the use of deception is inevitably highly ambiguous since a great deal of information relevant to understanding the structure of the situation must be withheld from the subject. Thus, the subject is especially motivated to try to figure things out and likely to develop idiosyncratic interpretations. Third, the use of deception, by its very nature, causes the experimenter to transmit contradictory messages to the subject. In his verbal instructions and explanations he says one thing about the purposes of the experiment; but in the experimental situation that he has created, in the manipulations that he has introduced, and probably in covert cues that he emits, he says another thing. This again makes it imperative for the subject to seek his own interpretation of the situation.

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. . . I have already stressed that I would not propose the complete elimination of deception under all circumstances, in view of the genuine conflict of values with which the experimenter is confronted. What is crucial, however, is that we always ask ourselves the question whether deception, in the given case, is necessary and justified.

\* \* \*

[My] final suggestion is that we invest some of the creativity and ingenuity, now devoted to the construction of elaborate deceptions, in the search for alternative experimental techniques that do not rely on the use of deception. . . .

## NOTES

### NOTE 1.

JOHN LOFLAND  
REPLY TO DAVIS\*

\* \* \*

. . . Mr. Davis [in response to an experiment reported by Mr. Lejeune and myself]

\* 8 *Social Problems* 365-367 (1961). Reprinted by permission. [The author no longer adheres to the views expressed in this NOTE. His present position is reflected in the following NOTE.]

writes that A.A. members will possibly be unhappy with our experiment and as a result make it difficult to get their cooperation in future research. This is a conceivable outcome in any field research, and one upon which we always take a chance. Mr. Davis' statement, in this case, assumes two things, both of which are unlikely. First, that A.A. members will read the report and, second, that they will be unhappy about what is reported. Concerning the former, the article was not written or reported to facilitate viewing by A.A. members; it is presented in scientific writing and appears in a professional journal, both of which very effectively limit its audience. As to the latter, if some members do read it, it is not evident that the reaction will be "indifferent," or as Mr. Davis means, hostile. In fact, personally, quite contrary to Mr. Davis' personal feelings, I judge the experiment to be favorable to A.A., and do not think that members will necessarily judge it any differently. . . .

\* \* \*

It is generally agreed that our obligation to science is the objective, full and unbiased execution and reporting of observation. It is also generally agreed that our obligation to the profession is to conduct research in ways that will not injure the general reputation of the profession or thwart subsequent access to research settings and thus hinder the development of the discipline. In his letter, Mr. Davis does not confine himself to his duty of upholding *these* obligations. He almost exclusively devotes his attention to extrascientific and extraprofessional standards because they appear to be different than his own. We all recognize that such statements are not, and cannot, be made as a scientist or professional; they are simply not part of the legitimate criteria for judging research. Therefore, as scientists and professionals, we have no reason to be concerned with Mr. Davis' opinions of our personal moral standards; but this is not to say that personal standards are unimportant, quite the reverse as I will indicate below.

Beyond the fact that protestations like this are not relevant, except to one's self, and that they are rather presumptuous, there is, for those who would persist, the important problem of ever generalizing a personal standard and holding it nonviolable for the profession or, indeed, for one's self.

The difficulties involved in asserting immutable professional standards of conduct are clearly illustrated in Mr. Davis' personal stand-

ard that one should not study groups unless they know about it and give their permission. A professional rule to this effect would not only make for great past, present and future loss to the discipline, but would be an active violation of many people's moral standards who think that there are some groups, such as professional crime and fascist groups, that should be studied whether they are asked and give permission or not. In other words, in accepting this rule, we could not study "bad" groups, which, as it happens, are also especially likely to be "groups that do not want to be studied." Furthermore, conceivably, it might be important enough to the discipline to justify studying a group even though the particular group refuses. I suspect that Mr. Davis, in taking a second look, would agree.

\* \* \*

[O]ur personal moral obligations and conflict of roles should not, and probably could not, be determined and/or solved by the intervention of the profession at large or other professionals speaking personally. It is probably an inescapable and insoluble part of the attempt to be both a scientist and a person in human groups that one must suffer the pains so eloquently portrayed by Mr. Davis, and that he must make his personal moral decisions alone, based on the situation. In addition, it is also doubtful that any amount of pleading for others to adopt one's own standards or prolonged demands to the profession for an edict of relief alters one's own, or others', personal dilemmas. The most legitimate and workable solution for the profession would appear to be the one we already have: each man works out, as best he can, his own, as Mr. Davis has so aptly captured it, "moral integration."

NOTE 2.

JOHN LOFLAND  
DEVIANCE AND IDENTITY\*

\* \* \*

[T]here are . . . difficulties involved in the very character of the empirical materials classified under the topic of deviance. Deviance, by definition, is disapproved, and it therefore occurs in secret or at least is carried on in a very circumspect manner. Moreover, the study of episodic or isolated deviant acts is seriously ham-

pered by their unpredictability. How, for example, would one arrange to study interaction scenes of homicide? There is little doubt that sound and film records of the interaction in the hours before the occurrence of acts of assault, rape, robbery, embezzlement or homicide would be of enormous value in understanding the constraining character of immediate space-time-bound locales. But it is less than clear how one is to know where and when to be in order to make such observations. Even if the "who, where and when" were foreknown, it is less than clear how one would arrange to be present without changing the entire situation and conducting some different outcome. . . .

\* \* \*

[T]he moral implications of the methodological requirements of full, precise, systematic knowledge of deviance convince me that the moral price we would have to pay for full knowledge is much higher than the value of that knowledge. I, for one, would not be willing to condone—in fact, would actively oppose—the initiation of a series of precise experimental manipulations aimed at sorting out the exact effects of the various factors involved in the reconstitution of personal identity. Nor could I countenance the invasion of privacy that would be required for the natural-setting study of homicide, embezzlement or other deviant acts. The "research" activities of Nazi Germany taught us (or should have) very well that there are definite moral limits on what can be done in the name of science. In matters as fundamental as identity change and deviance, we must, as moral men who happen also to be social scientists, make what we can of the variations that occur in the natural world and that we can discover without undue invasions of privacy. We must hold in check our impulse to implement our research technology in ways that violate our beliefs about the essential dignity and inviolate character of human beings. And too, a variety of episodes in the 20th century occurring under totalitarian (and not so totalitarian) regimes have made quite clear the untenable character of arguments which justify some present condition of human debasement, degradation, suffering and even death for the sake of "enormous" benefits that will accrue to future generations—whether these benefits are to derive from knowledge gained or from something else. Ironically, of course, some such claims of future good based on present evil are probably true. But their truth or falsity is quite beside the point.

\* Englewood Cliffs: Prentice-Hall, Inc. 300-301 (1969). © 1969. Reprinted by permission of Prentice-Hall, Inc., Englewood Cliffs, N.J.

The point is, rather, that, with few exceptions (as perhaps when the future ends are quite concrete and immediate and as when the present suffering is voluntarily undertaken), the sacrifice of human beings for laudable ends is not morally acceptable.

b.

**Margaret Mead**  
**Research with Human Beings—A Model**  
**Derived from Anthropological Field Practice\***

In recent years there have been a great many experiments and investigations reported in which deliberate falsification has been introduced. Instructed stooges have been directed to deny sensory evidence, or to mimic pain that they did not feel, or to obstruct situations planned by their peers. Investigators have posed as possible converts to flying-saucer cults. Under the guise of "participant observation," various forms of "cover" have been developed for social investigators, which have later been revealed to the public in the reports on the experiments. These are, I believe, all deeply unsatisfactory in several ways:

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[P]erhaps even more serious than the effect upon the subject or object or unwarned collaborator, which in most cases is brief and transitory, is the effect upon the investigator himself. Ethically, it means that he becomes accustomed to tricking, deceiving, and manipulating other human beings and, to that extent, to denigrating their humanity. Besides the ethical consequences that flow from contempt for other human beings, there are other consequences—such as increased selective insensitivity or delusions of grandeur and omnipotence—that may in time seriously interfere with the very thing which he has been attempting to protect: the integrity of his own scientific work. Encouraging styles of research and intervention that involve lying to other human beings therefore tends to establish a corps of progressively calloused individuals, insulated from self-criticism and increasingly available for clients who can become outspokenly cynical in their manipulating of other human beings, individually and in the mass.†

\* 98 *Daedalus* 361, 374–377 (1969). Reprinted by permission of *Daedalus*, Journal of the American Academy of Arts and Sciences, Boston, Massachusetts.

† Here again, we should mention the other effect. Instead of being cynical about his manipula-

Both of these undesirable consequences are prevented to some extent by the honest belief that the deception is absolutely necessary to the conduct of an experiment and that the experiment itself, must be performed. Arguments of this sort can be advanced for both laboratory and field tests of new drugs: Elimination of other factors of suggestion and belief must be made by the use of placebos, and so forth. When astronauts are being selected, it is vital to know their ability to withstand various kinds of strain, and this can only be found out by tests involving deception of various sorts, such as simulation, secret observation by long-distance TV, concealed indicators, and so forth. The ethical difficulties involved in even these situations are attested to by the number of cases where the experimenter with a new drug, for example, will insist on trying it on himself first.

Many of the situations where concealment is genuinely necessary and the experiment cannot be performed in some other way can be handled by general assent from the subjects, who know that they are agreeing to being deceived for the purposes of the experiment itself. Individuals who have particular diseases, conscientious objectors who wish to make a contribution to human welfare, candidates for dangerous secret activities or especially exacting forms of warfare or exploration, and students of psychology may volunteer to participate in activities where they consciously abrogate some of the dignities and freedoms that are associated with the status of a fully healthy, free citizen of a free society. The medical profession demands this of many patients, lawyers demand it of clients—"just put yourself in my hands and trust my judgment"—priests demand it of penitents, and parents demand it of their children. Such a status is not ennobling; it may be necessary; and it can be defined so that consent is given in such a general way that the particularities of placebos, stooges,

tions, the experimenter or interventionist who deceives may also take refuge behind the great good that he is doing to the subject as a patient or to other men who will benefit from his activities—in developing a new drug or writing advertising copy that will induce people to spend their money more wisely. Here the need to justify demeaning other human beings, in particular respects, is compensated for by another kind of delusion of grandeur: that of becoming someone who benefits mankind on a large scale, the omnipotent theorist, the all-knowing teacher, the scientist working for the intelligence agency of a particular government or conspiring with some secret plot to save the world.



and fabricated situations are left intact to test the physiological or psychological behavior of the subject. But the crucial question must be whether the deception is absolutely necessary in order to perform an experiment which is itself necessary. By the automatic inclusion of deception in a research or treatment plan, many research workers are simply relieved of any obligation to make new research designs which would not involve any deception at all.

c.

**Kai T. Erikson**  
**A Comment on Disguised Observation**  
**in Sociology\***

\* \* \*

[Another] problem with disguised observation . . . has to do with the sociologist's responsibilities to his colleagues. It probably goes without saying that research of this sort is liable to damage the reputation of sociology in the larger society and close off promising areas of research for future investigators. . . . And it is also true in the wider sense that any research tactic which attracts unfavorable notice may help diminish the general climate of trust toward sociology in the community as a whole. So long as this remains a serious possibility, the practice of disguised observation becomes a problem for everyone in the profession; and to this extent, it is wholly within the bounds of professional etiquette for one sociologist to challenge the work of another on this score.

This objection has been raised several times before, and the answer most often given to it is that the people who are studied in this fashion—alcoholics or spiritualists or mental patients, for example—are not likely to read what we say about them anyway. Now this argument has the advantage of being correct a good deal of the time, but this fact does not prevent it from being altogether irrelevant. To begin with, the experience of the past few years should surely have informed us that the press is more than ready to translate our technical reports into news copy, and this means that we can no longer provide shelter for other people behind the walls of our own anonymity. But even if that were not the case, it is a little absurd for us to claim that we derive some measure of protection from the narrowness of our audience when we devote so much time trying to broaden it. The fact is that

we are increasingly reaching audiences whose confidence we cannot afford to jeopardize, and we have every right to be afraid that such people may close their doors to sociological research if they learn to become too suspicious of our methods and intentions.

The [next] objection to be raised here, if only as a note in passing, concerns the responsibilities the profession should accept toward its students. The division of labor in contemporary sociology is such that a considerable proportion of the data we use in our work is gathered by graduate students or other apprentices, and this proportion is even higher for research procedures that require the amount of energy and time necessary for participant observation. Of the dozen or more observers who took part in the studies I have cited, for example, all but one was a graduate student. Now a number of sociologists who have engaged in disguised observation have reported that it is apt to pose serious moral problems and a good deal of personal discomfort, and I think one might well argue that this is a heavy burden to place on any person who is, by our own explicit standards, not yet ready for professional life. I am not suggesting here that students are too immature to make a seasoned choice in the matter. I am suggesting that they should not be asked to make what one defender of the method has called "real and excruciating moral decisions" while they are still students and presumably protected from the various dilemmas and contentions which occupy us in meetings like this—particularly since they are so likely to be academically, economically, and even psychologically dependent upon those elders who ask them to choose.

The [last] objection I would like to raise here about the use of undercover observation is probably the most important—and yet the most remote from what is usually meant by the term "ethics." It seems to me that any attempt to use masquerades in social research betrays an extraordinary disrespect for the complexities of human interaction, and for this reason can only lead to bad science. Perhaps the most important responsibility of any sociologist is to appreciate how little he really knows about his intricate and elusive subject matter. We have at best a poor understanding of the human mind, of the communication signals that link one mind to another, or the social structures that emerge from those linkages—and it is the most arrant kind of over-simplification for us to think that we can assess the effect which a clever costume or a few

\* 14 *Social Problems* 366, 368–371 (1967). Reprinted by permission.



studied gestures have on the social setting. The pose might "work" in the sense that the observer is admitted into the situation; but once this passage has been accomplished, how is he to judge his own influence on the lives of the people he is studying? . . .

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. . . It may be possible for a trained person to rearrange the slant of his body and re-set his facial muscles to approximate the bearing of someone else, but his performance will never be anything more than a rough imposture. Now we know that these various physiological, linguistic, and kinetic cues play an important part in the context of human interaction, but we have no idea how to simulate them—and what is probably more to the point, we never will. For one thing, we cannot expect to learn in a matter of hours what others have been practicing throughout a lifetime. For another, to imitate always means to parody, to caricature, to exaggerate certain details of behavior at the expense of others, and to that extent any person who selects a disguise will naturally emphasize those details which *he* assumes are most important to the character he is portraying. In doing so, of course, he is really only portraying a piece of himself. It is interesting to speculate, for example, why the Air Force lieutenant mentioned earlier thought he needed to present himself as a near-delinquent youth with a visible layer of personal problems in order to pose as an enlisted man. Whatever the reasoning behind this particular charade, it would certainly be reasonable for someone to suspect that it tells us more about the investigators' impression of enlisted men than it does about the men themselves—and since we have no way of learning whether this is true or not, we have lost rather than gained an edge of control over the situation we are hoping to understand. What the investigators had introduced into the situation was a creature of their own invention, and it would be hardly surprising if the results of their inquiry corresponded to some image they had in advance of the enlisted man's condition. (It is perhaps worth noting here that impersonation always seems easier for people looking down rather than up the status ladder. We find it reasonable to assume that officers "know how" to portray enlisted men or that sociologists have the technical capacity to pose as drunks or religious mystics, but it is not at all clear that the reverse would be equally true.)

\* \* \*

## NOTE

MARGARET MEAD  
THE CASE OF JOHN HOWARD GRIFFIN,  
AUTHOR OF *Black Like Me*\*

During [a] discussion . . . the question was raised as to whether John Howard Griffin's darkening his skin by a biochemical method so that he would be taken for a Negro and be able to explore the treatment of Negroes in the Southeast was an inadmissible type of deception. . . . It is an interesting, perhaps crucial, case in many ways.

John Howard Griffin is a novelist, who became blind during the early days of World War II, when he had to abandon a medical career and became actively involved in helping Jewish children escape from the Nazis. He returned, completely blind, to Texas with his family and lived on a small farm for twelve years, where he wrote six successful novels. In 1957, he regained his sight and was horrified, when he could *see*, to realize how close the caste relationships in the South were to the racial attitudes that he had abhorred when practiced by the Nazis. His attention became fixed on the question of color and the responses which white and black people made to color. In order to explore this more thoroughly, he took a treatment that darkened his skin, was highly dangerous, and, in the form in which he took it, did him some irrevocable harm although the skin coloration was reversible. He then traveled through the South, finding out not what it was like to be a Negro, but how the indignities heaped on anyone suspected of being a Negro, and the help freely given by Negroes to others with a black skin, looked to a white man reared as he had been, protected from this knowledge.

As a commentary on this case I would say: (1) he was not a scientist but a writer with an ethical mission searching for material, as writers do search for material; (2) he took tremendous risks both with his personal safety, his sanity, and ultimately, after the book was published, with the life and the safety of his family—far out of proportion to any risk to which he exposed any of the people whom he met; (3) as his goal was to probe a situation—as a white man taken

\* 98 *Daedalus* 381-382 (1969). Reprinted by permission of *Daedalus*, Journal of the American Academy of Arts and Sciences, Boston, Massachusetts.

for a black man—his means were completely appropriate to his ends. Individuals whom he deceived in passing were not experimental subjects or part of any experimental situation, nor were they identified in a way through which they could even recognize themselves again.

I believe this case falls quite outside the range of those scientific explorations which I have condemned, in which (1) the deception is unnecessary, (2) is damaging to subject and experimenter, and (3) when revealed is damaging to the trust of the public in scientists as such.

## 10.

### Interests of Society?

a.

#### Julius Seeman Deception in Psychological Research\*

\* \* \*

[T]he central dilemma posed by the use of deception . . . is the conflict between the rights of the individual and the needs of society. Those who justify the use of deception argue that the accumulation of scientifically derived knowledge sometimes exacts a price from individuals, and that this knowledge is worth the price. What does our society have to say on this point? The democratic ethic reflects continuing tensions generated by conflicts between individual and collective good. These conflicts rarely fail to take account of the central position that the rights of the individual hold in our social system. Even where there is collective danger, individual rights have a pivotal position. For example, our laws provide that even in time of war a person conscientiously opposed to military service may undertake civilian service.

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. . . It is possible that the most correct position with regard to the use of deception may turn out to be an absolutist position. Such a position would advance the ends-means argument; namely, that the outcome of any process is inexorably embedded in the means used. Thus, a process that used deceptive means could not lead to a "good" end.

There is much to be said for such a view, both ethically and pragmatically. In learning ex-

periments, for instance, it is clear that the end point of a learning curve is lawfully related to antecedent properties. In psychotherapy, the outcome is simply the end point of a process continuum. In an analogous way, the end point of an experience in which deception is embedded is functionally related to the deception process.

The ends-means view happens to be the view I hold. But one does not need to take an absolutist position in order to have reservations about the ethics of deception. One could argue that there are instances of innocuous deception that have no adverse consequences. This still leaves the question open with regard to other studies. The ethical issue is drawn most sharply in those studies that use noxious deception. . . .

\* \* \*

There is one final consideration with respect to the use of deception. It is a difficult point to make because it requires a reexamination of the psychologist's most basic assumptions about his discipline. An empirical discipline takes for granted that the primary goal of its efforts is the accumulation of knowledge. Psychologists have been trained to a finely honed position with regard to empirical processes and they have learned to value knowledge above all else. Thus it is scarcely possible to argue for the importance of a nonempirical process. Yet that is what I propose. I suggest that the ultimate goal of the scientific enterprise ought to be not knowledge but wisdom. Knowledge alone has a neutrality that can be deadly in our time. It can destroy as well as create. The existence of a Hiroshima in man's history demonstrates that knowledge alone is not enough, and that the old question of "knowledge for what?" must still be asked. If knowledge in psychology is won at the cost of some essential humanness in one person's relationship to another, perhaps the price is too high.

## NOTE

DIANA BAUMRIND  
PRINCIPLES OF ETHICAL CONDUCT IN THE  
TREATMENT OF SUBJECTS\*

\* \* \*

There are no absolute principles of good and evil, and this all major systems of morality and mythic representations acknowledge. More-

\* 24 *American Psychologist* 1025, 1026-1028 (1969). Reprinted by permission.

\* 26 *American Psychologist* 887, 890 (1971). © 1971, American Psychological Association. Reprinted by permission.

over, there is no human activity that under certain circumstances, settings, times, and places could not be viewed as good and under other circumstances as evil. But a given action under a given set of circumstances can be judged as good or evil; else what is the significant sphere of a code of ethics? It is my judgement that the research laboratory is the wrong place for Machiavellian encounters, and that this is the wrong time. I would judge many of [the experimenter's] actions as evil, for the precise reason that they are committed by a professional or scientist in the conduct of his work against an individual who has accepted an invitation to be a research subject. There is a time and place for deceit, inflicting pain, and putting other people down. But the psychologist's laboratory is not the place, and this is certainly not the time. . . . I am among those who believe that by such practices "behavioral research is contributing to the moral ills of society and that the influence is a direct one". . . . The influence of the research psychologist, such as it is, great or small, should be used as a positive moral vector and not a negative one. The research psychologist has many privileges not possessed by other people with whom the subject deals, and these privileges are granted to him on the assumption that he will be responsible, trustworthy, and altruistic in the conduct of his professional life. Fundamental moral principles of reciprocity and justice are violated when the research psychologist, using his position of trust, acts to deceive or degrade those whose extension of trust is granted on the basis of a contrary role expectation. It is unjust to use naïve, that is, trusting subjects, and then exploit their naïveté, no matter if the directly resulting harm is small. The harm is cumulative to the individual and society. At this time and in this place it is "evil" for research psychologists in pursuit of professional objectives to contribute an iota to "the attrition of human relationships in depersonalization and distrust," and the research enterprise does not intrinsically require that they do so.

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b.

**Joseph H. Fichter and William L. Kolb**  
**Ethical Limitations on Sociological**  
**Reporting\***

. . . Real urgency must be defined in terms of the pressing needs of a group, community, or

\* 18 *American Sociological Review* 544, 550 (1953). Reprinted by permission.

society, or in terms of some impending problem of which the scientist but not the group or community being studied is aware. Rights and duties are never unqualified in society and one of the qualifications seems to be that the society sometimes has a prior right to information which is necessary and useful for itself even though it may be harmful to an individual or sub-group.

The social scientist may find himself in one of several moral situations when he is trying to determine whether or not the social need is greater than the individual or group right. If the duly appointed authorities of a community or of the larger society believe certain information to be vitally needed, there is a *prima facie* case for the scientist to reveal such information. However, these authorities must show to the scientist the ground for the need. If he does not know and cannot find out from the authorities whether there is an urgent need for certain data which will be harmful to individuals and sub-groups, he is free of moral obligation to reveal it. If he is certain that the information is not necessary, he may in good conscience refuse to reveal it even though the authorities demand to know it. It must be recognized that this freedom in such instances is moral and not legal, and he may have to pay a price for his refusal.

In a similar manner the obligations which the scientist has to the group studied may require the revelation of information damaging to individuals or sub-groups. In this instance the scientist himself is likely to be the best judge of the need for his data. If he understands and accepts the basic values of the group and takes his obligation to the group seriously, he may find it imperative to disclose such information. Since he cannot plead ignorance, and since there is no demand from competent higher authority, the responsibility for the assessment of urgency rests squarely on the scientist.

Finally, even though neither the higher authority nor the representatives of the group studied place any demands upon him, he may become aware of facts which are vitally needed by the social group studied or by the society. In such cases he must not only accept the responsibility for violating the rights of individuals and groups, but also must arrive at his decision with very little outside aid. In clear-cut instances where the comparison and balancing of the rights of the various claimants can be easily accomplished, the decision may be easily reached. But it is certainly in this area that the researcher will be forced to consider most thoroughly the im-

portance which he, himself, has placed on the value of the information in its relation to the needs of the group.

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### NOTES

#### NOTE 1.

ROSS STAGNER  
PROBLEMS CONCERNING FEDERAL SUPPORT  
OF SOCIAL SCIENCE RESEARCH\*

\* \* \*

No one has ever reported that an atom protested invasion of its privacy when bombarded by a particle accelerator. This type of research handicap is unique to investigations focused on the behavior of human beings. A man may plausibly object to questions about the violence of his temper, his impulses to attack and destroy others, his hostility toward minority groups, and so on. Yet the real hazard to society, and indeed to our civilization, rests not in the atom bombs but in the violence and aggression within human beings who can trigger these bombs.

Social scientists, of course, have a genuine obligation to devise protections for the right of privacy, and to avoid mere psychic voyeurism. At the same time they have a compelling obligation to accumulate data—and meaningful generalizations—about the powerful impulses of loyalty, hostility, fear, and ambition which shape human history. It seems to me that, on the whole, the record of the social scientists in this area of protecting privacy has been quite good. Ethical standards have been codified and students in these fields have been trained, as in medical and legal training, to observe these standards. Psychologists, have, I think, done a very good job in this regard. Renewed attention to this matter in recent years guarantees, according to most scholars, that researchers will exercise extreme caution in this respect.

\* \* \*

There is an obvious conflict between the need of society to know and the right of the individual to dignity and privacy. The familiar analogy is that of medicine. For a thousand years or more, religious taboos forbade dissection of a

dead body. This prohibition was based on an appeal to the dignity of the individual, and the right of his relatives to know that his body was not violated. The consequence was that medical research and medical practice were blocked. It was only when a few brave souls defied the taboo and began such research that modern medicine began its remarkable advances. Clearly the scientists are under heavy obligation to avoid research techniques which endanger the health of their subjects; but there is also some need to consider the potential lifesaving and health-bringing developments in this equation. The medical profession has only recently been compelled to take a new look at this question. The social sciences must also consider it carefully.

My point in raising the medical analogy is that great social dangers cry for investigations which may be blocked by excessive emphasis on the right of privacy. Consider the case of the rapist, the violent criminal. As a youth he may certainly object to "prying questions" which might reveal his explosive, destructive, antisocial tendencies. Yet society is clearly entitled to look for measures to protect women from his hostile sexuality. A loaded way to phrase this question is to ask how we balance the right of the young man to privacy against the right of the young woman to walk safely in the streets. A more defensible question is: How can social scientists gather the data which we so desperately need, the basic information for the prevention and correction of violent behavior, with proper consideration for the right to privacy?

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My purpose in elaborating on this point about the privacy is also to suggest that people in general, and the Congress in particular, must become aware of the possible conflicts between values, all of which are respected in our society: the value of knowledge and the value of privacy, the value of the individual and the value of society as a whole. When belief in an earth-centered universe was enforced by religious and civil punishment, no progress was made in astronomy. When similar taboos forbade anatomical dissection, no progress was made in medicine. The Congress should, in my opinion, recognize that no one of these is absolute; that the welfare of large numbers of individuals needs to be weighed against the privacy of a few. Obviously I am not suggesting that the proposed National Social Science Foundation should be authorized to underwrite unrestrained and frivolous prying

\* National Foundation for Social Sciences—Hearings on S.836 before the Subcommittee on Government Research of the Senate Committee on Government Operations, 90th Congress, 1st Session 757-760 (1967).

into people's private lives. What must be said is that it will be futile to appropriate large sums of money for social science research if the investigators are forbidden to use psychological procedures which penetrate deeply into the thoughts and emotions of the person studied.

There has been, for example, some concern expressed on the floor of Congress in recent years about the use of personality tests. I hear that they have actually been banned for certain purposes within the Federal Government. This is probably a minor matter, because the validity of most such tests for employment purposes has not been adequately demonstrated. On the other hand, if agencies supporting basic social research should ban such tests, much important work will simply be impossible. Let me mention again the problem of rape and other violent assaults on persons. Our only rational hope is to try to identify the potential criminal and reeducate him before he begins these kinds of activities. Now I do not know how you put a dollar value on the trauma to a young woman who is raped, nor do I know how to put a dollar value on the invasion of privacy of the young men who might be studied in such a research program. But I do believe that, if the Congress is sincere in its desire that advances be made in the prevention of crime, legislation must be drafted in such a way that the research use of personality tests is permissible within the code of ethics of the American Psychological Association.

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#### NOTE 2.

DIANA BAUMRIND  
SOME THOUGHTS ON ETHICS OF RESEARCH—  
AFTER READING MILGRAM'S "BEHAVIORAL  
STUDY OF OBEDIENCE"\*

\* \* \*

[T]he subject is not always treated with the respect he deserves. It has become more commonplace in sociopsychological laboratory studies to manipulate, embarrass, and discomfort subjects. At times the insult to the subject's sensibilities extends to the journal reader when the results are reported. Milgram's study is a case in point. . . .

\* \* \*

Milgram . . . partially explains the subject's destructive obedience as follows, "Thus they as-

sume that the discomfort caused the victim is momentary, while the scientific gains resulting from the experiment are enduring. . . ." Indeed such a rationale might suffice to justify the means used to achieve his end if that end were of inestimable value to humanity or were not itself transformed by the means by which it was attained.

The behavioral psychologist is not in as good a position to objectify his faith in the significance of his work as medical colleagues at points of breakthrough. His experimental situations are not sufficiently accurate models of real-life experience; his sampling techniques are seldom of a scope which would justify the meaning with which he would like to endow his results; and these results are hard to reproduce by colleagues with opposing theoretical views. Unlike the Sabin vaccine, for example, the concrete benefit to humanity of his particular piece of work, no matter how competently handled, cannot justify the risk that real harm will be done to the subject. I am not speaking of physical discomfort, inconvenience, or experimental deception per se, but of permanent harm, however slight. . . .

c.

#### Henry K. Beecher Research and the Individual— Human Studies\*

\* \* \*

When the wonders of penicillin were new, but recognized, and the supply heartbreakingly meager, a small shipment finally arrived in North Africa during World War II. The hospital beds were overflowing with wounded men. Many had been wounded in battles; many had also been wounded in brothels. Which group would get the penicillin? By all that is just, it would go to the heroes who had risked their lives, who were still in jeopardy, and some of whom were dying. They did not receive it, nor should they have; it was given to those infected in brothels. Before indignation takes over, let us examine the situation. First, there were desperate shortages of manpower at the front. Second, those with broken bodies and broken bones would not be swiftly restored to the battle line even with penicillin, whereas those with venereal disease, on being treated with penicillin, would in a matter of

\* 19 *American Psychologist* 421-422 (1964). Reprinted by permission.

\* Boston: Little, Brown and Co. 209-210 (1970). Reprinted by permission.

days free the beds they were occupying and return to the front. Third, no one will catch osteomyelitis from his neighbor; the man with venereal disease remains, until he is cured, a reservoir of infection and a constant threat. In terms

of customary morality, a great injustice was done; in view of the circumstances, I believe that the course chosen was the proper one.

\* \* \*

## CHAPTER SEVEN

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# What Consequences to Society Should Affect the Authority of the Investigator?

In the previous chapter we explored the nature and extent of the investigator's authority in his interactions with individual subjects. Whenever these interactions interfere with the rights and well-being of research subjects, they also threaten the community as a whole. Thus, the problems presented for analysis in Chapter Six merge with those selected for this chapter. In some situations, however, the predictable and unpredictable risks for society go beyond the harm done to individual subjects. Accordingly, this chapter focuses on those situations in which the investigator's actions bring him into conflict with the rights and interests of the community. We have selected three categories of actual or potential harm to society, arising from interferences with: (1) human behavior, (2) human biology, and (3) mores and laws.

Since these investigations also expose individual subjects to harm, the questions set forth in Chapter Six should be asked once again. One question comes into sharper focus: When and to what extent should the scientist's freedom of inquiry be curtailed "in the interests of society"? Clearly, some experiments already performed and some now being contemplated may ultimately have far-reaching consequences for society. Investigations into ways of radically altering the psychology and biology of man have already proceeded to a considerable extent in the laboratory or in "therapeutic" settings. Once the means are at hand, the impetus to employ them more extensively may be irresistible unless prior thought has been given to the limits of their employment. In addition, some of these studies may have been carried out in violation of existing laws, while others may have been shelved in deference to

legal barriers. Thus, decisions to experiment or not experiment which involve the interests of society have perhaps too often been made unilaterally by investigators.

Our continuing objectives remain (1) to identify those consequences to society which should be labeled harmful and (2) to determine whether the decision to permit or preclude these consequences should be left to the discretion of the investigator alone or whether it requires approval from other participants in the human experimentation process. In examining these materials the following specific questions should also be considered:

1. When, if ever, in the development of a new procedure, should society make judgments about research activities?
2. What persons or institutions should have the authority to establish guides and criteria for conducting investigations with human beings, either in "therapeutic" settings, limited human trials, or mass applications?
3. What persons or institutions should be given authority to decide which investigations violate existing laws and which are exempt from criminal or civil liability because of their significance to science and society?

## A. What Constitutes Harm?

### 1.

#### Interferences with Human Behavior

a.

#### **José M. R. Delgado** **Evolution of Physical Control of the Brain\***

During the last decade we have reached an historical turning point because of the development of methods which permit the coordination and synthesis of physical, physiological, pharmacological, and psychological research. . . . [S]cience has developed a new electrical methodology for the study and control of cerebral functions in animals and humans. Learning, emotions, drives, memory, consciousness, and other phenomena which in the past belonged only in the realm of philosophy are now the subjects of neurophysiological experimentation. In the last few years, the scalpel of the brain surgeon has modified psychological reactions and a wealth of wonder drugs has liberated many patients from mental institutions.

I am not so naive as to think that cerebral

research holds all the answers to mankind's present problems, but I do believe that an understanding of the biological bases of social and antisocial behavior and of mental activities, which for the first time in history can now be explored in the conscious brain, may be of decisive importance in the search for intelligent solutions to some of our present anxieties, frustrations, and conflicts. Also, it is essential to introduce a balance into the future development of the human mind, and I think that we now have the means to investigate and to influence our own intellect.

\* \* \*

The social interaction of animals requires continuous mutual adaptation, and activities depend on a variety of factors, including sensory inputs, problem-solving capacity, emotional background, previous experience, conditioning, drives, instincts, and intelligent integration of all these processes. In spite of the extraordinary complexity of these supporting mechanisms, there is experimental evidence that electrical stimulation of specific areas of the brain may influence social interaction such as contactual relations, hierarchical situations, submissive manifestations, sexual activity, aggressive behavior, and social fear. . . .

\* James Arthur Lecture. New York: The American Museum of Natural History 2-3, 27-28, 30, 32, 41-42, 45-46, 48-51 (1965). Reprinted by permission.



... The fact that one animal can be electrically driven to fight against another has been established. In [an] experiment, stimulation of the tectal area in a male cat evoked the well-known pattern of offensive-defensive reactions. When this animal was placed on a testing stage in the company of a larger cat, they enjoyed friendly relations, lying close to each other and purring happily until the smaller cat was stimulated in the tectal area. At this moment, it started growling, unsheathed its claws, and launched a fierce attack against the larger animal which flattened its ears, withdrew a few steps, and retaliated with powerful blows. The fight continued as long as the stimulation was applied. The effect could be repeated, and the stimulated cat always took the initiative in spite of the fact that it was smaller and was always overpowered in the battle. After several stimulations, a state of mistrust was created between the two animals, and they watched each other with hostility.

\* \* \*

Rhesus monkeys are destructive and dangerous creatures which do not hesitate to bite anything within reach, including leads, instrumentation, and occasionally the experimenter's hands. Would it be possible to tame these ferocious animals by means of electrical stimulation? To investigate this question, a monkey was strapped to a chair where it made faces and threatened the investigator until the rostral part of the caudate nucleus was electrically stimulated. At this moment, the monkey lost its aggressive expression and did not try to grab or bite the experimenter, who could safely put a finger into its mouth! As soon as stimulation was discontinued, the monkey was as aggressive as before. Later, similar experiments were repeated with the monkeys free inside the colony, and it was evident that their autocratic social structure could be manipulated by radio stimulation. In one case in which the boss monkey was excited in the caudate nucleus with 1.5 mA for five seconds every minute, after several minutes the other monkeys started to circulate more freely around the cage, often in proximity to the boss, and from time to time they crowded him without fear. The intermittent stimulation continued for one hour, and during this time the territoriality of the boss dropped to zero, his walking time was diminished, and he performed no aggressive acts against the other members of the colony. About 12 minutes after the stimulation hour ended, the boss had reasserted his authority, and

his territoriality seemed to be as well established as during the control period...

\* \* \*

Elemental psychic phenomena such as hunger and fear can be analyzed in both animal and man, but processes like ideation and imagery that are expressed verbally can be studied only in human beings...

\* \* \*

In three different patients, thoughts and expressions with sexual content were induced by electrical stimulation of the temporal lobe. The first case, S.S., was an intelligent and attractive woman, 32 years old, who had suffered from uncontrollable epileptic attacks for several years. During the interviews she was usually reserved, but the first time that point A in the second temporal convulsion was excited with 6 volts, she became visibly affected, holding the hands of the therapist to express her fondness for him and to thank him for all his efforts. Several minutes later, after another stimulation of the same point, she started to say how much she would like to be cured so that she might marry, and other stimulations of point A were also followed by flirtatious conversation. The provocative play and ideas expressed under stimulation of point A did not appear following stimulation of other cerebral points and contrasted with this woman's usually reserved spontaneous behavior.

The second patient, V.P., was a woman 36 years old who had suffered from epilepsy since childhood. Point C in the temporal lobe was excited five times at intervals of from five to 10 minutes, and after each stimulation the patient's mood became friendlier; she smiled, questioned the therapist directly about his nationality, background, and friends, and declared that he "was nice," that his country (Spain) "must be very beautiful," that "Spaniards are very attractive," and she ended with the statement "I would like to marry a Spaniard." This particular train of thought and manner of speaking seemed completely spontaneous, but it appeared only after stimulation of point C in the temporal lobe, and no such shift to a flirtatious mood was noted in her spontaneous conversations following stimulations of other cerebral points.

The third case of evoked change in sexual ideology was a young epileptic boy, A.F., who, following stimulation of point LP 5-6 in the left temporal cortex, suddenly began to discuss his desire to get married. After subsequent stim-

ulations of the point, he elaborated on this subject, revealed doubts about his sexual identity, and voiced a thinly veiled wish to marry the male interviewer.

\* \* \*

Probably the most significant conclusion derived from electrical stimulation of the awake brain is that functions traditionally related to the psyche such as friendliness, pleasure, and verbal expression can be induced, modified, and inhibited by direct stimulation of cerebral structures. This discovery may be compared with the revolutionary finding almost two centuries ago that contraction of frog muscle may be induced by electricity without need of the soul's "animal spirits," because experimental analysis of mental functions can now proceed without implicating metaphysical entities. Research concerning the electrical driving of emotions, anatomical correlates of memory, or electrical signals related to learning does not interfere with personal ideas about the natural or supernatural destiny of man and does not involve theological questions, which should be disassociated from neurophysiological inquiry. . . . The task that we are facing is the correlation of neuro-anatomy and physiology with mental functions; the investigation of cerebral areas involved in psychic manifestations; the analysis of their electrical and chemical background; and the development of methods to induce or inhibit specific activities of the mind.

. . . Human behavior, happiness, good, and evil are, after all, products of cerebral physiology. In my opinion, it is necessary to shift the center of scientific research from the study and control of natural elements to the analysis and patterning of mental activities. There is a sense of urgency in this redirection because the most important problem of our present age is the reorganization of man's social relations. While the mind of future generations will be formed by pedagogic, cultural, political, and philosophical factors, it is also true that education is based on the transmission of behavioral, emotional, and intellectual patterns related to still unknown neuro-physiological mechanisms. Investigators will not be able to prevent the clash of conflicting desires or ideologies, but they can discover the neuronal mechanisms of anger, hate, aggressiveness, or territoriality, providing clues for the direction of emotions and for the education of more sociable and less cruel human beings. The precarious race between intelligent brains and unchained atoms must be won if the human race is going to survive, and learning the biological

mechanisms of social relations will favor the cerebral victory.

\* \* \*

From its beginning, wiring of the human brain aroused emotional opposition even among scientists, while similar wiring of the heart or of the bladder has been received enthusiastically. The difference in attitude was no doubt related to a more or less conscious personal fear that our identity could be attacked and that our mind could be controlled. Personal traits such as friendliness, sexual inclination, or hostility have already been modified during cerebral stimulation, and we can foresee other influences on emotional tone and behavioral reactions. Electricity is only a trigger of pre-existing mechanisms which could not, for example, teach a person to speak Spanish, although it could arouse memories expressed in Spanish if they were already stored in the brain.

Entering into the field of speculation, I would like to comment on one question which has already caused widespread concern. Would it be feasible to control the behavior of a population by electrical stimulation of the brain? From the times of slavery and galleys up to the present forced-labor camps, man has certainly tried to control the behavior of other human beings. In civilized life, the intervention of governments in our private biology has become so deeply rooted that in general we are not aware of it. Many countries, including the United States, do not allow a bride and groom to marry until blood has been drawn from their veins to prove the absence of syphilis. To cross international borders, it is necessary to certify that a scarification has been made on the skin and inoculated with smallpox. In many cities, the drinking water contains fluoride to strengthen our teeth, and table salt is fortified with iodine to prevent thyroid malfunction. These intrusions into our private blood, teeth, and glands are accepted, practised, and enforced. Naturally, they have been legally introduced, are useful for the prevention of illness, and do generally benefit society and individuals, but they have established a precedent of official manipulation of our personal biology, introducing the possibility that governments could try to control general behavior or to increase the happiness of citizens by electrically influencing their brains. Fortunately, this prospect is remote, if not impossible, not only for obvious ethical reasons, but also because of its impracticability. Theoretically it would be possible to regulate aggressiveness, pro-

ductivity, or sleep by means of electrodes implanted in the brain, but this technique requires specialized knowledge, refined skills, and a detailed and complex exploration in each individual, because of the existence of anatomical and physiological variability. The feasibility of mass control of behavior by brain stimulation is very unlikely, and the application of intracerebral electrodes in man will probably remain highly individualized and restricted to medical practice. Clinical usefulness of electrode implantation in epilepsy and involuntary movements has already been proved, and its therapeutic extension to behavioral disorders, anxiety, depression, and other illness is at present being explored. The increasing capacity to understand and manipulate mental functions of patients will certainly increase man's ability to influence the behavior of man.

If we discover the cerebral basis of anxiety, pleasure, aggression, and other mental functions, we shall be in a much better position to influence their development and manifestations through electrical stimulation, drugs, surgery, and especially by means of more scientifically programmed education.

These possibilities pose tremendous problems. As Skinner asked recently, "Is the deliberate manipulation of a culture a threat to the very essence of man or, at the other extreme, an unfathomed source of strength for the culture which encourages it?" Scientific discoveries and technology cannot be shelved because of real or imaginary dangers, and it may certainly be predicted that the evolution of physical control of the brain and the acquisition of knowledge derived from it will continue at an accelerated pace, pointing hopefully toward the development of a more intelligent and peaceful mind of the species without loss of individual identity, and toward the exploitation of the most suitable kind of feedback mechanism: the human brain studying the human brain.

b.

#### **Hudson Hoagland Potentialities in the Control of Behavior\***

\* \* \*

The idea of the control of one person by another usually elicits strong adverse reactions in people. We treasure our convictions of freedom,

and know either at first hand or vicariously the misery produced by coercion and tyranny. But we often fail to recognize that we are continually controlled in a variety of ways. Sanctions are derived from parents and other representatives of society, by laws and customs, and by the impact of irrational persuasion through myths and symbols that appeal to our subconscious drives, and may have little to do with the reason and logic we believe we use in making choices. A huckster or political propagandist may make us wish to have things we would be better off without. We are none the less controlled because we wish to do the things we do.

The great problem of control of behaviour resides in the question of who controls whom and for what purposes. It is clear that control by a Hitler or a Stalin is bad; but control is real and pervasive. How can it be used to advance human welfare?

We control each other in a great variety of ways. Force and the threat of force, which are clearly objectionable, may not be used but education, persuasion and moral pressure have the same effects. Cajolery, seduction, incitement and a variety of other techniques are used. B. F. Skinner has pointed out that ethical counter-controls in most countries prevent exploitation by the use of force and deception. But he emphasizes that there is real danger that the rapid development of new techniques of control will outstrip counter-control. Despite objections, science will increasingly facilitate control of human behaviour and it must be used wisely if we are to avoid disaster.

The behavioral sciences have developed new methods to modify and direct conduct. Examples are Pavlovian conditioning and the conditioning methods developed by Skinner, which have become widely used in studies of animal and human behaviour. By the use of appropriate reinforcing stimuli, behaviour may be modified and directed. The techniques involve carefully programmed rewards, reinforcing the subject's known hierarchies of values. Operant conditioning is the basis of the programming of teaching machines which are increasingly being used in education. The use by advertisers and others of subliminal messages in television has caused alarm and been made illegal in some countries. The effectiveness of this clandestine form of subconscious communication is, however, questionable.

C. H. Waddington, in his book *The Ethical Animal*, has considered that the long range objectives of the control of behaviour are ethical

\* Gordon Wolstenholme, ed.: *Man and His Future*. Boston: Little, Brown and Co. 299, 303-310, 312-313 (1963). Reprinted by permission of J. & A. Churchill, London.

systems, the values of which may be judged in relation to their ability to further a desirable evolutionary direction, unique for mankind, and he discusses the nature of this evolutionary progress. Human culture, he points out, is based on a mechanism that requires people to be brought up in such a way that they accept beliefs given them by others such as parents and other influential persons in authority. Of course such beliefs are subject to later testing and rejection or retention, but before this can happen ideas must be transmitted as a form of social heredity. Ideas thus function in cultural evolution in a way analogous to genes in biological evolution, and Henry A. Murray has referred to germinal ideas as *idenes*.

The moulding of the newborn human individual into a being ready to believe what it is told seems to involve many very peculiar processes, which at present may be explained as the formation of the superego and the repression of the id, to use Freudian terminology. A frequent result of the process seems to be that people believe too much and too strongly. The process that evolution has provided us with seems often to lead to considerable exaggeration of the ability to believe.

Waddington argues that many of the world's evils and social ills stem from over-activity of the superego, leading to the acceptance of socially regressive beliefs with undesirable impact upon politics, religion and group identifications. Intense and irrational loyalties stemming from early authoritarian acceptance of communication have repeatedly led to fanaticism, bigotry and wars. One has but to recall pictures in the American press of squawking New Orleans women with children in their arms, hurling imprecations at a white father taking his small daughter to a desegregated school, to see pathological ethics in action. As Brock Chisholm has pointed out, most of the ethical beliefs we hold so strongly are established by accidents of birth and what we learn, hit or miss, before we are seven years old. Emotionally charged prejudices are propagated from generation to generation by parental and adult prestige. The strongest beliefs may bear little relation to the common good. The world has continually been sundered by the hates of rival groups and these could, in the nuclear age, soon render man an extinct species.

The rate of increase of scientific information is said now to be doubling every ten years, and its technological applications are changing

society in ways for which there are no precedents. The fate of man has become the prize in a gruelling race between education and disaster. Traditional methods of education and ethical transmission appear to be inadequate, and the behavioural sciences so far have not been effective in meeting major challenges of the twentieth century. Fear that the behavioural and social sciences may be used for evil purposes has slowed their development and blocked their use for constructive purposes. We need a larger investment of talent in these fields, commensurate with their importance. As someone has said, understanding the atom is child's play compared to understanding child's play.

\* \* \*

Psychopharmacology is a new empirical field that has developed rapidly over the last decade, and the use of drugs for the treatment of psychiatric disorders has furnished its major thrust. The pharmaceutical industry has produced hundreds of compounds faster than they can be tested in the clinic. These substances fall roughly into five groups. There are the stimulant drugs, such as ephedrine and its derivatives, which increase wakefulness and decrease fatigue under some conditions but also have some undesirable side effects on the central nervous system. The anti-depressant drugs include iproniazid (Marsilid) and a number of other monoamine oxidase inhibitors, together with some anti-depressants of other chemical types. These agents may produce euphoria, increase verbal productivity, speed reaction times and otherwise act as stimulants, but their principal value is in combating severe depressions of mental patients. The tranquillizers are a third group extensively employed in the treatment of disturbed mental patients, including schizophrenics. These drugs include chlorpromazine and a variety of other phenothiazine derivatives, as well as reserpine and a few related *Rauwolfia* alkaloids. A fourth category consists of substances that act as mild tranquillizers and sedatives. One of these, meprobamate, is sold under a number of trade names of which Miltown is perhaps the best known, and another is methaminodiazepoxide (Librium). These drugs may relieve neurotic anxiety without producing the sedative effects of barbiturates and bromides.

A fifth group of psychoactive drugs produce transient psychotic states. Their primary value is for research purposes in producing

model psychoses in normal persons. Some effects of some of this group have been known for a long time. In crude form, as they occur in native plants, they have been used to produce mystical states during primitive religious rites. They include mescaline, psilocybin, the powerful synthetic psychogen LSD-25 (lysergic acid diethylamide) and other synthetic products.

All these drugs except those of the fifth group primarily affect mood. None of them acts upon the information content of the brain. The tranquilizers and psychic energizers are primarily responsible for the large increase in discharge rates of mental patients from hospitals in recent years.

The promiscuous use of the milder tranquilizers has given cause for alarm. These substances can inhibit initiative, vigour and drive, and may have deleterious side effects. They constitute the largest item of sale in American drug stores today. Barbiturate sleeping pills, bootlegged from druggists, are being used as a substitute for alcohol by some juvenile groups. A drink called a "goof ball" is made from sleeping pills dissolved in Coca Cola, and barbiturate addiction has become a serious problem among some teenagers.

At present there seems to be little likelihood of the deliberate use of any of the known psychoactive drugs for the control of the behaviour of normal people. Even in the hands of a dictator, it is hard to see how any of these compounds could be used effectively to manipulate the actions of a population towards directed ends. Although these drugs may relieve depression and reduce anxiety in neurotic and psychotic patients, they can only disturb normal persons and make them miserable. Ephedrine and its derivatives may briefly spur a fatigued individual to greater output of activity but the subsequent hangover can negate such transient benefit. The functions of normal healthy organs, including the brain, have not so far been improved significantly by the use of drugs. . . .

There are historical examples of the use of drugs to control populations. Alcohol was used deliberately by some of our American forebears to debilitate and destroy the will to resist of some Indian tribes, and oriental despots have promoted the use of opium by subject populations for similar purposes. The consumption of tobacco and alcoholic beverages is promoted by commercial interests for their own profit—a control, in general, approved by the public. But peo-

ple tend to resent the use of chemical agents when urged upon them for their own good. Irrational opposition to vaccination in the past and to the fluoridation of water supplies today are cases in point. Despite the magnitude of the population problem, many most in need of birth control refuse the use of oral contraceptives even in the absence of religious taboos.

It has been popularly believed that drugs have been employed extensively in brain-washing procedures in Communist-controlled countries. However, from the evidence available, this has not been the case. According to reliable reports, coercion of persons for the purpose of extracting confessions has involved methods similar to police state practices used since the time of Napoleon. Neither scientifically directed Pavlovian conditioned reflex procedure nor pharmacology appear to have been used in any significant way in breaking the morale of political prisoners.

Extensive work by neurophysiologists, using operations on the brains of animals, has shown that it is possible markedly to modify emotional and aggressive behaviour. When experimental lesions in monkeys are carefully restricted to the pyriform lobe of the amygdaloid complex and hippocampus without interference with neocortical regions, most fear and anger responses disappear, without gross motor or sensory deficiencies. Although these animals can express anger and rage in response to appropriate stimuli, they are rendered remarkably docile and fearless, and their behaviour is accompanied by a reduction in sexual activity. Studies of cats, including the lynx, show there is marked docility following bilateral lesions of the pyriform lobe. But the amygdalotomized cat can be turned into a vicious and rageful animal by additional superimposed lesions in the ventro-medial nucleus of the hypothalamus. Changes have been reported in the hierarchical position of individual rhesus monkeys from dominant to submissive positions in the pecking order following amygdalotomy, and clinical observations indicate that some amygdala lesions in man are followed by diminished social aggressiveness.

It thus appears that surgical operations on the brain's limbic system can markedly change emotional behaviour. Presumably chemical agents may ultimately be found which will act selectively on specific brain centres and have similar effects. It has been reported that cats exposed to certain agents of potential use in chemical warfare are terrified at the sight of mice.

Despite the values of the neurosurgical findings to medicine, it is difficult to see any practical application of psycho-surgery in the future, to enable men deliberately to control each other's behaviour in any socially significant way.

There is however one field which may hold promise for constructive purposes. It is possible that agents may be found to facilitate learning, memory and recall. It would clearly be desirable to find chemical and pharmacological procedures to facilitate processes of education, even at the risk of their perversion for political purposes.

\* \* \*

Holger Hyden and his collaborators have developed elegant microchemical methods to study individual neurones in different parts of the nervous system. . . . Of special interest in this connexion was the finding that tricyano-amino-propine administered to human subjects is followed by an increase in suggestibility. Hyden considers that this substance or others might affect mental states in such a way that a police-controlled government, by putting such agents in drinking water, could make propaganda more palatable. . . .

\* \* \*

#### NOTES

##### NOTE 1.

DAVID KRECH  
PSYCHOCHEMICAL MANIPULATION AND  
SOCIAL POLICY\*

\* \* \*

It is my considered judgment—well, fairly well considered—that within 5 to 10 years there will be available a regimen combining psychological and chemical measures that will significantly increase the intelligence of man. This troubles me, and should trouble you, for reasons that may soon become clear. To give some substance to these predictions and forebodings, let me cite first some supporting experimental chapter and verse.

I start with Prof. James McGaugh's demonstration that certain central nervous system stimulants can improve an animal's ability to learn. . . . The experimental mice were presented with the task of learning to choose a white alley leading to food over a black alley. Note that the

saline control animals averaged about 20 errors before they learned always to choose the white alley, but the Metrazol (pentylene-tetrazol)-dosed animals improved with increasing strengths of the drug until, at the dose level of 10mg/kg of body weight, the mice solved the problem after but 5 errors; beyond that dosage there was no appreciable improvement. There seems to be a limit to the intellectual power of even a hopped-up California supermouse. In another experiment two different strains of mice were trained to solve a simple maze. Note first that we had here hereditary differences in learning ability: a relatively bright strain and a relatively stupid one. Secondly, note that the stupid treated with 10 mg/kg of pentylene-tetrazol did as well as their untreated but hereditarily superior colleagues. Chemotherapy here compensated for the stupid parents. In another series of experiments McGaugh found that different drugs worked differentially for different strains, individuals, and intellectual tasks. Thus, for some problems picrotoxin helped the dull animal but not the bright, while for other problems pentylene-tetrazol seemed to help everyone.

Does all of this mean that we will soon be able to substitute an inexpensive get-smart pill for our expensive school enrichment programs? The answer is no, as our Berkeley experiments on the effects of experience on the brain suggest. Let me describe a typical experiment. At weaning age, 12 rats are put into a psychologically enriched living group while their twin brothers are placed in a psychologically impoverished group. They all have the same food, of course. All 12 enriched pups live in 1 large cage equipped with inviting rat toys in a well-lighted, noisy, and busy laboratory. As the rats grow older, they are given various little learning tasks to master for which they are rewarded with bits of sugar. While these animals are enjoying the richest intellectual environment that Berkeley can provide for rats (but they are not on drugs!), their brothers lead quite different lives. Each impoverished animal lives out his solitary confinement in a small cage situated in a dimly lit and quiet room. It is rarely handled by its keeper and never invited to solve problems or join in fun and games with fellow rats or graduate students. At the age of 105 days all the rats are sacrificed, and their brains are analyzed morphologically and chemically.

This standard experiment that I have just described has been repeated dozens of times and has yielded the [following] results. . . . As the

\* 67 *Annals of Internal Medicine*, Supplement 7: 19-24 (1967). Reprinted by permission. © 1967, American College of Physicians.

more fortunate litter mate lives out his life in the enriched condition, the bulk of his cortex expands and grows deeper and heavier than that of his culturally deprived brother. Part of this increase in cortical mass is accounted for by an increase in the number of cortical glia cells, part by an increase in the size of the neuronal cell bodies and their nuclei, and part by an increase in the diameter of the cortical blood vessels. Biochemical changes also occur. The enriched brain shows both more acetylcholinesterase and more cholinesterase activity.

Now, what does all this mean? Let me return to McGaugh's results for a moment. Whether a drug will improve an animal's learning ability will depend, of course, upon how the drug changes the chemistry of the animal's brain. And it is now clear from our own work that the chemical status of the brain, *before the introduction of any drug*, is partly dependent upon the psychological milieu in which the animal has been living. Therefore, putting McGaugh's results and our results together, it seems clear that *how* a drug, introduced from the outside, will change the brain chemistry and, thus, affect learning will depend upon the organism's psychological environment. I am not talking about some sort of mysterious interaction between "psychological forces" and "chemical compounds." I am talking about interactions between chemical factors in the brain induced by environment and chemical factors introduced into the brain by injections or pills. This is what lay behind my opening conclusion that within a few years psychology and chemistry will be able to raise the intelligence of man significantly.

Why does this worry me? To answer this question, I shall ask you to take a very brief look at some human data and then join me in an old parlor game. [In] the theoretical IQ distribution for our population . . . the mental retardates are separated into the so-called "organics"—comprising those retardates whose difficulties can be traced to clearly identifiable physical defects such as phenylketonuria or serious head injuries—and the "familial retardates"—made up of children with IQ's between 40 and 70 in whom there has been found no clear physiological defect. The causes cited for familial retardation include defects in brain biochemistry, hereditary factors, and cultural factors; indeed, this group is often labeled "the cultural familial retardates."

Now the most likely development from the research that I have been discussing will undoubtedly produce effective treatment for many

of the cultural familial retardates. But if we will be able to raise the IQs of the cultural familial retardates, how about the "cultural familial geniuses"? And what about the many millions of men, women, and children in the largest group of all—the "cultural familial mediocrities"? Let me suggest three possible answers.

The first possibility I will label "Brave New World, Mark I." Here . . . we assume that our psychological chemical procedures will raise the intelligence of all men and women so that the distribution curve would shift in the direction of higher IQ levels. Now, let us play the old parlor game of "what if?". Remember how it goes? You say, "What if through a genetic mutation induced by radioactive fallout from our liberty-loving anticommunist, democratic, nuclear-testing program, all babies were born with three arms? What changes do you foresee?" And your guests begin to speculate. Someone suggests that the whole clothing industry would have to be retooled—three sleeves instead of two; the deodorant industry would boom; speakers would have to revise their "on-the-one-hand and on-the-other-hand" clichés; handholding under the dinner table would be facilitated, leading to an increase in off-the-reservation dalliance, divorce, and eventual total moral decay. Very well. What would happen if, through psychochemistry, we raise the IQ level of most people by 20 points? What new demands would this place on our educational facilities and practices? What political changes might such a population bring about? What moral changes? How about religious practices and institutions?

Let me propose another possibility and another set of questions. You will remember that McGaugh found that some drugs can help only the duller strains and individuals of his experimental animal populations. Perhaps we shall find that with the human being we can raise only the lower IQs, the higher IQs being resistant to further improvement. . . . In "Brave New World, Mark II" we would have relatively little spread from the brightest to the dullest. Who now will be the hewers of wood and the drawers of water and the inhabitants of the slums and the wastelands—and who, the WASPS and the gentry? What changes will all of this induce when we are all pretty much alike intellectually? How long can we remain segregated into different political, economic, and social groups?

Let me end with the "Mark III" model. You will also remember that different drugs may be effective for different kinds of problems. On the



human level this means that we may be able, through psychochemistry, to raise verbal abilities in some, arithmetic reasoning in others, artistic abilities in still others. Now, who gets what raised and who decides for whom? The parent? The family pediatrician? The family physician? The school board? And on what basis do they decide? On the effectiveness of the pharmaceutical industry's advertising? On the effectiveness or the persuasiveness of detail men? On the ability to pay for the more expensive abilities? These problems—surpassingly strange in their novelty, bafflingly complex, and of serious import—are problems with which you will inevitably become intimately involved. Yet it seems clear to me that the physician alone does not have the wisdom and the knowledge to handle these matters. Here, most certainly, the physician cannot be allowed to write social policy on his prescription pad. And here, most certainly, the physician should be prepared to welcome guidance from the laity and even accede gracefully to social control.

NOTE 2.

PERRY LONDON  
BEHAVIOR CONTROL\*

The first true technology of individual behavior control through verbal information was probably psychotherapy, especially the multitude of systems and subsystems called insight therapy, which aims to help people solve personal problems primarily by special ways of talking to them and listening to them talk. . . .

Psychotherapy is a technologically primitive means of behavior control compared to what will come after it, but it is significant in its own right because of the breadth of its applications, if not their power, and because it embodies virtually all the ethical problems which conscientious students of behavior control must encounter. . . .

\* \* \*

Even were its scope less broad, psychotherapy would still be an important prototype of informational behavior-control technology because of its noncoercive character. All psychotherapies are merely special cases of the many kinds of situations in which some rational, persuasive, and nonviolent means are needed for controlling individual behavior or for teaching responsible agents of society how to do so. Part of its value

as an area of study is that it involves the development of controls under conditions of maximum disadvantage to any controlling agency. "The therapist," as Neal Miller of Rockefeller University puts it, "does not have direct control over the important rewards and punishments in the patient's environment." Neither coercion by police power nor continuous charge over rewards and punishments, such as parents have, are ordinarily available to a psychotherapist; this turns his operations into relatively pure attempts to influence with only limited resources, the chief of which is language. It is not cynical, therefore, to say that the systematic persuasion methods which are psychotherapy are salesmanship elevated to the level of technology.

Because it relies on this most complex medium—the "higher processes" of language and symbol (sometimes considered the only uniquely human attributes)—psychotherapy is also a straightforward extension of education which, in the ethical perspective of our age, is regarded as the antithesis of control through coercion. And because it addresses individuals directly, it may be the closest thing extant to an agency for individual control that also satisfies the modern morality of freedom and individual choice.

\* \* \*

All forms of psychotherapy aim to control behavior which, by one standard or another, is considered mentally deranged, diseased, disturbed, or otherwise disordered. For this reason, psychotherapists commonly refer to their methods as techniques of treatment rather than control. Such terminology makes no difference to their operations.

\* \* \*

C.

**Arnold M. Ludwig, Arnold J. Mary,  
Phillip A. Hill, and Robert M. Browning**  
**The Control of Violent Behavior through  
Faradic Shock\***

This study concerns an evaluation of the use of faradic shock as punishment for the purpose of suppressing the violent, potentially homicidal behavior of a hospitalized, chronic paranoid schizophrenic patient. The uniqueness of the study lies in four general areas: a) the type of

\* New York: Harper & Row 39-41 (1969). Reprinted by permission of Harper & Row, Publishers.

\* 148 *Journal of Nervous and Mental Disease* 624-627, 633-637 (1969). © 1969, The Williams & Wilkins Co., Baltimore, Maryland. Reprinted by permission.



patient treated, b) the particular kinds of behaviors chosen for modification, c) the fact that this procedure was administered against the expressed will of the patient and d) the clinical-experimental format employed.

\* \* \*

[P]ertinent to the present study is the previous use of electric shock as an aversive conditioning agent. In most of these clinical studies, the investigators have used a special apparatus, capable of delivering about 70 to 100 volts (the actual voltage can be adjusted by a rheostat) through electrodes on a cuff applied to the patient's forearm, wrist or calf. The procedure involves shocking the patient immediately after the repeated visual presentation (actual object, picture, slide) of his deviant interest. The conditions treated by this means include cases of compulsive gambling, homosexuality, compulsive eating, fetishism, transvestism, car stealing, obsessional ruminations, smoking, writer's cramp, alcoholism, and even habitual blushing, compulsive copulation with sheep and marital infidelity. It is important to emphasize that in all these cases the "voluntary" consent and active cooperation of the patients were required as a precondition for treatment.

Of a somewhat different nature are the studies concerned with autistic and severely retarded children. Lovaas and co-workers, pioneers in this field, have used slapping, electrified grills and the cattle prod (the "shock stick") as a means of controlling and modifying self-mutilative, aggressive and other forms of maladaptive behavior in these children. Employing similar techniques, other investigators have extended this work and reported equally impressive therapeutic results in otherwise hopeless, self-destructive children. In these cases, of course, the consent and active cooperation of the children were not necessarily sought.

Three factors contributed to our choice of a punishment treatment paradigm: a) the dangerousness to self and others of the patient's behavior, b) the inability of previously employed treatment methods to modify this behavior and c) the far more drastic nature of other possible treatment alternatives.

The patient was a 31-year-old female with an established diagnosis of chronic paranoid schizophrenia. She had been hospitalized for the better part of 9 years. Her intelligence and verbal facility were normal, but the content of her ideation and speech was predominantly vindictive

and accusatory with marked delusions of persecution. Although she manifested agitation and depression on occasion, this symptomatology would usually undergo a rapid paranoid transformation whereby she would begin to blame others for her predicament. She showed poor socialization, resisting most efforts of both patients and staff to engage her in conversations or activities, and responded poorly to praise or other positive social reinforcers. The most serious feature of her behavior was her frequent, vicious physical assaultiveness toward staff and fellow patients. Less frequently, she had been known to set fire to herself or apply lit cigarettes to her skin. This self-destructive behavior, however, had not been observed during the last 2 years of her hospitalization.

Housed on various locked wards over the years, she had succeeded in terrorizing patients and staff alike. She would bully and threaten patients into giving her cigarettes, money and other articles. In regard to staff, she would threaten to kill them or their families if they did not accede to her wishes or did not leave her alone. Often these threats would be translated into physical assaults. The predictable result was that patients and staff treated her gingerly and tended to keep a "safe distance."

Although many of her physical attacks upon staff were in response to limit-setting attempts, there were other instances where there were no discernible antecedents for this violence. These latter situations were especially frightening because of their lightening suddenness and unexpected quality. For example, she would walk up to staff members or patients and without warning punch them in the face. In other instances, she would lie in wait for her intended "victims" in the bathroom or some other unsupervised ward area and attack them mercilessly once they entered.

Over the long period of her hospitalization, the patient had been exposed to almost every variety of psychiatric therapy. Massive doses of tranquilizers and sedatives, electroconvulsive therapy, intensive milieu, group and individual psychotherapy, and a variety of special psychosocial techniques had been tried—all to no avail in curbing her assaultive behavior. There were, to be sure, brief periods of quiescence of her symptomatology, but these seemed relatively independent of her treatment at those times.

Clearly, the management of this patient represented a serious medical-psychiatric problem and something had to be done before she killed

someone or was seriously injured or killed herself through the retaliation of another patient. Faced with the ineffectiveness of prior treatment approaches, we were forced to consider other more extreme procedures such as prefrontal lobotomy, shackling her in physical restraints, or isolating her through prolonged seclusion. We were naturally reluctant to resort to these procedures and believed it would be far more humane and potentially therapeutic to try to modify her behavior through aversive conditioning. After securing the support of a group of board-certified psychiatrists and obtaining the necessary administrative clearance, we initiated the treatment program.

The aims of the treatment program were essentially two-fold: a) the elimination of serious aggressive-assaultive behavior and b) the stimulation of appropriate, socialized, responsible behavior. In order to realize the first goal, we felt it would be important not only to apply punishment (*i.e.*, faradic shock) systematically upon the appearance of behaviors defined as physical aggression but also to suppress certain antecedent, related behaviors. It was our plan to establish a hierarchy of responses associated with aggression and then proceed to modify each successive level in this hierarchy in a stepwise manner through the aversive therapy paradigm. . . .

To achieve the second goal, namely the fostering of prosocial responses in this patient, we planned to provide copious positive reinforcement in terms of praise, compliments and other more material rewards for all desirable and adaptive behavior. This intensive program of positive social reinforcement was to accompany the aversive conditioning aspects of treatment throughout the entire program.

There were a number of reasons for choosing the cattle prod as the means of delivering the aversive stimulus or punishment. From a technical standpoint, this instrument (Sabre-Six model, Hot Shot Products Co.) seemed to represent an excellent device for providing a potent, noxious stimulus. It was capable of producing a faradic shock spike of approximately 1400 volts at 0.5 milli-amperes, the resulting pain lasting only as long as the current was permitted to flow. From the standpoint of safety, the shock caused no tissue damage or other adverse physical effects. In comparing the treatment to such standard psychiatric procedures as electro-convulsive therapy or even psychotropic medication, the possibility of serious physical side effects was regarded as far more remote. Moreover, when compared to

the dangers and relative unpredictability of onset and duration of action of other aversive agents, such as emetic and muscle-paralyzing drugs, this instrument was far safer and could be applied in a more specific manner with a minimal time lag between the appearance of the undesirable behavior and the aversive stimulus. Also, from a practical standpoint, the instrument was portable, inexpensive and easy to use.

\* \* \*

#### *General Clinical Observations*

It seems reasonable to presume that if a stimulus is to be defined as aversive or punishment, it should elicit a reaction indicative of displeasure, discomfort or pain. The more profound the reaction, the greater should be the individual's efforts to avoid behavior associated with the application of the aversive stimulus. There were a number of clinical observations that indicated that the faradic shock possessed all the properties of a punishing stimulus for our patient. After being shocked, the patient would let out a cry, stiffen all over and then begin whimpering.

We had anecdotal confirmation from the patient herself that the faradic shock was specifically instrumental in making her cautious about striking out. For example, in the early stages of the therapy program it was common to hear her exclaim "The only thing holding me back is the faradic stimulator," "If it wasn't for the faradic shock, you'd be on your ass," "You are doing this to me, getting away with everything because of the faradic stimulator," "I wouldn't hit you . . . I don't want that faradic thing," "Maybe I better not say that . . . it might be blaming. It's better to be safe than sorry."

Of incidental interest was the anticipated finding that the patient was quite capable of making the mental connection between the performance of a punishable act and the subsequent punishment, regardless of the time lag between these events. Pertinent to this matter was the patient's response to one instance of delayed punishment when she chided a staff member by saying "You're supposed to do this later after I say it (referring to level III behavior)."

In contrast to our impressions about the potency of faradic shock as an aversive stimulus are our observations on the relative impotence of primary rewards (goodies, cigarettes) or social rewards (praise) to shape or reinforce constructive, adaptive behavior. For the most part, the patient typically had responded to praise, emotional warmth and rewards in an antagonistic

and surly manner. Although we continued to provide intensive and copious positive reinforcements for appropriate behavior throughout the entire treatment program, including the several base line periods, it was not until the spontaneous appearance of the approach behavior following the start of the level III program that positive reinforcement (particularly social) seemed to take on increasingly appropriate emotional valence.

#### *Concluding Comments*

Although we were able to establish "effective" control over the intensity, duration and frequency of aggression and its antecedents, these behaviors were never eliminated completely and, therefore, control over them could not be regarded as absolute. Throughout the entire treatment program, there were occasional flareups of each of the three levels of punishable behaviors. . . .

Of great clinical interest was the spontaneous appearance of relatively new behaviors following the suppression of the patient's dominant aggressive responses. Generally, these outlets proved to be milder, less destructive and more innocuous than the deviant behaviors they substituted for, and they were relatively short-lived. We suspected that the reason for their transiency was that they did not serve as satisfactory outlets for her feelings.

Aside from a reduction in aggressive behaviors, we also noted a concomitant increase in more socialized behaviors following the punishment of each successive level in the hierarchy of aggressive and antecedent behaviors. It was not simply that the patient became more tractable, compliant or automaton-like in response to these punishment procedures: rather, the patient gradually began to show tentative warmth to others (*i.e.*, "I like you . . . no I don't"), engaged in occasional humorous interchanges and showed some diminution in her general hating and surlyness. . . .

One possible mechanism for the shift from antagonistic to approach behaviors, as well as for her general clinical improvement, involves the concept of "functional incompatibility." . . . For . . . new behaviors to become established, the stereotyped behaviors must first be reduced or eliminated. With our patient, an even clearer incompatibility between her prior aggressive and hating mode and any prosocial responses could be demonstrated.

From several of the patient's statements, such as "If I can't fight you guys, I'll join you.

. . . Nothing else works," we might also invoke the mechanism of "identification with the aggressor." With such a view, we would expect that the patient, feeling impotent and frightened when having her habitual sources of displaying power over others blocked, would attempt to regain strength and security through her identification with the staff. . . .

Perhaps the most straightforward explanation for the increase in the patient's social approach behavior related to the obvious change in attitudes and behavior of the staff and fellow patients toward her once her aggressive tendencies were reduced. Before this reduction took place, the staff might have felt that they were being warm and accepting much of the time with the patient, but it was difficult to relate with unconditional positive regard to someone who punched them in the jaw just 1 week ago. With this threat of harm diminished, the staff could be more genuinely affectionate, thereby increasing the possibility that the patient could respond in kind. . . .

\* \* \*

Another factor contributing to the relative success of the aversive therapy program seemed related to the subjective meaning of punishment for the patient. From clinical observations, we had become convinced that the patient, at some level of consciousness or volition, wanted to be effectively controlled and have limits set and enforced. In the course of our therapy program, we collected much anecdotal "evidence" that seemed to support this view. For example, shortly after the initiation of the punishment program for level I behaviors, the patient eloped from the hospital. Following previous elopements, she was always picked up by the community authorities and escorted involuntarily back to the hospital. On this particular occasion, she returned on her own, claiming "They're trying to kill me out there . . . at least in here you'll protect me!" Also, during the course of the program, she made the following revealing statement: "I know what you're trying to do. . . . You're trying to make a human being out of me."

From a therapeutic standpoint, we must regard the aversive conditioning program as a qualified success. Although the patient remains far from "cured," she nevertheless has progressed considerably from the primitiveness, violence and malignant paranoia which characterized her pretherapy behavior. Most important, she has begun to display many positive attributes which make her more approachable by others

and more responsive to social reinforcers. However, even with these gains, we anticipate having to deal with periodic flare ups of aggressive and antecedent behaviors. We are not so naive as to expect the aversive program alone to be sufficient to eliminate the patient's core psychopathology and completely reverse the effects of the superimposed psychopathology acquired, and inadvertently reinforced by others, over long years of institutionalization. We also have no way of predicting just how lasting even her current gains will be. It is quite possible that if and when the patient becomes inured to faradic shock, she may lapse to former behavior patterns rather than continue to show improvement. Nevertheless, with the threat of violence reduced and with the beginning responsiveness to social reinforcers, we now at least have a therapeutic foothold for exploring less drastic psychosocial procedures.

In conclusion, we wish to state that we are well aware of the many sensitive ethical issues associated with the use of punishment as therapy. No responsible clinicians can embark on this type of treatment program without first asking and answering for themselves the crucial questions dealing with the use of coercion for the modification of human behavior, the rights of patients and the time-worn ends-means controversy. Since we have dealt with many of these issues elsewhere, we feel that there is little to be gained from repeating our views. Suffice it to say that given the dangerousness of the patient's behavior, the ineffectiveness of prior treatment approaches and the safety of faradic shock as compared to the possible consequences of more drastic therapies, we were able to conduct this program with few ethical qualms. Even if the program had not proved successful, we would have felt that the exploration of this therapeutic modality was ethically justified.

## NOTES

### NOTE 1.

ROBERT G. HEATH  
ELECTRICAL SELF-STIMULATION OF THE  
BRAIN IN MAN\*

Two patients were used in the study. Patient No. B-7, age 28, with a diagnosis of narcolepsy and cataplexy, had failed to respond to conventional treatments. He had electrodes implanted

by the method developed in our laboratory into 14 predetermined brain regions and fixed to remain in exact position for prolonged study.

Patient No. B-10, age 25, a psychomotor epileptic with episodic brief periods of impulsive behavior uncontrolled with the usual treatments, had 51 leads implanted into 17 brain sites.

\* \* \*

Stimuli were delivered from a specially constructed transistorized self-contained unit which was worn on the patient's belt. The unit generated a pre-set train of bi-directional stimulus pulses each time that one of the 3 control buttons was depressed. Each button directed the pulse train to a different electrode pair permitting the operator a possible selection of cerebral sites. . . .

\* \* \*

*Patient No. B-7.* After randomly exploring the effects of stimulation with presses of each of the 3 buttons, Patient No. B-7 almost exclusively pressed the septal button.

Stimulation to the mesencephalic tegmentum resulted in a prompt alerting, but was quite aversive. The patient, complaining of intense discomfort and looking fearful, requested that the stimulus not be repeated. To make certain that the region was not stimulated, he ingeniously modified a hair pin to fit under the button which directed a pulse train to the mesencephalic tegmentum so it could not be depressed.

Hippocampal stimulation was mildly rewarding.

Stimulation to the septal region was the most rewarding of the stimulations and, additionally, it alerted the patient, thereby combating the narcolepsy. By virtue of his ability to control symptoms with the stimulator, he was employed part-time, while wearing the unit, as an entertainer in a night club.

The patient's narcolepsy was severe. He would move from an alert state into a deep sleep in the matter of a second. Recognizing that button pressing promptly awakened him, fellow patients and friends occasionally resorted to pushing the button if he fell asleep so rapidly that he was unable to stimulate himself.

The patient, in explaining why he pressed the septal button with such frequency, stated that the feeling was "good"; it was as if he were building up to a sexual orgasm. He reported that he was unable to achieve the orgasmic end point, however, explaining that his frequent,

\* 120 *American Journal of Psychiatry* 571-576 (1963). Copyright 1963, the American Psychiatric Association. Reprinted by permission.

sometimes frantic, pushing of the button was an attempt to reach the end point. This futile effort was frustrating at times and described by him on these occasions as a "nervous feeling."

*Patient No. B-10.* Studies conducted on the psychomotor epileptic patient were more varied and provided more information concerning subjective responses. . . .

The button most frequently pushed provided a stimulus to the centromedian thalamus. This stimulus did not, however, induce the most pleasurable response; in fact, it induced irritability. The subject reported that he was almost able to recall a memory during this stimulation, but he could not quite grasp it. The frequent self-stimulations were an endeavor to bring this elusive memory into clear focus.

The patient most consistently reported pleasurable feelings with stimulation to two electrodes in the septal region and one in the mesencephalic tegmentum. With the pleasurable response to septal stimuli, he frequently produced associations in the sexual area. Actual content varied considerably, but regardless of his baseline emotional state and the subject under discussion in the room, the stimulation was accompanied by the patient's introduction of a sexual subject, usually with a broad grin. When questioned about this, he would say, "I don't know why that came to mind—I just happened to think of it."[\*] The "happy feelings" with mesencephalic stimulation were not accompanied by sexual thought.

\* \* \*

Changes in parameters of stimuli to a given region of the brain, including current intensity, wave form, pulse width, and frequency, in many

[\*] . . . Patient No. B-10, the psychomotor epileptic, was stimulated in the septal region during a period when he was exhibiting agitated, violent psychotic behavior. The stimulus was introduced without his knowledge. Almost instantly his behavioral state changed from one of disorganization, rage, and persecution to one of happiness and mild euphoria. He described the beginning of a sexual motive state. He was unable, when questioned directly, to explain the sudden shift in his feelings and thoughts. This case demonstrates a phenomenon which appears to be consistent and which has been repeated in a large number of patients in our laboratories. This phenomenon is the ability to obliterate immediately painful emergency emotional feelings in a human subject through introduction of a pleasurable state by physical or chemical techniques.

instances altered the patients' responses. This has similarly been reported with animal ICSS [Intracranial Self-Stimulation].

Information acquired from the patients' reporting of their reasons for button pressing indicates that all ICSS is not solely for pleasure. The highest rate of button pressing occurred with Patient No. B-7 when he was somewhat frustrated in his pleasurable pursuit and as he attempted to achieve an orgasmic end point. In Patient No. B-10 the highest rate of button pressing also occurred with frustration, but of a different type, evolving with attempts to bring into focus a vague memory that ICSS had evoked. The subject's emotional state in this instance built into strong anger. It was interesting that the patient would button press to stimulate the region within the centromedian thalamus for a prolonged period, but at a slower rate when buttons providing more pleasurable septal and tegmental stimulation were also available. Depression of the septal button, with resultant pleasant feelings, alleviated the painful emergency state, according to the subject's report, and thereby provided him comfort to pursue his quest for the fleeting memory.

With septal stimulation in other patients, as well as the two subjects discussed here, a sexual motive state has frequently been induced in association with pleasurable response. This sexual state has not developed in association with pleasurable feelings during stimulation to other regions.

\* \* \*

#### NOTE 2.

SIR JOHN ECCLES  
EXPERIMENTS ON MAN IN  
NEUROPHYSIOLOGY\*

\* \* \*

There is another kind of investigation that is often carried out and which I am extremely concerned about—investigations involving so-called indwelling electrodes. These are placed deep in various parts of the brain and X-ray controlled to see where they are. The electrodes are often as much as 2 or 3 millimetres in diameter and have multiple channels on them. They are inserted through a trephine hole in the skull and often stay in for weeks or months; the subjects

\* V. Fattorusso, ed.: *Biomedical Science and the Dilemma of Human Experimentation*. Paris: Council for International Organizations of Medical Sciences 22-23 (1967). Reprinted by permission.

who go home have a little cap on their skull and you can at any time have them back in the laboratory and lead from the hippocampus, the thalamus, or any part of the cortex where the electrodes happen to be lodged.

This, I think, causes fantastic damage to the brain. I do not believe that the subjects have been informed at all of what has been done; they are often under treatment for psychosis and various kinds of epilepsy and so on. This kind of investigation simply horrifies me. I do not believe that this damage to the brain is justifiable on any consideration whatsoever. At least, there should be a tremendous investigation on anthropoid apes using these techniques before they are used on human subjects. And, if there is going to be an investigation on human subjects, then I believe the investigators themselves should be the people who take the electrodes, at least in the first instance. I make this statement categorically.

\* \* \*

#### NOTE 3.

MARTIN J. REIMRINGER, STERLING W.

MORGAN, AND PAUL F. BRAMWELL

SUCCINYLCHOLINE AS A MODIFIER OF ACTING-OUT BEHAVIOR\*

Atascadero State Hospital is a maximum security facility designed to treat mentally disordered sex offenders, the acting-out mentally ill patient, and the criminally insane. Concentrated within its buildings are those individuals whose behavior has been such as to have them considered acute dangers to society.

Within the 1500 patient population, there exists a small number of individuals who continue their aggressive, acting-out behavior during their hospitalization. These patients have frequently been engaged in fights, verbal threatening, deviant sexual behavior, and stealing. Usually, they are not cooperative and are not involved with the hospital treatment program. The procedure followed to curb this unacceptable behavior has been to transfer the patient to a ward for the acutely disturbed, medicate him and, if necessary, place him in restraint until the acute stage was past. Such a technique while having historical precedent is both time consuming and costly in terms of individual benefit. A search for another technique was begun.

The drug, succinylcholine (anectine), is a neuromuscular blocking agent used primarily as

an adjuvant to electroshock treatment and surgical anesthesia. A single intravenous dose of 20 to 35 mg. becomes maximally effective within 34-40 seconds, progressively producing fasciculations and paralysis first in the small, rapidly moving muscles of the fingers, toes and eyes through to the intercostal muscles and the diaphragm. At this point, a period of apnea is produced. During the entire period of muscular paralysis there is neither loss of memory and consciousness nor clouding of the sensorium. Due to the rapid hydrolysis of the anectine by the pseudocholinesterase of the liver and the plasma the effect of succinylcholine is very rapid and complete recovery from a single dose is usually within five minutes. The general psychological effect is to produce a decidedly unpleasant and fearful sensation.

Succinylcholine offers an easily controlled, quickening, fear-producing experience during which the sensorium is intact and the patient rendered susceptible to suggestion.

Ninety male patients were used in an exploratory study to determine the effectiveness of succinylcholine as an agent in behavior modification. The criteria of selection varied, but included persistent physical or verbal violence, deviant sexual behavior, and lack of cooperation and involvement with the individual treatment program prescribed by the patient's ward team. The subjects included overtly psychotic mentally retarded and sociopathic patients.

The number of treatments was predetermined by the ward doctors. The patient was administered an initial intravenous dose of 20 mg. of succinylcholine. Subsequent treatments sometimes required an increase in the dosage in order to meet the desired criteria of 1¼ minute period of apnea. Positive pressure breathing equipment was at the treatment site if the patient's pulse slowed during period of apnea.

After respiration stopped, the talking phase of the treatment began. Both negative and positive suggestions spoken in a confident, authoritarian manner were made by the male technician. The negative suggestions concerned the obliteration of unacceptable behaviors such as fighting and stealing. Positive suggestions focused upon the patient's becoming involved with patient government, the taking of individual responsibility and an increase in constructing socialization. These suggestions continued throughout the period of apnea and until the patient could verbally respond to the technician. After the treatment was completed, the patient was re-

\* *Clinical Medicine* 28-29 (July 1970). Reprinted by permission.

turned to his ward and no other special attention formally given to him.

The data revealed the following results: Improvement was noted in 61 of 90 patients, or 68 per cent; 16 patients, or 18 per cent, were considered temporarily improved; no change was noted in 12 men, or 13 per cent; and in one patient (one per cent) the result was increased violence.

The subjects were rated on the basis of the frequency of acting-out behavior subsequent to the treatments. At Atascadero State Hospital special incident reports are made for each episode of aggressive and/or unacceptable behavior. The subjects were rated as improved if no incident reports appeared in their record for a period exceeding three months. Temporary improvement was given when there were no incident reports for a one-to-three-month period. The "no change" category included those patients whose frequency of acting-out behavior remained approximately the same after treatments as prior to the use of anectine. Only one patient appeared to have shown an adverse reaction to the treatments. Some patients have extended their trouble-free behavior to eighteen months.

\* \* \*

## 2.

### Interferences with Human Evolution

a.

#### Bentley Glass Human Heredity and Ethical Problems\*

The discoveries of molecular biology and genetics during the past twenty years are now generally acclaimed as the most significant scientific advances of our present generation, just as the understanding of the forces of nuclear energy in the atom were those of the preceding generation. Like the application of nuclear energy to both destructive and constructive purposes, the application of the spectacular finding that deoxyribonucleic acid (DNA) is the chemical basis of heredity offers man a magnificent extension of power over nature and at the same time lays

on his conscience a frightening responsibility lest that power be misused.

Within these past twenty years it has been demonstrated conclusively how the DNA of the cell replicates and how errors in replication, errors producible either by high-energy radiation or by certain chemical agents, give rise to mutations, that is, produce permanent hereditary changes most of which are detrimental. Next was discovered the way in which the genetic code which is formed by the sequence of nucleotides in the DNA molecule is transcribed to ribonucleic acid (RNA) and the way in which the messenger molecules of RNA, after migrating from the nucleus of the cell to the cytoplasm, are transcribed on the ribosomes into specific sequences of amino acids in a polypeptide molecule. The problem of protein synthesis, which lies at the basis of all formation of living structures and all control over living processes, has been clarified beyond wildest expectations. The "one gene—one enzyme" hypothesis has become the well established "one gene (or cistron)—one polypeptide" theory, transformed into doctrine or dogma, so quickly has it become a basic concept of biology.

[R]apidly and step by step, the way has been opened to producing a great variety of genes which might be introduced into living cells in order to supply various hereditary deficiencies. Maybe by using a harmless virus as a carrier for such genes, in the manner known to geneticists as transduction, genes can be introduced into particular organs where they are needed and made a part of the regular replicating genome of the cell. Or perhaps, like the original classical transformation experiments of Avery, McCloud, and McCarty with *Pneumococcus* in the 1940's, treatment of deficient cells and organs with the isolated or synthesized active genes will be feasible. In either case, what Joshua Lederberg has called euphenics, the specifically desired modification of the phenotype of a defective organism by treatment, will have advanced a major stride.

The earlier stages of euphenics involved finding out just what gene product was missing in the body, or just what gene substrate, accumulating unused, was proving to be toxic. . . . Later developments of euphenics involved identification of the specific enzymes which are wholly or partly missing in particular hereditary conditions, in the hope that introduction of the enzymes into the body might allay the malady. This hope proved not well grounded, since most enzymes are large protein molecules, and are

\* First Annual Address to the Society for Health and Human Values, Los Angeles, California 1-15 (Oct. 29, 1970). Unpublished manuscript printed by permission of the author who retains all rights.



either digested when administered by mouth or are likely to produce allergic hypersensitivity if obtained from sources other than man himself. Or, if neither of these difficulties arises, the enzyme molecule is often unable to enter the cells where it must function. Hence only relatively small proteins, such as insulin, or enzymes which function extracellularly, such as serum cholinesterase, can be effectively administered, and even in these cases treatment is limited by the available supply.

The newest phase of euphenics, just beginning, may well surmount these difficulties by transferring to the defective person the missing gene itself. As in the case of proteins, the problem is first to get the effective gene into the cell and next to replace the incapacitated allele in the recipient's chromosomes. By means of carrier viruses this may turn out to be feasible. Recent successes in infecting mice with transducing viruses offer considerable encouragement. It may become possible to infect with the virus the specific organs or tissues which require the activity of the gene, without endeavoring to transform the entire body of the recipient genetically.

Since there is still no hint that we will ever be able to produce desired mutations to order, or indeed to eliminate the occurrence of mutations we do not want, we must in coping with genetic disease resort either to methods of euphenics or to eugenic measures that prevent the birth of the genetically handicapped. Recent advances in the detection of the normal, heterozygous carriers of genes producing recessive disorders and of the affection of a fetus at an early age greatly enhance our opportunities to reduce specific disorders. As for the former, the list of heterozygous "inborn errors of metabolism" which can be detected biochemically or by karyotype analysis is increasing so rapidly, at a rate of three or more each year, that already over sixty can be detected and the identity of the critical marriages in which two carriers are wed could theoretically be established. However, even in the case of the most prevalent of these, cystic fibrosis, which affects about one birth in each 2000, the frequency in the population of heterozygous persons (1 in 22) signifies that only one in 484 matings, random with respect to the possession of the gene, will run the high genetic risk, one in four, of producing an affected child. This frequency is not enough to require that all marrying persons be screened to determine the carriers among them, although perhaps all first cousins who marry should be tested, since the rarer the

condition the greater the probability of consanguinity among the parents. But when, a few years from now, the number of heterozygous conditions which can be detected exceeds one hundred, and tests can be made both cheaper and simpler, multiple testing will become routine. The legal requirement in some states at the present time that all newborn infants be tested routinely for phenylketonuria points the way. This condition, the adverse effects of which upon mental development can be counteracted by controlled diet if detected and treated early enough in life, affects only about one child in 40,000. The heterozygous carriers, on the other hand, are at a ratio in the population of one per cent. If the test for the heterozygous condition were as simple and as cheap as that for the homozygous defect, it would be far better to test everyone for presence of the recessive gene, much as we test and identify everyone in respect to their ABO and Rh blood groups. Such tests, carried out in the maternity hospital for each baby and recorded in a computer system, would enable appropriate measures to be taken. What would these be?

Before grappling with this difficult question, let me describe another scientific advance of the past fifteen years that, like the biochemical detection of heterozygous carriers, opens the way to new eugenic and euphenic measures. Cytological methods enabling the investigator to examine the karyotype, that is, the chromosome constitution, of a person with minimal difficulty are now available. After the first development in 1955 of the techniques for examination of smears of whole cells treated to spread out the chromosomes, to stop cells just prior to metaphase during cell division, and to stain them appropriately, great advances were made in the detailed identification and characterization of the normal human chromosome complement, or karyotype. Lejeune and his colleagues pioneered in the detection of chromosome abnormalities of the karyotype by discovering in 1959 that Down's syndrome, familiarly called mongolism, is regularly characterized by the presence of an extra chromosome, one of the smallest in the set, known as Chromosome 21. . . . Usually these chromosome conditions can be identified from cultured skin cells. . . . Most recently, a technique known as amniocentesis has been developed. By means of a hypodermic needle, inserted into the amniotic fluid surrounding a fetus, amniotic fluid containing cells which may be cultured and examined can be withdrawn. Thus pre-



natal diagnosis may be performed, either chromosomal or biochemical. Puck has recently stated that one per cent of all human births carry a chromosome abnormality, and that the cost of caring for the Down's syndrome "mongols" in the United States is alone estimated, according to the National Foundation, to be \$1.7 billion annually. Obviously, the cost of developing reliable and inexpensive methods of detecting the carriers of such conditions, when transmissible, and of coding the population genetically could be largely offset by savings from the elimination of even one such defect.

It is necessary, however, to examine the difficulties that stand in the way. In the case of Down's syndrome and other monosomic or trisomic conditions of the karyotype, most affected individuals are sterile or lack normal sexual drives. Hence most affected cases arise through errors in chromosome distribution (*i.e.* non-disjunction) occurring in the reproductive cells of the parents. In this respect they resemble fresh mutations of the genes, especially dominant mutations. Two alternative approaches may be taken. One may attempt, by treatment, to correct the condition and restore development to normality. In the case of a dominant mutant such as retinoblastoma, a condition once invariably fatal in childhood, surgical removal of the affected eye, if performed soon enough and with full removal of the cancerous condition, permits the child to retain the vision of one eye and to grow normally to adulthood. The sequel is alarming, since most of these patients then marry and pass on to half of their children, on the average, the devastating genetic disorder which will again require radical surgery. One is forced to ask whether it would not be better in some way, in these circumstances, to force the person involved to remain childless. The second alternative is more applicable to a condition such as mongolism, in which reproduction of the affected person is very unlikely but the cost of care—the burden upon family and upon society—is very great. Early diagnosis of the condition by amniocentesis would permit surgical abortion of the affected fetus.

Mongolism, and probably chromosomal abnormalities in general, increase sharply in frequency with advancing age of the parents, particularly the mother. In mothers over 35 years of age the incidence of Down's syndrome among births rises to 2 per cent, and in mothers over 40 years of age to 10 per cent or higher. It follows that even if universal fetal testing cannot

be legally prescribed, the testing of all prospective births to mothers over the age of 35 years would be unquestionably desirable. For other conditions it is the more advanced age of the father which is involved. The ethical question at once becomes clear. Inasmuch as no effective treatment for mongolism or other chromosomal abnormalities capable of surviving birth is known, and inasmuch as the social and psychological cost to both family and society is in general so high, abortion is regarded by many geneticists as the best way to handle the matter. Yet where society has banned abortion no improvement in the quality of the population by reducing the one per cent of births with chromosome abnormalities is to be expected.

The ethical problem is more acute in the case of a simple dominant gene of malignant character, such as that which produces retinoblastoma. By permitting the survival and reproduction of an affected person in this case there is inevitably a rise in the frequency of the condition in future generations, unless stern measures such as sterilization of the surviving retinoblastoma cases is made mandatory or unless by genetic counsel they are indeed persuaded not to have children. In the latter case, since contraceptive measures are not perfect and may be carelessly utilized, offspring may be produced in spite of good intentions.

... Most serious of all is the fact that many socially undesirable traits depend upon multiple genes. General mental inferiority, grading down into imbecility and idiocy, is of this nature. In such cases it may in time be possible to learn how to modify the environment so as to prevent any manifestation of overt disease or inferiority. Yet by surrounding ourselves with an ever more artificial environment, we unwittingly modify the rigor of natural selection in many ways. The price we must pay, in the end, for the mercies of medical care and surgical aid is a dysgenic increase in the frequencies of certain detrimental genes the effects of which we have learned to ameliorate. Thousands of diabetics who in a former day would have died early in life are now saved by insulin to live relatively normal lives, and of course to pass on to their descendants the genes responsible for their diabetes. Myopia is no longer, as it may have been in man's early hunting existence, a grave handicap in life; hearing aids alleviate certain types of deafness. No one, I think, would have it otherwise. Yet to contemplate the man of tomorrow who must begin his day by adjusting his spectacles and his hear-

ing aid, inserting his false teeth, taking an allergy injection in one arm and an insulin injection in the other, and topping off his preparations for life by taking a tranquillizing pill, is none too pleasant. To say the least, medical science steadily increases the load it must carry.

A still more serious difficulty in the analysis of the genetic load of detrimental genes and in the choice of policies to be pursued toward that load lies in the existence of genes which are detrimental in one environment but confer a benefit in another, or of genes in which the heterozygote is more highly favored by selection than either of the homozygous types. A now classic example of both of these situations is that of sickle hemoglobin. The homozygote with two sickle hemoglobin alleles almost invariably dies of a fatal anemia, in spite of the utmost that can be done for him by means of blood transfusions. The homozygote with only standard hemoglobin falls prey, in heavily malarial regions, to that once greatest of human killers. The heterozygote, possessing half sickle hemoglobin and half standard hemoglobin, in such regions far surpasses in survival and reproductive capacity the two homozygotes.

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The measures of negative eugenics prove ineffective, just as those of euphenics prove self-defeating in a long-term evolutionary sense. Segregating defectives in institutions where they cannot reproduce is temporary, and too often occurs only after the act. Sterilization would be effective only in the case of a simple, readily detected dominant condition, but is of little use in the far more numerous cases of irregularly expressed dominant conditions, recessive traits, and especially multifactorial traits. It cannot discriminate between the alleles whose heterozygote is superior to the homozygous types. Free birth control and contraceptives may be important in population control, but would have little to contribute to the genetic well-being of a population unless accompanied by a level of genetic understanding, counselling, and conscience that today seems remote. . . .

We must turn, then, from negative aspects to positive eugenics, more popularly termed "genetic engineering" today. . . .

For any program of positive selection or genetic manipulation we must clearly establish goals and standards. Difficult, yet easiest, would be to determine that a certain allele  $A^1$  was inferior in quality to its competitor, allele  $A$ . Then

we might substitute allele  $A$  for allele  $A^1$  either euphenically, by introducing the effective gene into the tissues of the recipient whose own allele is  $A^1$ , or eugenically, by substituting  $A$  for  $A^1$  in the same individual's reproductive cells. It has not been demonstrated at the present date that the latter transformation is feasible, but conceivably a harmless virus carrier might transduce immature sperm cells or ova as readily as cells of liver or kidney. Let us concede that, could this be done in the case of a sufferer from retinoblastoma, the action would be laudable. For the overwhelming majority of genetically conditioned traits, however, no such simple solution is possible. Clearly, for the most significant traits the values and goals are not for science alone to impose—we are concerned with social values, and which of these, we must ask, is preeminent? I once suggested that we might agree upon such goals as "freedom from gross physical or mental defects, sound health, high intelligence, general adaptability, integrity of character, and nobility of spirit." I did not imply that these characteristics were in any case fully or even partially genetic in nature. H. J. Muller selected a different list: "genuine warmth of fellow feeling and a co-operative disposition, a depth and breadth of intellectual capacity, moral courage and integrity, an appreciation of nature and art, and an aptness of expression and of communication;" on the physical side, "to better the genetic foundations of health, vigor, and longevity; to reduce the need for sleep; to bring the induction of sedation and stimulation under more voluntary control; and to develop increasing physical tolerances and aptitudes in general." I suspect that any other scientist will select a different, though overlapping, list.

Nor can we select for such qualities without having ways and means of defining them precisely and measuring them at least roughly in a quantitative way. Obviously, the psychologist and the sociologist will need to do a great deal of preliminary work before genetic analysis and understanding of these traits become possible. At present, human behavior genetics is still a young, rather undeveloped field.

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It should be feasible, long before the year 2000, to bank human reproductive cells of both sexes in frozen state, as we now do with the sperms of domestic animals, especially sheep and cattle. In this way the reproductive cells of selected individuals might be utilized, even long

after death, to produce in the laboratory embryos that might be implanted in the womb of a foster mother, or even, after sufficient development of techniques, to be grown in bottle cultures. The latter "brave new world" technique I do not expect to see realized by the turn of the century. On the other hand, I do expect that techniques will be developed for the cultivation in the laboratory of portions of human ovary and testis permitting successful continuous production of mature ova or sperms. Recent successes in the production of mature ova from cultured mouse ovaries lead me to expect that only persistence by a sufficient number of skilled biologists is needed to attain successful cultivation of human reproductive organs, continuous production of eggs and sperms, and formation by fertilization in the laboratory of as many human embryos as may be wished.

In 1970 piglets conceived in Canada were flown across the Atlantic Ocean in a tube and implanted in a sow of a different breed. The parents were three sows and a boar of the Canadian Yorkshire breed, and the embryos were conceived on March 16 on a Canadian Department of Agriculture farm at Hull, Quebec. The foster mother, at Weybridge, Surrey, has delivered her litter of "improved" piglets. It seems quite clear that the techniques I am describing need only to be applied to mankind, and improved in minor ways.

Geneticists are looking forward to the day when they can practice genetic surgery, that is, really reach in and transform a defective gene and make it functional again. That will not be easy. In the first place, it would have to be done either in the very young embryo, before the cells have begun to multiply, or in the reproductive cells that actually function in making new individuals. . . .

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Another new technique that has extraordinary possibilities lies in the extension to man of the vegetative propagation of the individual. In frogs it has proved possible to implant the nucleus of a skin cell or an intestinal cell in a fertilized frog egg whose own nucleus has been surgically removed or inactivated by ultraviolet light. Young frogs have now been reared to adulthood from such eggs, and prove to have all the genetic characteristics of the strain that provided the implanted nucleus, not those of the original strain or species from which came the egg. It is thus plausible to think that human individuals

might be multiplied from tissues—even long-stored frozen cells—by implantation of the somatic nucleus from such cells into freshly obtained ova. A multiplied clone of Einsteins or Sophia Lorens might turn out to be disappointing on the average, although as identical genetically as any monozygous twins. The experiment would be intriguing.

I am frequently asked why anyone should wish to pursue these goals: "Aren't the age-old ways of making babies good enough?" Several reasons may be given why exploration of such new possibilities is desirable. Only by studying the development of the human embryo and fetus under continuous observation and under various conditions can life scientists really learn what factors produce particular kinds of abnormalities and how these may be corrected or avoided. Moreover, the practice of "prenatal adoption," that is, the implantation of a healthy selected embryo in a foster mother's womb, appears to have fewer religious and legal objections than the present practice of artificial insemination of a woman, without consent of her husband, or even with it. For prenatal adoption is clearly a true adoption of a child, *not* a usurpation of a husband's or wife's right to procreate. The development of the implanted fetus within the mother and its normal delivery at full term will surely engender the maternal and paternal feelings of the "parents" far more fully than adoption of a child already several years old. Moreover, most couples who are sterile might in this way have the full experiences of parenthood, apart from transmitting their own genes.

There are other reasons why such practices might be adopted, if not in the United States, then possibly in other countries. Banking of reproductive cells taken from individuals around 20 years of age may serve to prevent the accumulation of detrimental mutations with advancing age. . . . In the presence of the threat of nuclear war, the safest way to ensure the survival and genetic health of future generations would be to carry on these artificial reproductive practices of banking and culture selection, fertilization, and subsequent implantation in subterranean laboratories safe from direct nuclear radiation and fallout.

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Now we come to the most serious aspect of what I term our present crisis of values and goals. How can one select good strains of reproductive cells? If the same material is used to pro-

duce a great many embryos which are reared into babies, they will be too much alike, like members of the same caste in *Brave New World*. This difficulty might be avoided by never using a single line of reproductive cells more than a few times. There is another difficulty. Nearly all of us carry some defective genes. The average is probably around eight. We lack visible signs of defect only because most defective genes are recessive; that is, must be inherited in a double dose, coming from both father and mother, in order to produce an evident defect. As long as we have one working gene belonging to any particular pair of genes, enough of the protein it controls is made to satisfy general needs. Close relatives, however, have a greatly heightened probability of carrying the same defective gene because of possessing a common ancestor. It would therefore be necessary to have strict rules to prevent offspring being produced by persons derived from the same lines of banked or cultured reproductive cells, and careful records on the lineage of each person would have to be kept.

In a population suffering severely from overcrowding and subject to rigorous limitations of births, eugenics might be related rather simply to the measures for population control. For example, if a couple that has used up its coupons for two babies wanted additional children they might be required to meet certain genetic tests before receiving a special permit. Some additional children, above the limitation of two per couple, would be needed in some families to maintain the population at the same level, since some women have no children or only one, for a variety of reasons. The simplest eugenic test, yet one that in the long run might be quite effective in improving the population, would be simply to examine the first two children in order to assure that neither one was physically or mentally below average. Beyond the application of so simple a test, eugenic selection runs into frightful dilemmas. Who really possesses a "good" genotype? . . .

. . . When I read in the Bill of Human Rights of the United Nations that one inalienable right of the individual is to reproduce, and that the right of every person to have a family is a basic human right that must not be infringed, I wonder whether this "right" is indeed to remain unrestricted. Is it not equally a right of every person to be born physically and mentally sound, capable of developing fully into a mature individual? Has society, which must support at great cost the burden of genetic misfortune resulting

from mutation, chromosomal accident, and prenatal harm inflicted by trauma or virus, no right at all to protect itself from the increasing misfortune? Should not the abortion of a seriously defective fetus be obligatory? Should not the loss of a defective child be recompensed by the opportunity to have another, a sound, child by prenatal or postnatal adoption? Can we not devise laws and practices that will improve, even though slowly, the quality of our population while we retain individual choice and freedom to a great extent? Cannot the substitution of a greater freedom of choice in new respects compensate for the restriction of some time-honored privileges?

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I wonder what will be the effect of a complete liberation of the sexual life from its relationship to reproduction upon society and upon the family in particular. Recently Robert Morrison of Cornell University has pointed out the grave threat to the continuance of the family as the basic social unit. Can we safely, after a million years of human and prehuman evolution during which the family has been the basis of all protection, education, and nurture, give it up? What will be the psychological consequences of a population with no personal ties either to the older generation or to the younger generation? Can we look forward to a brotherhood of mankind when there are no more parents, brothers, or children, only unrelated people?

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b.

**Joshua Lederberg  
Experimental Genetics and  
Human Evolution\***

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The elaboration of eugenics is, however, not the main purpose of a discussion of human evolution, except for the one point—the added difficulties it creates for any measure of human value. If this subject were not at the heart of the eugenic controversy, it would be arrogant to insist on the discussion of it.

Reconsider how we must reevaluate the cumulative score of a human genotype regarded over a lifetime, and for its contribution to the

\* 100 *American Naturalist* 519, 522–529, 531 (1966). Copyright © 1966 by the American Naturalist. Reprinted by permission.

human future. Besides present perplexities, look to future perturbations:

1) *Durability*. The mere extension of life-span alters the scores. Performance must be measured over the whole term of life, not based only on youthful precocity.

2) *The eutheic context*. Educational opportunity and practice are changing rapidly. Consider

*Recognition of individual diversity*. Educators have begun to learn, and exercise the knowledge, that children vary widely in the details of their information-processing machinery, e.g., the relative acuity of their sensory modalities. Many "dull" children must be reclassified as over-specialized; we might well make virtue out of necessity in enabling each child to exploit his inherent skills. . . .

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Western culture and its limited population is being succeeded by a much broader world culture. Is there much point in setting eugenic standards relevant only to a small minority of the world's population even as we watch the unprecedented breakdown of intercultural barriers? The jet airplane has already had an incalculably greater effect on human population genetics than any conceivable program of calculated eugenics.

3) *The world situation*. The central problem for the species must bias any momentary evaluation. Until recently, this was perceived as agricultural efficiency. Hunger still haunts the earth, but we might just manage to marshal the technical resources to assuage it. The specter of the industrialized world is suddenly nuclear suicide, and this has already led to some concern as to the biological adaptation of the species most appropriate to an age dominated by nuclear power. Political institutions are likely to change course much more rapidly than any biological response. As has been pointed out repeatedly, adaptability is man's unique adaptation.

This begs the question how to anticipate future needs, how far adaptability can be generalized, and how well it can compete, in any well-defined microniche, with more rigorous specialization. To put it another way, how do we identify the most adaptable genotypes now living and what is the price, to the detriment in special skills, of this adaptability?

4) *Response to euphenics*. The medico-technological context of human performance is more predictable than the socio-political. We are already committed to the attempted eradication

of infectious agents like malaria, tuberculosis, cholera, variola, and poliovirus. In consequence, any breakdown of public health services can be catastrophic by exposing large, imperfectly immunized populations to these parasites. If the interplay of Hemoglobin S and malaria is a useful model, genetic adaptations to a germ-free environment are taking place too; chemical pollution might replace germs as a major selective factor except that its cumulative impact on adults is less cogent than acute infanticide. The context of modern man, in fact, includes steadily increasing reliance on medicine, i.e., euphenics, from ovulation onwards. It makes as little sense to decry genetic adaptations to this as to other components of civilized life. The quality of a genotype cannot now be evaluated in terms of a hypothetical state of nature (wherein we would quickly grunt in chilly displeasure at our unfurred skins), but must match the pragmatic expectations of the milieu of the individual and his descendants. In fact selection is so slow, especially for rare genes, as to make this a theoretical issue for some time. It would be a tour-de-force to demonstrate any change in the frequency of a specific deleterious gene in a human population that could be unambiguously traced to a relaxation of natural selection against it. In comparison to the pace of medical progress, these exigencies are trivial.

As medical practice evolves so does the evaluation of health and vigor. What has happened to pancreatic diabetes is happening to phenylketonuria, and is bound to happen to many other biochemical and developmental diseases. Indeed, it would be no surprise to find compensating advantages, in certain contexts, for some of these genotypes.

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Recall that the most successful exercises in plant breeding have not established pure lines of vigorous individuals. Instead, somewhat overspecialized strains are nurtured and the latent resources of individually unpromising parents are merged in vigorous hybrid off-spring. (A good farmer has learned how common sense conflicts with reality when he tries to use ears of hybrid corn as seed for another generation.)

5) *Social adjustment*. We are on the shakiest ground trying to sort out the genetic basis of such social diseases as crime and delinquency. In any case we have a long way to go in elucidating how nature and nurture interact in this field; e.g., what penalty the species would suffer

by extirpating every gene that might in some environment contribute to crime and rebellious behavior. Instability of family life, the estrangement of the generations, and the shallowness of human communication are more prevalent and cumulatively more serious diseases than violent crime, and must be given equal account in any effort to define the "good man," or in any lament of human deterioration.

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6) *The sexual dimorphism.* Most eugenic discussions have been overwhelmingly male-oriented, as is academic life. Western culture is more paradoxical than ever in its assignment of roles to women, and thereby in the design of their education and the advertised criteria of feminine success, stressed by conflicting demands for decoration and utility, dependence and initiative. The lack of useful occupation for many older women is a premonition of the leisure society where "work may become the prerogative of a chosen elite." Half the beneficiaries of eugenic design will be women. Will their creativity and happiness be augmented in a genotype that recombines XX and a set of male-oriented autosomes? Or shall we bypass the dimorphism and evolve a race where this does not matter? To shout "Vive la difference" and then ignore it is hypocrisy.

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7) *The leisure society.* This discussion has been dominated by criteria of performance at work. The whole framework may be obsolescent on the time scale of a few generations. As machines come to do almost all of the work, and this must include managerial and inventive tasks as well as clerical and manual, what are the relevant human values? Will not boredom be the most pernicious disease, and a zest for life without the compulsion of labor the rare essential for the species? Play rather than work will be the substratum of human activity, and the transmutation of play into cultural progress will replace the underpinning by industrial and military technology of its superstructure of basic science.

Perhaps the scientist who works for his joy in it is the most nearly pre-adapted for that topsy-turvy world, obviously an impeccable criterion for eugenic choice.

This leads us finally to algeny. Man is indeed on the brink of a major evolutionary perturbation, but this is not algeny, but *vegetative propagation*. . . .

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Vegetative or clonal reproduction has a certain interest as an investigative tool in human biology, and as an indispensable basis for any systematic algenics; but other arguments suggest that there will be little delay between demonstration and use. Clonality outweighs algeny at a much earlier stage of scientific sophistication, primarily because it answers the technical specifications of the eugenicists in a way that Mendelian breeding does not. If a superior individual (and presumably then genotype) is identified, why not copy it directly, rather than suffer all the risks of recombinational disruption, including those of sex. The same solace is accorded the carrier of genetic disease: why not be sure of an exact copy of yourself rather than risk a homozygous segregant; or at worst copy your spouse and allow some degree of biological parenthood. Parental disappointment in their recombinant offspring is rather more prevalent than overt disease. Less grandiose is the assurance of sex-control; nuclear transplantation is the one method now verified.

Indeed, horticultural practice verifies that a mix of sexual and clonal reproduction makes good sense for genetic design. Leave sexual reproduction for experimental purposes; when a suitable type is ascertained, take care to maintain it by clonal propagation. The Plant Patent Act already gives legal recognition to the process, and the rights of the developer are advertised "Asexual Reproduction Forbidden."

Clonality will be available to and have significant consequences from acts of individual decision—Medawar's piecemeal social engineering—given only community acquiescence or indifference to its practice. But here this simply allows the exercise of a minority attitude, possibly long before its implications for the whole community can be understood. Most of us pretend to abhor the narcissistic motives that would impel a clonist, but he (or she) will pass just that predisposing genotype intact to the clone. Wherever and for whatever motives close endogamy has prevailed before, clonism and clonishness will prevail.

Apogamy as a way of life in the plant world is well understood as an evolutionary cul-de-sac, often associated with hybrid luxuriance. It can be an unexcelled means of multiplying a rigidly well-adapted genotype to fill a stationary niche. So long as the environment remains static, the members of the clone might congratulate themselves that they had outwitted the genetic load;

and they have indeed won a short-term advantage. In the human context, it is at least debatable whether sufficient latent variability to allow for any future contingency were preserved if the population were distributed among some millions of clones. From a strictly biological standpoint, tempered clonality could allow the best of both worlds—we would at least enjoy being able to observe the experiment of discovering whether a second Einstein would outdo the first one. How to temper the process and the accompanying social frictions is another problem.

The internal properties of the clone open up new possibilities, e.g., the free exchange of organ transplants with no concern for graft rejection. More uniquely human is the diversity of brains. How much of the difficulty of intimate communication between one human and another, despite the function of common learned language, arises from the discrepancy in their genetically determined neurological hardware? Monozygotic twins are notoriously sympathetic, easily able to interpret one another's minimal gestures and brief words; I know, however, of no objective studies of their economy of communication. For further argument, I will assume that genetic identity confers neurological similarity, and that this eases communication. This has never been systematically exploited as between twins, though it might be singularly useful in stressed occupations—say a pair of astronauts, or a deep-sea diver and his pump-tender, or a surgical team. It would be relatively more important in the discourse between generations, where an older clonant would teach his infant copy. A systematic division of intellectual labor would allow efficient communicants to have something useful to say to one another.

The burden of this argument is that the cultural process poses contradictory requirements of uniformity (for communication) and heterogeneity (for innovation). We have no idea where we stand on this scale. At least in certain areas—say soldiery—it is almost certain that clones would have a self-contained advantage, partly independent of, partly accentuated by, the special characteristics of the genotype which is replicated. This introverted and potentially narrow-minded advantage of a clonish group may be the chief threat to a pluralistically dedicated species.

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My colleagues differ widely in their reaction to the idea that anyone could conscientiously risk

the crucial experiment, the first attempt to clone a man. Perhaps this will not be attempted until gestation can be monitored closely to be sure the fetus meets expectations. The mingling of individual human chromosomes with other mammals assures a gradualistic enlargement of the field and lowers the threshold of optimism or arrogance, particularly if cloning in other mammals gives incompletely predictable results.

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Paradoxically, the issue of "subhuman" hybrids may arise first, just because of the touchiness of experimentation on obviously human material. Tissue and organ cultures and transplants are already in wide experimental or therapeutic use, but there would be widespread inhibitions about risky experiments leading to an object that could be labelled as a human or para-human infant. However, there is enormous scientific interest in organisms whose karyotype is augmented by fragments of the human chromosome set, especially as we know so little in detail of man's biological and genetic homology with other primates. This is being and will be pushed in steps as far as biology will allow, to larger and larger proportions of human genome in intact animals, and to organ combinations and chimeras with varying proportions of human, subhuman, and hybrid tissue (note actual efforts to transplant primate organs to man)...

## NOTE

DAVID M. RORVICK  
THE TEST-TUBE BABY IS COMING\*

\* \* \*

[W]e are poised for the performance of a medical procedure as dramatic as, and almost certainly more far-reaching than, the heart transplants of the 1960's—embryo implants and transplants. . . . Some recent announcements of successful test-tube fertilizations have been received by the press as "firsts" when, in fact, Dr. John Rock of Harvard, the birth-control pioneer, achieved this back in the 1940's.

He obtained ripe eggs from female patients and then exposed them to sperm in the test tube. No special medium was used to incubate the eggs, and success was slight. None of Dr. Rock's "test-tube babies" grew beyond the three-cell stage. . . .

\* *Look* 83-86 (May 18, 1971). Reprinted by permission of David M. Rorvick.



Beginning in the 1950's, Dr. Landrum B. Shettles, an assistant professor of obstetrics and gynecology at Columbia University's College of Physicians and Surgeons, refined Dr. Rock's experiments to an art. He became the first to demonstrate conclusively that *in vitro* ("in glass") fertilization of the human ovum is possible. In the course of performing various operations requiring abdominal incision into the peritoneal cavity of the female, Dr. Shettles pierced the ovaries of his patients with a syringe and aspirated, or drew up into the syringe, some of the eggs from their follicles. At the same time, without harming the patient in any way, he drew off some of the follicular fluid, excised tiny pieces of the tubal fimbriae, the fingerlike projections at the end of the Fallopian tubes that "pick up" the egg when it is ejected from the ovary, and aspirated some of the mucosa that abounds within the tubes. From all of this, he formulated the culture mediums in which the egg could mature and undergo fertilization in the test tube.

When the egg, bathed in follicular fluid, was ready for fertilization, Shettles placed it in a sterile Petri dish containing another culture medium. Here the prime ingredient was ovulation mucus taken from the mid-cervical canal of the woman who provided the egg. Into this, he "unleashed" millions of sperm cells and let them fight their way to the egg—just as they would in nature. At the right moment, he added tubal mucosa and the tiny bits of tubal fimbriae in order, again, to provide the chemical components that the sperm cells normally encounter when they pass from the uterus into the tubes, still in pursuit of the egg.

Thus, he became the first man in the world to witness the drama of human fertilization. . . .

\* \* \*

Dr. Shettles . . . grew several of his test-tube babies to the so-called blastocyst stage (64 cells and up). It is at this stage that the egg would normally attach itself to the lining of the uterus, having first made a leisurely five-day trip down the Fallopian tube. When Dr. Shettles first began experimenting, however, too little was known to attempt implanting one of his test-tube embryos in the uterus. Therefore he was forced to sacrifice what he had created.

\* \* \*

Dr. Rock, writing in the *American Journal of Obstetrics and Gynecology* in 1958, said: . . . "The time may be rapidly approaching when the

poor woman whose tubes have been excised, yet who still wants a baby, will rejoice that Dr. Shettles will be able to extract an ovum from her ovary . . . then fertilize the egg *in vitro* . . . and finally put it back in the uterus. Thus will he impregnate the woman in spite of the fact that she has no tubes."

Experiments in the 1960's, demonstrating that embryos could, with good results, be transferred from one animal to another, suggested still other possibilities. It appeared that an egg produced by one woman could be implanted in another for any of a number of reasons. Perhaps the donor of the egg, in one instance, has viable tubes but, because of a heart condition, cannot risk the rigors of pregnancy. Still, she *could* procure another woman (probably a sister or other close relative) to carry her child for her. A woman who has viable tubes and normal uterus but nonfunctioning ovaries, on the other hand, could seek out an egg donor, have the ovum fertilized in the test tube by her husband's sperm and then implanted in her uterus. This projected procedure is called "prenatal adoption" and is simply an extension of artificial-insemination techniques already in wide use. Finally, a completely healthy young woman, reluctant to remove herself from a rewarding career for even a few months, might still produce a child by *hiring* another woman to carry her embryo to term.

\* \* \*

Understandably . . . the leaders in the field are maneuvering with great caution. Neither Dr. Shettles nor his chief competitor, a British team led by Dr. Patrick Steptoe of Oldham General Hospital, Lancashire, and Dr. R. G. Edwards of Cambridge University, are presently granting interviews. They prefer, instead, to work quietly behind the scenes while they perfect their techniques. Some of their recent results, however, have been reported in various technical journals, and it has now become possible to make some assessment of their progress.

The British, though they have only recently duplicated Dr. Shettles' decade-old success with *in vitro* fertilization, now appear to be making rapid progress, thanks to a concerted team effort and ample research funds. In 1970, they were at last able to grow several test-tube babies to the blastocyst stage. Since then, they have been examining advanced embryos for cellular defects, to be certain that test-tube fertilization does not result in anomalies that could cause birth defects. The embryos they finally implant, however, will



not have the benefit of such examinations, since these would be disruptive to normal growth. "It will probably call for a brave decision," Dr. Step-toe has said, referring to the first implant attempt. . . .

Dr. Shettles, meanwhile, continues to solo, and has only recently applied for Federal funds to help his research. Nonetheless, he has just revealed . . . that he has *already* performed an embryo-transplant operation in the human, a historic step. The circumstances, however, were such that the embryo was not permitted to develop for any length of time. As Dr. Shettles reports it, a nearly mature egg was aspirated from its follicle in the ovary of a woman undergoing an operation to correct a defect in one of the Fallopian tubes. The egg was matured *in vitro*, fertilized with sperm from the woman's husband, grown in a culture for five days to the blastocyst stage and then implanted in the uterus of a second woman. The menstrual cycles of the two women had already been synchronized to insure a hospitable reception for the transplanted embryo.

Two days after the transplant, a previously scheduled hysterectomy (for cancer of the cervix) was performed on the recipient. The implanted embryo was then located, with a dissecting microscope, in the lining of the excised uterus. An examination showed that it had implanted properly. It consisted of several hundred cells at this point and, in Dr. Shettles' words, "no contraindication for continued development was discernible." The objective of the procedure was not to produce a term baby but to determine whether implantation could even occur, given the circumstances of *in vitro* fertilization, and to provide various data before proceeding to "the real thing."

\* \* \*

C.

**Charles L. Remington**  
**An Experimental Study of Man's Genetic**  
**Relationship to Great Apes, by Means of**  
**Interspecific Hybridization\***

*Introduction*

Experimental crosses between separate species are becoming a principal source of biological knowledge of evolutionary relationships. Important hybridization studies have been carried

out among mammals, birds, reptiles, amphibians, fishes, several phyla of invertebrates, and most plant groups, but the one species that most interests us, *Homo sapiens*, is not known to have ever been tested in crosses. By contrast, *Homo sapiens* is among the few best-studied species in almost every biological discipline, including anatomy, biochemistry, cytology, ecology, embryology, endocrinology, ethology, epidemiology, genetics, histology, neurobiology, paleontology, parasitology, pathology, physiology, population genetics. Humans are interested in biology because they want and need to know themselves, their relationships to the rest of present day life, and the history of their species. The very great scientific value of well-performed hybridization experiments emphasizes the high priority that should be given to a thorough study of the interspecific genetics of *Homo* with one or more of the three types of pongids (chimpanzee, gorilla, orang) most similar to *Homo*. Because there is a strong possibility that hybrid analysis will significantly clarify and alter present views of our relationships and our past history, such an analysis will not be neglected much longer.

In humane cultures, biomedical researchers have been reluctant to experiment with man, and the philosophical taboos linked with this reluctance have surely been a major factor in postponing hybridization studies until now. However, two other causes may be even more important in the delay: (1) only relatively recently have zoologists in general begun to do sophisticated hybridization research and thereby to perfect the experimental design; (2) for the unique problems of *Homo* x pongid crosses, much of the useful technology mentioned later under "Procedure" is only now being worked out by biomedical specialists in such fields as human and bovine artificial insemination and human tissue culture.

The scientific importance of a *Homo* hybridization experiment is primarily in the great value scientists generally give to knowledge of ourselves. As with every area of biology except the study of mentality, so with hybridization, experiments involving *Homo* are not necessary for our development of generalizations about organisms. Other species of animals with hybridizable relatives would tell us as much about speciation theory, and many such groups would be easier and less expensive to hybridize.

The experiment's human interest value is too obvious to deserve much justification. In fact, an obstacle and distraction to the fullest accom-

\* Unpublished manuscript (1971). Reprinted by permission of the author who retains all rights.

plishment of the ideal *Homo* experiment would be the universal interest there would be in the progress and results of the study. The world press, if aware of the research, would perhaps try to report it as fully as moon exploration or the first heart transplants, an alarming prospect to a scholar in the ivory tower.

The human welfare value of hybridization is not easy to predict. There are dimly perceived biomedical applications that would probably be worth exploring once the initial results of hybridizing *Homo* were available. There are even vaguer speculations on the cultural uses of half-human livestock which are not the forte of the hybridizing biologist but which deserve some debate by thinkers of various backgrounds; at least one famous novel has this theme.

#### *Basic Protocol for Hybridization Experiments*

*Step 1:* Cross ♂ Species A with ♀ Species B. Measure hybrids' developmental rate and physiological normalcy in comparison with non-hybrid Species A and Species B. Record in hybrids the external appearance of all characters likely to alter during growth, such as hair character, color, and distribution. (If possible, actual parents should be tested for fertility by also being mated with their own species; for long-lived animals such as mammals, this would be satisfied if the ♀ parents were chosen from young individuals that have already had offspring and the ♂ parents were proven sires.) Mating even one pair of parents provides significant data, but it is important to try to have at least three replications (i.e., three different pairs of parents), to minimize the possibility that the findings from a single pair were atypical. (For example, suppose that sperm in a single human ejaculate were nonfunctional one time in five; there would thus be a twenty percent likelihood that a single *Homo* x pongid mating would appear sterile from this cause alone. Getting this result twice in a row would only have a four percent likelihood, and thrice in a row less than one percent. Consistent results in three replicates would thus reduce to a negligible level the chance of aberrancy in each of the many results of the experiment.) If hybrids fail to mature, determine the precise development stage at death. If ♂ hybrids do mature sexually, analyze chromosome homology by observing synapsis at meiosis I, from testicular samples (needle biopsies need not harm ♂ mammals).

*Step 1A.* Cross ♂ Species B with ♀ Species A. Same procedures as in Step 1.

*Step 2.* Backcross hybrids to both parental species, mating ♂  $F_1$  with ♀ Species A and ♀ Species B, and ♀  $F_1$  with ♂ Species A and ♂ Species B. (Do not waste hybrids by doing genetic  $F_2$ 's, i.e., sib matings of  $F_1$  hybrids.) Same procedures as with Step 1. This step provides the crucial measure of interspecific hybrid sterility. The backcross offspring provide essential data on heredity of discrete interspecific differences (color, form, electrophoresable enzymes, behavior, etc.).

#### *Plan of this Experiment*

##### *1. Species to Be Used.*

Because specialists in taxonomy, anatomy, and paleontology are agreed that *Homo* is more closely related to the Pongidae than to any other kind of primate, the ultimate aim of the experiment is to cross *Homo* with at least one pongid species. The gibbons (Hylobatidae) are less similar to *Homo* and are much smaller, so they are less promising than pongids. Using the dissimilarity criterion (but not size), the orangutan (*Pongo pygmaeus*) also ranks rather low. Of the three remaining species, the pigmy chimpanzee (*Pan paniscus*) is both smaller and much rarer than the chimpanzee and is not a prime candidate. That leaves the gorilla, *Gorilla gorilla* (of which there are at least two general geographic types that appear so unlike that they may ultimately prove to be separate species) and the common chimpanzee (*Pan troglodytes*). The gorilla appears to be anatomically and biochemically the most similar to *Homo* and has the best size range, but it has become rare and very expensive and is not ideally suited for breeding in captivity, even of its own kind. By elimination, therefore, *Pan troglodytes* best combines the characteristics of an optimal parental species of the *Homo* hybridization: (1) taxonomic similarity to *Homo*; (2) ease of breeding in captivity; (3) abundant availability; (4) reasonable similarity in size; (5) familiarity to psychologists, anatomists, physiologists, cytologists, and biochemists, so that characteristics of the  $F_1$  hybrid *Homo sapiens* x *Pan troglodytes* could be compared immediately with both parents without new research on an unknown parent. In the rest of this account, the generic name *Pan* will refer only to *P. troglodytes*. Should this cross fail, however, it would still be necessary to attempt the *Homo* x *Gorilla* cross before concluding that *Homo* is not hybridizable with any other species. Although this last combination would al-

ways be of great interest, if the *Homo x Pan* cross succeeds, trying the *Gorilla* cross would be much less important. For a modern analysis of relatedness, all of the following should eventually be carried out, in about this priority ranking: (1) *Homo x Pan*; (2) *Pan x Gorilla*; (3) *Pan x Pongo*; (4) *Homo x Gorilla*; (5) *Homo x Pongo*; (6) *Gorilla x Pongo*; (7) *Pan x Hylobates*.

## 2. Procedure.

A classical hybridization experiment usually includes reciprocal crosses; e.g., ♂ *Homo x* ♀ *Pan* and ♀ *Homo x* ♂ *Pan*. However, a recent survey of interspecific hybrids throughout the animal kingdom shows that reciprocal crosses usually give the same or nearly the same results. Therefore, it is probably satisfactory for this experiment to make only the Step 1 cross: female chimpanzee inseminated with human sperm. However, when tissue-culturing techniques are developed (probably soon) allowing mammalian zygotes to be formed and reared in culture, *Homo* ova should obviously be treated with *Pan* (and *Gorilla*, *Pongo*, *Hylobates*, and even *Macaca*) sperm, in order to study at least the early stages of embryonic development in the reciprocal hybrid.

This experiment would include the involvement of specialists in several fields, in order to obtain all the important types of data and to maximize chances for success in Steps 1 and 2. In addition to the principal investigator, a speciation geneticist, the following collaborators would be arranged:

(a) primate-care specialists, probably in one of the National Regional Primate Laboratories, to care for the mothers and to be prepared to bottle-raise hybrid infants;

(b) an obstetrical surgeon, available should caesarian section of hybrids be required, and possibly to perform abortions if necessary to remove and analyze embryos that do not develop to parturition;

(c) artificial insemination specialists to treat the *Homo* sperm, inseminate the *Pan* females, and attempt to use known fertility treatments should fertilization be difficult to produce;

(d) a human cytologist to analyze the hybrids' chromosomes, especially during meiotic synapsis after obtaining bioptic testicular samples;

(e) a protein specialist, to investigate electrophoretically the enzymes and other proteins

of the hybrids (DNA and cytochrome sequencing might be needed as well);

(f) psychometrists or ethologists, to assay behavioral characteristics of the hybrids, including learning, memory, emotions, facial expressions, and manipulative ability;

(g) specialists in dermatoglyphics, hair micro-structure, hair pigmentation, and other details of the hybrid phenotype;

(h) a mammalian tissue-culture specialist already working on fertilization and early development in culture.

## Ethical and Legal Aspects

In the course of many years of gathering together the world's scientific literature related to animal hybridization, I have been able to discern several patterns that recur in so many different kinds of animals that they are probably applicable to *Homo x pongid* crosses: (1) mammals this similar probably can be cross-fertilized, and there is a reasonable likelihood that the embryos will develop fully and that the infants will grow past the nursing stage; (2) there is a small chance that sexually mature hybrids will show at least some successful gametogenesis and therefore allow a measure of fertility; (3) in line with Haldane's "Rule," it is fairly likely that maturing F<sub>1</sub> hybrids will all be female (the homogametic sex); (4) conspicuous interspecific differences tend to exhibit simple Mendelian heredity, and the hybrids therefore will tend to be like one parent in some prominent characters, the other parent in some, and to show blending inheritance in a few, and none of these characters can have its dominance relations reliably predicted prior to experimental tests; (5) size tends to be polygenic, however, and the foetus would therefore tend to be intermediate in size between *Homo* and pongid foetuses, making it desirable to use genetically small *Homo* as the sperm source; (6) many hybrids show heterosis (hybrid vigor), including long life. These guesses suggest that *Homo* can be hybridized and hybrids reared, that they may all be females, that they will resemble *Homo* in some characters and the pongid parent in others, unpredictably, and that the hybrid will be largely but perhaps not totally sterile and may have a long life-potential.

Ethically, the scientist might be expected to have the same responsibilities for humane care of these hybrids as for any other experimental mammals. He should also have the same freedom to operate on the hybrids and to sacrifice them

for study, although most of the essential data could probably be obtained without sacrificing.

Legally, it appears appropriate that the contribution of one-half of the genetical material by *Homo* should not make the hybrid subject to the legal protections and obligations of a human in the nation in which the experiment is carried out. However, if a hybrid were successfully backcrossed to *Homo*, the new offspring would of course be 0.75 *Homo*, and very interesting legal and ethical questions would then arise. Backcross hybrids, incidentally, would be likely to have substantially higher fertility than  $F_1$  hybrids. If a hybrid were successfully backcrossed to the pongid species, on the other hand, backcross hybrids would raise even fewer questions of legal humanness than would the  $F_1$  hybrids.

Other challenging questions in this realm might be raised if the ♀ parent of the cross were a human female volunteer, but the present experimental design is not concerned with this reciprocal combination except in the future situation in which human ova could be cultured and fertilized outside the uterus.

#### NOTE

SIR WILLIAM BLACKSTONE  
COMMENTARIES ON THE LAWS OF ENGLAND\*

\* \* \*

A monster, which hath not the shape of mankind, but in any part evidently bears the resemblance of the brute creation, hath no inheritable blood, and cannot be heir to any land, albeit it be brought forth in marriage: but, although it hath deformity in any part of its body, yet if it hath human shape it may be heir. This is a very ancient rule in the law of England; and its reason is too obvious and too shocking to bear a minute discussion. . . . [Those who are born with a form not human are not considered children; as when a woman by a perversion of nature brings forth something monstrous or prodigious. Nevertheless the offspring to which nature has only added or from which withheld something, as if it should have six or only four fingers, ought to be reckoned among children, and though its limbs be useless or distorted, yet it is not a monstrous birth. (Bracton)]

\* \* \*

\* *Book the Second—of the Rights of Things* 246–247 (1756).

### 3.

#### Interferences with Mores and Laws

a.

#### John McLean Morris and Gertrude van Wagenen Compounds Interfering with Ovum Implantation and Development— The Role of Estrogens\*

In an effort to discover an effective post-coital antifertility agent, a variety of compounds . . . have been studied. A number of these which have proved extremely effective in inhibiting ovum implantation and development in the rabbit and other species have been found to have little or no effect in the primate.

Three years ago, work was commenced on the first compound which met the fundamental requirements of being nontoxic, nonteratogenic, and 100 per cent effective: ORF-3858 (2-methyl-3-ethyl-4-phenyl-Δ-cyclohexenecarboxylic acid). The "all-or-none" nature of the response in the rabbit and the absence of any fetal abnormalities in marginal doses suggested the value of trial in the primate.

The similarity of the menstrual cycle of the rhesus monkey (*Macaca mulatta*) to that of the female human being makes any data concerning mating and pregnancy in this animal of significant interest. ORF-3858 proved effective in the macaque. When administered orally in 10 mg. doses for the first six days following positive mating (three days for possible sperm survival plus three for effective period of action) there were no pregnancies.

Investigations to study the mechanism of action of this compound revealed that it had certain estrogenic properties, prompting a further evaluation of estrogens themselves.

It has been known for many years that estrogens interfere with early pregnancy in the rabbit and other species. With the control of conception assuming today such tremendous importance, it is surprising to note, as Parkes does, that "there has been, so far as I know, no determined effort to see whether the administration of estrogen during the third week of the human cycle would prevent any implantation that might otherwise take place."

\* 96 *American Journal of Obstetrics and Gynecology* 804–808, 810–812 (1966). Reprinted by permission.

Estradiol-17 $\beta$ , stilbestrol, ethinyl estradiol, and mestranol (as well as ORF-3858) will not only prevent implantation, but when given to the rabbit later, in larger doses, will also result in placental separation and death of the fetus. . . .

In spite of such observations in lower species, it is obvious that human pregnancy cannot be interrupted in such fashion by the administration of estrogens. Similarly, in the macaque, the oral or intramuscular administration of stilbestrol or estradiol dipropionate after implantation (on days 18 to 167) appeared to have little effect on the development of the fetus. . . . Of 20 animals so treated, 3 aborted, an incidence of 15 per cent. This does not vary significantly from the 10 per cent expected abortion rate in this colony. There were no fetal abnormalities produced.

Efforts were undertaken, however, to see if estrogens in sufficient dosages in the primate might prevent implantation if administered immediately after mating. Observations on the timing of ovulation in macaques in our laboratory indicate that days 11 and 12 constitute the mode, and that 32 per cent of the first positive matings at this time were followed by pregnancy when the monkeys were carefully selected (on the basis of such factors as age) for probable fertility.

*Experimental animals.* A total of 28 monkeys were employed in this study, 26 multiparous females and 2 nulliparous. For the 26 multiparous monkeys, 42 pregnancies were recorded during the 3 years before the experiments on contraception were begun, and 18 were pregnant in the year that immediately preceded the experiments. Although identical breeding conditions have been maintained during the period of contraceptive trial, no pregnancies have occurred when ORF-3858 (10 mg.), stilbestrol (1 to 25 mg.), or estradiol (10 mg.) was given orally for 6 days following positive mating. In 3 years of normal breeding, there were 204 positive matings with 42 pregnancies; in up to 2 years of normal breeding with treatment, there were 321 positive matings without pregnancy. . . .

\* \* \*

#### *Preliminary clinical trials*

In spite of confidence that success in the macaque should be paralleled by success in man, initial human experimentation was undertaken with some trepidation.

The first cases were rape cases. All of the

subjects received 50 mg. of stilbestrol for 4 to 6 days after exposure. The chance of pregnancy following rape is uncertain for many obvious reasons. Sometimes no sperm could be found in the cervix or vagina. In a few instances, temperature charts were started; if no rise occurred, no drug was given. In most of the cases accepted for treatment, exposure occurred near midcycle and fern crystallization of cervical mucus as well as presence of sperm were demonstrated. In this small series of patients, none has become pregnant so far. . . .

A limited number of courageous volunteers furnished an opportunity for further and more adequate observation. Coitus took place at midcycle near the time of the temperature rise. . . .

\* \* \*

In these preliminary trials there have been no pregnancies. While of interest, these clinical studies are incomplete and have as yet no statistical significance.

\* \* \*

Just how these compounds prevent implantation is unknown. Accelerated tubal transport may be a factor, but this does not explain their effectiveness in the rabbit after implantation. . . .

\* \* \*

Regardless of the mechanism involved, the implication of these observations is apparent: the administration of these agents in the third week of the menstrual cycle in women may inhibit implantation of the fertilized ovum. . . .

\* \* \*

#### NOTE

##### CONNECTICUT GENERAL STATUTES (1958)

\* \* \*

Sec. 53-29. *Attempt to procure miscarriage or abortion.* Any person who gives or administers to any woman, or advises or causes her to take or use anything, or uses any means, with intent to procure upon her a miscarriage or abortion, unless the same is necessary to preserve her life or that of her unborn child, shall be fined not more than one thousand dollars or imprisoned in the State Prison not more than five years or both.

Sec. 53-30. *Abortion or miscarriage.* Any woman who does or suffers anything to be done, with intent thereby to produce upon herself mis-

carriage or abortion, unless necessary to preserve her life or that of her unborn child, shall be fined not more than five hundred dollars or imprisoned not more than two years or both.

Sec. 53-31. *Encouraging the commission of abortion.* Any person who, by publication, lecture or otherwise or by advertisement or by the sale or circulation of any publication, encourages or prompts to the commission of the offenses described in section 53-29 or 53-30, or who sells or advertises medicines or instruments or other devices for the commission of any of said offenses except to a licensed physician or to a hospital approved by the state department of health, or who advertises any so-called monthly regulator for women, shall be fined not more than five hundred dollars or imprisoned not more than one year or both.

Sec. 53-32. *Use of drugs or instruments to prevent conception.* Any person who uses any drug, medicinal article or instrument for the purpose of preventing conception shall be fined not less than fifty dollars or imprisoned not less than sixty days nor more than one year or be both fined and imprisoned.

\* \* \*

b.

**William H. Masters and Virginia E. Johnson**  
**Human Sexual Inadequacy\***

While developing therapy concepts and procedural patterns at onset of the clinical investigative approach to sexual dysfunction in 1959, there were many severe problems to be faced. One of the most prominent concerns was the demand to develop a psychosocial rationale for therapeutic control of unmarried men and women that might be referred for treatment. During the past 11 years, 54 men and 3 women were unmarried when referred by their local authority with complaints of sexual dysfunction. In a statistical breakdown relative to intake diagnosis, 16 men were premature ejaculators, one was an incompetent ejaculator, 21 were primarily impotent, and 16 were secondarily impotent. The three women were orgasmically dysfunctional, one primarily and two situationally (coital orgasmic inadequacy).

The immediate problem to be faced was the obvious clinical demand for a female partner

—a partner to share the patient's concerns for successful treatment, to cooperate in developing physically the suggestions presented during sessions in therapy, and, most important, to exemplify for the male various levels of female responsiveness. All of these factors are essential, if effective sexual functioning is to be returned to the sexually inadequate man. In brief, someone to hold on to, talk to, work with, learn from, be a part of, and above all else, *give to and get from* during the sexually dysfunctional male's two weeks in the acute phase of therapy.

The term *replacement partner* is used to describe the partner of his or her choice brought by a sexually inadequate unmarried man or woman to share the experiences and the education of the clinical therapy program. *Partner surrogate* has been reserved to indicate the partner provided by the cotherapists for an unmarried man referred for treatment who has no one to provide psychological and physiological support during the acute phase of the therapy. . . .

Thirteen of the 54 nonmarried men brought replacement partners of choice who were most willing to cooperate with the therapists to enable their sexually dysfunctional men to establish effective sexual performance. The three nonmarried women also brought replacement partners of their choice to participate in therapy. These replacement partners were men with whom they had established relationships of significant duration, as well as the personal warmth and security that develops from free exchange of vulnerability and affection.

Partner surrogates have been made available for 41 men during the last 11 years. This situation has involved basic administrative and procedural decisions. Should the best possible climate for full return of therapeutic effort be created for the incredibly vulnerable unmarried males referred for constitution or reconstitution of sexual functioning; or should there be professional concession to the mores of society, with full knowledge that if a decision to dodge the issue was made, a significant increase in percentage of therapeutic failure must be anticipated? Unmarried impotent men whose dysfunctional status could be reversed to allow assumption of effective roles in society would continue sexually incompetent. From a clinical point of view there really was only one alternative. Either the best possible individual return from therapeutic effort must be guaranteed the patient, or the Foundation must refuse to treat unmarried men or

\* Boston: Little, Brown & Co. 146-155 (1970). Reprinted by permission.

women for the symptoms of sexual inadequacy. Either every effort must be made to meet the professional responsibility of accepting referrals of severely dysfunctional men and women from authority everywhere in or out of the country, or admission to clinical procedure must be denied. It would have been inexcusable to accept referral of unmarried men and women and then give them statistically less than 25 percent chance of reversal of their dysfunctional status by treating them as individuals without partners. This figure has been reached by culling the literature for material published from other centers, since it is against Foundation policy to treat the sexually dysfunctional individual as a single entity. If the concept that therapy of both partners for sexual inadequacy has great advantage over prior clinical limitations to treatment of the sexually dysfunctional individual without support of marital partner, then partners must be available. Statistically there no longer is any question about the advantage of educating and treating men and women together when attacking the clinical concerns of male or female sexual inadequacy. For these reasons the therapeutic technique of replacement partners and partner surrogates will continue as Foundation policy.

It should be emphasized that no thought was ever given to employing the prostitute population. For reasons that will become obvious as the contributions of the replacement-partner and partner-surrogate populations are described, so much more is needed and demanded from a substitute partner than effectiveness of purely physical sexual performance that to use prostitutes would have been at best clinically unsuccessful and at worst psychologically disastrous.

Women volunteered for this assignment of partner surrogate. Over the last 11 years, 13 women have been accepted from a total of 31 volunteers for assignment as partner surrogates. Their ages ranged from 24 to 43 years when they joined the research program. Although all but two of the women have been previously married, none of the volunteers were married when living their role as a partner surrogate.

The levels of formal education for the partner surrogates were high-school graduate (3), additional formal secretarial training (2), college matriculation (2), college graduates (4), and post-graduate degrees in biological and behavioral sciences (2). Nine of the 13 women had a child or children before joining the program. Ten of these women also were committed

to full-time employment outside of their role as partner surrogate; one did part-time volunteer work and the remaining two were caring for very young children.

Every effort has been made to screen from this section of the total research population women with whom the cotherapist did not feel totally secure attitudinally or socially, and approximately 60 percent of those women volunteering for roles as partner surrogate were not accepted. Of the 13 women accepted, 6 had previously served as members of the study-subject population during the physiological investigative phase of the research program, and 7 volunteered their services for this specific clinical function.

The reasons expressed for such voluntary cooperation were varied but of real significance. During the screening process, each woman was interrogated in depth while the interviewers were acquiring medical, social, and sexual histories from which to evaluate the individual's potential as a partner surrogate. The investigation was conducted by male and female interrogators both singly and in teams. If interrogation indicated potential as a substitute partner, the three involved individuals (volunteer and interrogators) discussed this concept in detail, examining both the positive and the negative aspects of such a service. No attempt ever was made to persuade any women to serve as a partner surrogate. Volunteers who showed hesitancy or evidence of personal concern were eliminated from this potential role in the research program.

Of major interest was the fact that 9 of the 13 volunteers were interested in contributing their services on the basis of personal knowledge of sexual dysfunction or sex-oriented distress within their immediate family. . . .

\* \* \*

Of the remaining four volunteers, three women had more prosaic reasons for essaying the role of a partner surrogate. The expressed needs were unresolved sexual tensions, a need for opportunity of social exchange, and an honest interest in helping dysfunctional men repair their ego strengths as sexually adequate males. Finally, a physician, frankly quite curious about the partner-surrogate role, offered her services to evaluate the potentials (if any) of the role. When convinced of the desperate need for such a partner in the treatment of sexual dysfunction in the unmarried male, she continued as a part-



ner surrogate, contributing both personal and professional experiences to develop the role to a peak of effectiveness.

\* \* \*

The specific function of the partner surrogate is to approximate insofar as possible the role of a supportive, interested, cooperative wife. Her contributions are infinitely more valuable as a means of psychological support than as a measure of physiological initiation, although obviously both roles are vitally necessary if a male's inadequacies of sexual performance are to be reversed successfully. . . .

\* \* \*

When assuming an active role in the clinical therapy of any sexually incompetent man, the partner surrogate is given detailed information of the individual male's psychosexual background and the cause for and specifics of his sexual dysfunction, and is kept thoroughly informed on a day-to-day basis as to the professional teams' concept of therapeutic progress. No other identifiable personal details of the involved male other than name are ever provided. Even this is masked if the name is well-known. The patient is cautioned against providing relevant personal information. In the same vein, the partner surrogate never exchanges any personal information that might lead to her identification in the future.

Shortly after the roundtable discussion . . . , the first meeting between the patient and his partner surrogate is arranged. The first meeting is always limited to a social commitment. Usually the couple go to dinner and spend a casual evening in order to develop communication and comfort in each other's company. . . .

\* \* \*

Once social exchange has been established, the partner surrogate moves into a wife's role as the treatment phase is expanded. She joins the sexually inadequate male in both social and physical release of the tensions that accrue during the therapy. With the exception of attending the individual therapy sessions, every step that a wife would take as a participant with her husband in the therapeutic program is taken with the dysfunctional nonmarried male by his partner surrogate.

\* \* \*

In view of the statistics there is no question that the decision to provide partner surrogates

for sexually incompetent unmarried men has been one of the more effective clinical decisions made during the past eleven years devoted to the development of treatment for sexual inadequacy.

\* \* \*

Thirteen women have accompanied unmarried men to the Foundation, agreeing to serve as replacement partners to support these men during treatment for sexual dysfunction. In all instances both individuals were accepted in therapy with full knowledge of the referring authority. Since the women were selected by the men involved, they were accepted as if they were wives. They were interrogated in depth and attended all therapy sessions. They lived with the unmarried males as marital partners, in contrast to the partner surrogate, who spent only specific hours during each day with the sexually dysfunctional male. Details of treatment for the various forms of male sexual dysfunction need not be repeated; clinical situations with replacement partners are managed in the same way as with wives.

\* \* \*

Three unmarried women referred to the Foundation brought with them replacement partners of their choice. In each instance the current relationship was one of significant duration. The shortest span of mutual commitment was reported as six months. Two of the three women have previously been married. The replacement partners were treated as husbands of sexually inadequate wives. They attended all sessions and went through in-depth history-taking to provide information sufficient to define their roles in providing relief for their distressed women companions.

## NOTES

### NOTE 1.

#### SEX RESEARCHERS ARE SUED BY EX-PATIENT FOR \$750,000\*

Dr. William Masters and Mrs. Virginia Johnson, the sex researchers, have been sued for \$750,000 on the ground that they used the plaintiff's wife to give sexual treatment to two men patients.

The suit was filed yesterday in United States District Court in St. Louis by George E. Calvert,

\* *The New York Times* 20, col. 1 (August 26, 1970). © 1970 by the New York Times Co. Reprinted by permission.



who stated in his complaint that he lived in New Hampshire.

Dr. Masters and Mrs. Johnson issued a statement saying "Our reaction is that any such charge is ridiculous. We can prove it."

Mr. Calvert alleged that he and his wife, Barbara, had been patients of Dr. Masters and that the doctor had "breached the patient-doctor relationship in procuring the said Barbara Calvert to engage in sexual intercourse" with two co-defendants identified as "John Doe 1" of New York and "John Doe 2" of Virginia.

Mr. Calvert alleged that Dr. Masters and Mrs. Johnson had paid Mrs. Calvert \$500 and \$250 for her services to the two John Doe defendants. He contended that they charged John Doe 1 \$3,000 for his treatment and John Doe 2 an unspecified amount.

\* \* \*

The husband charged that Dr. Masters and Mrs. Johnson had induced Mrs. Calvert to keep her activities a secret from him.

\* \* \*

#### NOTE 2.

##### MISSOURI REVISED STATUTES (1949)

§563.010. [A]ny person who procures a female inmate for a house of prostitution; or who shall induce, persuade, encourage, inveigle or entice a female person to become a prostitute; or who by promises, threats, violence or by any device or scheme, shall cause or influence a female person to become an inmate of a house of . . . assignation, . . . or any person who . . . by abuse of any position of confidence or authority shall cause or influence any female person to enter any place within this state in which prostitution is practiced, encouraged, or allowed, . . . or who shall receive or give, or agree to receive or give any money or thing of value for procuring or attempting to procure any female person to become a prostitute or to come into this state or leave this state for the purpose of prostitution shall be guilty of pandering, and upon conviction shall be punished by a fine of not less than one hundred dollars nor more than one thousand dollars or by imprisonment in the penitentiary for a term of not less than two years nor more than five years.

§563.130. [A]ny person or persons or corporation who shall directly or indirectly establish, keep, permit or maintain any . . . assignation house . . . in this state shall in addition to other

penalties prescribed by the laws of the state of Missouri be deemed guilty of a nuisance, and all buildings, erections, rooms and places, and the ground itself in or upon which such . . . assignation house . . . is conducted, permitted, carried on, maintained or continued are also declared nuisances, and all such nuisances shall be enjoined and abated as herein provided.

#### NOTE 3.

##### HUSBAND AND WIFE— CRIMINAL CONVERSATION\*

\* \* \*

§476. [W]henever a third person commits adultery with either spouse, he or she commits a tortious invasion of the rights of the other spouse, from which a cause of action for criminal conversation arises. At common law such a cause of action exists in favor of the husband against one who commits adultery with his wife, and this right of action is not affected by the Married Women's Property Acts. The wife also, under the Married Women's Acts, according to the weight of authority, may maintain an action for criminal conversation with her husband. . . .

The causes of action for criminal conversation and for alienation of affections are alike in that each arises from the marriage relation, and each is for a tort against the right to consortium; but they differ in that the latter is for loss of consortium or affection and does not necessarily, though it may, involve adulterous intercourse, whereas the former is for the adulterous intercourse or criminal conversation, and the alienation of affections and other consequent injuries to the consortium, such as loss of service, are only matters of aggravation and not necessary to the cause of action. . . .

\* \* \*

§478. [I]t is no defense to an action for criminal conversation that the defendant was led into the adulterous intercourse through the acts and practices of the plaintiff's spouse instead of being himself the seducer, although that fact may be shown in mitigation of damages.

\* \* \*

\* 41 *American Jurisprudence* 2d §§476, 478. Rochester, N.Y.: The Lawyers Co-operative Publishing Company 402-404 (2d ed. 1968). Copyright © 1968 by Jurisprudence Publishers, Inc. Reprinted by permission.

c.

**State v. Daniel Richard Martin\***

SUPERIOR COURT  
LITTLETON COUNTY  
OCTOBER 4 & 5, 1955  
LITTLETON, CONNECTICUT  
STATE  
V.  
DANIEL RICHARD MARTIN

Before

Honorable ELMER W. RYAN, Judge

Appearances

THOMAS F. WALL, ESQ., State's Attorney  
CHARLES R. EBERSOL, ESQ., Defendant's  
Counsel

\* \* \*

THE CLERK: How old are you, Mr. Martin?

THE ACCUSED: Fifty-two.

THE CLERK: You are charged in the first count of the information of the State's Attorney with the crime of indecent assault. How do you wish to plead?

MR. EBERSOL: May it please the Court, we would like permission to file a plea of *nolo contendere* to this and on the second count of the information.

MR. WALL: No objection by the State's Attorney, Your Honor.

THE COURT: Well, no objection by the State; plea of *nolo contendere* is accepted. . . .

MR. WALL: In view of the plea, I move, Your Honor, that a finding of guilty be entered on the basis of the plea.

THE COURT: Finding of guilty is entered on both counts.

MR. WALL: The defendant, Your Honor, is a physician. He was born in Bloomington, Illinois in 1902. Comes from a very good family. He has been known fairly generally as Richard Martin although his name is stated correctly in the substitute information. He has never married. He attended the elementary and high schools of his home town, graduated from college, graduated from a prominent medical school in 1927. He served as House Officer in training at his medical school at the hospital connected therewith two years; and he came to Connecticut in 1929 where he was physician for a private school

in this county for five years. For five years after that, he taught pediatrics at a medical school and then he taught the same subject at another medical school from 1939 to 1946, and he returned to Littleton in 1946, and he has practiced pediatrics there until his arrest. He maintained an office in Littleton although his name did not appear at the office on the door. He was in the same office with three other physicians. These charges originated during the month of August, a complaint to my office concerning the Doctor by a mother of three children aged nine, ten and thirteen.

In the Spring of this year, one of these three children, the middle one, aged ten—I believe he was probably nine years of age when he was first treated—went to a clinic as a result of a very severe, chronic condition involving an extreme case of stuttering—stuttered to such an extent that he barked like a dog; and he went for psychiatric treatments and took them for a short period of time at this clinic; and, at that time, it was recommended that he be under the care of Dr. Martin.

Now as I have stated, Dr. Martin practiced pediatrics in his office. . . . He lived pretty generally in the woods about two or three miles away from his office at a very beautiful spot on top of a mountain in Littleton owned by a public-spirited person, well-known in the community who had confidence in the Doctor and felt that he could use her land for what you might call a "camp" or a "clinic." . . .

The Doctor had, for a number of years, taken boys and, sometimes, girls to this camp; and so far as any of the clinics or outside agencies . . . knew, the Doctor was using psychiatric treatment of his own on these boys although he made it plain that he was not a psychiatrist; and his treatment, so far as anyone knew, was to have these boys reside in a cabin which was very, very crude on the side of the lake. . . .

And the theory as stated to agencies and institutions who came in contact with Dr. Martin was that these boys would get back to nature, that they would take care of their physical needs chopping wood, cooking their own meals, and making their own beds; and that personal observation on the part of the Doctor, and supervision upon his part, and the beautiful surroundings, and taking advantage of keeping these boys busy at their occupations of maintaining themselves at the camp would have some therapeutic value; and over the years, the Doctor acquired a very good reputation for caring for these boys;

\* Richard C. Donnelly, Joseph Goldstein, and Richard D. Schwartz: *Criminal Law*. New York: The Free Press 11-28 (1962). Reprinted by permission of The Macmillan Company.

and he evidently had some results that were looked upon as—by others as having been good. As a matter of fact, the case of this ten-year-old boy was also a case which was widely talked of in psychiatric circles as attesting to the Doctor's reputation. He was told on a number of occasions that this young boy had been cured, or very nearly cured, of his habit of barking like a dog, and that the Doctor was given some credit for it.

. . . In the early Spring, probably in April, when recommendation was made that the—this nine- or ten-year-old boy be sent to Dr. Martin, he went down there to the camp; and he remained there fairly steadily from April until the summer for quite some time. The Doctor told him that—and told the family—that it was best for them not to go near him, or not to take him away from the camp; and after he had been there for a number of months, the Doctor asked the family to send down his older brother aged thirteen and the younger brother aged nine to the camp in order that he might study their case histories to give him a better understanding of the case of the ten-year-old, the middle of the three.

Shortly after the arrival of the ten-year-old boy, about a week after he went to the cabin, the doctor started to commit homosexual acts upon the ten-year-old boy; and according to the statement of the ten-year-old boy, these acts took place twice a week over a period of four months which would indicate approximately, according to my calculations from this statement, approximately thirty-two homosexual acts. Those were acts where the Doctor, himself, committed acts upon the boy and had the boy commit acts upon him; and upon one occasion, there was an act described by the boy as an act by the Doctor attempting to commit sodomy.

\* \* \*

The thirteen-year-old boy stayed down there for about a week; and he made other visits which lasted, all together, about three weeks.

The nine-year-old boy went there for a week in the early part of the summer, and he was there from August first to August seventh, the week before the arrest. When the nine-year-old boy got back to his home after a visit there, through an incident of the mother warning the nine-year-old boy to stay away from a certain person in their neighborhood, having nothing to do with this case, who was considered by the mother as being queer, the young boy—nine-year-old—made a statement to his mother in answer to this caution. Quote, "Why, is he a pig like Dr. Martin?"

She then questioned the nine-year-old boy, and the nine-year-old boy related circumstances to the mother of a disgusting nature of the very same conduct as has been related, and—.

THE COURT: What you say "the very same type of conduct," are you referring to homosexual practices?

MR. WALL: They were, Your Honor. There were at least three acts during August with this nine-year-old boy and one act that was either sodomy or bordered upon sodomy; and this nine-year-old boy was also involved—which is not accountable, necessarily, to the Doctor—with an act with a fifteen-year-old boy who was also down there for treatment. In other words, it was—as far as this nine-year-old boy being there a week or two, he just got into a nest of homosexuality over which the Doctor evidently presided. He didn't see these acts, but nevertheless, having participated in them also, in an approving way, it was not to be expected that the children, themselves, would refrain from them between themselves; and this fifteen-year-old boy did molest the nine-year-old boy while he was there.

All this was told to the mother by the nine-year-old boy. Perhaps not all of it, but enough that the mother immediately notified the authorities, and she got the other children back from there without exciting suspicion; and statements were taken from the children—the three of them; and the thirteen-year-old boy told a similar story of at least six acts over a two-week period that he was there, all with Dr. Martin, all upon his solicitation.

. . . There was also this fifteen-year-old boy who had been at the camp for one year and three months; and there was another sixteen-year-old who had been there for a number of years; and there were acts related between the fifteen-year-old boy and the sixteen-year-old boy. Now, there is no act between the Doctor and the fifteen-year-old boy, but there was of the sixteen-year-old boy.

Now, this sixteen-year-old boy had acts with the ten-year-old boy, with the one who was being treated there; and he also had acts with the fifteen-year-old boy, and he also had acts with Dr. Martin once a month over a period of the two years that he was there; and the acts with the sixteen-year-old boy at some times were either sodomy or very close to sodomy. . . .

\* \* \*

The information which we have concerning the ten-year-old boy concerning his—the prog-

ress of his stuttering was that while there appeared to be a slight improvement while he was there for a while, that, actually, the trouble got worse and the trouble is worse than ever at the present time.

However, this particular case has been cited as showing some of the remarkable cures that the Doctor effects. I don't say that the Doctor doesn't effect some cures in some phases of medicine. I believe he is quite a capable man in his own field. He certainly has a very good reputation in his field; and he, aside from his homosexual proclivities, is able to treat people rather well and has an understanding of human nature. . . .

\* \* \*

MR. EBERSOL: [I]n my talks with Mr. Wall, the question of sodomy never came up; and had there been any such charge, they would have been denied.

I believe, as he stated—he was certainly speaking the truth—that the statement made by these youths might be so interpreted. As he said, they were susceptible of several interpretations.

There is a host of people who would like to be in my shoes right now—other lawyers, doctors, clergymen, teachers, heads of schools, public officials, social workers, leaders in the field of working with disturbed children, prominent people in public life, rich and intellectual, poor, humble and uneducated, mothers and fathers of radically sick children, and some of these once-derelict and lost boys, themselves, would like to be here in Court to speak; and all these and many others who would fill this courtroom many times over would attest to Dr. Martin's medical skill, to his integrity and courage, and attest to his living selfless and untiring ministrations to children and parents alike; and, in a sense, I am their agent as well as the attorney for the defendant.

Never, I am sure, in the history of Little-town County Jail, has one of its involuntary guests received so many visitors or such a flood of mail. I have here just a few of the letters the doctor has received. These letters are the sincere outpourings of people—of people of all ages and from every walk of life who knew the accused or his good works or both; and what do they say with one accord?

Simply stated it is, "We have faith in your integrity and know that you never did anything which you did not feel was in the very best interests of your boys." Would that all of them might speak for themselves, but the nature of our plea does not permit it. Therefore, I would like to give

The Court a few excerpts of representative letters.

\* \* \*

From a mother in Wilberham: "Our prayers are with you, and we hope that they and those of so many people in Wilberham help a little." There is a postscript. "As I have said to you before, we have felt that one of the finest memories of childhood we could give our youngsters is knowing Dr. Martin. How long will the world take to realize that all the great advancements of science have been made by men working alone?"

\* \* \*

From a master in a private school: "We think of you constantly and are desperately sorry that your wonderful work has had to be interrupted. I want to add that our belief in your personal integrity has not been changed. We realize that your medical skill and knowledge caused you to take steps not readily understood by a layman."

From a foster-mother: "We have felt deeply grateful to you these past years for the kind, considerate care you have given our young—the foster children and our own two—and have been impressed and inspired by the work you have been doing in rehabilitating disturbed and unhappy children. I wish all parents could come to talk to you a couple of times a year. It's like getting one's tires retreaded, everything goes along so much more smoothly."

And finally, a very thoughtful letter from parents in Massachusetts—one, a PTA President. . . .

"It appears to us that you are suffering the penalty that often lands on forerunners, both in science or art. The threat of popular disapproval or of law-infringement is set up to warn off the faint-hearted or the criminal. It is an effective barrier to all but the incurably courageous and the incurably vicious individuals. So, in the doghouse or the jailhouse, we find the best and the most mixed-up together, or, rather, to use a less harsh term, the most creative and the most destructive. How can human justice tell them apart? In a sense, both are threats to stable status quo society; and yet the first group are the seeds of tomorrow's best harvest.

"This is all rather trite and obvious, but it is in these terms that we see your offense. We start from the basis that if a man is to be judged by the fruits of his work, you must be rated as

an exceptionally successful healer of radically sick children. The product, where human spirits are concerned, bespeaks the process; hence, we have always been quite content that you should work your magic in your own way, feeling that only the soundest sort of therapy could bring about the clarity or serenity which you seem to be able to leave in your patients and their parents.

"If other doctors, using more conservative methods, could match your score, then it might be said that radical procedures were not justified. But the size and character of your practice is evidence enough that your ways are sound and practical. No doubt, it would be easy to find points where your methods carried you across the frontiers of the legal or the moral codes and make you vulnerable to accusations like the present ones; but we do not see that such pin-point out-of-context challenges have any validity. They may be true but, lacking the whole truth, they are a kind of lie about you and your purposes.

"And, of course, we, ourselves, always come back to the hard practical fact that we have in our own family evidence of your skill, first-hand knowledge of the good you have done. No accusations made by others will ever outweigh this, for us."

This last letter clearly defines the purpose of our defense; namely, to give these acts which, out of context, the State regards as deeds of a lecherous homosexual, their true background and context in which we firmly believe they are the acts of a skillful, highly experienced professional and a serious student of difficult medical problems who has helped some very sick children and who may help other doctors to cure such sick children.

\* \* \*

As Mr. Wall has stated, the project involved a woods cabin on a lake, part of an old farm, four miles from the village of Littletown. During nine years, twenty-two children stayed there for long periods of time—two to four years, and many more for a shorter time. This project, let me emphasize, was separate from his clinical pediatric office practice in Littletown.

Patients at the cabin project were referred by various children's agencies, both public and private, by clinics, by courts, hospitals, schools, physicians, clergymen, nurses, and by the friends or relatives of patients who had been there.

Except in the very beginning, almost all of the referrals came because the referring agency

or the person had direct knowledge of the result in the case of a patient who had been there. No publicity or promotion methods were used ever. As Mr. Wall stated, he didn't even have his name on the door of the clinic office; didn't have a registered telephone. No record was kept of the numerous applications.

The accused has estimated for me that the children accepted in this project were less than one percent of those for whom some sort of application was made. This demand reflected, not only the lack of resources for such children, but also the knowledge of the results that he was achieving.

As for the financing of the cabin project, the parents or the agencies were expected to pay the expenses of the child's care, if they were able. No child was ever refused for lack of money. Occasionally, it was necessary, not only to keep the child for nothing, but to send money home for his family in order to insure his having a family when he needed one.

\* \* \*

Most of the cabin children were those who were—who are currently called "disturbed". . .

\* \* \*

. . . Other terms that might be or have been applied to them are "worried," "lost," "frightened," "defeated," "discouraged," "crazy," "neglected," "confused," "delinquent," and "bad." Disturbed children might, from the Doctor's point of view, be viewed as those who have stepped out of the pattern or path of growth far enough or for long enough time that they have become a worry to themselves, to their families, their teachers, or their neighborhoods.

The people of this Village of Littletown became accustomed to seeing derelict children such as these become respectable junior citizens of the community. At one time, three of the cabin boys were heads of young people's groups in the three churches in the community. Only on Thursday, last, did a county newspaper report that one of the boys who was at the cabin at the time of the Doctor's arrest—and who is still in Littletown in a foster home—had just been elected head of his church young people's group after the arrest of the Doctor.

There never was a routine program—the methods used to accomplish these results were experimental and unorthodox. The approach was to induce the child to go back in his life to the age when his trouble started, and then to guide

him anew up to his present age along lines which would be more comforting to him and more acceptable to others—regression, and then progression or re-education.

There never was a routine program of management of a patient at the cabin. In each case, the program was improvised to fit the needs of the child, whatever they were. To induce the regression, what might be called a "permissive environment" was created. In his first few weeks, the child was usually with the Doctor continuously, night and day, and was allowed—even encouraged—to do about as he pleased. Introduced to this exceedingly free environment, not every child regressed. As soon as he was really sure that no one really cared what he did and that no one was going to try to make him behave himself, and that adult approval was not going to be meted out according to his acts, a child might show signs of real relief and immediately begin to behave in accordance with social standards which he had continually flouted before.

Usually, after an initial period of permissiveness, the Doctor closed in on a child slowly, very slowly, much as he describes he used to harness a colt when he was young—a little restraint, a small requirement, mild denials, and this while the child was more or less alone with the Doctor.

However, permissiveness was not the only technique that was used. Both public and private schools were attended by the cabin children. Inducting a cabin child into the elementary public school life by degrees was made easier because Dr. Martin was the school physician and spent many hours there in certain seasons. Being neither teacher nor parent, and not in a position of authority, he had unusual freedom and opportunity to observe the children, both normal and disturbed. In addition to the schools, the almost weekly dances at the community house were part of the laboratory where the Doctor made his studies of these cabin children.

Now it is time for me to move from the general background to the specific charges of this information; and Mr. Wall has had complete freedom, as we had agreed previously, in mentioning cases not included in this information. So will I.

In March of this year, the cabin project included eight children—four in full, actual residence in the cabin, itself, and four in foster homes in the Village. With the exception of one child who had returned to the cabin because of a school failure, all of these children were fairly advanced in their course, sufficiently stable so

that they could attend school successfully and avoid overt anti-social acts.

This, then, was the situation at the cabin when the ten-year-old boy—unnamed by Mr. Wall but, for the purposes of my statement I shall call George—came at the request of his mother and the Salisbury Health Center. Being people of small income and large family expenses, the parents could not pay the child's expenses—and here we must bear in mind that the payments for the child were always for bare expenses, not one cent for professional services by the Doctor. The importunities of the family and the plight of the child were such that the Doctor agreed to take him, money or no money; and, later, after some negotiations, it was arranged that the Torrington Welfare Department should pay the cost of his care and the Diocesan Bureau would supervise his case.

Let me now describe the boy's arrival in the clinic in the accused's own words. "He stood in the center of the main waiting room, scrubbed, brushed, polished, carefully dressed, erect, healthy looking, smiling; not merely smiling, but actually radiant with enthusiastic excitement, the living picture of every parental person's dream of a child. Before I could say a word, he greeted me in as winning a way as I have ever seen. Handsome looks, good-will, charm, self-confidence, poise—perfection in a child not yet ten years old. This, the picture, then, save for one detail. Every few seconds, this perfection would be ripped by a yelp, bloodcurdling, inhuman, feral, his mouth and throat retching open as if turning inside out to rid him of something, unbelievably horrid. As abruptly as it came, this spasm passed, the perfect child, apparently unaware that anything had happened, going right on with his prattle. To the beholder, his yelp was no more shocking than his eerie ignorance of it."

This little boy, the Doctor later learned, had not only been excluded from public school in his town, never advancing beyond the first grade, but also from his church because of his affliction. He could not go to the movies, he could not be taken in the stores or even on the streets without extreme embarrassment to his companions.

From that first day, the Doctor and George, awake and asleep, spent more than three thousand hours in each other's company, through many of which the Doctor was striving to think himself into George, studying his every movement, every word, every facial expression with utter absorption. How many psychiatrists that we

know would be willing to make such an intensive study while at the same time conducting a busy clinical practice in the mornings and maintaining a simple cabin life for four to eight boys? How many of us fathers in this courtroom have ever devoted so much time, energy and love to one of our own children?

\* \* \*

[At] the cabin, then, in this first period, most of the other boys were three to six years older than George. His eating, his sleeping habits taught the Doctor nothing about him. He showed exaggerated modesty, never undressing before the other children even when they went swimming. A hard player, he liked rough-housing, tackling, wrestling, and was an astonishingly good baseball player. In this type of play, it never seemed to occur to him or to the other children that he was weaker or smaller or younger.

In sedentary play, or in almost any other relationship, they relegated him to his little boy status. Building something was one of his favorite forms of solitary play, and he was both persistent and good at it. In handling smaller children at the clinic, he was almost as proficient as an experienced adult. At the cabin, he would cut wood, build a fire, try to cook a meal, do all sorts of difficult things—and without being asked to do them. He was most gregarious, every new face a new friend. Active as he was, he seldom had any accident; and in the first few months, the Doctor does not remember his ever crying. He talked excessively and took over the whole conversation of the cabin. Even so, he had difficulty in expressing himself. Words just wouldn't come when he called them. He was totally unable to read and could not even recognize his own name in print.

When George was alone with the Doctor, he occasionally treated the Doctor as a contemporary playmate; occasionally, as a father, more often as a fellow-worker, perhaps, but most of the time as a physician; and he would present his own case to the Doctor almost as intently as if he were some elderly, chronic invalid in the hospital.

In the first period of ten weeks at the cabin, his chief symptom, this abrupt outcry, did not change appreciably; if anything, for the worse. The Doctor's most intent study of it showed him no connection between this terrible noise and any aspect of his life, his mood, his companions, his activity, anything. After a day or two at the

cabin, when he had grown easy with the Doctor, he began to show two more habits, as he called them. He would double his right fist and hit his pubic region hard; and he would, at the same time, raise and twist his left leg and kick his buttocks with the whole inner surface of his foot. Throughout this first period of ten weeks, these two phenomena appeared and disappeared without any apparent rhyme or reason.

At about the end of the first ten weeks which had been spent in close observation of George, the Doctor was then estimating that it would take two years for him to get much result, and maybe five years to get well, if he ever could. At that time, the Doctor might be said to have thought of George like this: if one overlooked his main symptom, he was a marvelously adjusted child—good, obedient, perfect in almost every department of behavior, unbelievably effectual in all sorts of activities except school, and in all sorts of relationships. But, to the Doctor's mind, he was a brave, long-practiced, tested and durable fake, a fabricated construction after what he thought his world expected of him, a shell of a boy; and deep inside this shell was some hidden, unrecognizable something, pressing uncontrollably and senselessly up to the surface to explode in this yelp.

Now we come to a May evening—and in this, we do not agree with the presentation of the State that these acts occurred within a few weeks after [George] had been there. . . .

\* \* \*

. . . He had been playing about the clinic while the Doctor was at work there in the morning; and they worked together in the garden and cabin in the early afternoon. In the late afternoon, after school, he played with the other children while the Doctor was preparing their dinner in the cabin on the stove which The Court has a picture of. The meal over, the dishes washed, two children studying in the cabin, two children studying in the farm house kitchen. The Doctor and George have tended to the evening chores, feeding rabbits and chickens. The air is raw; and, rather than disturb the boys studying in the cabin and in the kitchen, they go into the barn—again, only fifty or seventy-five feet from the farmhouse; and the Doctor dropped down on some loose hay in the barn, George beside him.

The Doctor was lying on his back, half-asleep, his arm around George's shoulders; George beside him, his arm—his right arm across the Doctor's chest. His hand wandered in-



side the Doctor's shirt, stroked his belly; and, up to this time, it was accidental, drowsy, much like as though he were casually petting a friendly dog; and then George grew tense and purposeful as if some inhibition had been suddenly removed; and in the permissive pattern, the Doctor kept patting his back as if he were still absent-minded. As compulsively as if nothing could stop it, George's hand explored the Doctor's genitals, grabbed his penis firmly, and began a masturbatory movement. This act, as it grew in crescendo, was suddenly interrupted by the sound of the boys leaving the farmhouse.

And never had the Doctor been more alert to a child's looks and actions than he was to George throughout the next two days. The only change that he could detect was an increased, if anything, a small increase in his yelping, and a sort of unwillingness to meet the Doctor's eye, most unlike his usual open gaze. Two nights later, the chores over and the setting much like two evenings before, George said to the Doctor in a subdued way, quite without his usual brash confidence, "I'm tired. Can't we lie on the hay?"

This time, George quickly had the Doctor's penis in his hand and wasted no time, both manually and orally, bringing on an orgasm; the Doctor patted his back encouragingly. He then went to sleep on the hay, and the Doctor went in the farmhouse to take the night telephone calls.

When George awoke the next morning, he found the Doctor cooking his breakfast; and no matter how hard he studied the Doctor's face, George could find nothing there but loving approval. Not then did they mention this sex act nor have they ever since; but as the Doctor stood there, cooking breakfast in the early dawn, George seemed to shake himself free of something, not unlike a dog shaking himself as he comes out of a pond.

Perhaps an hour elapsed before the Doctor realized that James [*sic*] was no longer yelping; and as the usual day wore on, there was still no yelping. Others in the clinic and the village and, finally, James [*sic*] himself, toward noon, realized that he was no longer yelping. Never, after that time, did the Doctor hear the full cry again; and as if at a turn of a switch, George entered the second phase of his cabin life.

Almost a complete metamorphosis came over George. His shiny, outer shell almost visibly crumpled. He could with ease, now, go to the movies, go to his church Mass, to the school commencement. He no longer made himself work. Before imperturbable, he now sulked and fretted over any little act that didn't please him,

weeping real tears if there was some hurt to his feelings. His effectiveness with small children at the clinic ceased, and he could no longer be trusted with them. His skill in baseball was no use to him, any more, because he preferred to push little sticks around little trucks in the dust. His exaggerated modesty disappeared, and he needed to talk less. He suddenly showed the first steps toward ability to read.

And the second week of June, the Doctor let George go home for a time, explaining to his family and to George, himself, that he considered the ending of the noise to be just the end of his symptom and not the end of his disease. Apparently, they were so excited about it that they told the social worker in charge that he was cured and home for good. At this time, it was the Doctor's intention to spend the rest of the summer preparing George for school.

The third phase for George began with his return from his rather unsuccessful visit home in June. This period marked the beginning of his progression or re-education. As part of the Doctor's program, life grew harder and harder for George. Reading lessons took on a fixed schedule. Hating walking as he did, he was required to walk a mile to his tutor's house. Many little obligations were imposed, and many privileges denied. The sexual activity with the Doctor which occurred several times during the second period was not conspicuously refused by the Doctor, but he saw to it there was no time for it.

George used to tag along up to the Hague apartment where the Doctor would retreat to read a while, occasionally. Merely coming to sit on the edge of the old cot as the Doctor lay there, reading, seemed to be an intimacy sufficient to keep down the old anxiety of George. If the Doctor had said in so many words that the old genital activity was over, especially if he had shown any disgust or disapproval at this time, the Doctor thought that George would not have done so well.

With this harder program, though he showed numerous tics and other evidences of strain, he tolerated the steadily increasing pressure of the regimen, very well. He still kept his easy friendliness, with the Doctor; and in George's eye, the accused believes, he was always a doctor. Such, then, was the beginning of the rebuilding, the progression of George in this third period when it was abruptly halted on August fourteenth by the Doctor's arrest.

So far, I have said nothing about the other members of the family with whom George had lived. It is axiomatic that, in working with chil-

dren, that a ten-year-old child, if he has lived with them, is as much a product of his family that he can not be considered apart from them. From years of practice and experience, the Doctor had become an astute observer of people; and his report on George's family certainly demonstrates it. Father, mother, uncle, aunt, two sisters, sixteen and eighteen years of age, three brothers, one of whom I shall refer to as "Peter," the thirteen-year old; one, nine, whom I shall call "Mark"; and one, four, whom I shall call "Philip." Peter, the thirteen-year-old; Mark, nine; and Philip, four.

Of these nine people, although they made a complex, perhaps trying, family group for George, only three seemed to the Doctor to really stir him; namely, his mother, his uncle and Mark, the nine-year-old.

It was an important part of the Doctor's work with the cabin-child to invite the relatives in and to associate them with the child in varying combinations and situations which were calculated to show the structure of the relationship. Although Peter, thirteen, did not appear to upset George, he threw some light on George's case. He first came to the cabin to visit George along with the other children, the mother and the uncle in April, one month after—approximately one month after George came there. After that, he began coming to the cabin whenever possible—and not as Mr. Wall has suggested on invitation of the Doctor; by his own request. Every time he would leave, he would ask if he could come back again; and there were dozens of times—for an hour, a day, overnight, a weekend; and in the summer, for two continuous weeks. All this time, the Doctor was carefully studying him and his relationship to George.

After the Doctor's first sexual experience with George, he was quite busy with the clinic, and Peter was not invited to the cabin for some time; and when he was, for the first time, afterwards, the Doctor saw to it that he and George did not have a moment together unless the Doctor was within earshot. He was, then, convinced that Peter could not have heard of George's experience before Peter had almost exactly a similar one with the Doctor. That is, it was not the Doctor being the aggressor, but the permissive environment, and the boy taking the initiative. After that, further acts of this nature with Peter had only—were only in continuance and furtherance of this study.

Mark, the nine-year-old, had much greater impact on George. Historically, he was conceived unintentionally while George was just a

few months old; and when George was five or seven months old, still almost an integral part of his mother, he was sent to his uncle and aunt who were childless. Thus, for George, the world fell apart when he was, in a sense, put out of his home by Mark, this now almost perfect little boy—I am speaking of Mark, age of nine, healthy, bright, successful in school, the kind of child that George was trying so desperately to be.

Helping George in his relationship with Mark, the Doctor considered to be a fundamental step in his recovery. As these boys, in George's third period, were going to be dumped together like puppies within a litter in a few weeks, and as the Doctor had discovered that both George and Peter were practiced in sex play—I think it's important to emphasize that it was the type of sex play that was not phantasy, and neither were they able to know of the acts which they did without having done it with someone older than themselves before the Doctor found this out—so it was of importance for the Doctor to learn whether or not Mark had had any of this experience because he felt that George's yelp had a sex-guilt expression—or had been a sex-guilt expression, and that this sex-guilt was pretty well now gone as far as George was concerned. So that this perfect younger brother, Mark, was just the one who would make George feel most guilty if he were in the habit of sex play with him, and then the symptoms might return.

As Mark was such a snuggling child with the Doctor and so demonstrative physically, it was easy for the Doctor to learn whether he had the same desire for genital play as his two older brothers had shown. It seemed quite clear to the Doctor that he had not. He would lie beside the Doctor with his hands on the Doctor's genitals with no apparent interest, excitement or revulsion much as he might have considered the Doctor's ear or nose. After a little of this, the Doctor was sure that Mark did not know of the sort of sex play which his brothers knew so well; and, further, the Doctor believed that he had learned this knowledge of Mark without arousing or scaring or harming him, at all, as he continued to maintain his same affectionate, trusting ways with the Doctor. Here, I do not wish to imply that all the sexual acts of the Doctor with the boys, on his part, were merely permissive; but I do assert that they were all engaged in by him with the same purpose in mind.

\* \* \*

These acts are condemned and prohibited

by law as indecent, unchaste, impure, obscene. This, the Doctor knew and understood when he engaged in them; but it appears to me, the motive and intent of the actor may and does change the whole character of the act and its consequences. Killing on the battlefield is sanctioned in wartime; and, often, the killer is rewarded with medals while killing in peacetime is murder, punishable by death.

With The Court's indulgence, I would like to read from a letter I received last week from an outstanding authority in the field of child guidance and family life who, with her husband, both psychologists, have conducted a school for disturbed children for more than twenty years. These people are in Court, here, today, Your Honor.

After several talks with the Doctor and reading his report of the cabin project, she wrote me of her great concern about the possibility of the Doctor's pleading guilty to these charges. I am quoting. "It would seem to me that he had every professional and human urgency to do exactly as he did, and I would wish that he would fight for that right. Perhaps I can make myself clearer with a simple example. Two men may cut open a woman's stomach. The one, Jack, the Ripper, commits a criminal act of assault. The other, Dr. John Doc, Surgeon, performs a life-saving miracle. Both men have performed, essentially, the same act. The intent of one is sadistic. The intent of the other is lifesaving and knowledge-seeking for the purpose of further lifesaving. The result of one is death; the result of the other is a chance for life. Are both these men to be regarded in the same light?"

"Must the surgeon when attacked by ignorant, if well-meaning persons, plead guilty to assault along with Jack, the Ripper; and, if he does, does this not help to identify his act, in the public mind, with that of Jack?"

"The Law necessarily follows, rather than precedes human experience. But if scientific exploration ceases until legal processes catch up, where would human progress be? History has presented us, again and again with the dilemma of brave men of insight and vision who have elected to proceed at whatever personal cost with the task of blazing new trails. The legal challenge to establish the right of such men to work must be one of the powerful inducements that attracts men like yourself to the legal profession.

"There is evidence, and lots of evidence, that Dr. Martin was on the right track. To take just one small bit—for years, there have been available to therapists, and even to educators,

flexible dolls, male and female, adult and child and baby, equipped with the appropriate genitalia for their sex and age. These have been used by psychiatrists, therapists and some nursery school teachers to permit distressed children to act out their sexual perplexities and to vent their sexual feelings. Often enough, there has been relief of anxiety symptoms; providing, of course, that the therapist has lovingly accepted all acts performed upon the dolls and between the dolls as manipulated by the child.

"Young children, and children not yet seriously disturbed, might respond reasonably well to this ventilation of anxiety, but one could hardly imagine that the seriously disturbed children brought to Dr. Martin, the so-called hopeless cases, could respond to anything or any person external to him. To use psycho-lingo, their transference was to him. It seems obvious that these youngsters had to work through their anxiety in the acquiescence and acceptance of his own person.

"Knowing what small amount I do about children, it seems to me that Dr. Martin did an enlightened act of professional and personal giving of himself that could be conjectured to have made cure possible for these children. And the indisputable fact is that the children did, thereafter, take the clearly recognizable first steps toward recovery."

\* \* \*

"Scientifically, he has given us clues to understanding children and the deep roots of their disturbances that few other scientists have even dared to look at, let alone expose. I feel that my own knowledge of children and effectiveness to them in time of trouble has been vastly increased by these observations and perceptions." That's all.

May I respectfully call to The Court's attention the fact that members of the accused's family, his two brothers, one a college president, one a physician, another a sister who has come on from Illinois, his home state, are here. They, who would be his severest critics, if they believed him to be wrong, they don't have their heads lowered in shame because they have seen him before on the forefront of progress.

In addition to the members of his family, two of the physicians who were associated with him in his office practice in Littleton are in Court; and since they cannot testify here, I asked one of them for his Credo on the Doctor which he has given me permission to read to The Court since he cannot testify.

"To my knowledge, Dr. Martin has been an outstanding practitioner of medicine for many years. He has been at the head of two departments of pediatrics at major university colleges of medicine. He has maintained a large practice of pediatrics locally, doing much charity work. He has been sought after as advisor and lecturer on pediatrics and family problems. He has a tremendous number of devoted proteges, many of whom occupy positions of importance in public life. He is a man of many diverse talents and has done much of basic importance in bacteriology and serology, has published work of fundamental importance on undulant fever and streptococcal diseases.

"He has always been known as a fearless pioneer in new fields. He has never sought favor, wealth or self-aggrandizement. He is primarily interested in fundamental biologic processes.

"He has been active and effective in the field of pediatric psychiatry for many years. He has intentionally taken on only a few, but the most challenging and difficult cases for psychiatric treatment. He has had real success in rehabilitation of seriously diseased minds.

"His actions, as described by himself, represent the exploration of little known problems with equally little known techniques. The problems were unorthodox; the approach equally so. To assume that he allowed himself to indulge in self-gratification ignores a completely selfless past and loses sight of the incredible amount of time and energy devoted to maintaining, feeding and teaching the boys under his care. This was a twenty-four hour, seven-day job without interruption. This was the work of an exceptionally devoted man, for a man with very unusual singleness of purpose.

"There is no doubt in my mind, speaking as a physician, but that Dr. Martin's actions represent an extension of scientific research into the sexual problems of adolescents. I am not competent to judge as to the value of what he accomplished with them or discovered. I have no doubt as to his motive."

\* \* \*

In answer to the charge of the state that the accused is a lecherous pervert preying on young children, his friends, his neighbors, parents and children would answer that his Spartan existence four miles from town without a car, in all seasons of the year, his selfless and untiring dedication to his desire to help children and to pierce and throw back the darkness of ignorance and the unknown, his remarkable self-discipline, his

infinite patience and understanding refute and belie that charge. In his own words, the accused's own reply is that he is a serious student of difficult medical problems, who has helped some very sick children and who may help other doctors to cure such sick children.

Already, his punishment has been extreme for, as soon as he was arrested and the accusations against him made public, the Doctor he was, was ruined; and the man he was, was broken. In losing his license, [\*] sure, he has lost, not only his means of livelihood, but more important, his opportunity to go on serving children and parents as he had done so unselfishly and so successfully in the past.

In jail for only seven weeks, but dishonored for a lifetime, and already gravely punished at the age of fifty-two, he has, I sincerely believe, discharged his debt to society. Therefore, I re-

[\*] The license or certificate of registration of any licensed or registered practitioner of the healing arts in this state may be revoked, suspended, or annulled, or such practitioner may be reprimanded or otherwise disciplined, after notice and hearing, on the recommendation of the examining board representing the branch of the healing arts practiced by such practitioner for any cause named below. . . . The causes for which a license or certificate of registration may be revoked, suspended or annulled or for which a practitioner may be reprimanded or otherwise disciplined are as follows: Conviction in a court of competent jurisdiction, either within or without this state, of any crime involving moral turpitude, of any infamous crime or any crime in the practice of his profession; immoral, fraudulent, dishonorable or unprofessional conduct; illegal, incompetent or habitually negligent conduct in the practice of the healing arts; habitual intemperance in the use of spirituous stimulants or addiction to the use of morphine, cocaine or other habit-forming drugs; advertising in connection with the practice of the healing arts which is found by the board representing the branch of the healing arts practiced by the practitioner to be deceptive, misleading, extravagant, improbable or untrue; aiding or abetting the unlawful practice of any branch of the healing arts; failure to record a license or certificate of registration as required by law; insanity of the practitioner; fraud or deception in obtaining a license or certificate of registration. The clerk of any court in this state in which a person practicing any branch of the healing arts has been convicted of any crime as described in this section shall, immediately after such conviction, transmit a certified copy, in duplicate, of the information and judgment, without charge, to the state department of health, containing the name and address of the practitioner, the crime of which he was convicted and the date of conviction. [Conn. Gen. Stat. §20-45 (1958)]

spectfully recommend to The Court that there be imposed a suspended sentence with probation. Thank you for your kindness.

THE COURT: May I ask one question so that I may fully understand Counsel's claims? As you related the story, these children were, in a sense, the aggressors. Do you claim that at no time was the defendant the aggressor?

MR. EBERSOL: No, Your Honor, I did not.

THE COURT: I didn't think you did. I thought you made it clear, but I wanted to be—I wanted to be certain of the position of the defense.

MR. EBERSOL: I made the statement that he was the aggressor but that the aggressions were for the same purpose and in the same medical pattern as the permissiveness, that he considered them just as necessary.

THE COURT: I am speaking of acts other than those which you have taken the trouble to describe.

MR. EBERSOL: That's right, Your Honor; and so am I.

THE COURT: Very well. Thank you.

MR. EBERSOL: Thank you.

MR. WALL: My brother has made a remarkable defense of this man. As I gather it, the crux of his defense is that, at all times, he was acting for the interests of these children and that these acts upon the children were in the nature of therapy—described as "blazing new trails," "spiritualization of the sexual act," making these young men—these boys—or "freeing these boys of their infantile needs"—I quote these things—"unlearning homosexuality". . . .

\* \* \*

. . . Now, as to therapy, in the first place, it is inconceivable to me that such acts can constitute therapy. Course, I am not the greatest physician or scientist in the world, and I don't set myself up against psychiatrists of great renown; but I don't see that there is any psychiatrist of great renown who is advancing any such theory here. We have just a defendant who is accused of these most repulsive acts in connection with young children, trying to defend himself; and he says it's "therapy."

If there is any great mind, any great psychiatrist who can—who has advanced any such theory, and that appears in the literature, anywhere, why, I presume that that might have some bearing upon the Court.

Actually, we, with the limited facilities available to us, to the State, did consult a psychiatrist.

We consulted the very psychiatrist who was connected with the clinic which referred this ten-year-old boy to the defendant; and he advises us that there is nothing in the literature of psychiatry which supports any such fantastic theory.

Now, also, as to his claim of therapy, he had a ten-year-old boy that he was treating for an unfortunate condition to which he was subject. I have no doubt that he did try to rid that young boy of his condition. I have no doubt that he may have had some skill in that regard; but I have no doubt, either, Your Honor, that the—as to the effect of the homosexual acts upon the boy. I don't believe that it's possible for any—I wouldn't have believed it possible to have anyone come into a court of law and state that the—that such acts with a ten-year-old boy could constitute, by a man of fifty-two years of age, theretofore respected in his profession, that such acts could be considered as therapy; and to prove that he didn't consider it as therapy, Your Honor, and to prove, also, that to use the defendant's attorney's statement that "the State assumed he indulged for self-gratification," and to prove that he did indulge in these acts for self-gratification, Your Honor, the nine-year-old boy didn't need the therapy.

The nine-year-old boy didn't bark like a dog. The thirteen-year-old boy didn't bark like a dog. There was no necessity for any therapy between the Doctor and the nine-year-old boy and the thirteen-year-old. There was no expectation of any continuation of treatment in the future. They were only there to visit for a few days at a time, coming there for a few days at a time. All he could be considered to be doing was debauching them, getting them into the type of vice which is—up until today, Your Honor, I thought definitely was disgraceful conduct; and I thought it was generally so regarded. Certainly, it is against the law. Certainly, parents who entrust their children to physicians for treatment should not be expected to hear from that physician, at a later time, "that I committed homosexual acts on your boy for your boy's own good, and that I committed homosexual acts upon the boy to whom you entrusted me for treatment, and also his older brother, and also his younger brother because I thought it would help the boy entrusted to me for treatment." It just is inconceivable, Your Honor, that parents should have to accept any such statement.

\* \* \*

. . . Your Honor. I feel that, despite the bril-

liance of my brother's statement to the Court in defense of the indefensible, that by having a defendant ask an attorney to make a statement such as that before the Court, that is a fraud upon the Court, Your Honor, to have the defendant ask an attorney to tell a story such as—I believe my brother is very, very well-intentioned; but to my mind, it adds insult to the injury here to have a defendant insist upon that type of a defense being made, that he was in the course of a scientific investigation, that it was therapy; and under those circumstances, Your Honor, he doesn't stand before Your Honor as someone sorry that he has disobeyed the law and telling Your Honor that he wishes no longer to continue to disobey the law. He comes before Your Honor to say, "I am a scientist. This is just a portion of my science. This is the way that I treated these children." And he takes credit for some of his undoubted skill as a physician in the past and by claiming that the homosexual activities were integrated with other treatments which he gave to his charges. I don't say that the Doctor hasn't done things for people in the past. He undoubtedly has done a great many things for people in the past; but to have prostituted children aged nine, ten, thirteen, and to claim that they—that he was completely justified in doing so is not coming before your Court—before this Court with the type of defense which would involve the extension of any mercy to him.

I think, Your Honor, that the only possible disposition of this case is a long State's Prison sentence. . . .

\* \* \*

MR. EBERSOL: If it please the Court, I would like the privilege of remarking only with respect to Brother Wall's final statement—that I think, from my earlier statement, it should be apparent to the Court that, with the exception of the nine-year-old brother, that each and every one of these boys had a previous history of sexual perversion, that they were not learning new tricks from the Doctor.

THE COURT: I beg your pardon. I didn't hear the last—

MR. EBERSOL: They were not learning new tricks from the Doctor. I have already stated the reason and purpose for the involvement of the two boys; and although there are five statements there, there are statements from only three of the cabin boys.

MR. WALL: I might say, Your Honor, that any previous sexual interest these boys may have

had is based entirely upon what the defendant, himself, states. There is nothing in our file that indicates any previous sexual experience by these children, absolutely nothing.

\* \* \*

THE COURT: I went over the Doctor's notes in full, and very thoroughly, last night. I spent a great deal of time reading and studying these documents; and they are very, very revealing. There isn't any question in my mind—and I say this, not to rub salt in the wound, but as a matter of fact—that the defendant is a sex pervert. There isn't any question in the mind of the Court, either, that the defendant was the aggressor in these acts.

I think, however, that Defense Counsel stated well and accurately that this man is a man of outstanding ability, that he is a man who, unquestionably, could be a healer among these children.

I was impressed with his deep-seated knowledge of these youngsters, of the time that he spent with them and of the effort that he put into it.

I was also impressed with the fact that he has this very sad affliction which makes him a subject not fit to be associated with children. That's the sad part of it because there, probably, is where his greatest ability lies—in treating children.

With reference to punishment, these matters I consider very difficult ones to deal with. I think every judge does. The matter is serious as far as using these homosexual acts as therapy; as far as the claim that he is a pioneer in this field, the Court must reject that. That just isn't so. The facts indicate that the acts took place with two children who are in no way involved in treatment. The excuse or justification that these things took place to determine what had been the child's experience prior thereto for the purpose of therapy for the child—that, the Court can't agree with. In considering punishment, I recognize the fact that here is a man of great ability, that no matter how small the punishment or how great, he has been severely punished by the public disgrace and humiliation to which he has been subjected already. At the same time, I recognize the fact that he just can't resist these tendencies. I wish there were a physician to which I could send this defendant. I don't know of any. He's the only physician who can do anything for himself; and I hope he can because I sentence him with the utmost regret because of his outstanding

ability, because of what could well be a genuine interest in children and the treatment of them; but the public is entitled to some protection.

The sentence is to the State prison for a term on the first count of not less than one nor more than three years. On the second count, three years.

That means that you have a sentence of not less than one nor more than six years. The length of time that you stay there depends upon you and depends upon the judgment of the Parole Board and the authorities in the prison. I hope that you will do the things which will get you out of there and into productive work as soon as possible; and I wish that there were a physician who could do something for you.

### NOTE

#### CONNECTICUT GENERAL STATUTES (1949)

\* \* \*

Sec. 8359. *Indecent assault.* Any person who shall commit an indecent assault upon another person shall be imprisoned not more than ten years. The overt act or acts of which such assault consists need not be otherwise described in a complaint under this section than as an inde-

cent assault, unless the accused shall request the court that it be particularly described in such complaint. It shall be no defense to a complaint under this section that the person assaulted shall consent to the act of violence or to the act of indecency, and this section shall not affect the penalty for sodomy.

Sec. 8369. *Injury or risk of injury to children.* Any person who shall wilfully or unlawfully cause or permit any child under the age of sixteen years to be placed in such a situation that its life or limb is endangered, or its health is likely to be injured, or its morals likely to be impaired, or shall do any act likely to impair the health or morals of any such child, shall be fined not more than five hundred dollars or imprisoned not more than ten years or both.

Sec. 8544. *Bestiality and sodomy.* Any person who shall have carnal copulation with any beast, or who shall have carnal knowledge of any man, against the order of nature, unless forced or under fifteen years of age, shall be imprisoned in the State Prison not more than thirty years.

Sec. 8553. *Fornication or lascivious carriage.* Any person who shall be guilty of fornication or lascivious carriage or behavior shall be fined not more than one hundred dollars or imprisoned not more than six months or both.

## B.

### In Determining the Investigator's Authority, What is the Relevance of:

#### 1.

##### Choice of and Attitude toward Subjects?

##### a.

##### Vernon H. Mark and Frank R. Ervin Violence and the Brain\*

The problem that modern biological and social scientists have in trying to deal with violence is much like the problem that 19th century neurologists and psychiatrists had with "insanity"—learning enough about its causes and natural history to be able to assess individual cases and treat each patient properly. In the 19th century the insane asylums were full of people

lumped together as "crazy," but who in reality had many different diseases. Some actually had pellagra. Once doctors could recognize vitamin deficiencies, and know how to prevent and treat them, people no longer developed pellagra-related symptoms of mental illness. Others were suffering from the late stages of syphilis, and still others from undiscovered brain tumors; both conditions became accessible to diagnosis and treatment, thereby shrinking the "crazy" category still further. In short, the more psychiatrists and neurologists learned about the various causes of "insanity," the more effectively were they able to differentiate between mental conditions that appeared to be the same, but, because they had different causes, required very different methods of treatment.

We are in the same state today vis-a-vis violence: we need to find out more about the condition and learn how to sort out its different

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causes, so that we can decide which are the most important biological and/or social factors in each in each individual case, and then treat each patient appropriately.

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If detecting a potentially violent individual is the first order of business for meaningful investigation, the second is to improve our treatment methods. This could well start with a re-evaluation of the kinds of psychotherapy given to violent patients. As certain forms of conditioning (i.e., conditional reflex therapy—à la Pavlov) have been shown to improve or alleviate special kinds of temporal lobe epilepsy, applications of improved techniques in behavior therapy might have important consequences for the psychotherapeutic treatment of impulse disorders.

The recent pharmacologic advances in anti-convulsants and tranquilizers presage not the end but the beginning of a psychopharmacological revolution. Many new and important chemical agents and drugs will be added to our armamentarium for the treatment of impulsive and violent behavior. But even with these new psychotherapeutic and medicinal tools, some people with brain disease may still require surgical treatment for the control of violence. How can we improve our surgical operations? By making smaller and more precise lesions within the brain? By using electrical stimulation . . . instead of making destructive brain lesions? Perhaps a prolonged therapeutic effect can be obtained by the introduction of chemical agents into focal areas of the brain to produce chronic chemical stimulation over a long period of time.

[A]s long as senseless killings and brutality are acceptable events in our cities, on our highways, and in our foreign relations, then identifying any violent individual as unique will continue to be very difficult indeed. How, in fact, can society even define what is "abnormal" under these circumstances? Only when our society—through its educational, religious, family, and governmental structures—clearly defines and uniformly reacts to violence as being unacceptable, will we be able to approach the situation in a truly rational way.

The definition of "unacceptable violence" is, of course, a major stumbling block. What is "unacceptable violence"? The "law and order" faction of our society might define any liberal group protest as falling into this category, while protesting groups might label any action of police

against demonstrators as "police brutality"—a clear-cut case of "unacceptable violence." Some minority groups have gone even further and, having identified the deprivation of civil rights as a form of violence, have equated this term with physical violence, and justified physically violent retaliation.

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b.

### **Herman J. Muller Means and Aims in Human Genetic Betterment\***

The main thesis I wish to uphold . . . is the following. For any group of people who have a rational attitude toward matters of reproduction, and who also have a genuine sense of their own responsibility to the next and subsequent generations, the means exist right now of achieving a much greater, speedier, and more significant genetic improvement of the population, by the use of selection, than could be effected by the most sophisticated methods of treatment of the genetic material that might be available in the twenty-first century. The obstacles to carrying out such an improvement by selection are psychological ones, based on antiquated traditions from which we can emancipate ourselves, but the obstacles to doing so by treatment of the genetic material are substantive ones, rooted in the inherent difficulties of the physicochemical situation.

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[T]he earlier stages of genetic surgery, if it does come into use, will doubtless be concerned mainly with repairing germ cells having certain rare and extreme genetic defects, such as the idiocy caused by failure in handling the amino acid phenylalanine. In this negative role of the technique there will be no more question of values than there is for a surgeon of the more usual kind, for everyone would agree that such marked defects are undesirable. There is nevertheless a problem of values or ethics in deciding for which people it is justifiable to make an effort if, as seems likely, the effort were too great to be applicable to every such case. But in making these decisions we would be thrown back on the whole ethical question of who should reproduce, and to what extent. And until genetic

\* T. M. Sonneborn, ed.: *Control of Human Heredity and Evolution*. New York: The Macmillan Co. 100, 109-115 (1965). Reprinted by permission.

surgery acquires such finesse as readily to make a sage out of a simpleton, a saint out of a scamp, and a Samson out of a shrimp, we will continue to have this problem with us.

Conceivably, we might some day achieve such seeming miracles of genetic metamorphosis as these, by manipulating certain specific genetic sites or chromosomes and thus providing genes that exerted major influences on the general abilities in question. It would at the same time be desirable, in such cases, to obtain as high concentrations as possible of the very numerous correlative or modifying genes that act, sometimes very subtly, to support the major ones, as by regulating and giving balance to their expression. However, it would be a task of transcendent magnitude, intricacy, and reconditeness to do all this by genetic surgery for any one individual. Moreover, every individual to be operated on would present his own unique complex of labyrinthine problems of this sort.

If at length, however, the techniques were mastered that did enable genetic surgeons to tackle in a really practicable way the stupendous tasks of producing to order genetically improved types of human beings, then they would find themselves face to face with the age-old problem of what human values they should strive for—a problem here couched in genetic terms. This problem, even in its genetic form, is not unique for genetic surgery, however. It applies equally to any conceivable scheme for genetic betterment, and it has been used by critics as an argument against any such attempt.

For persons who would concede the desirability of human genetic betterment, or at least the need of merely preventing genetic deterioration, the possibility of conducting it by some kind of parental selection should not be overlooked, for the technical difficulties of such an approach are incomparably less than those of genetic surgery, in view of the enormous wealth of diverse genetic combinations that are already in existence in any human population. Moreover, the potentialities of these combinations can be assessed in a rough and ready way by methods similar to those obtaining under natural selection, namely, by using the criterion of the given individual's performance. It seems truly perverse for people to wait until they can take the long way around and manufacture genetic constitutions to order, when they are, in large measure, already available. Let us then consider in what ways the genetics of existing populations might be influenced through parental selection, so as to de-

crease the frequency of genetic defects and to increase the abundance of traits that are considered desirable.

We may first dismiss as obviously biased and pernicious the claims of racists who see in their own race a markedly superior type of humanity or who, conversely, single out certain other races for special condemnation. We may likewise give short shrift to those old-style eugenicists who, regarding economic, social, or educational status as a reliable enough criterion of genetic fitness, have advocated measures that would make it easier for the so-called upper classes to have children and harder for the so-called lower classes.

A recent, rather sophisticated modification of the last-mentioned notion is the proposal that certain types of occupations should be so designed as to be attractive to persons of genetically less desirable types, and that the circumstances of these jobs should be so arranged as to make it relatively inconvenient and unattractive for these workers to have children. On the other hand, conversely, certain other types of occupations should, in this view, be so contrived as to be attractive to persons of more fortunate endowments, and these occupations should be associated with ways of living made conducive to the rearing of large families. It is hard to criticize this proposal seriously unless it is spelled out much more concretely. We should, however, point out that exactly this matter of finding a suitable concrete form for it would be a source of much difficulty, especially since the system of values of genetic traits implied in the given concrete arrangement would have to be one that people could agree on. We should also point out that it is highly unlikely that any democratic society would consent to having such intentional restraints imposed on it.

In general, any type of eugenics in which, as in this proposed case, the standards and values are decided upon by governmental bodies is to be regarded with suspicion, even if the government is of some democratic form, for, as yet at least, governments, including relatively enlightened ones, represent in some respects the lowest common denominator of progressive thinking. (In fact, a distinguished scientist on reading this remark has commented that it is in his opinion an understatement.) Nevertheless, it can be heartily agreed that enlightened governments, by their support of free public education, freedom of expression, and scientific research, do indirectly fill a highly useful function in human ad-

vancement. That is, they can increase people's opportunities for finding roads to progress.

Now among the most important educational needs of modern populations are those in the area of genetics and evolution. The so-called "common man" already has sufficient native intelligence and social consciousness to be able, when suitably taught, to appreciate the importance of both a good heredity and a good environment, to realize that the betterment of both is to be sought for, and to find gratification in contributing efforts of his own for these common purposes of mankind. Moreover, he is so constituted as readily to adopt a value system in which high regard is given to such primary human psychological attributes as those of sympathy, moral courage, reasonableness, and creativity. This being the case, the most fundamental basis is at hand, through education, for preparing people to follow, voluntarily, courses of action that will on the whole be conducive to the genetic betterment of the species.

Among such courses of action the type most commonly thought of consists of the exercise of more restraint in having children on the part of the genetically less well endowed and the raising of larger families by the better endowed. To this recommendation it should be answered, first, that few people of inferior mentality are willing to appraise themselves as below the average in this respect. Second, those of lower-than-average moral fiber can hardly be expected to exercise unusual restraint in the interest of a higher moral fiber for mankind in general. On the other hand, third, persons of higher-than-average mental ability or of unusually conscientious or considerate disposition are often the very ones most likely to limit their families, in order to enable both themselves, their spouses, and the children whom they do have to live a life more rewarding in other respects. Certainly people's estimates of themselves and of those closest to them are notoriously biased and unrealistic.

At best, then, the attempt to inculcate policies of this kind could have but a small positive effect on genetic trends. Perhaps this effect would be hardly enough to counteract fully the trend toward genetic deterioration that must exist today in technically advanced countries, with their low death and birth rates. . . .

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[F]or the present and a considerable period to come our knowledge of what genetic defects people carry in hidden form will be so fragmen-

tary as to be of little use for the purpose of substantially reducing the frequency of severe genetic defects—the main objective of negative eugenics today. Of far greater consequence for the population, however, than the avoidance of the sporadic outcroppings of such hidden defects would be the raising of the genetic level in regard to the abilities and proclivities of greatest human importance. In order to achieve major practical results along these lines it would not be necessary, nor would it for a long time be possible, to arrive at an exact knowledge of the genes and gene-differences involved. They are undoubtedly very numerous, but they give evidence of having, on the whole, a fair amount of dominance. Thus—except for the confusing influence of cultural and other environmental differences—a kind of over-all estimate of an individual's genetic level in regard to these attributes, one having considerable validity, could be obtained by considerations of his actual performance or, as the geneticist would say, his phenotypic classification (not meaning merely his looks!). This is in fact how nature operates in the process of natural selection and how man has operated in past times in the artificial selection of other animals and of plants, and the method has obviously worked. In such overall appraisals, moreover, one or more highly valuable traits often more than make up for considerable shortcomings.

A heredity clinic would hardly dare to offer advice along these lines to people in general, and, if it did, the advice would probably be discounted and resented. Many people would hold, and often quite rightly, that the fact of the advisor's being a geneticist or a physician does not necessarily make him a good judge of what constitute the higher human values, or of the degree to which they, the judged, measure up to reasonable standards in these respects. Yet, as we have noted previously, their own judgments of themselves would also tend to be biased, as they might well admit themselves during moods of unusual calm and objectivity. Does this situation force us to conclude, then, that all doors to parental selection of a salutary and significant kind are closed for the human species?

There has for some time been still another possible method of parental selection, which in large measure avoids these difficulties. This is a method I . . . have recently called germinal choice. . . .

Unlike what is true of other forms of parental selection that have been suggested for man,

this method does not work by attempting to influence either the size of families that people have or their choice of marriage partners. Neither does it attempt to influence people's evaluations of themselves. Its proposed mode of procedure is to establish banks of stored germ cells (spermatozoa), eventually ample banks, derived from persons of very diverse types but including as far as possible those whose lives had given evidence of outstanding gifts of mind, merits of disposition and character, or physical fitness. From these germinal stores couples would have the privilege of selecting such material, for the engendering of children of their own families, as appeared to them to afford the greatest promise of endowing their children with the kind of hereditary constitution that came nearest to their own ideals.

As an aid in making these choices there would be provided as full documentation as possible concerning the donors of the germinal material, the lives they had led, and their relatives. The couples concerned would also have advice available from geneticists, physicians, psychologists, experts in the fields of activity of the donors being considered, and other relevant specialists, as well as generalizers. In order to allow a better perspective to be obtained on the donors themselves and on their genetic potentialities, as well as to minimize personality fads and to avoid risks of personal entanglements, it would be preferable for the material used to have been derived from donors who were no longer living, and to have been stored for at least 20 years. The technique of preparing semen in a medium containing glycerine and keeping it at the temperature of liquid nitrogen . . . provides a reliable and relatively inexpensive means of maintaining such material for an unlimited period without deterioration.

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c.

**Frank J. Ayd, Jr.**  
**Fetology—Medical and Ethical Implications**  
**of Intervention in the Prenatal Period\***

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. . . Dr. Geoffrey Chamberlin at King's College Hospital, London, England . . . has conducted experiments with living human fetuses. . . . In his initial experiments, eight living human fetuses, weighing from 300 to 980 grams, were obtained by hysterotomy for the therapeutic

abortion. In seven cases the amniotic sac was removed intact from the uterus; the other fetus was placed in normal saline solution at the operating table. To prevent any respiration, Dr. Chamberlin reported, the fetuses were kept under artificial liquor amnii in a tank. Within 12 minutes after removal from the womb, the umbilical vessels—vein and both arteries, where possible—were cannulated. In each fetus, Dr. Chamberlin said, "The cannulas were passed well beyond the abdominal wall, along the hypogastric arteries, and hopefully into at least the internal iliac vessels." Then the eight fetuses were kept alive for varying intervals on total perfusion via the artificial placenta.

Dr. Chamberlin reported that "the longest survival in this series came with the largest fetus." Delivered from a 14-year-old girl, this 980-gram male fetus . . . "was cannulated without trouble about 11 minutes after the placenta was separated. A brisk spontaneous flow was noted 22 minutes postpartum; the fetus was kept on the circuit for 5 hours and 3 minutes.

"Only when a cannula slipped out by accident and could not be reintroduced was the experiment halted.

"Irregular gasping movements, twice a minute, occurred in the middle of the experiment," Dr. Chamberlin declared. "Once the perfusion was stopped, however, the gasping respiratory efforts increased to 8 to 10 per minute." The fetus's death was recorded 21 minutes after disconnection from the artificial placenta.

"Continuous electrocardiographic monitoring throughout the experiment, using standard I leads, revealed a regular pulse that varied from 120 to 90. When the circuit was stopped, the heart slowed, fibrillated and eventually ceased beating.

"Maintained at 39° to 42° C in a water bath, with 15-to-40-ml-per-minute extracorporeal flow rates, the fetus seemed undisturbed. He made 'occasional stretching limb movements, very like the ones in other human work,' reported by a Swedish team under Dr. Bjorn Westin."

Although none of the living human fetuses connected to the artificial placenta survived for more than five hours and three minutes, these human experiments were rated as successful. . . . The human fetuses used in these experiments are alive. What are their rights? Since they are incapable of giving consent to their use as experimental subjects, who can morally and legally give consent for them—their mothers, their fathers, both parents, or the state? . . .

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\* 169 *Annals of the New York Academy of Sciences* 376, 379 (1970). Reprinted by permission.

## NOTE

PAUL RAMSEY  
THE ETHICS OF GENETIC CONTROL\*

\* \* \*

. . . In the case of cloning a man, the question is what to do with mishaps, whether discovered in the course of extracorporeal gestation in the laboratory or by monitored uterine gestation. In case a monstrosity, a subhuman or parahuman individual, results, shall the experiment simply be stopped and this artfully created human life killed? In mingling individual human chromosomes with those of the "higher" mammals (given sufficient dosage and "a few years"), what shall be done if the resulting individual lives seem remarkably human? Moreover, Lederberg not only contemplates experiments "augmenting" animal cell cultures with "fragments of the human chromosome set," the reverse is also to be done: "Clonal reproduction and *introduction of genetic material from other spheres*" are two paths already opened up in *human evolution* (italics added). Lederberg "infers" these twin genetic policies instead of taking the other and somewhat longer road to genetic engineering. But surely these are paths no less fraught with mishaps knowingly if not intentionally created. We must face the grave moral question of what to do with them.

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## 2.

## Benefits to Individuals?

a.

Robert L. Sinsheimer  
The Prospect of Designed Genetic Change†

"It has now become a serious necessity to better the breed of the human race. The average citizen is too base for the everyday work of modern civilization. Civilized man has become possessed of vaster powers than in old times for good or ill but has made no corresponding advance in wits and goodness to enable him to direct his conduct rightly." This was written in

\* Kenneth Vaux, ed.: *Who Shall Live? Medicine, Technology, Ethics*. Philadelphia: Fortress Press 92-93 (1970). Copyright © 1970 by Fortress Press. Reprinted by permission.

† 32 *Engineering and Science* 8, 13 (April 1969). Reprinted by permission of the California Institute of Technology.

1894 by Sir Francis Galton. The concerns of the present are clearly not new.

It has long been apparent that you and I do not enter this world as unformed clay compliant to any mold. Rather, we have in our beginnings some bent of mind, some shade of character. The origin of this structure—of the fiber in this clay—was for centuries mysterious. In earlier times men sought its trace in the conjunction of the stars or perhaps in the momentary combination of the elements at nativity. Today, instead, we know to look within. We seek not in the stars but in our genes for the herald of our fate.

. . . For the first time in all time a living creature understands its origin and can undertake to design its future. . . . Even in the ancient myths man was constrained by his essence. He could not rise above his nature to chart his destiny. Today we can . . . envision that chance—and its dark companion of awesome choice and responsibility.

\* \* \*

It is worthwhile to consider specifically wherein the potential of the new genetics exceeds that of the old. To implement the older eugenics of Galton and his successors would have required a massive social program carried out over many generations. Such a program could not have been initiated without the consent and cooperation of a major fraction of the population, and would have been continuously subject to social control. In contrast, the new eugenics could, at least in principle, be implemented on a quite individual basis, in one generation, and subject to no existing social restrictions.

The old eugenics would have required a continual selection—for breeding—of the fit, and a culling out of the unfit. The new eugenics would permit in principle the conversion of all of the unfit to the highest genetic level.

The old eugenics was limited to a numerical enhancement of the best of our existing gene pool. The horizons of the new eugenics are in principle boundless—for we should have the potential to create new genes and new qualities yet undreamed. But of course the ethical dilemma remains. What are the best qualities, and who shall choose?

It is a new horizon in the history of man. Some may smile and may feel that this is but a new version of the old dream of the perfection of man. It is that, but it is something more. The old dreams of the cultural perfection of man were always sharply constrained by his inherent, inherited imperfections and limitations. Man is

all too clearly an imperfect and flawed creature. Considering his evolution, it is hardly likely that he could be otherwise. To foster his better traits and to curb his worse by cultural means alone has always been, while clearly not impossible, in many instances most difficult. It has been an Archimedian attempt to move the world, but with the short arm of a lever. We now glimpse another route—the chance to ease the internal strains and heal the internal flaws directly, to carry on and consciously perfect far beyond our present vision this remarkable product of two billion years of evolution.

I know there are those who find this concept and this prospect repugnant—who fear, with reason, that we may unleash forces beyond human scale and who recoil from this responsibility. I would suggest to them that they do not see our present situation whole. They are not among the losers in that chromosomal lottery that so firmly channels our human destinies. This response does not come from the 250,000 children born each year in this country with structural or functional defects, of which it is estimated 80% involve a genetic component. And this figure counts only those with gross evident defects outside those ranges we choose to call natural. It does not include the 50,000,000 “normal” Americans with an IQ of less than 90.

We who are among those who were favored in the chromosomal lottery and, in the nature of things, it will be our very conscious choice, whether as a species we will continue to accept the innumerable, individual tragedies inherent in the outcome of this mindless, age-old throw of dice, or instead will shoulder the responsibility for intelligent genetic intervention.

As we enlarge man's freedom, we diminish his constraints and that which he must accept as given. Equality of opportunity is a noble aim given the currently inescapable genetic diversity of man. But what does equality of opportunity mean to the child born with an IQ of 50?

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b.

**José M. R. Delgado**  
**Physical Control of the Mind—Toward a**  
**Psychocivilized Society\***

\* \* \*

The most alarming aspect of ESB [electrical stimulation of the brain] is that psychological

reactivity can be influenced by applying a few volts to a determined area of the brain. This fact has been interpreted by many people as a disturbing threat to human integrity. In the past, the individual could face risks and pressures with preservation of his own identity. His body could be tortured, his thoughts and desires could be challenged by bribes, by emotions, and by public opinion, and his behavior could be influenced by environmental circumstances, but he always had the privilege of deciding his own fate, of dying for an ideal without changing his mind. Fidelity to our emotional and intellectual past gives each of us a feeling of transcendental stability—and perhaps of immortality—which is more precious than life itself.

New neurological technology, however, has a refined efficiency. The individual is defenseless against direct manipulation of the brain because he is deprived of his most intimate mechanisms of biological reactivity. In experiments, electrical stimulation of appropriate intensity always prevailed over free will; and, for example, flexion of the hand evoked by stimulation of the motor cortex cannot be voluntarily avoided. Destruction of the frontal lobes produced changes in affectiveness which are beyond any personal control.

The possibility of scientific annihilation of personal identity, or even worse, its purposeful control, has sometimes been considered a future threat more awful than atomic holocaust. Even physicians have expressed doubts about the propriety of physical tampering with the psyche, maintaining that personal identity should be inviolable, that any attempt to modify individual behavior is unethical, and that methods—and related research—which can influence the human brain should be banned. The prospect of any degree of physical control of the mind provokes a variety of objections: theological objections because it affects free will, moral objections because it affects individual responsibility, ethical objections because it may block self-defense mechanisms, philosophical objections because it threatens personal identity.

These objections, however, are debatable. A prohibition of scientific advance is obviously naive and unrealistic. It could not be universally imposed, and, more important, it is not knowledge itself but its improper use which should be regulated. A knife is neither good nor bad; But it may be used by either a surgeon or an assassin. Science should be neutral, but scientists should take sides. . . .

. . . It is accepted medical practice to try and

\* New York: Harper & Row 213–217, 220, 238–241, 244 (1969). Copyright © 1969 by José M. R. Delgado. Reprinted by permission.

modify the antisocial or abnormal reactions of mental patients. Psychoanalysis, the use of drugs such as energizers and tranquilizers, the application of insulin or electroshock, and other varieties of psychiatric treatment are all aimed at influencing the abnormal personality of the patient in order to change his undesirable mental characteristics. The possible use, therefore, of implanted electrodes in mental patients should not pose unusual ethical complications if the accepted medical rules are followed. Perhaps the limited efficiency of standard psychiatric procedures is one reason that they have not caused alarm among scientists or laymen. Psychoanalysis requires a long time, and a person can easily withdraw his cooperation and refuse to express intimate thoughts. Electroshock is a crude method of doubtful efficacy in normal people. Although electrical stimulation of the brain is still in the initial stage of its development, it is in contrast far more selective and powerful; it may delay a heart beat, move a finger, bring a word to memory, or set a determined behavioral tone.

When medical indications are clear and the standard therapeutic procedures have failed, most patients and doctors are willing to test a new method, provided that the possibility of success outweighs the risk of worsening the situation. The crucial decision to start applying a new therapeutic method to human patients requires a combination of intelligent evaluation of data, knowledge of comparative neurophysiology, foresight, moral integrity, and courage. Excessive aggressiveness in a doctor may cause irreparable damage, but too much caution may deprive patients of needed help. The surgical procedure of lobotomy was perhaps applied to many mental patients too quickly, before its dangers and limitations were understood; but pallidectomy and thalamotomy in the treatment of Parkinson's disease encountered formidable initial opposition before attaining their present recognition and respected status.

While pharmacological and surgical treatment of sufferers of mental illness is accepted as proper, people with other behavioral deviations pose a different type of ethical problem. They may be potentially dangerous to themselves and to society when their mental functions are maintained within normal limits and only one aspect of their personal conduct is socially unacceptable. The rights of an individual to obtain appropriate treatment must be weighed with a professional evaluation of his behavioral problems—and their possible neurological basis—which necessitates a value judgment of the person's behavior in com-

parison with accepted norms. One example will illustrate these considerations.

In the early 1950s, a patient in a state mental hospital approached Dr. Hannibal Hamlin and me requesting help. She was an attractive 24-year-old woman of average intelligence and education who had a long record of arrests for disorderly conduct. She had been repeatedly involved in bar brawls in which she incited men to fight over her and had spent most of the preceding few years either in jail or in mental institutions. The patient expressed a strong desire as well as an inability to alter her conduct, and, because psychiatric treatment had failed, she and her mother urgently requested that some kind of brain surgery be performed in order to control her disreputable, impulsive behavior. They asked specifically that electrodes be implanted to orient possible electrocoagulation of a limited cerebral area; and if that wasn't possible, they wanted lobotomy.

Medical knowledge and experience at that time could not ascertain whether ESB or the application of cerebral lesions could help to solve this patient's problem, and surgical intervention was therefore rejected. When this decision was explained, both the patient and her mother reacted with similar anxious comments, asking, "What is the future? Only jail or the hospital? Is there no hope?" This case revealed the limitations of therapy and the dilemma of possible behavioral control. Supposing that long-term stimulation of a determined brain structure could influence the tendencies of a patient to drink, flirt, and induce fights; would it be ethical to change her personal characteristics? People *are* changing their character by self-medication through hallucinogenic drugs, but do they have the right to demand that doctors administer treatment that will radically alter their behavior? What are the limits of individual rights and doctors' obligations?

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When an individual's behavior is judged unfit by members of a society, the consequences may be forceful deprivation of liberty and incarceration. If the individual is confined to a mental hospital, his undesirable conduct may cause the authorities to administer drugs, by forceful injection if necessary, in order to change or control his behavioral responses. In the early 1950s an historical precedent was established for the deliberate destruction of part of the brain as a legal treatment for criminals. A man apprehended in Pittsburgh after committing a series of



robberies was given the alternative of a long jail sentence or submission to frontal lobe surgery which might curb or eliminate his future criminal behavior. Lobotomy was performed, and, although initially the patient appeared better adjusted socially, several months later he committed more thefts. When he realized that the police were closing in, he wrote a letter to the surgeon expressing appreciation for his efforts and regret that the operation had not been successful. Hoping that the study of his case might help others, he donated his brain to the surgeon and committed suicide by shooting himself through the heart. In spite of this therapeutic failure, the possibility of surgical rehabilitation of criminals has been considered by several scientists as more humane, more promising, and less damaging for the individual than his incarceration for life.

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[T]he desires and mechanisms for choice are determined mainly by early childhood experiences, cultural imprinting, and learned patterns of response. Newborn babies are completely dependent on parental care for the quality and quantity of sensory inputs as well as for food and warmth. The elements offered by the environment are almost infinite, but only a limited number are used to structure each individual. Their selection depends on chance, which among many variables includes the presence and behavior of parents and teachers. We must recognize that initially an individual has no control over the sensory inputs which mold his mind, and that during the decisive years of childhood, when each of us receives emotional impacts, behavioral formulas, and ideological frameworks, we are unable to search independently for alternatives. Our initial personality is structured in a rather automatic way when our capacity for intelligent choice has not developed.

If we accept that early experience is decisive for the establishment of personal identity, then we must accept that individual mental structure is not self-determined but hetero-determined by the interaction of genes received from our ancestors and information received from the environment and culture. Where, then, is the freedom to construct personal identity? To clarify these ideas, let us remember that liberal societies are based on the principle of individual self-determination, with the assumption that each human being is *born free* and has the *right* to de-

velop his own mind, to construct his own ideology, to shape his own behavior, and to express his personality without external pressures or indoctrination. The role of education, which involves both parents and schools, is to help these processes evolve with due respect for the individual. One of the main goals in these societies is "to find ourselves," and to develop our potential while remaining independent and self-sufficient. Privacy has a high priority in its intellectual, emotional, material, and territorial aspects, and personal freedom stops with interference in the rights of others.

This kind of liberal orientation has great appeal, but unfortunately its assumptions are not supported by neurophysiological and psychological studies of intercerebral mechanisms. The brain of the newborn lacks the stored information, neuronal circuits, and functional keyboards prerequisite to the formulation of choice. The infant may have the right to be free, but he has neither the option nor the biological mechanisms for free behavior. Confrontation with a multiplicity of choices may create confusion and anxiety in a child who does not yet possess the necessary mental sophistication to choose. This frustrating situation of inadequacy may have a traumatic and deforming effect rather than a constructive and positive one. The brain of an adult usually possesses the capacity to select a response but even it is not self-sufficient; for the brain needs constant environmental inputs in order to preserve mental normality, and a flow of information is necessary to make each judgment against the background of experience.

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The brain, *per se*, with all its genes, is not sufficient for the development of a mind in the absence of external information, and the content of this information is decisive in the establishment of mental structure. Even the withdrawal of parental care is a factor which can irrevocably shape future behavior of the young, as demonstrated by Harlow's neurotic motherless monkeys and by the emotional and mental handicap suffered by homeless children.

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Whatever we do or fail to do when we are in charge of a baby will influence his future mental structure. Our attitude, therefore, should not be to close our eyes and accept chance, but to investigate the extracerebral and intracerebral

elements which intervene in the formation of personality. To study what is going on inside of the brain is as important as to consider the other aspects of education and behavior.

These remarks are intended both to demonstrate that we should not base our interpersonal relations on false or unproved assumptions and to indicate the need to study these problems experimentally within the framework of intracerebral physiology. As our power to influence the mental structure of man continually increases, we face the question of the kind of people we would like to create. We must realize that parents and educators are imprinting and manipulating the minds and personalities of young people in any case, and that we are responsible for giving coherent form and ethical purpose to the psychogenetic elements transmitted to the child. The issue is whether violence and other behavioral patterns are inborn and inevitable or whether they are mainly related to a cultural learning which may be influenced by intelligent planning.

Ecological forces cannot be ignored or destroyed. Liberation from and domination of the environment became possible when we discovered the laws of nature and directed them with our intelligence. We cannot ignore the biological laws of the mind either. We should use our intelligence to direct our behavior, rather than accept its determination by unknown forces. Through education we should provide awareness of the elements, including intracerebral mechanisms, which intervene in the formation of personal identity, and we should teach the processes of decision-making and intelligent choice. Personal freedom is not a biological gift but a mental attribute which must be learned and cultivated. To be free is not to satisfy sexual drive, to fill an empty stomach, or to quarrel with our wives because of our repressed fears or mother-infant relations. Freedom requires the recognition of biological drives and their intelligent direction through processes of sublimation, substitution, postponement, or simply their satisfaction with civilized refinement and enjoyment. Individual freedom will increase when we understand the setting of our personality in early childhood and when we develop means for the intelligent evaluation and emotional interpretation of the information reaching us from the environment.

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The old temple inscription "Know Thyself" is often repeated today, but perhaps it is not ade-

quate. It should declare "Construct Thyself" as well. Shape your mind, train your thinking power, and direct your emotions more rationally; liberate your behavior from the ancestral burden of reptiles and monkeys—be a man and use your intelligence to orient the reactions of your mind.

\* \* \*

## NOTES

### NOTE 1.

FRANK J. AYD, JR.

FETOLOGY—MEDICAL AND ETHICAL  
IMPLICATIONS OF INTERVENTION IN THE  
PRENATAL PERIOD\*

\* \* \*

... Edwards and his associates at Cambridge University reported that they had fertilized human egg cells *in vitro*, after overcoming the problem of sperm incapacitation. They inseminated 56 eggs, but only seven were truly fertilized, five of which were abnormal. There are several possible explanations for the abnormalities, one of which is that the experimental techniques themselves damaged otherwise healthy human oocytes. Obviously, many difficulties must be surmounted, but the hope of growing a human embryo in a test tube has been stimulated, and more efforts to achieve this growth can be expected. The objective is not to create a test-tube baby, but to gain more understanding of infertility and sterility problems, of human reproduction and growth, and ultimately to implant laboratory-grown blastocysts into women who are infertile because they lack ovaries or because their fallopian tubes are blocked. The latter may be a laudable goal, but are the means ethically unobjectionable? At present, there is no possible way of sustaining the life and development of human embryos *in vitro*; hence, their death or deliberate destruction is inevitable. This being so, should not scientists, out of respect and reverence for human life, refrain from this form of human experimentation and seek to attain their objectives in other ways? Purposely to initiate human life while knowing that it cannot survive cannot be justified by extolling the knowledge acquired and its potentially useful applications. However desirable, a good end does not authorize the use of any means. We must ask not only if *in vitro* fertilization of human

\* 169 *Annals of the New York Academy of Sciences* 376-378 (1970). Reprinted by permission.

ova for these ends is immoral, but also if it is a warning of a dehumanizing trend in science.

For many valid reasons, scientists desire to know the sex of a human embryo as early as possible and to chart fetal maturation until birth. Not only are some doing cytological examinations of ova from women with somatic chromosome mosaicism, but some also are contemplating sexing the blastocyst by removing it before nidation in the uterus, excising a fragment of trophoblast and determining the sex-chromatin score of stained trophoblast cells. If the blastocyst is of the desired sex, it would be returned to the mother or to the uterus of a suitable foster parent, hopefully, to implant and grow. This has not been done in humans, but, if it becomes feasible, it would be used to control the sex of human babies, not merely to satisfy a parental whim, but to control or eradicate serious sex-linked diseases. If it should be demonstrated that a blastocyst has a sex-linked disease (for example, hemophilia), it would not be replaced in the womb. Edwards and Gardner justify this by asking: "Would not the non-replacement of a blastocyst be (socially and ethically) far more acceptable than a full-scale abortion of the implanted, thriving fetus—the only alternative today?" Implicit in this question is the conviction that there is no alternative to abortion, that the handicapped should be denied the right to life, and that physicians should have the authority to make and act on this judgment. This compels me to ask: What kind of medical scientist is he who prefers to eradicate suffering rather than face the responsibility of relieving it? What kind of a society would condone such conduct by medical scientists? . . .

\* \* \*

#### NOTE 2.

HAROLD M. SCHMECK, JR.  
VIRUS IS INJECTED INTO TWO CHILDREN IN  
EFFORT TO ALTER CHEMICAL TRAIT\*

Scientists here and abroad are watching with interest the progress of two little girls in West Germany who have been infected deliberately with a virus that causes harmless wart-like growths in rabbits.

The two girls, one about 2 years old and the

other about 7, were given the virus in the hope that it might correct a serious hereditary defect. The project is one attempt to alter intentionally an inborn chemical trait, a feat that has never been achieved or even been considered a practical possibility.

The feat seems theoretically possible because the virus is believed to carry a genetic blueprint for making an important enzyme that the two sisters lack. If the virus infection introduces these genetic instructions into the children's cells, the girls might begin to manufacture the missing enzyme.

The two girls were born with a hereditary biochemical defect that left them unable to make the enzyme arginase. The enzyme normally breaks down the chemical arginine from ordinary food. Lacking the enzyme, the girls accumulate large excesses of arginine in their blood. This has produced mental retardation and other serious problems in the older girl, and the younger is developing in the same fashion.

\* \* \*

Dr. Rogers [of Oakridge National Laboratory in Tennessee] said it was probably too late to do anything for the older girl. The main hope is that the progression of ill effects in the younger sister can be arrested.

The possibility arises because of the discovery years ago of a peculiarity of the Shope papilloma virus, the virus being given to the two girls. The discoverer of the virus, Dr. Richard Shope of the Rockefeller Institute for Medical Research, injected himself with it in the mid-1930's and suffered no ill effects then or later, but his blood levels of arginine did drop noticeably and stayed low for a long time.

During the mid-1960's Dr. Rogers and his co-workers published a report noting that about half the laboratory workers in laboratories studying the virus also had low arginine levels. It appeared that accidental infections with the virus had given the workers an excessive ability to make arginase.

\* \* \*

The prospect of using a virus to make an essentially genetic change of this sort has long been discussed as a theoretical possibility for the distant future. Two children whose inborn defect matches the possible corrective action of the virus may be providing an early test of that idea.

\* *The New York Times* 28, col. 3 (September 21, 1970). © 1970 by The New York Times Company. Reprinted by permission.

c.

**Louis Jolyon West**  
**Ethical Psychiatry and Biosocial Humanism\***

\* \* \*

A member of the APA was expelled because he publicly described and justified sexual intimacies with female patients as beneficial therapeutic procedures. But no complaint had been made by a patient in this case. What, therefore, was the written and published criterion for ethical behavior that this physician violated? Was it the Oath of Hippocrates? Other facets of that oath have recently fallen in the face of legal reforms, for example, concerning abortions. The former member in question could not really be accused of advertising, since his revelation was made in a professional journal. Even a challenge that he was administering a valueless procedure in the name of therapy could be argued, since clinicians from Hippocrates to Freud have noted the beneficial effects of sexual intercourse in certain cases.

Even if the Ethics Committee turned to the community for support in the form of laws forbidding extramarital sexual congress, it appears that such statutes may soon fall before the growing acceptance of the principle set forth in the Wolfenden Report that sexual acts between consenting adults in private are not a matter for concern under the criminal code. Nevertheless, I feel certain that the overwhelming majority of psychiatrists would agree that it is unethical to seduce patients and foolish (if not outrageous) to call it treatment.

## 3.

## Pursuit of Knowledge

a.

**Robert Reinhold**  
**Psychologist Arouses Storm by Linking**  
**I.Q. to Heredity†**

A storm is brewing over a suggestion by a leading educational psychologist that intelligence is determined largely by heredity and cannot be altered significantly by improving environment.

\* 126 *The American Journal of Psychiatry* 226, 228-229 (1969). Copyright 1969, the American Psychiatric Association. Reprinted by permission.

† *The New York Times* 52, col. 4 (March 30, 1969). © 1969 by The New York Times Company. Reprinted by permission.

For this reason, argues the psychologist, Dr. Arthur R. Jensen of the University of California at Berkeley, compensatory education programs designed to raise the intelligence of disadvantaged children by enriching their cultural surroundings are misdirected.

Further, he theorizes, the measured mental differences between racial and ethnic groups are rooted in inborn genetic differences that are as much a part of group identity as skin color, hair texture and blood chemistry.

Such hereditary factors, he believes, may account for the fact that Negroes average 15 points below whites on I.Q. tests. Recent evidence, he adds, indicates that children from Negro and other disadvantaged groups do poorly in "cognitive" learning—the ability to reason abstractly—while they do well in "associative" learning, which involves rote learning and memory.

If this theory can be substantiated, says Dr. Jensen, then "the next step will be to develop the techniques by which school learning can be most effectively achieved in accordance with different patterns of ability."

This is the chief conclusion of a controversial 123-page study by Dr. Jensen in the current issues of *The Harvard Educational Review*, a student-run publication of the Harvard Graduate School of Education.

Dr. Jensen's theories are not new, but the force with which he presents them has rekindled issues that have long divided geneticists, psychologists and educators. . . .

\* \* \*

Predictably, the Jensen article has prompted heated reaction from those who say it lends support to racist claims that Negroes are inferior to whites intellectually.

\* \* \*

In addition, a number of geneticists and psychologists believe Dr. Jensen may have overinterpreted the limited scientific evidence available—particularly on the question of racial differences.

"From the standpoint of methodology, this is as difficult an issue as geneticists confront today," said a leading human geneticist. "It is conceivable there are significant mental differences between blacks and whites—but we simply do not have the information to reach a valid conclusion. Negroes have been subjected to so many

subtle kinds of discrimination that we cannot compare the two groups."

However, Dr. Jensen says his study was intended as an objective scientific analysis of an area that he feels has long been taboo.

\* \* \*

In an interview, Dr. Jensen indicated that he would not recommend widespread use of his ideas without further research. "I'm trying to stimulate more research," he said. "There should not be any fear in finding biological differences between groups."

Reaction to the article has been swift. . . .

Another leading geneticist, Prof. James F. Crow of the University of Wisconsin, said he agreed "for the most part" with Dr. Jensen's analysis of the high heritability of I.Q. but disagreed on the interpretation.

"No matter how high the heritability," Dr. Crow said "there is no assurance that a sufficiently great environmental difference does not account for the differences in the two means [between Negro and white I.Q.'s], especially when one considers that the environmental factors may differ qualitatively in the two groups."

\* \* \*

## NOTES

### NOTE 1.

LAWRENCE E. DAVIES  
HARRASSMENT CHARGED BY AUTHOR OF  
ARTICLE ABOUT NEGROES' I.Q.'s\*

An educational psychologist who describes himself as a liberal and a strong civil rights advocate has been the subject in the last month of what he calls a campaign of harrassment depicting him as bent on promoting racism.

Repeated calls for the dismissal of Dr. Arthur L. Jensen from the faculty of the University of California, Berkeley, have been sounded since an article by him was published in the winter issue of the *Harvard Educational Review*.

\* \* \*

The militant Students for a Democratic Society began in mid-April to attack the 45-year-old educator, a Berkeley faculty member for 11 years. A sound truck operating in the vicinity of the campus demanded "Fight racism, fire Jensen," and circulars described him as "Berkeley's white supremacist crusader."

\* *The New York Times* 33, col. 1 (May 19, 1969). © 1969 by The New York Times Company. Reprinted by permission.

\* \* \*

In the last three weeks hardly an issue of *The Daily Californian*, the campus newspaper, has been published without one to three letters from students and faculty members assailing or defending Dr. Jensen. News articles and editorials have also kept the issue alive.

Dr. John Searle, professor of philosophy and chairman of the Berkeley academic freedom committee, said that the committee members had discussed the Jensen case.

"Any attempt to fire him would be a violation of academic freedom," Professor Searle told an inquirer, "but no such attempt has been made. . . ."

\* \* \*

### NOTE 2.

COUNCIL OF THE SOCIETY FOR THE  
PSYCHOLOGICAL STUDY OF SOCIAL ISSUES  
STATEMENT\*

As behavioral scientists, we believe that statements specifying the hereditary components of intelligence are unwarranted by the present state of scientific knowledge. [W]e believe that such statements may be seriously misinterpreted, particularly in their applications to social policy.

The evidence of four decades of research on this problem can be readily summarized. There are marked differences in intelligence test scores when one compares a random sample of whites and Negroes. What is equally clear is that little definitive evidence exists that leads to the conclusion that such differences are innate. The evidence points overwhelmingly to the fact that when one compares Negroes and whites of comparable cultural and educational background, differences in intelligence test scores diminish markedly; the more comparable the background, the less the difference. There is no direct evidence that supports the view that there is an innate difference between members of different racial groups.

[A] more accurate understanding of the contribution of heredity to intelligence will be possible only when social conditions for all races are equal and when this situation has existed for several generations. . . . Social inequalities deprive large numbers of black people of social, economic, and educational advantages available to a great majority of the white population. The exist-

\* 39 *Harvard Educational Review* 625-627 (1969). Reprinted by permission.

ing social structures prevent black and white people even of the same social class from leading comparable lives. In light of these conditions, it is obvious that no scientific discussion of racial differences can exclude an examination of political, historic, economic, and psychological factors which are inextricably related to racial differences.

\* \* \*

[A] number of Jensen's key assumptions and conclusions are seriously questioned by many psychologists and geneticists.

The question of the relative contributions of heredity and environment to human development and behavior has a long history of controversy within psychology. Recent research indicates that environmental factors play a role from the moment of the child's conception. The unborn child develops as a result of a complex, little understood, interaction between hereditary and environmental factors; this interaction continues throughout life. To construct questions about complex behavior in terms of heredity *versus* environment is to oversimplify the essence and nature of human development and behavior.

\* \* \*

The Council . . . reaffirms its long-held position of support for open inquiry on all aspects of human behavior. We are concerned with establishing high standards of scientific inquiry and of scientific responsibility. Included in these standards must be careful interpretation of research findings, with rigorous attention to alternative explanations. In no area of science are these principles more important than in the study of human behavior, where a variety of social factors may have large and far-reaching effects. When research has bearing on social issues and public policy, the scientist must examine the competing explanations for his findings and must exercise the greatest care in his interpretation. Only in this way can he minimize the possibility that others will overgeneralize or misunderstand the social implications of his work.

#### NOTE 3.

##### DARTMOUTH BLACKS BAR PHYSICIST'S TALK\*

Dr. William Shockley, a Nobel prize winner in physics, was prevented from delivering a

paper on genetic racial differences today when about 30 Negro students applauded continuously and would not stop.

Dr. Shockley was to have delivered the paper at the fall meeting of the National Academy of Sciences at Dartmouth College.

Dr. Shockley, a professor of engineering science at Stanford University, had provoked controversy in the last year with his views that inheritance, rather than environment, may be the major factor in intelligence.

\* \* \*

There had been some speculation that Dr. Shockley would not be well received on the campus. A letter from the National Academy of Sciences was read before Dr. Shockley's introduction. It read in part: "As a member of the National Academy of Sciences, Dr. William Shockley has the privilege of addressing the academy on matters he believes deserving of its interest. For several years he has used this opportunity to attempt to persuade the academy to sponsor or encourage research to determine the relative importance of genetic inheritance as a factor in human intelligence, separate and apart from environmental factors."

"However," the letter continued, "the academy considers that the field is already being actively pursued. Whereas the academy encourages work in this, as in all other legitimate fields of inquiry, it regards all studies in this area with great reservation. Since Dr. Shockley has offered no new research program and no new approach . . . the N.A.S. does not endorse his recommendations."

#### NOTE 4.

##### LUTHER J. CARTER NAS AGAIN SAYS NO TO SHOCKLEY\*

For more than 4 years now, William Shockley, a Stanford physicist who shared a Nobel Prize for his part in inventing the transistor, has been carrying on a dogged campaign to have the National Academy of Sciences encourage research in "dysgenics." As he defines it, dysgenics has to do with the "retrogressive evolution" of a population through the reproduction, in disproportionately large numbers, of its genetically inferior elements. Specifically, Shockley is afraid that the U.S. population is declining

\* *The New York Times* 37, col. 2 (Oct. 16, 1969). © 1969 by The New York Times Company. Reprinted by permission.

\* 168 *Science* 685 (1970). Copyright 1970 by the American Association for the Advancement of Science. Reprinted by permission.

in quality through the reproduction of large numbers of Negroes of low I.Q., a view which he says can in no sense be ascribed to a "racist" motivation. Last week, the academy rebuffed Shockley's latest attempt to have it go on record as favoring dysgenics research. His proposed resolution to that effect was not seconded. He found some satisfaction, however, in the as yet unreleased report of an academy committee.

This committee was appointed by Philip Handler, president of the academy, after an academy meeting last October at which Shockley had again raised the dysgenics research issue. Kingsley Davis, a sociologist at Berkeley, was named chairman. According to Shockley, the report of the Davis committee, which the academy received but took no action on, acknowledges that study of racial and hereditary differences is "proper and socially relevant."

"The report indicates that members of the committee 'variously' regarded the impact of suppressive attitudes on research in this area," Shockley told *Science*. "The difference in viewpoints on research taboos about human quality problems are enormous in my opinion. I think the word 'variously' does not portray this."

Shockley added, however, that "my general reaction is that this report represents enormous progress over the one issued in 1967." Here Shockley was alluding to a 1967 report of the academy which concluded in part by questioning "the social urgency of a crash program to measure genetic differences in intellectual and emotional traits between racial groups." "In the first place, if the traits are at all complex [as the report had said they would surely be], the results of such research are almost certain to be inconclusive," this report said. "In the second place, it is not clear that major social decisions depend on such information; we would hope that persons would be considered as individuals and not as members of groups."

\* \* \*

#### NOTE 5.

SCOTT THACHER  
SHOCKLEY STICKS BY RACE THEORIES\*

Professor William B. Shockley proposes scientific research to determine whether the genetic potential for intelligence of black people differs from whites. Because of his insistence on spreading this view, he is probably one of the most dis-

liked and controversial figures on campus, a person many prefer to ignore.

\* \* \*

Joshua Lederberg, chairman of the Department of Genetics, and a Nobel Prize winner claims Shockley has already made his own conclusions about black-white differences. "He has chosen to ventilate his own inflammatory prejudgement of the results of research thinly disguised as a 'question.'"

However, the crux of Shockley's disagreement with many other scientists lies in the emphasis he places on heredity, as opposed to environment, in determining the intelligence and motivation of an individual.

On the nature-nurture argument, he says, "there is one piece (of evidence) that it's irresponsible not to accept." He states it as follows: "Heredity is more than twice as important as environment in determining intelligence as measured by IQ tests in families that adopt one of a pair of white identical twins."

He does not extrapolate this to the difference in average intelligence between races. The distribution of black IQ's is offset downwards by about 15 points from the IQ's of whites. However, Shockley's opinion is that less than half of this difference is accounted for by a different environment.

It is a nonfact that simply "blames the Negro IQ deficit on cultural disadvantages," says Shockley. "There is a whole pattern of relationships which appear to be explainable far more simply on the basis of racial genetic differences than in any other way."

He cites evidence which shows that blacks perform better on tests of verbal skills than they do on reasoning, numerical, or spatial ones, compared to the relative levels for whites. In addition, he observes that socially valuable character traits may be correlated with IQ. He says this correlation "can be four times greater for whites than for Negroes."

Shockley submits that these things may be evidence for racial genetic differences that are not greatly affected by environmental ones. He has proposed to study this further by comparing the racial compositions and average IQ's of different black populations living in uniform slum environments.

This could be done by measuring the frequency of a certain blood type that is very rare among the Africans from which the blacks came, but found in 43 per cent of all Caucasians. In a

\* *Stanford Daily* 4, col. 1 (Dec. 2, 1970). Reprinted by permission.



letter to *Scientific American* he writes that "my own preliminary research suggests that an increase of 1 per cent in Caucasian ancestry raises Negro IQ about one point on the average for low IQ populations."

\* \* \*

Shockley's ideas are contested by academicians here in two main areas. First, they challenge his view that genetic factors pretty well limit intelligence to the level we observe. This idea implies that the difference in black and white IQ distributions cannot be eliminated solely by educational means.

\* \* \*

The other area of disagreement is over the possibility of a genetic basis for racial IQ differences. A member of the Department of Genetics, suggested in *Scientific American* that this question "will be almost impossible to answer satisfactorily before the environmental differences between U.S. blacks and whites have been substantially reduced."

Dr. John L. Black, director of the Counseling and Testing Service, agrees with both views. While the study of racial differences in intelligence "might be interesting research, this has little practical significance."

In addition, he is of the opinion that the difference would be far more difficult to find than Shockley thinks. "What little talents we have might better be put more directly to the solution of educational problems."

\* \* \*

NOTE 6.

SCOTT THACHER  
SELF-ESTEEM PLAYS PART IN SHOCKLEY'S  
RESEARCH\*

Professor William Shockley is quite candid about why he pursues the topic of worsening heredity among black people. "It's what I will think of myself. . . . It's very self-oriented."

The decline of genetic potential among all races, "might well be the most important single factor in the future of the nation, maybe even the future of the world. No one else of equal eminence is tackling this one."

He feels pride is not unnatural when one undertakes a difficult project. "In the end you will be very concerned with your self-esteem

and this seems to be the nature of man or thinking man."

This is one of the conclusions that Shockley the philosopher draws from his three humanistic postulates. The first postulate is that a humane civilization depends on "man's concern for the emotions experienced by his fellow creatures." The second is that "The truth shall make you free."

He states the third, "Terminal Self-Esteem," as follows: "During the last rational five minutes of my life, I hope to consider that since 1967 I have used my capacities close to their maximum potential, in keeping with the objectives of Nobel's Will, of conferring the greatest benefit on mankind."

\* \* \*

"His whole thrust is for rational criteria," says [Professor William] Spicer, who has known him for several years. "His whole conflict comes from people who use arguments he feels don't have a rational basis." When dealing with social questions, "He has more faith than I do that you can isolate all the variables."

A student who worked for Shockley last summer concurs: "The simplicity of his models is very nice to use." But he is unsure "whether you can really compute human intelligence with one or two variables. . . . His (Shockley's) limitation to the direct and physical sciences is his greatest danger."

Other men, such as Joshua Lederberg, feel that Shockley's advent into the fields of sociology and genetics has been something less than professional. Lederberg calls Shockley's recent pronouncements "an abuse of his privilege as a scientist."

\* \* \*

NOTE 7.

JOHN WALSH  
NATIONAL ACADEMY OF SCIENCES—  
AWKWARD MOMENTS AT THE MEETING\*

\* \* \*

The Academy grappled again with the controversial subject of "behavioral genetics" and voted by a decisive margin not to encourage expanded federal research on the effects on intelligence of genetic differences. At the same time

\* *Stanford Daily* 3, col. 3 (Dec. 3, 1970). Reprinted by permission.

\* 172 *Science* 539-541 (1971). Copyright 1971 by the American Association for the Advancement of Science. Reprinted by permission.

that members turned down a committee recommendation that the Academy form an internal working group to study the feasibility of long-term research on the subject. The Academy members did, however, approve a recommendation favoring cooperation to put research in behavioral genetics in a broader scientific context.

\* \* \*

Academy action on the behavioral genetics issue came in response to persistent efforts by William Shockley, co-developer of the transistor, to persuade the Academy to back research to establish the scientific basis of what he has called the "racial genetic intellectual disadvantages of the nation's black minority". . . .

A Shockley proposal for Academy action in 1969 led to formation of a blue-ribbon Academy study group headed by Kingsley Davis of the University of California at Berkeley. The report of the Davis committee was circulated to the Academy membership in advance of this year's meeting and last Wednesday was accepted by the members after the second and third of three recommendations had been rejected.

\* \* \*

The first recommendation,† which the membership approved is, according to [Academy president Philip] Handler, addressed "to the academic world in a general way." Despite its vagueness, the recommendation appears to go somewhat further than previous Academy statements on the issue, and Shockley has been quoted as saying he feels the action indicated the Academy has "faced down the road" toward further action.

In the body of its brief report, the committee stresses that "the genetic aspects of behavioral characteristics in men are very inadequately known." And Handler in commenting on the report said "the important aspects of human behavioral genetics have to do with individual differences rather than differences between groups." Knowledgeable observers say the Academy faced the dilemma on the behavioral genet-

† We recommend exploration of means for closer cooperation among those concerned with research and training in psychology, education, behavioral genetics, and neurobiology. Such cooperation will be especially valuable to the extent that it contributes to broader training and the extension of competent research that combines the insights and techniques of behavioral genetics with those of other fields. [*Ibid* at 540.]

ics issue of appearing to limit scientific inquiry on the one hand or of backing research on the other and sought to steer between the two shoals.

\* \* \*

b.

### **President's Commission on Obscenity and Pornography Report and Model Statute\***

\* \* \*

*Section 2. Sale and display of explicit sexual material to young persons.* (a) Purpose. It is the purpose of this section to regulate the direct commercial distribution of certain explicit sexual materials to young persons in order to aid parents in supervising and controlling the access of children to such material. The legislature finds that whatever social value such material may have for young persons can adequately be served by its availability to young persons through their parents.

(b) Offenses Defined. A person is guilty of a misdemeanor if he

(i) knowingly disseminates explicit sexual material . . . to young persons. . . .

\* \* \*

(e) Defenses. It shall be an affirmative defense to a prosecution under this section for the defendant to show:

(i) That the dissemination was made with the consent of a parent or guardian of the recipient. . . .

\* \* \*

A primary basis for the Commission's recommendation for repeal of adult legislation is the fact that extensive empirical investigations do not indicate any causal relationship between exposure to or use of explicit sexual materials and such social or individual harms such as crime, delinquency, sexual or nonsexual deviancy, or severe emotional disturbances. The absence of empirical evidence supporting such a causal relationship also applies to the exposure of children to erotic materials. However, insufficient research is presently available on the effect of the exposure of children to sexually explicit materials to enable us to reach conclusions with the same degree of confidence as for adult exposure. Strong ethical feelings against experimentally exposing children to sexually explicit materials considerably reduced the possibility of gathering the necessary data and information regarding young persons.

\* Washington, D.C.: U.S. Government Printing Office 56-57, 66-67 (1970).

In view of the limited amount of information concerning the effects of sexually explicit materials on children, other considerations have assumed primary importance in the Commission's deliberations. The Commission has been influenced, to a considerable degree, by its finding that a large majority of Americans believe that children should not be exposed to certain sexual materials. In addition, the Commission takes the view that parents should be free to make their own conclusions regarding the suitability of explicit sexual materials for their children and that it is appropriate for legislation to aid parents in controlling the access of their children to such materials during their formative years. The Commission recognizes that legislation cannot possibly isolate children from such materials entirely; it also recognizes that exposure of children to sexual materials may not only do no harm but may, in certain instances, actually facilitate much needed communication between parent and child over sexual matters. The Commission is aware, as well, of the considerable danger of creating an unnatural attraction or an enhanced interest in certain materials by making them "forbidden fruit" for young persons. The Commission believes, however, that these considerations can and should be weighed by individual parents in determining their attitudes toward the exposure of their children to sexual materials, and that legislation should aid, rather than undermine, such parental choice.

\* \* \*

#### 4.

#### Values of Investigator?

a.

#### Carl R. Rogers and B. F. Skinner Some Issues Concerning the Control of Human Behavior\*

I. SKINNER

\* \* \*

[T]he control of human behavior has always been unpopular. Any undisguised effort to control usually arouses emotional reactions. We hesitate to admit, even to ourselves, that we are engaged in control, and we may refuse to

control, even when this would be helpful, for fear of criticism. . . . Intelligent men and women, dominated by the humanistic philosophy of the past two centuries, cannot view with equanimity what Andrew Hacker has called "the specter of predictable man." Even the statistical or actuarial prediction of human events, such as the number of fatalities to be expected on a holiday weekend, strikes many people as uncanny and evil, while the prediction and control of individual behavior is regarded as little less than the work of the devil. I am not so much concerned here with the political or economic consequences for psychology, although research following certain channels may well suffer harmful effects. We ourselves, as intelligent men and women, and as exponents of Western thought, share these attitudes. They have already interfered with the free exercise of a scientific analysis, and their influence threatens to assume more serious proportions.

Three broad areas of human behavior supply good examples. The first of these—*personal control*—may be taken to include person-to-person relationships in the family, among friends, in social and work groups, and in counseling and psychotherapy. Other fields are *education* and *government*. A few examples . . . will show how nonscientific preconceptions are affecting our current thinking about human behavior.

People living together in groups come to control one another with a technique which is not inappropriately called "ethical." When an individual behaves in a fashion acceptable to the group, he receives admiration, approval, affection, and many other reinforcements which increase the likelihood that he will continue to behave in that fashion. When his behavior is not acceptable, he is criticized, censured, blamed, or otherwise punished. In the first case the group calls him "good"; in the second, "bad." This practice is so thoroughly ingrained in our culture that we often fail to see that it is a technique of control. Yet we are almost always engaged in such control, even though the reinforcements and punishments are often subtle.

\* \* \*

[C]oncepts of choice, responsibility, justice, and so on, provide a most inadequate analysis of efficient reinforcing and punishing contingencies because they carry a heavy semantic cargo of a quite different sort, which obscures any attempt to clarify controlling practices or to improve

\* 124 *Science* 1057-1065 (1956). Reprinted by permission.

techniques. In particular, they fail to prepare us for techniques based on other than aversive techniques of control. Most people would object to forcing prisoners to serve as subjects of dangerous medical experiments, but few object when they are induced to serve by the offer of return privileges—even when the reinforcing effect of these privileges has been created by forcible deprivation. In the traditional scheme the right to refuse guarantees the individual against coercion or an unfair bargain. But to what extent *can* a prisoner refuse under such circumstances?

We need not go so far afield to make the point. We can observe our own attitude toward personal freedom in the way we resent any interference with what we want to do. Suppose we want to buy a car of a particular sort. Then we may object, for example, if our wife urges us to buy a less expensive model and to put the difference into a new refrigerator. Or we may resent it if our neighbor questions our need for such a car or our ability to pay for it. We would certainly resent it if it were illegal to buy such a car (remember Prohibition); and if we find we cannot actually afford it, we may resent governmental control of the price through tariffs and taxes. We resent it if we discover that we cannot get the car because the manufacturer is holding the model in deliberately short supply in order to push a model we do not want. In all this we assert our democratic right to buy the car of our choice. We are well prepared to do so and to resent any restriction on our freedom.

But why do we not ask *why* it is the car of our choice and resent the forces which made it so? Perhaps our favorite toy as a child was a car, of a very different model, but nevertheless bearing the name of the car we now want. Perhaps our favorite TV program is sponsored by the manufacturer of that car. Perhaps we have seen pictures of many beautiful or prestigious persons driving it—in pleasant or glamorous places. Perhaps the car has been designed with respect to our motivational patterns: the device on the hood is a phallic symbol; or the horsepower has been stepped up to please our competitive spirit in enabling us to pass other cars swiftly (or, as the advertisements say, "safely"). The concept of freedom that has emerged as part of the cultural practice of our group makes little or no provision for recognizing or dealing with these kinds of control. Concepts like "responsibility" and "rights" are scarcely applicable. We are prepared to deal with coercive measures, but we have no traditional recourse with respect to other

measures which in the long run (and especially with the help of science) may be much more powerful and dangerous.

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Government has always been the special field of aversive control. The state is frequently defined in terms of the power to punish, and jurisprudence leans heavily upon the associated notion of personal responsibility. Yet it is becoming increasingly difficult to reconcile current practice and theory with these earlier views. In criminology, for example, there is a strong tendency to drop the notion of responsibility in favor of some such alternative as capacity or controllability. But no matter how strongly the facts, or even practical expedience, support such a change, it is difficult to make the change in a legal system designed on a different plan. When governments resort to other techniques (for example, positive reinforcement), the concept of responsibility is no longer relevant and the theory of government is no longer applicable.

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The uneasiness with which we view government (in the broadest possible sense) when it does not use punishment is shown by the reception of my utopian novel, *Walden Two*. This was essentially a proposal to apply a behavioral technology to the construction of a workable, effective, and productive pattern of government. It was greeted with wrathful violence. *Life* magazine called it "a travesty on the good life," and "a menace . . . a triumph of mortmain or the dead hand not envisaged since the days of Sparta . . . a slur upon a name, a corruption of an impulse." Joseph Wood Krutch devoted a substantial part of his book, *The Measure of Man*, to attacking my views and those of the protagonist, Frazier, in the same vein, and Morris Viteles has recently criticized the book in a similar manner in *Science*. Perhaps the reaction is best expressed in a quotation from *The Quest for Utopia* by Negley and Patrick:

"Halfway through this contemporary utopia, the reader may feel sure, as we did, that this is a beautifully ironic satire on what has been called 'behavioral engineering.' The longer one stays in this better world of the psychologist, however, the plainer it becomes that the inspiration is not satiric, but messianic. This is indeed the behaviorally engineered society, and while it was to be expected that sooner or later the principle of psychological conditioning would be

made the basis of a serious construction of utopia—Brown anticipated it in *Limanora*—yet not even the effective satire of Huxley is adequate preparation for the shocking horror of the idea when positively presented. Of all the dictatorships espoused by utopists, this is the most profound, and incipient dictators might well find in this utopia a guidebook of political practice."

One would scarcely guess that the authors are talking about a world in which there is food, clothing, and shelter for all, where everyone chooses his own work and works on the average only 4 hours a day, where music and the arts flourish, where personal relationships develop under the most favorable circumstances, where education prepares every child for the social and intellectual life which lies before him, where—in short—people are truly happy, secure, productive, creative, and forward-looking. What is wrong with it? Only one thing: someone "planned it that way." If these critics had come upon a society in some remote corner of the world which boasted similar advantages, they would undoubtedly have hailed it as providing a pattern we all might well follow—provided that it was clearly the result of a natural process of cultural evolution. Any evidence that intelligence had been used in arriving at this version of the good life would, in their eyes, be a serious flaw. . . .

The dangers inherent in the control of human behavior are very real. The possibility of the misuse of scientific knowledge must always be faced. We cannot escape by denying the power of a science of behavior or arresting its development. It is no help to cling to familiar philosophies of human behavior simply because they are more reassuring.

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If the advent of a powerful science of behavior causes trouble, it will not be because science itself is inimical to human welfare but because older conceptions have not yielded easily or gracefully. We expect resistance to new techniques of control from those who have heavy investments in the old, but we have no reason to help them preserve a series of principles that are not ends in themselves but rather outmoded means to an end. What is needed is a new conception of human behavior which is compatible with the implications of a scientific analysis. All men control and are controlled. The question of government in the broadest possible sense is not how freedom is to be preserved but what kinds

of control are to be used and to what ends. Control must be analyzed and considered in its proper proportions. No one, I am sure, wishes to develop new master-slave relationships or bend the will of the people to despotic rulers in new ways. These are patterns of control appropriate to a world without science. They may well be the first to go when the experimental analysis of behavior comes into its own in the design of cultural practices.

## II. ROGERS

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I am sure we agree that men—as individuals and as societies—have always endeavored to understand, predict, influence, and control human behavior—their own behavior and that of others.

I believe we agree that the behavioral sciences are making and will continue to make increasingly rapid progress in the understanding of behavior, and that as a consequence the capacity to predict and to control behavior is developing with equal rapidity.

I believe we agree that to deny these advances, or to claim that man's behavior cannot be a field of science, is unrealistic. . . .

I believe we are in agreement that the tremendous potential power of a science which permits the prediction and control of behavior may be misused, and that the possibility of such misuse constitutes a serious threat.

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With these several points of basic and important agreement, are there then any issues that remain on which there are differences? I believe there are. They can be stated very briefly: Who will be controlled? Who will exercise control? What type of control will be exercised? Most important of all, toward what end or what purpose, or in the pursuit of what value, will control be exercised?

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[L]et us review very briefly the various elements that are involved in the usual concept of the control of human behavior as mediated by the behavioral sciences. . . .

1) There must first be some sort of decision about goals. Usually desirable goals are assumed, but sometimes, as in George Orwell's book *1984*, the goal that is selected is an aggrandizement of individual power with which most

of us would disagree. In a recent paper Skinner suggests that one possible set of goals to be assigned to the behavioral technology is this: "Let men be happy, informed, skillful, well-behaved and productive." In the first draft of his part of this article, which he was kind enough to show me, he did not mention such definite goals as these, but desired "improved" educational practices, "wiser" use of knowledge in government, and the like. In the final version of his article he avoids even these value-laden terms, and his implicit goal is the very general one that scientific control of behavior is desirable, because it would perhaps bring "a far better world for everyone."

Thus the first step in thinking about the control of human behavior is the choice of goals, whether specific or general. It is necessary to come to terms in some way with the issue, "For what purpose?"

2) A second element is that, whether the end selected is highly specific or is a very general one such as wanting "a better world," we proceed by the methods of science to discover the means to these ends. We continue through further experimentation and investigation to discover more effective means. The method of science is self-correcting in thus arriving at increasingly effective ways of achieving the purpose we have in mind.

3) The third aspect of such control is that as the conditions or methods are discovered by which to reach the goal, some person or some group establishes these conditions and uses these methods, having in one way or another obtained the power to do so.

4) The fourth element is the exposure of individuals to the prescribed conditions, and this leads, with a high degree of probability, to behavior which is in line with the goals desired. Individuals are now happy, if that has been the goal, or well-behaved, or submissive, or whatever it has been decided to make them.

5) The fifth element is that if the process I have described is put in motion then there is a continuing social organization which will continue to produce the types of behavior that have been valued.

Are there any flaws in this way of viewing the control of human behavior? I believe there are. In fact the only element in this description with which I find myself in agreement is the second. It seems to me quite incontrovertibly true that the scientific method is an excellent way to discover the means by which to achieve our

goals. Beyond that, I feel many sharp differences, which I will try to spell out.

I believe that in Skinner's presentation here and in his previous writings, there is a serious underestimation of the problem of power. To hope that the power which is being made available by the behavioral sciences will be exercised by the scientists, or by a benevolent group, seems to me a hope little supported by either recent or distant history. It seems far more likely that behavioral scientists, holding their present attitudes, will be in the position of the German rocket scientists specializing in guided missiles. First they worked devotedly for Hitler to destroy the U.S.S.R. and the United States. Now, depending on who captured them, they work devotedly for the U.S.S.R. in the interest of destroying the United States, or devotedly for the United States in the interest of destroying the U.S.S.R. If behavioral scientists are concerned solely with advancing their science, it seems most probable that they will serve the purposes of whatever individual or group has the power.

But the major flaw I see in this review of what is involved in the scientific control of human behavior is the denial, misunderstanding, or gross underestimation of the place of ends, goals, or values in their relationship to science. . . .

In sharp contradiction to some views that have been advanced, I would like to propose a two-pronged thesis: (i) In any scientific endeavor—whether "pure" or applied science—there is a prior subjective choice of the purpose or value which that scientific work is perceived as serving. (ii) This subjective value choice which brings the scientific endeavor into being must always lie outside of that endeavor and can never become a part of the science involved in that endeavor.

Let me illustrate the first point from Skinner himself. It is clear that in his earlier writing it is recognized that a prior value choice is necessary, and it is specified as the goal that men are to become happy, well-behaved, productive, and so on. I am pleased that Skinner has retreated from the goals he then chose, because to me they seem to be stultifying values. I can only feel that he was choosing these goals for others, not for himself. I would hate to see Skinner become "well-behaved," as that term would be defined for him by behavioral scientists. His recent article in the *American Psychologist* shows that he certainly does not want to be "productive" as that value is defined by most psychologists. And the most awful fate I can

imagine for him would be to have him constantly "happy." It is the fact that he is very unhappy about many things which makes me prize him.

In the first draft of his part of this article, he also included such prior value choices, saying for example, "We must decide how we are to use the knowledge which a science of human behavior is now making available." Now he has dropped all mention of such choices, and if I understand him correctly, he believes that science can proceed without them. He has suggested this view in another recent paper, stating that "We must continue to experiment in cultural design . . . testing the consequences as we go. Eventually the practices which make for the greatest biological and psychological strength of the group will presumably survive."

I would point out, however, that to choose to experiment is a value choice. Even to move in the direction of perfectly random experimentation is a value choice. To test the consequences of an experiment is possible only if we have first made a subjective choice of a criterion value. And implicit in his statement is a valuing of biological and psychological strength. So even when trying to avoid such choice, it seems inescapable that a prior subjective value choice is necessary for any scientific endeavor, or for any application of scientific knowledge.

I wish to make it clear that I am not saying that values cannot be included as a subject of science. It is not true that science deals only with certain classes of "facts" and that these classes do not include values. It is a bit more complex than that, as a simple illustration or two may make clear.

If I value knowledge of the "three R's" as a goal of education, the methods of science can give me increasingly accurate information on how this goal may be achieved. If I value problem-solving ability as a goal of education, the scientific method can give me the same kind of help.

Now, if I wish to determine whether problem-solving ability is "better" than knowledge of the three R's, then scientific method can also study those two values but *only*—and this is very important—in terms of some other value which I have subjectively chosen. I may value college success. Then I can determine whether problem-solving ability or knowledge of the three R's is most closely associated with that value. I may value personal integration or vocational success or responsible citizenship. I can determine

whether problem-solving ability or knowledge of the three R's is "better" for achieving any one of these values. But the value or purpose that gives meaning to a particular scientific endeavor must always lie outside of that endeavor.

\* \* \*

My point then is that any endeavor in science, pure or applied, is carried on in the pursuit of a purpose or value that is subjectively chosen by persons. It is important that this choice be made explicit, since the particular value which is being sought can never be tested or evaluated, confirmed or denied, by the scientific endeavor to which it gives birth. The initial purpose or value always and necessarily lies outside the scope of the scientific effort which it sets in motion.

Among other things this means that if we choose some particular goal or series of goals for human beings and then set out on a large scale to control human behavior to the end of achieving those goals, we are locked in the rigidity of our initial choice, because such a scientific endeavor can never transcend itself to select new goals. Only subjective human persons can do that. Thus if we chose as our goal the state of happiness for human beings (a goal deservedly ridiculed by Aldous Huxley in *Brave New World*), and if we involved all of society in a successful scientific program by which people became happy, we would be locked in a colossal rigidity in which no one would be free to question this goal, because our scientific operations could not transcend themselves to question their guiding purposes. And without laboring this point, I would remark that colossal rigidity, whether in dinosaurs or dictatorships, has a very poor record of evolutionary survival.

If, however, a part of our scheme is to set free some "planners" who do not have to be happy, who are not controlled, and who are therefore free to choose other values, this has several meanings. It means that the purpose we have chosen as our goal is not a sufficient and a satisfying one for human beings but must be supplemented. It also means that if it is necessary to set up an elite group which is free, then this shows all too clearly that the great majority are only the slaves—no matter by what high-sounding name we call them—of those who select the goals.

Perhaps, however, the thought is that a continuing scientific endeavor will evolve its own goals; that the initial findings will alter the



directions, and subsequent findings will alter them still further, and that science somehow develops its own purpose. Although he does not clearly say so, this appears to be the pattern Skinner has in mind. It is surely a reasonable description, but it overlooks one element in this continuing development, which is that subjective personal choice enters in at every point at which the direction changes. The findings of a science, the results of an experiment, do not and never can tell us what next scientific purpose to pursue. Even in the purest of science, the scientist must decide what the findings mean and must subjectively choose what next step will be most profitable in the pursuit of his purpose. And if we are speaking of the application of scientific knowledge, then it is distressingly clear that the increasing scientific knowledge of the structure of the atom carries with it no necessary choice as to the purpose to which this knowledge will be put. This is a subjective personal choice which must be made by many individuals.

Thus I return to the proposition with which I began this section of my remarks—and which I now repeat in different words. Science has its meaning as the objective pursuit of a purpose which has been subjectively chosen by a person or persons. This purpose or value can never be investigated by the particular scientific experiment or investigation to which it has given birth and meaning. Consequently, any discussion of the control of human beings by the behavioral sciences must first and most deeply concern itself with the subjectively chosen purposes which such an application of science is intended to implement.

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### III. SKINNER

I cannot quite agree that the practice of science *requires* a prior decision about goals or a prior choice of values. The metallurgist can study the properties of steel and the engineer can design a bridge without raising the question of whether a bridge is to be built. But such questions are certainly frequently raised and tentatively answered. Rogers wants to call the answers "subjective choices of values." To me, such an expression suggests that we have had to abandon more rigorous scientific practices in order to talk about our own behavior. In the experimental analysis of other organisms I would use other terms, and I shall try to do so

here. Any list of values is a list of reinforcers—conditioned or otherwise. We are so constituted that under certain circumstances food, water, sexual contact, and so on, will make any behavior which produces them more likely to occur again. Other things may acquire this power. We do not need to say that an organism chooses to eat rather than to starve. If you answer that it is a very different thing when a man chooses to starve, I am only too happy to agree. If it were not so, we should have cleared up the question of choice long ago. An organism can be reinforced by—can be made to "choose"—almost any given state of affairs.

Rogers is concerned with choices that involve multiple and usually conflicting consequences. I have dealt with some of these elsewhere in an analysis of self-control. Shall I eat these delicious strawberries today if I will then suffer an annoying rash tomorrow? The decision I am to make used to be assigned to the province of ethics. But we are now studying similar combinations of positive and negative consequences, as well as collateral conditions which affect the result, in the laboratory. Even a pigeon can be taught some measure of self-control! And this work helps us to understand the operation of certain formulas—among them value judgments—which folk-wisdom, religion, and psychotherapy have advanced in the interests of self-discipline. The observable effect of any statement of value is to alter the relative effectiveness of reinforcers. We may no longer enjoy the strawberries for thinking about the rash. If rashes are made sufficiently shameful, illegal, sinful, maladjusted, or unwise, we may glow with satisfaction as we push the strawberries aside in a grandiose avoidance response which would bring a smile to the lips of Murray Sidman.

People behave in ways which, as we say, conform to ethical, governmental, or religious patterns because they are reinforced for doing so. The resulting behavior may have far-reaching consequences for the survival of the pattern to which it conforms. And whether we like it or not, survival is the ultimate criterion. This is where, it seems to me, science can help—not in choosing a goal, but in enabling us to predict the survival value of cultural practices. Man has too long tried to get the kind of world he wants by glorifying some brand of immediate reinforcement. As science points up more and more of the remoter consequences, he may begin to work to strengthen behavior, not in a slavish devotion

to a chosen value, but with respect to the ultimate survival of mankind. Do not ask me why I want mankind to survive. I can tell you why only in the sense in which the physiologist can tell you why I want to breathe. Once the relation between a given step and the survival of my group has been pointed out, I will take that step. And it is the business of science to point out just such relations.

The values I have occasionally recommended (and Rogers has not led me to recant) are transitional. Other things being equal, I am betting on the group whose practices make for healthy, happy, secure, productive, and creative people. . . .

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If we are worthy of our democratic heritage we shall, of course, be ready to resist any tyrannical use of science for immediate or selfish purposes. But if we value the achievements and goals of democracy we must not refuse to apply science to the design and construction of cultural patterns, even though we may then find ourselves in some sense in the position of controllers. Fear of control, generalized beyond any warrant, has led to a misinterpretation of valid practices and the blind rejection of intelligent planning for a better way of life. In terms which I trust Rogers will approve, in conquering this fear we shall become more mature and better organized and shall, thus, more fully actualize ourselves as human beings.

b.

**Arthur J. Dyck**  
**Ethical Issues in Community and**  
**Research Medicine\***

. . . What tasks and what judgments accrue to physicians as physicians? What are they trained to do and what ought they be trained to do? By what primary principles and modes of ethical reasoning is and ought their practice to be governed? Let us briefly examine a specific instance in which current medical practice is involved sometimes implicitly, sometimes explicitly, in conflicting ways of answering these questions.

In a recent series of articles in the *Journal*, Milunsky et al. review the current state of prenatal genetic diagnosis and argue for the widespread use of amniocentesis. As they use the

term, amniocentesis refers to the aspiration of fluid from the amniotic sac for the purpose of making cytogenetic studies. The immediate rationale for this use of amniocentesis is to advance the practice of genetic counseling by basing predictions of an increasing number of diseases upon actual diagnoses in utero instead of calculated probability risks. The advantages that they cite for this more accurate counsel is that it reassures couples regarding the normality of the fetus, it permits the decision to intervene through abortion where abnormalities are detected, and it provides a way of preventing the births of infants with irreparable genetic defects and fatal genetic diseases. Milunsky and his co-workers recognize that this last use of amniocentesis changes the traditional role of the physician so far as he can now predict diseases accurately before birth and provide the means of preventing the birth of a child with mental defects or fatal diseases. Hence, we enter a new era of social and preventive medicine.

Throughout the discussion of the diagnostic use of amniocentesis as advocated by Milunsky et al., there is no explicit recognition of the fetus as a patient. Apparently, genetic counseling does not include the task of preparing a family to accept and care for a defective child. "Therapy" at the present time is aimed at the family and not the fetus. In an earlier essay, John W. Littlefield spoke specifically to this issue:

Prenatal genetic diagnosis will constitute a major medical advance only if therapy can be given once a diagnosis is made. Eventually and occasionally, this may be prenatal therapy for the fetus. . . . But society and the professions must appreciate and accept that the proper therapy now is for the family, and at times that means abortion.

Clearly, neither reassurance for the family nor abortion provides therapy for the fetus. The hope is held out for eventual and occasional therapy for the fetus, with no explicit reference to the kinds of postnatal therapy presently available. For now, the treatment of the diseases of this "sometime patient" is, in this view, achieved by its elimination as a patient and as a living entity.

Suppose a physician argues that deciding whether to treat the fetus as a patient is a judgment that a physician as physician is not in a position to make. In effect, this is the view taken by Milunsky et al. when they suggest that physicians and society are, and should remain, impartial or neutral regarding decisions by families about

\* 284 *New England Journal of Medicine* 725-726 (1971). Reprinted by permission.

whether to use amniocentesis and whether to abort the fetus where deformities are detected. This point of view is strange considering both the traditional role of physicians and current medical practice.

As the physician's role was traditionally depicted in the Hippocratic Oath and many subsequent codes, he was expected to be the physician advocate of both the pregnant woman and developing life within the womb. If, as Milunsky et al. suggest, both physician and society should be impartial regarding the use of amniocentesis to prevent diseases by eliminating the diseased, what advocate is left for defenseless life? Are physicians about to abandon also their time-honored role as advocates on behalf of the hopelessly ill, the unconscious and the experimental subject who is uninformed? And even if one wishes to leave the exact status of the fetus as a human life an open question, should it not be part of the special responsibility of the physician, as it certainly has been traditionally, to err on the side of saving and fostering human life rather than to develop or encourage programs that selectively prevent such life?

To drop the fetus as a patient at this time in history is also incongruous with the aims and increasing accomplishments of contemporary fetology. Furthermore, why should physicians claim increasing responsibility for defining and specifying the end of human life, and decreasing responsibility for defining and specifying the beginning of human life? Why, for example, should the absence of signs of brain activity spell death while signs of brain activity in the eight-week-old fetus are largely unheralded as signs of human life? If physicians nevertheless insist that specifying when human life begins is not a medical decision, by what warrant do they decide that the fetus is not a patient and that his life is dispensable? As Ramsey has indicated, the medical warrant for recommending abortion occurs only when a fetus threatens the life or the health of a pregnant woman.

To decide that a given set of diseases is to be eliminated by elimination of the diseased is one of the principles on which programs of eugenics and euthanasia rest. Decisions of this kind are surely not morally neutral. What special competence does a physician have to decide that a society ought to prefer death to giving custodial or remedial care for those who require it? Milunsky et al. cite the costs of care for the mentally retarded in Massachusetts. What a meager sum this is as compared to the amount of money

being spent for destroying lives in Vietnam! If saving money is important, why not save much more money and save lives as well by thinking of other costs that could be cut? One of the problems here is that, as the physicians strive to contribute to social well-being, they find that only certain kinds of actions are predictably within their power as physicians. Hence, they look to the surest way in which they can affect social policy. Their warrant for doing so is very unclear, and, whereas we can vote out those who might suggest legislation that permits or encourages selective killing, including capital punishment and the like, our recourse in forming the conscience of physicians is less certain. Heretofore, in the area of abortion, we have generally put constraints upon physicians and others on behalf of the fetus. The assumption that the use and application of amniocentesis is a neutral sphere for physicians and society presupposes that, for physicians and society, abortion is not a moral issue, and that existing or future laws do or will assure that abortions are decided solely by families and physicians. To go that way is not morally neutral, and it is not life affirming.

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#### NOTE

JAMES K. GLASSMAN  
HARVARD GENETICS RESEARCHER QUILTS  
SCIENCE FOR POLITICS\*

Last November a team of Harvard scientists announced that it had isolated a pure gene from a strain of bacterial virus for the first time in history. Now, one of the principal members of the team has decided to give up science and become a full-fledged political activist. The scientist is James Shapiro, who is 26 years old and a research fellow in bacteriology and immunology at the Harvard Medical School. . . . Nobelist Salvador E. Luria and other experts in Shapiro's field consider him one of the most promising molecular geneticists in the nation.

Shapiro discussed his three main reasons for quitting science in a recent interview. First, he believes that the work he does will be put to evil uses by the men who control science—in government and in large corporations—in the way that atomic energy, for example, was put to evil uses. Second, he refuses to contribute to a

\* 167 *Science* 963 (1970). Copyright 1970 by the American Association for the Advancement of Science. Reprinted by permission.

system that does not allow "the people" to have a say in deciding what work scientists do. Third, he thinks that the most important problems the country faces, such as health care and pollution, need political solutions more urgently than scientific ones.

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Shapiro's decision was foreshadowed by statements he made when isolation of the gene was announced. With Beckwith and Lawrence Eron, a third-year Harvard Medical School student, he warned that the work could be perverted and used for evil purposes, such as genetic manipulation in human beings. Shapiro said at the press conference, "We did this work for scientific reasons, also because it was interesting to do. But scientists generally have the tendency not to think too much about the consequences of their work while doing it. But now that we have, we are not entirely happy about it. This is a problem in all scientific research, the bad consequences we cannot control. Many of us are upset that science and technology have been used, as in Vietnam, on innocent people. I don't think we necessarily have the right to pat ourselves on the back."

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## 5.

### Interests of Science?

#### J. Kenneth Benson and James Otis Smith The Harvard Drug Controversy— A Case Study of Subject Manipulation and Social Structure\*

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In 1960 Dr. Leary and Dr. Alpert obtained a supply of psilocybin from Sandoz Pharmaceuticals, Inc., and began research on the "consciousness-expanding" properties of the drug. The research began as a thoroughly respectable inquiry in a burgeoning field of research. Alpert was an assistant professor of Clinical Psychology and of Education and Leary a lecturer on Clinical Psychology at Harvard. Both were associated with the Center for Research in Person-

ality, a research arm of Harvard's Department of Social Relations. Both men were respected members of their profession. . . .

In initiating the research, Leary and Alpert entered a rapidly developing field of investigation. Research on the effects of peyote, mescaline, psilocybin, d-lysergic acid diethylamide (LSD-25) and other materials with similar effects has been greatly accelerated within the last twenty years. Psychiatrists and others have been interested in the psychotomimetic properties of the materials. Some have believed that the substances produced a temporary psychosis which might be studied in search of pharmacological explanation and treatment for psychosis. Although enthusiasm for this view has waned, it is still the basis for some research. In addition, the military has been interested also in the psychotomimetic properties of the substances. Some persons have argued that these materials might be used to produce a temporary state of insanity in an enemy which would permit conquest without death and destruction.

Numerous investigators have been concerned with the mystical, insightful experiences reported to them from the ingestion of psilocybin, LSD-25, and related materials. A variety of reports of mystical insight, consciousness-expansion, and increased understanding of self as a consequence of exposure to the drugs have appeared in the literature. Some psychiatrists and clinical psychologists have been interested in the therapeutic potential of such experiences. It has been suggested that mind-altering substances might be profitably employed as an adjunct to psychotherapy or even as a therapy in itself. Other reports support the idea that these materials can be helpful in the rehabilitation of criminals, alcoholics, and narcotics addicts. It has also been suggested that the pain and anxiety of dying patients can be somewhat relieved by chemically induced insight and understanding.

Leary and Alpert were interested in the study of the mystical insight and understanding produced by LSD and related drugs. However, their interest extended beyond the strictly therapeutic potential of the drugs. They argued that exposure to the substances could provide an expansion of consciousness which would enhance one's creativity, intelligence, understanding of life, and social adjustment. While some of their research involved a rehabilitation program for criminals, the bulk of their efforts were expended in studies of "normal" subjects.

As the research progressed, the activities of

\* G. Sjöberg, ed.: *Ethics, Politics, and Social Research*. Cambridge, Mass.: Schenkman Publishing Co. 117-119, 123-126, 128-131 (1967). Reprinted by permission.

Leary and Alpert reportedly became more promotional and less restrained. Concerned for the welfare of students, Harvard University officials reached an agreement with Leary and Alpert in the fall of 1961 that undergraduates would not be used as research subjects. In March, 1962, the misgivings of some colleagues in the Department of Social Relations led to a meeting of the Center for Research in Personality, at which the Leary-Alpert research was roundly criticized. Published accounts of the meeting stimulated a widespread controversy culminating in an investigation of the project by the Massachusetts Public Health Department. The decision of the investigators to permit the research to continue, with minor changes, seemed to signal a reduction in the level of public controversy, although the efforts of Harvard officials and the Department of Social Relations to establish satisfactory arrangements for the control of the drugs were unsuccessful.

The controversy bloomed anew in the fall of 1962 when Leary and Alpert returned to Harvard after a summer of drug research in Mexico. In October they formed an organization called the International Federation for Internal Freedom (IFIF) which was to sponsor and encourage research with hallucinogenic drugs.

University officials became alarmed by reports of extensive illicit use of hallucinogens by Harvard undergraduates. Although responsibility for this development could not be readily assigned to the psilocybin researchers, two university officials—Dean John U. Monro and Health Services Director Dana Farnsworth—issued public statements warning students of possible harm and decrying the sophisticated, intellectual promotion of the hallucinogens. . . .

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. . . Some critics, especially behavioral scientists, attacked the psilocybin research for what they regarded as a departure from the proper goals and methods of science. The overlap between research and application within the project runs counter to the view that research activity should be insulated from problems of practical application.

The psilocybin investigators were impressed with the potential benefits of the consciousness-expanding drugs. In a paper presented in 1961, Leary argued as follows:

The basic aim of physical science is to reduce human helplessness in the face of the physical environment. Physical science has other goals, of course: to un-

derstand, to explain, to control, to measure, to predict. . . . Why explain? Why predict? To lessen fearful ignorance. The technologies which have grown up around the physical sciences, engineering, medicine, also take as their goal the reducing of human helplessness . . . and the social technologies—psychiatry, social work, applied psychology—is not their goal the reduction of confusion and the increase in human freedom?

When people come to us and ask us to change their behavior, why can't we do it? Why can't we teach them to see the game structure of human society?

Change in behavior can occur with dramatic spontaneity once the game structure of behavior is seen. The visionary experience is the key to behavior change.

The most efficient way to cut through the game structure of Western life is the use of drugs, consciousness-expanding drugs.

Leary and Alpert contended that consciousness-expanding drugs would eventually produce extensive changes both in individuals and in social systems. They sometimes interpreted opposition to their research as an attempt to defend the status quo. For example, in a paper entitled "The Politics of Consciousness-Expansion" they argued that because LSD can change the functioning of the nervous system it proves a threat to the established social order and therefore challenges "every branch of the Establishment." They characterized the fear consciousness-expansion caused among the Establishment as "more frightening than the Bomb!" They argued that this fear was the result of potential socio-political change rather than of physical or physiological change occurring in individual subjects. "Man" they state "is about to be changed . . ." and the ". . . present social establishments had better be prepared for the . . . floodtide, two billion years building up. The verbal dam is collapsing. Head for the hills, or prepare your intellectual craft to flow with the current."

It is, of course, not necessarily unscientific to believe that one's research is of practical value. As clinical psychologists, Leary and Alpert were members of a profession highly committed to the practical application of research findings. Researchers in other areas, e.g., the sociology of deviant behavior, have been much concerned with the utilization of research findings in prevention and rehabilitation. However, Leary and Alpert went even further, attempting to engage in research and in practical application concurrently:

The goal of the research sessions run by the Harvard-IFIF group was not to produce and study frightening disturbances of consciousness (which was the goal of most psychiatric investigations of model psychoses), but to produce the ecstatic experiences, to expand consciousness, to provide the subject with the most memorable, revelatory, life-changing experience of his life. . . .

From the beginning of our research, our attention was directed to the engineering of ecstasy, the preparation for, the setting for, the architecture of ecstasy.

Apparently, the production of ecstatic experiences was both a means to the discovery of the causes of such experiences and a desired end-product of the research. Such a combination of theoretical and practical interests is, of course, not unique to the psilocybin project. Frequently, in drug experimentation the investigator hopes that his research will cure or prevent illness among research subjects while at the same time providing valuable knowledge. In studies of delinquency control and prevention one may hope not only to discover causes and cures but also to reduce delinquency within the population studied.

The combination of research and application becomes objectionable to many when the applied concerns of the investigator interfere with his search for valid knowledge. Such interference was alleged by critics of the Harvard psilocybin project. Some contended that the scientific goals of the psilocybin investigation were eventually obscured in the attempt to produce mystic ecstasy as an end in itself. Consider, for example, the comments of Brendan A. Maher, chairman of the Center for Research in Personality at the time of the controversy:

Taking a drug, sitting in a fox-hole, falling in love, or falling out of an airplane all provide experiences. To the extent that we engage in any of these activities because the experience is an end in itself, then we are doing it—to speak colloquially—for “kicks.” A university is an institution intended to provide a rather special set of experiences; experiences that lead to increased competencies, capacities for intellectual self-discipline, interest in examining all of the evidence and an understanding of the intellectual history of man. Experience *per se* is not part of a University's commissariat. . . .

Among the members of the faculty at the Center there was serious concern when it became apparent that not only were students being indoctrinated in the belief that communicable knowledge was the end-product of some kind of pointless “game,” but that the drug experience was being held out to them as a kind of redemption from the rigors of rationality.

Similar concern was reportedly voiced by Dr. Herbert C. Kelman (then a lecturer in Social Psychology at Harvard) at a meeting of the faculty and students of the Center for Research in Personality. He contended that “the program has an anti-intellectual atmosphere. Its emphasis is on pure experience, not on verbalizing findings.”

The methods as well as the aims of the psilocybin research were criticized. The setting in which drugs were administered and the participation of research workers in the drug sessions led to considerable opposition.

Drug sessions were often held in private homes and apartments. Research subjects were led to expect and to prepare for pleasant, insight-provoking experiences. The research setting was pleasant, relaxed, and supportive. Music, paintings, books, and drinks were sometimes provided. Research workers often took the drug with the subjects.

The careful arrangement of the research setting was based on several considerations. First, the investigators believed the expectations of research subjects and the social context of the drug sessions to be important determinants of reaction to psilocybin. They argued that many of the negative reactions reported by some other investigators were consequences of negative pre-exposure attitudes and of threatening research settings. Second, the investigators felt that the participation of research workers in the drug sessions facilitated favorable reactions by eliminating the social distance between the subject and the observer. Third, the investigators thought that reactions to psilocybin were properly understood only by persons who had themselves been exposed to the drug.

The procedures intended by Leary and Alpert to be provocative of desirable psilocybin reactions and of valid scientific data were seen by some critics as conducive to a party-like atmosphere inappropriate to scientific research. The critics charged that the participation of research workers in the drug sessions precluded rather than facilitated the collection of reliable information.

The negative reactions of behavioral scientists to the psilocybin project included criticism of goals and methods in conjunction with concern for the well-being of research subjects and the control of drugs. The matter of subject health was not dealt with separately as an ethical issue apart from other issues. Instead, behavioral scientists reacted to the combination of poten-

tially harmful operations and questionable purposes and methods. If the research had been regarded as both worthy in its aims and rigorous in its methodology, there might have been far less furor over subject health. Similarly, if the research had not involved a potential threat to subject health, there probably would have been far less controversy over its methods and purposes.

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. . . Another issue in the Harvard controversy concerns the limits of academic freedom. It is clear that the activities of Leary and Alpert were restricted to some extent by social control measures. Early in the research, the Harvard administration reached an agreement with the psilocybin investigators that undergraduates would not be used as research subjects. Later, at the insistence of the Massachusetts Public Health Department, the presence of a medical doctor at each drug administration became mandatory. At several junctures, Harvard officials issued public statements which were construed as criticism of the promotional activities of the psilocybin investigators. Still later, the psilocybin research was separated from Harvard because of the failure to devise control measures satisfactory both to the research workers and to representatives of the Laboratory of Social Relations. Finally, both Leary and Alpert were dismissed from their appointments on the Harvard faculty.

Despite the restrictions described above, it has been argued in some quarters that the principle of academic freedom was not violated. Obviously, the argument hinges upon one's concepts of academic freedom. In fact, much of the Harvard psilocybin controversy consists of a protracted, though muted, debate over the meaning of academic freedom.

The debate concerns two major issues. First, what kinds of activities are protected by the guarantees of academic freedom? Second, what types of control may be exercised over research activities without violating academic freedom?

The controversy can be partly understood as a debate concerning the scope of the term "research." Psilocybin investigators were given occasionally to very broad usage of that term. For example, in a mimeographed form letter of April, 1963, and appearing under an IFIF letterhead, Leary argued in effect that these substances are powerful agents for developing human potentialities. Because they are funda-

mentally educational rather than medical instruments, their use and availability should follow the educational model. Leary also stated that anyone who could benefit from the experience and who had some training in the area should be able to undertake *research* into the expansion of his consciousness.

By contrast, various social control agents seem to have been intent upon utilizing a narrower concept of "research." A distinction between research and nonresearch activities seems implicit in the responses of Harvard officials. To our knowledge, administrative officials never publicly criticized the formal research activities of Leary and Alpert. They did, however, on several occasions criticize the intellectual promotion of the hallucinogens. . . .

A clear attempt to delimit the concept of research was apparent in the negotiations within the Laboratory of Social Relations intended to establish acceptable conditions for the control of the drug. Robert F. Bales, Director of the Laboratory, and others, felt that the Laboratory could not continue to assist the psilocybin investigators in any way unless promotional uses of the project's psilocybin supply (e.g., to impress prospective financial supporters) were eliminated. The "non-research" uses of psilocybin led to severance of the connection between the psilocybin project and the Laboratory of Social Relations.

\* \* \*

. . . Brendan A. Maher argued that academic freedom should not be construed to mean that incompetence is approved. As Maher put it in a general statement preceding his description of the Harvard controversy,

It is difficult to see how academic freedom is threatened by the expectation that a scholar demonstrate his competences, especially where there is the slightest possibility that harm may be caused to others by an unskilled performance. . . . Academic freedom does not include a license to be incompetent where it is possible for competence to be provided.

\* \* \*

Thus, in many quarters there seems to have been an attempt to define the guarantees of academic freedom so as to exclude some of the activities of Leary and Alpert from their protection.

In addition, those dealing with the psilocybin research came to grips with the problem of research control. Does the principle of academic freedom mean that the investigator should be



free to investigate any and all topics without interference? In the case at hand, the attempt to establish control led to severance of the connection between Harvard and the psilocybin project. Efforts were made to establish a committee within the behavioral science faculty which would oversee the psilocybin research, at least to the point of determining the conditions under which the drug was to be administered. Since an agreement satisfactory to all parties could not be reached, the proposed committee was not established. However, the effort to establish such a committee indicates an apparent preference on the part of some persons involved for controls from within the behavioral science discipline rather than from administrative officers of the university. Intra-professional controls were apparently perceived as more palatable than extra-professional ones.

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#### NOTE

THE EDITORS OF NATURE  
WHAT COMES AFTER FERTILIZATION\*

Test tube babies may not be just round the corner, but the day when all the knowledge necessary to produce them will be available may have been brought a stage nearer by the work reported by Dr. R. G. Edwards and his colleagues. . . .

\* \* \*

The advantage of having these blastocysts in culture will be that it will then be possible to make a thorough study of the early stages of human embryology, for example to find out when different enzymes become active. There will be the opportunity to investigate the nature and time of onset of various biochemical abnormalities which are now attracting considerable attention in the medical world. So little is known about the vital early stage of human embryology that any efforts that are likely to lead to an increase in knowledge are surely praiseworthy. Any research that may help to show why embryonic development sometimes goes wrong seems to be a laudable enterprise. The fact that the techniques might one day be developed to make it possible to produce a fully grown human embryo *extra utero* should not be a restraint to progress. The day of the test tube

\* 221 *Nature* 613 (1969). Reprinted by permission.

baby is not here yet, and the advantages of this work are clear. These are not perverted men in white coats doing nasty experiments on human beings, but reasonable scientists carrying out perfectly justifiable research. One of the possible benefits of this research could be the treatment of some forms of infertility, probably in older women, who are thought to produce a high proportion of abnormal embryos which fail to develop.

But because the virtues of work like this seem self-evident to those most immediately involved, they should not fall into the trap of believing that everybody else feels the same. There is, for work like this, a real need to explain that the purposes of scientists are very different from those of Big Brother in George Orwell's *1984*. Unless this is done, there is a danger that the public may come to lose faith in science.

#### 6.

#### Interests of Society?

a.

#### Joshua Lederberg Curbs on Human Engineering Can Create Thought Control\*

\* \* \*

A pluralistic society must make the same response to the calculated use of human biochemistry against the overcentralization of power. On the same argument that we now universally accept for leaving the responsibility for the details of child-rearing and education in the hands of the family, I would advocate the utmost permissiveness with respect to individual use of biological innovations.

Effective sanctions on the part of the state to enforce Dr. Nirenberg's cautions would generate a police and thought-control bureaucracy exactly contrary to his fundamental humanistic aims. We have already experienced the sad consequences of the confusion of law with private morals in such areas as contraception and abortion and have only begun to extricate ourselves from their attendant hypocrisy and class discrimination.

Social order must, of course, place some limits on individual discretion. We do not, for

\* *The Washington Post* A 13 (Oct. 21, 1967).  
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example, allow a parent to leave a child utterly without education, partly because of the economic stress on the community, partly because of the way this alienates the child from his culture to what we regard as the child's disadvantage.

It is doubtful, however, whether we will ever know which knowledge is for the benefit of mankind. Was the invention of printing, autos, airplanes, radio, TV, nuclear energy? If we make the most energetic, immediately beneficial use of molecular biology in medicine, as I believe we should, we must also vigilantly pursue the further research and education needed for the utmost harmony of social and technological development.

b.

### **Marshall W. Nirenberg Will Society Be Prepared?\***

New information is being obtained in the field of biochemical genetics at an extremely rapid rate. Thus far, this knowledge has had relatively little effect upon man. More information must be obtained before practical application will be possible, and the technical problems that must be overcome are formidable. However, when these obstacles have been removed this knowledge will greatly influence man's future, for man then will have the power to shape his own biologic destiny. Such power can be used wisely or unwisely, for the betterment or detriment of mankind.

Salvador Luria has said: "the progress of science is so rapid that it creates an imbalance between the power it places in the hands of man and the social conditions in which this power is exerted. Then neither warnings of scientists, nor breadth of public information, nor wisdom of citizens may compensate for inadequacies of the institutional framework to cope with the new situations."

\* \* \*

What may be expected in the future? Short but meaningful genetic messages will be synthesized chemically. Since the instructions will be written in the language which cells understand, the messages will be used to program cells. Cells will carry out the instructions, and the program may even be inherited. I don't know

how long it will take before it will be possible to program cells with chemically synthesized messages. Certainly the experimental obstacles are formidable. However, I have little doubt that the obstacles eventually will be overcome. The only question is when. My guess is that cells will be programmed with synthetic messages within 25 years. If efforts along those lines were intensified, bacteria might be programmed within 5 years.

The point which deserves special emphasis is that man may be able to program his own cells with synthetic information long before he will be able to assess adequately the long-term consequences of such alterations, long before he will be able to formulate goals, and long before he can resolve the ethical and moral problems which will be raised. When man becomes capable of instructing his own cells, he must refrain from doing so until he has sufficient wisdom to use this knowledge for the benefit of mankind. I state this problem well in advance of the need to resolve it, because decisions concerning the application of this knowledge must ultimately be made by society, and only an informed society can make such decisions wisely.

### **NOTE**

**JOSHUA LEDERBERG  
DANGERS OF REPROGRAMMING CELLS\***

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. . . Nirenberg's . . . underlying concern, which I share, is that biological control might be used by a malevolent government to the peril of individual freedom. . . .

Presumably we have to be even more concerned about subtler mistakes. A well-intentioned government might impose rash commitments for the sake of short-term advantages. Plainly we must be very sensitive about innovations that, once introduced, constitute irreversible evolutionary deviations.

\* \* \*

. . . Our educational systems are certainly a form of psychological engineering scarcely different in fundamental principle from the biological interventions that our knowledge of nucleic acids is likely to bring about.

Our main concern must be to maximize the

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\* 158 *Science* 313 (1967). Copyright 1967 by the American Association for the Advancement of Science. Reprinted by permission.

role of individual decision. This could be defeated by overenthusiastic policing of personal initiative and experimentation as well as by premature positive measures imposed by the State.

In point of fact, we already practice biological engineering on a rather large scale by use of live viruses in mass immunization campaigns. While these are of indubitable value for preventing serious diseases, their global impact on the development of human beings of a wide range of genotypes is hard to assess at our present stage of wisdom. Crude virus preparations, such as some in common use at the present time, are also vulnerable to frightful mishaps of contamination and misidentification.

Live viruses are themselves genetic messages used for the purpose of programming human cells for the synthesis of immunogenic virus antigens. Nirenberg's cautions are just as relevant to considerations of contemporary policy as they are for the ever-widening applications of molecular biology in the near future.

c.

**Andrei D. Sakharov**  
**Thoughts on Progress, Peaceful Co-Existence,**  
**and Intellectual Freedom\***

Modern technology and mass psychology constantly suggest new possibilities of managing the norms of behavior, the strivings and convictions of masses of people. This involves not only management through information based on the theory of advertising and mass psychology, but also more technical methods that are widely discussed in the press abroad. Examples are biochemical control of the birth rate, biochemical control of psychic processes and electronic control of such processes.

It seems to me that we cannot completely ignore these new methods or prohibit the progress of science and technology, but we must be clearly aware of the awesome danger to basic human values and to the meaning of life that may be concealed in the misuse of technical and biochemical methods and the methods of mass psychology.

Man must not be turned into a chicken or a rat as in the well-known experiments in which elation is induced electrically through electrodes inserted into the brain. Related to this is the question of the ever increasing use of tranquil-

izers and antidepressants, legal and illegal narcotics, and so forth.

We also must not forget the very real danger mentioned by Norbert Wiener in his book "Cybernetics," namely the absence in cybernetic machines of stable human norms of behavior. The tempting, unprecedented power that mankind, or, even worse, a particular group in a divided mankind, may derive from the wise counsels of its future intellectual aides, the artificial "thinking" automata, may, as Wiener warned, become a fatal trap; the counsels may turn out to be incredibly insidious and, instead of pursuing human objectives, may pursue completely abstract problems that had been transformed in an unforeseen manner in the artificial brain.

### NOTE

JOSHUA LEDERBERG  
GENETIC ENGINEERING, OR  
THE AMELIORATION OF GENETIC DEFECT\*

\* \* \*

What . . . are the *problems* to which genetic science can be applied? Some may think of rescuing man from the prospect of nuclear annihilation by recasting the genes for aggression—or acquiescence—that are supposed to predestine a future of territorial conflict. Even if we postulate for sake of argument that we know the genetics of militarism, we have no way to apply it without solving the political problem that is the primary difficulty to begin with. If we could agree upon applying genetic (or any other effective) remedies to global problems in the first place, we probably would need no recourse to them in the actual event.

The converse argument applies to the gloomier predictions of totalitarian abuse of a genetic technology. The scenario of *Brave New World* is well advertised by now, and no one doubts that a modern slave state would reinforce its class stratification by genetic controls. But it could not do so without having instituted slavery in the first place, for which the control of the mass media presents much more immediate dangers than knowledge of DNA. It is indeed true that I might fear the control of my behavior through electrical impulses directed into my brain, but (possibly excepting television) I do not accept the implantation of the electrodes

\* *The New York Times* 15 (July 22, 1968).  
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\* 34 *The Pharos of Alpha Omega Alpha* 9, 10 (1971). Reprinted by permission.

except at the point of a gun: the gun is the problem.

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## 7.

### Limits of Prediction?

a.

#### Francis D. Moore Therapeutic Innovation—Ethical Boundaries in the Initial Clinical Trials of New Drugs and Surgical Procedures\*

\* \* \*

A familiar though remarkable historic example of therapeutic innovation which took place in Brookline, Massachusetts, about 250 years ago raised questions as appropriate for review in 1969 as they originally were in 1721.

An epidemic of smallpox had carried away many of the colonists in Boston and eastern New England in 1702. Accounts vary as to what brought word of smallpox inoculation to the keen ear of the Reverend Cotton Mather. But to this enterprising clergyman belongs the full credit for stimulating physicians to activity. Whether he saw accounts of the Turkish experiments or learned from talks with his own Negro slave that the practice of inoculation had been tried among primitive African populations (the latter being the more dramatic version which he himself preferred), the fact remains that he stimulated others to action with such promptness that the inoculation in Britain carried out by Lady Montagu took place only a few weeks prior to his, and the much larger New England experience became the prototype for widespread application both in this country and abroad.

The practice of inoculation against smallpox in the early eighteenth century consisted in the intentional infection of a normal person with virulent unattenuated smallpox virus obtained from a patient who himself might later die of the disease. This inoculation was done with the hope that the recipient would be afflicted with a mild case of smallpox—a "distinct case" as it was then called—and that the resultant "non-susceptibility" would last the rest of his life. By

contrast, the practice of vaccination introduced seventy-five years later by Jenner in England and, following his lead, by Waterhouse in the United States consisted in inoculating the recipient with the virus of the cowpox. This mild and rarely lethal disease confers immunity to smallpox by virtue of an antigen shared by the two viruses.

Cotton Mather could find none of his Boston medical cronies interested in such a heterodox undertaking. So he turned to the nearby town of Brookline where he discussed the matter with Zabdiel Boylston, then thirty-seven years of age. Boylston was the son of a doctor who had studied at Oxford, yet he himself had no medical degree. He was still a young man who had not emerged as a medical figure in a society that was already teeming with strong medical characters. Among these were the men who, a few years later, were to found the Harvard Medical School and the Massachusetts Medical Society. This large conservative wing of practitioners would have no part of the Reverend Mather's suggestion. But Zabdiel Boylston saw it for what it was—a chance to reduce the mortality from smallpox epidemics.

Accordingly, Zabdiel fetched some pus from a pock and proceeded to inoculate his thirteen-year-old son by rubbing this material on a scarification on the boy's arm. This epic experiment occurred on June 26 or 27, 1721. It is generally conceded that Boylston did not select himself for this experiment because he had already suffered the disease and was immune. In any event, the deed was successfully accomplished—at least the son did not die of the disease—and Boylston inoculated 247 persons in the next few months. Of these, six died. There was a clamorous and riotous opposition to the procedure both among fellow practitioners and among the laity who were aroused by their friends the doctors. Not long thereafter, out of a group of 5,759 cases of the naturally occurring disease, 844 died, according to Boylston's own account. Other figures from contemporary literature state that Boylston inoculated 242 persons of whom six died, and that there were 5,889 cases in the epidemic of whom 844 died. Whichever figures are correct, it was evident that the mortality was lower in the inoculated form of the disease, and that those who had been successfully inoculated rarely, if ever, contracted the naturally occurring epidemic form of smallpox.

After a time of persecution, Boylston won out. He was acclaimed and honored here and in England. The practice spread to the other col-

\* 98 *Daedalus* 504-507 (1969). Reprinted by permission of *Daedalus*, Journal of the American Academy of Arts and Sciences, Boston, Massachusetts.

onies. Benjamin Franklin, who had been a severe critic, later became a strong proponent. . . .

This was a lethal experiment. It carried a mortality of over 2 per cent. It was undertaken to protect the individual, and through him the larger group, from the ravages of an epidemic disease. It was undertaken by people who had little idea of the nature either of the disease or the infectious agent, although Mather wrote of the "animalculi" that were involved. The basis for any confidence that this experiment would be successful was in large part hearsay from the Middle and Far East. There was no animal trial or laboratory work. A cloud of fantasy and petty controversy surrounded the actual details of inoculation techniques. Little effort was made to isolate those who had been inoculated with the disease, and they could become carriers of a virulent virus. Curiously enough, opponents of the procedure based their claims on the assertion that inoculation would not protect against the epidemic disease; they were not so interested in its public hazards or mortality, although this hazard to society was quite evident at the time. (Princess Caroline, for example, following the lead of Lady Montagu, inoculated convicted criminals and pauper children before she did her own family. She evidently hesitated to inflict an experiment that she considered hazardous on people whom she considered to be of great importance.) Finally, and most remarkably, the entire mass experiment carried out in Boston and Brookline was proposed and urged by a man of the church and was opposed almost to a man by the medical profession.

Could this experiment be conducted in 1969? Certainly not. The mortality was prohibitive. There was no scientific basis or preliminary laboratory work. It is quite evident that both Cotton Mather and Zabdiel Boylston perceived a potential social benefit that was greater in their minds than the immediate sacrifice of six lives. From this experiment was born the initial awareness of active immunization as a means of protecting society against the scourge of epidemic disease. The first mass trials of the Salk polio vaccine and all the other inoculations from Pasteur to Enders went through moments when they shared precisely the same ethical problems faced by Mather and Boylston.

\* \* \*

The first use of ether anesthesia, the first injection of insulin, the first use of liver extract, and the first application to a patient of any one

of a host of new drugs are all part of the same ethical family. At the present time, we are engaged in one of the largest mass human experiments of this type ever considered: the widespread use of oral contraceptives. . . . Oral contraception has certain features that set it apart from ordinary therapeutic innovation because it is a medicinal treatment given to a healthy person to prevent a normal occurrence, rather than an inoculation given to prevent fatal epidemic disease or a drug (or operation) employed to treat human illness. Oral contraception must, therefore, be even more free of taint than innovations involved with the treatment of disease.

b.

### Panel Discussion Eugenics and Genetics\*

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BRONOWSKI: I find myself out of sympathy with much that has been said in Muller's and Lederberg's papers. That is because I really do not understand what problem you are trying to solve. If you are trying to upset violently the present gene frequencies in the population, then nothing that Muller proposes could do this. Just as Haldane has shown long ago that sterilization of the unfit would hardly have any influence on the proportion of recessive genes, so the multiplication of what we choose to call the fit can really have very little effect on the presence of recessives. (And no one who has known the children of accepted geniuses would suppose that the population would greatly benefit by there being several hundred of them.) If you are trying radically to change the gene frequencies, of course you can only do that in Crick's way, that is by forcibly preventing all but a few genes from reproducing. Even this supposes that you know (a) why you think a particular gene is good, and (b) what tests to apply in order to identify it.

However, I took Crick's remarks to be a *reductio ad absurdum* of the method of direct control of the gene frequencies. Indeed, we might achieve the same effect in a simpler way—by eating the children of the unfit, as Jonathan Swift suggested that the Irish poor should eat their own children. But what problem are we

\* Gordon Wolstenholme, ed.: *Man and His Future*. Boston: Little, Brown and Co. 284-297 (1963). Reprinted by permission of J. & A. Churchill, London.

trying to solve? What genes are we trying to boost? Muller asserts in his paper that there are reasons to believe that the human population is deteriorating, and Huxley in one phrase in his paper also implied this. I know of no evidence for that. I know of no evidence that the present human population is inferior, in any respect that one could quantify, to the human population 50 years ago. On the contrary, the only important experimental test of this assertion—the experimental intelligence testing of Scottish children which has been carried out over the past 25 years—produced exactly the opposite results. The human race seems to be improving itself by those natural means which I propose to continue to enjoy so long as I can!

MACKAY: I have been thinking of Shaw's mischievous remark: "What has posterity done for me that I should do anything for posterity?" Since the relation between individual responsibility and that of "society" is in fact still unclear, the notion of "our" responsibility to tinker with the genetic composition of posterity is doubly obscure. Without a much deeper analysis, the unguarded transfer to "society" of ideas proper to individual responsibility can mislead us into talking—and selling—moral nonsense.

That such nonsense has proved saleable, especially in Nazi Germany, should warn us against evaluating our plans for the race solely in terms of technical feasibility. We should, however, note one technical snag in any proposal to make the human genetic constitution self-regulating. I mean the difficulty of preventing the "goal-setting" from drifting or oscillating as time goes on, under the influence of external or even internal factors. Suppose, for example, that "we" (biologists? or politicians?) decided (and had the power) to make the next human generation of type "X." So far, perhaps, so good. But when we die, our place must presumably be taken by a new committee—which would presumably be of type "X." The question we must ponder is what kind of changes these men of type "X" would think desirable in their successors—and so on, into the future. If we cannot answer it, then to initiate such a process might show the reverse of responsibility, on any explication of the term.

In short, to navigate by a landmark tied to your own ship's head is ultimately impossible. If we are ever to make proper use of our growing eugenic powers, we shall need a wisdom greater than our own.

Here let us be quite candid. There is little agreement today that such wisdom is available, let alone as to its origin. But I believe strongly that this does not make discussion at this level pointless or impossible. For the beginning of wisdom is to ask the right questions; and it is by each faithfully drawing attention to—and listening to—questions which from a different viewpoint might not be raised, that we can most fruitfully cooperate for human welfare.

BROCK: I would like to echo Bronowski's question: What is the problem that Crick and Muller are trying to solve? And are they thinking about some other problems which would arise out of the solution of what they think they are trying to solve? Are we going to bring to humanity the happiness which undoubtedly we all want? Sir Julian asked the question: What are people for? I don't believe that any of us really knows the answer, but I suppose that self-expression and self-fulfillment must be among the objectives of mankind. This brings me to the psycho-emotional aspect of the woman who is denied children. Even where childlessness is inevitable, even when she is married to an impotent husband to whom she is devoted, the psycho-emotional effect of this situation on her is devastating. Admittedly the need to have children can be met up to a point by adoption, but not when there is another alternative. In my opinion no woman is going to be emotionally satisfied by the adoption of children, when she knows that she could have had children by her own parturition. If we are to have a healthy society, we must have a society in which such psycho-emotional upsets are reduced to the minimum. When it comes to the solution that Muller proposes I doubt that, even with improved biological education, many men will be emotionally satisfied by children not their own, if they are also able to have children in the normal way. And without this emotional satisfaction and fulfillment I doubt that we would have a healthy society. There may be a small group of "advanced" or otherwise abnormal people who would be satisfied, but the average man in my opinion would not be. I agree with Pirie that perhaps for many men it is the fun rather than the children that they desire, but this is not true of all men and is certainly not true of the average woman.

TROWELL: Speaking as a physician, I should like to emphasize the very profound psychological effect on both men and women who cannot

have progeny by the natural method. It has played havoc with many of my patients and some of my friends.

KLEIN: I agree that the psycho-emotional reaction of a man who cannot have children is very strong. There are a number of married people who have no children because although the man is potent, he is sterile; this is a most unhappy situation and it is very difficult to explain to a couple that a man can be both potent and sterile. I think more research is needed on the problem of sterility in the male.

If we ever adopt Muller's techniques, we shall have to have biographies, not only of the great man whom we are considering as a sperm donor, but also of his antecedents. However, the present state of our knowledge of human inheritance is extremely fragile. I think we are still at the beginning of the study of human heredity, and before applying it I feel we must know it much better.

May I finish with a story by the German biologist, von Uexküll, about a man who discovered his own shadow. This man came to believe that his shadow was a living thing. At first he imagined his shadow to be his servant, because it copied all his movements; but he gradually began to doubt this and to believe that he was imitating the shadow. Thereafter he showed more and more consideration for his shadow, allowing it to have his seat or bed while he himself remained uncomfortably to one side. This man was eventually reduced to being the shadow of his shadow. Perhaps we also are too conscious of our shadows and forget what we are ourselves.

LEDERBERG: In answer to Dr. Bronowski's question about our motivation, I think that most of us here believe that the present population of the world is not intelligent enough to keep itself from being blown up, and we would like to make some provision for the future so that it will have a slightly better chance of avoiding this particular contingency. I am not saying that our measures will be effective, but I think this is our motivation; it is not the negative but the positive aspects of genetic control that we are dealing with here.

On the other hand I have serious doubts about the proposals for controlling reproduction that have been presented to us. The aspects of *social* control that seem to be necessary to make these proposals technically effective are I think extremely offensive and extremely dan-

gerous, certainly in our present social context. But leaving the matter to individual choice, which from a social standpoint is the most ideal, is certainly not going to be technically effective. And if people are allowed to choose the fathers of their children, will they not choose just the more notorious projections of their own images, exaggerated by the publicity given to advertised donors?

COMFORT: Dr. Lederberg, what makes you think that we could make ourselves less likely to blow ourselves up by a genetic increase in intelligence?

LEDERBERG: I didn't say I thought we would succeed; I said I think this is our underlying motivation for attempting genetic control.

COMFORT: I should think that it is not so much low I.Q.'s, but personality problems and emotional disturbances which were the cause of our liability to blow ourselves up.

LEDERBERG: These are just as likely to be under genetic control.

COMFORT: They may be, but in man there is a large latitude for training. Dr. Trowell spoke about breeding a generation that displayed cruelty and efficiency. I think one could do this—or for that matter do the opposite—much more simply by *upbringing* than one can by trying to alter genetic constitution.

BRONOWSKI: I would still like an answer to my question. What is the evidence that genetically the human population is deteriorating?

HUXLEY: The evidence is mainly deductive, based on the fact that we are preserving many more genetically defective people than before, and are getting a lot of radioactive fallout. Meanwhile, the study of intelligence in Scottish children which you cited is not valid evidence. During the period between the first and the second tests, children generally were becoming larger, were developing more rapidly, and therefore were becoming more intelligent for their chronological age.

The important point, however, as Lederberg said, is not the negative one of deterioration (although it might become so if there were greatly increased fallout); the main thing is to aim at positive improvement. . . .

But the basic point was raised by MacKay, that you will have to nail your colours to some moral mast. In the present state of the world you will have to find a new moral mast to nail them to, and this will only come about by more knowledge and more education and more think-



ing; and this is a feedback process. At the moment the population certainly wouldn't tolerate compulsory eugenic or sterilization measures, but if you start some experiments, including some voluntary ones, and see that they work and if you make a massive attempt at educating people and making them understand what is at issue, you might be able, within a generation, to have an effect on the general population. After all, our moral values evolve like everything else and they evolve largely on the basis of the knowledge we have and share.

GLIKSON: Like Dr. Bronowski I do not see why we need the application of biological technology in changing the quantitative and qualitative composition of whole communities or of humanity. But I think emphasis should be laid on the dangers involved in the very development of such biological technology, because its application would most probably fall into the hands of political forces which would use it for quite different purpose than those anticipated here. . . .

PRICE: I would like to go further than Bronowski, and suggest that the psychosocial system might in its own bumbling homeostatic way actually be doing the right job. We know that a great deal of the performance of man depends as much on social environment as on genetics, and this environment might act in a way completely opposite to that which would be produced by the mechanisms of genetic control which we might introduce. For example, creativity, intelligence, and the leaning towards science are apparently, on the basis of historical evidence, enormously helped by such things as being first or only children, and by losing a parent before the age of ten; these things together improve your chance of being a good and creative scientist by something like a factor of ten. Now, if the better people are having small families, they are increasing the frequency of only children, thereby giving their group an increased chance of success; and to increase the number of people carrying these genes by encouraging larger families among the more intelligent people might be to deny the possibility of the very environment which would let these factors work.

PINCUS: I am very surprised to hear some people here say that genetics has taught us nothing about nature and that if we breed in a random manner by the old-fashioned methods, we shall get good genes. This is nonsense genetically: you don't get good genes by breeding in random fashion; you get good genes by selection. If, however, you want to emphasize the phenom-

enon of heterosis or hybrid vigour, as Huxley has done, and argue that the real reason for the success of the human race is that there is so much interbreeding that you are always getting heterosis phenomena, then I accept that you have an argument there. But if we are talking about genetic improvement, you have to select good genes.

TROWELL: Could I put in very briefly the point of view of the Roman Catholic church (speaking as someone who is not a Roman Catholic and who does not subscribe completely to that point of view)—their great emphasis on natural law. I think they would say that we should be very careful before we distrust what has worked for about a million years in the human species and for longer than that in the animal creation, for this is one aspect of natural law. . . .

\* \* \*

CLARK: Dr. Trowell used the phrase "natural law" in the sense of something which has been going on for a very long time. I would define it differently; it may coincide with what has been the practice of mankind or it may not. Several people have raised the question of what is the purpose of man on earth. I feel a bit hesitant at entering this field and would have preferred a professional to have tackled it—but the main purpose of man on earth is to love God and obey his commandments. I know that poses a difficulty for people who deny God's existence but I think they ought to take a look at this view, and consider how other conclusions follow from it. Cultural fulfilment and enjoyment are secondary purposes in man's existence, not his primary purpose.

\* \* \*

CRICK: I disagree strongly with Dr. Clark's remarks and with the standpoint from which he made them. It is clear that if we take the broad ethical question of ultimate ends we shall never reach any agreement. Moreover, those of us who are humanists have a great difficulty in that we are unable to formulate our ends as clearly as is possible for those of us who are Christians. Nevertheless there are some ends that we can all share, even though we have these differences. It is surely clear that good health, high intelligence, general benevolence—the qualities Muller listed—are desirable qualities which we would all agree on. We would agree also that these qualities are not uniformly distributed. There are peo-

ple who are deficient in intelligence, for example (I mention intelligence because this is something we can to some extent measure). Surely it is a very reasonable aim for us to try to increase that. Some of the arguments that "nature is doing it all right" may possibly be correct but they seem to me only to reflect conservatism and to have no real basis of fact. We are now in an environment that is changing very rapidly, and has been changing for the last few thousand years, but we evolved, as was made clear by Muller, over a much longer period of time in very different circumstances. Consequently, we should not *necessarily* go on as we are.

Are the methods for improvement which we have at our disposal effective? Now there are difficult technical questions here, but my point, which Huxley made rather strongly, is that we are likely to achieve a considerable improvement—not perhaps as fast as we could do by other methods or even as fast as may turn out to be necessary—by using a very primitive knowledge of genetics; that is, by simply taking the people with the qualities we like, and letting them have more children. Nobody is suggesting, at least it would be foolish if they did, that we should have *enormous* numbers of people all with one father; one should have a wide selection of donors and so get diversification. The difficulty I see concerns the techniques that are *socially* possible, in the present social context, and in the social context of the next twenty or thirty years—a context which will change and which to some extent our views may help to change. For example, psychological problems may arise in families with children who are not the children of the father. Whereas I reject utterly arguments about natural law, I am much concerned that evidence on the psychological problems in such families should be collected. We already have examples of families where the father is infertile and the mother has had a child by artificial insemination by a donor; I understand that the disturbance to family life is often not great in such cases. I agree entirely with Huxley that what is wanted here is some sort of limited programme to try and find the difficulties. Let us define our broad aims and then tackle the practical details.

MEDAWAR: I agree with a good deal of what Crick has just said, but I think we ought to be warned by the very diversity of opinion in this room. We all have a pretty good opinion of our own intellect and our worthiness to be sperm donors. But our opinions are extremely diverse, and my feeling at the moment is that human be-

ings are simply not to be trusted to formulate long-term eugenic objectives—least of all Roman Catholics. What frightens me about Muller and to some extent Huxley is their extreme self-confidence, their complete conviction not only that they know what ends are desirable but also that they know how to achieve them. I can perhaps imagine approving of the kind of scheme Muller has outlined if he put it this way: "we don't really know a great deal about human inheritance but with the co-operation of a number of volunteers let us put my scheme into practice and perhaps we shall learn from it."

HUXLEY: But surely Muller's point, and certainly mine, is not to think in terms of any definite eugenic ideal; the aim that I have in mind is the very general one of gradual improvement.

MEDAWAR: But you don't know how to do it! May I challenge you to explain Evelyn Hutchinson's paradox about homosexuality? The proportion of homosexuals has probably not declined over the period of recorded history; yet according to all selection theories which we are so confident about, the proportion should have declined on the reasonable grounds (a) that homosexual tendencies are to some extent genetically determined and (b) that homosexuals are on the whole less fertile (even if fractionally less fertile) than normal people. It follows that the genetic endowments that make for homosexuality or parasexuality in general should have declined. In fact they have done nothing of the kind. This means either that so deep-seated a trait as parasexuality or homosexuality is not genetically determined or that we don't really understand the mechanism of its inheritance.

HUXLEY: I didn't know about this paradox, and am afraid I can't answer that point. In any case I want to look at the problem from another angle. You say we must know more about the details of human genetics before we can think about improvement. I really don't see why. Darwin knew nothing about the details of reproduction, still less about genetics, and yet he was able to deduce a set of principles and a general theory of evolutionary transformation which have stood up to the test of time. Our new knowledge is merely permitting us to fill in the details and add a few minor modifications. What I want to stress is that if we can find the right method of exerting selective pressure, we could make for human genetic improvement. We must do it by way of experiment.

Dr. Trowell talked about breeding for efficiency. This is very important because, as psy-

chosocial organizations get more and more complicated, we need more and more good brains at the top to run them. If you assume as a first approximation that intellectual efficiency or intelligence has a strong genetic component, and that it is distributed according to the ordinary type of symmetrical frequency curve, you can calculate that a very small increase in the mean will produce a large percentage increase in the upper values; so far as I remember, if you could raise mean I.Q. from 100 to 101.5 you would raise the percentage of people with an I.Q. of 160 and over by nearly 50 per cent. The increased social and cultural efficiency resulting from a small difference in the number of outstandingly gifted

people is also very important in considering the problem of possible racial differences.

LEDERBERG: The converse of Huxley's calculation is that in order to shift the mean I.Q. by 1.5/100 you must increase the production of geniuses by 50 per cent. It is perhaps better to aim at just increasing the variance. The question is not whether we should think about doing eugenics; we certainly should, and should collect just as much information as possible. The point is whether we should embark on a concrete programme that is very costly in social and political stresses for an aim which isn't very well crystallized yet.

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### **PART THREE**

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## **The Authority of the Subject as Guardian of His Own Fate**

In Part Two we explored the investigator's authority in the human experimentation process. We sought to specify the nature and degree of harm to subjects, science and society as well as other identifiable elements of research design and objectives which suggest that other decisionmakers, besides the investigator, should participate in this process. We now focus on the subject in order to examine the extent and limits of his ability and authority to make decisions on his own behalf.

Belief in the idea of individual freedom is a cornerstone of the Western concept of man and society. The common law nurtures and protects individual freedom through the doctrine of self-determination, which confers on each person the right to pursue his own ends in his own way so long as he does not interfere with specified rights of other individuals or of the community. The requirement of consent is the primary means for implementing the abstract notion of self-determination. Tort law, for example, guards a man's property and person against interferences to which he has not consented. Similarly, a contract comes into being when two or more persons agree with each other that certain terms should govern their relationship.

In most commercial transactions, each party is responsible for informing himself about the terms and implications of the contract. However, when professionals intervene in the lives of others, a higher standard is imposed upon them. They may be held responsible not only for obtaining the layman's consent, but also for informing him of the consequences of their agreement.

The doctor-patient relationship has been the primary arena for the development of the rule that a professional is liable for damages if he intervenes (even if he acts with utmost care and for benevolent reasons) without his patient's "informed consent." From its origins in therapeutic settings, the requirement of informed consent has been adopted for experimental situations. In Chapter Eight we examine its history and the functions which it can serve in investigator-subject relationships, in order to analyze why and how it should be safeguarded by the participants in the human experimentation process.

The doctrine of self-determination and the requirement of consent have always had to contend with doubts about man's capacity to consent and with conflicts between man and society about society's prerogative to override consent in its own interests. These doubts and conflicts have not escaped the human experimentation process but, at present, are reflected only in a number of "exceptions" to the rule of consent rather than in an overall theory of consent which encompasses these issues. Chapters Nine and Ten examine the limits of self-determination and consent inherent in the nature of man and in the investigator-subject relationship as well as the limits imposed by the "claims" of society and the subject's own "best interests." This inquiry should begin both to define the authority and capacity of man to give consent and to identify the assumptions about man and society on which a theory of consent for human experimentation should rest.

Throughout we ask:

1. What values do law and the professions attempt to nurture and protect through the concept of self-determination and the requirement of "informed consent"?
2. How should self-determination and informed consent be defined in order to protect these values?
3. Under what circumstances is informed consent either ill-adapted or insufficient to protect these values?
4. Under what circumstances do self-determination and informed consent conflict with other values which the participants—investigator, subject, professions, and society—seek to protect and advance?
5. How should a subject's inability to consent affect his participation in the human experimentation process?

Since, under some circumstances, informed consent alone may not adequately safeguard the rights of subjects or provide the best means for resolving conflicts between the participants, the study of these materials should raise questions about additional rules and procedures for the control of human experimentation as well as about who should be given authority to formulate, administer, and review them.

## CHAPTER EIGHT

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# What Are the Functions of Informed Consent?

The concept of informed consent is a legal hybrid. The traditional function of consent was to differentiate those medical interventions which were legally permissible from those which would subject a physician to liability for an unauthorized “offensive touching” of his patient. Recently courts have concluded that a patient’s assent to a medical procedure is valid—a “voluntary” product of his “free will”—only if it is based on adequate information about the intervention including its attendant risks. The engrafting of the “information” component moved the concept beyond simple assault and battery law. A physician may now be held liable either for negligence in a malpractice suit, if he breaches his duty to inform a patient, or for battery, if his failure to inform is found to have vitiated the patient’s consent.

From these beginnings, the concept of informed consent has been accepted in case and commentary as a cardinal principle for judging the propriety of research with human beings. Yet law has neither defined sufficiently well the substance and ambit of informed consent in therapeutic settings nor determined clearly its functional relevance for human experimentation. Thus, in invoking informed consent like a talisman, lawyers, investigators, and courts often seem to overlook the fact that it lacks specific construction and remains an ill-defined concept.

This chapter examines first the constructions which courts and commentators have given to informed consent. It then explores the functions which informed consent could serve for the human experimentation process; these have been grouped into four categories. Most clearly, requiring informed consent serves society’s desire to respect each individual’s

autonomy and his right to make choices concerning his own life. Second, providing a subject with information about an experiment and encouraging him to be an active partner in the process may also increase the rationality of the experimentation process.

Third, securing informed consent protects the experimentation process by encouraging the investigator to question the value of the proposed project and the adequacy of the measures he has taken to protect subjects, by reducing civil and criminal liability for non-negligent injury to the subjects, and by diminishing adverse public reaction to an experiment. Finally, informed consent may serve the function of increasing society's awareness about human research. For instance, the need to obtain consent from large numbers of potential donors for the removal of their kidneys after death has led to an extensive program of information about renal transplantation. While the motivation for the information campaign was to recruit individual donors, it also enlightens the public at large.

The functions of informed consent, which are identified in this chapter, do not necessarily conflict with and may in some contexts reinforce one another. Nevertheless, each function suggests different substantive and procedural requisites for the definition of "consent." Thus an important task of this chapter is to arrive at a comprehensive functional definition of informed consent, in light of the competing theories and attitudes about individuals' right to and capacity for self-determination.

In studying the materials in this chapter, consider, in addition to the general questions posed in the introduction to this Part, the following specific questions:

1. What functions should informed consent serve in the human experimentation process? In seeking an answer to this question, must the "informing" and "consenting" components of this concept be considered separately?
2. What are the requisite elements of informed consent to implement these functions?
3. To what extent can the elements of informed consent, developed in therapeutic settings, be carried over by analogy to experimental contexts?
4. Under what circumstances and to what extent should the elements of informed consent be modified?
5. Should additional or alternative elements of consent be required when the experimental subject is also a patient?

## A.

### An Historical Perspective

#### 1.

##### From Status to Contract

a.

##### **Friedrich Kessler and Grant Gilmore Contracts—Cases and Materials\***

\* \* \*

Within the framework of a free-enterprise system the essential prerequisite of contractual

liability is volition, that is, consent freely given, and not coercion or status. Contract, in this view, is the "meeting place of the ideas of agreement and obligation." As a matter of historical fact, the rise of free and informal contract within western civilization reflected the erosion of a status-organized society; contract became, at an ever-increasing rate, a tool of change and of growing self-determination and self-assertion. Self-determination during the nineteenth century was regarded as the goal towards which society progressed; the movement of progressive soci-

\* Boston: Little, Brown and Co. 3-6 (2nd ed., 1970). Reprinted by permission.



eties, in the words of Sir Henry Maine, is a movement from status to contract. "It is through contract that man attains freedom. Although it appears to be the subordination of one man's will to another, the former gains more than he loses." Contract, in this view, is the principle of order par excellence and the only legitimate means of social integration in a free society. Translated into legal language this means that in a progressive society all law is ultimately based on contract. And since contract as a social phenomenon is the result of a "coincidence of free choices" on the part of the members of the community, merging their egoistical and altruistic tendencies, a contractual society safeguards its own stability. Contract is an instrument of peace in society. It reconciles freedom with order, particularly since with increasing rationality man becomes less rather than more egoistic.

\* \* \*

Small wonder that freedom of contract, as evolved in the spirit of *laissez-faire*, has found repeated expression in Anglo-American case law. It became the paramount postulate of public policy. "[I]f there is one thing which more than another public policy requires," Sir George Jessel, M.R., assures us, "it is that men of full age and competent understanding shall have the utmost liberty of contracting, and that their contracts entered into freely and voluntarily shall be held sacred and shall be enforced by Courts of Justice." True, fraud, misrepresentation and duress must be ruled out by the courts in the exercise of their function of making sure that the "rules of the game" will be adhered to. But these categories were narrowly defined (at least by the nineteenth century common law) due to the strong belief in the policing force of the market. Oppressive bargains, it was taken for granted, can be avoided by careful shopping around. Contracting parties are expected to look out for their own interest and their own protection. "Let the bargainer beware," as we were told, was (and to some extent still is) the ordinary rule of contract. It is not the function of courts to strike down improvident bargains. Courts have only to interpret contracts made by the parties. They do not make them. Within this framework contract justice is commutative and not distributive justice. This attitude is in keeping with liberal social and moral philosophy according to which it pertains to the dignity of man to lead his own life as a reasonable person and to accept responsibility for his own mistakes. . . .

\* \* \*

## NOTES

### NOTE 1.

FRIEDRICH KESSLER  
CONTRACTS OF ADHESION—SOME THOUGHTS  
ABOUT FREEDOM OF CONTRACT\*

With the development of a free enterprise system based on an unheard of division of labor, capitalistic society needed a highly elastic legal institution to safeguard the exchange of goods and services on the market. Common law lawyers, responding to this social need, transformed "contract" from the clumsy institution that it was in the sixteenth century into a tool of almost unlimited usefulness and pliability. Contract thus became the indispensable instrument of the enterpriser, enabling him to go about his affairs in a rational way. Rational behavior within the context of our culture is only possible if agreements will be respected. It requires that reasonable expectations created by promises receive the protection of the law or else we will suffer the fate of Montesquieu's Troglodytes, who perished because they did not fulfill their promises. This idea permeates our whole law of contracts, the doctrines dealing with their formation, performance, impossibility and damages.

Under a free enterprise system rationality of the law of contracts has still another aspect. To keep pace with the constant widening of the market the legal system has to place at the disposal of the members of the community an ever increasing number of typical business transactions and regulate their consequences. But the law cannot possibly anticipate the content of an infinite number of atypical transactions into which members of the community may need to enter. Society, therefore, has to give the parties freedom of contract; to accommodate the business community the ceremony necessary to vouch for the deliberate nature of a transaction has to be reduced to the absolute minimum. Furthermore, the rules of the common law of contract have to remain *Jus dispositivum*—to use the phrase of the Romans; that is, their application has to depend on the intention of the parties or on their neglect to rule otherwise. (If parties to a contract have failed to regulate its consequences in their own way, they will be supposed to have intended the consequences envisaged by the common law.) Beyond that the law cannot go. It has to delegate legislation to the contracting par-

\* 43 *Columbia Law Review* 629-630 (1943). Reprinted by permission.

ties. As far as they are concerned, the law of contract has to be of their own making.

Thus freedom of contract does not commend itself for moral reasons only; it is also an eminently practical principle. It is the inevitable counterpart of a free enterprise system. As a result, our legal lore of contracts reflects a proud spirit of individualism and of *laissez faire*. This is particularly true for the axioms and rules dealing with the formation and interpretation of contracts, the genuineness and reality of consent. . . .

\* \* \*

#### NOTE 2.

SCHLOENDORFF V. NEW YORK HOSPITAL  
211 N.Y. 127, 129, 105 N.E. 92, 93 (1914)

CARDOZO, J.

. . . Every human being of adult years and sound mind has a right to determine what shall be done with his own body; and a surgeon who performs an operation without his patient's consent commits an assault, for which he is liable in damages. (*Pratt v. Davis*, 224 Ill. 300; *Mohr v. Williams*, 95 Minn. 261.) This is true except in cases of emergency where the patient is unconscious and where it is necessary to operate before consent can be obtained. . . .

b.

**Slater v. Baker and Stapleton, C.B.**  
95 Eng. Rep. 860 (1767)

[PER CURIAM]

\* \* \*

Special action upon the case, wherein the plaintiff declares that the defendant Baker being a surgeon, and Stapleton an apothecary, he employed them to cure his leg which had been broken and set, and the callous of the fracture formed; that in consideration of being paid for their skill and labour, &c. they undertook and promised, &c; but the defendants not regarding their promise and undertaking, and the duty of their business and employment, so ignorantly and unskilfully treated the plaintiff, that they ignorantly and unskilfully broke and disunited the callous of the plaintiff's leg after it was set, and the callous formed, whereby he is damaged. The defendants pleaded not guilty, whereupon issue was joined, which was tried before the Lord Chief Justice Wilmut, and a verdict found for the plaintiff, damages 500*l*. The substance of the evidence for the plaintiff at the trial was, first a sur-

geon was called, who swore that the plaintiff having broken both the bones of one of his legs, this witness set the same; that the plaintiff was under his hands nine weeks; that in a month's time after the leg was set, he found the leg was healing and in a good way; the callous was formed; there was a little protuberance, but not more than usual, upon cross examination he said he was instructed in surgery by his father, that the callous was the uniting the bones, and that it was very dangerous to break or disunite the callous after it was formed.

John Latham, an apothecary, swore he attended the plaintiff nine weeks, who was then well enough to go home; that the bones were well united; that he was present with the plaintiff and defendants, and at first the defendants said the plaintiff had fallen into good hands; the second time he saw them all together the defendants said the same; but when he saw them together a third time there was some alteration; he said the plaintiff was then in a passion, and was unwilling to let the defendants do any thing to his leg; he said he had known such a thing done as disuniting the callous, but that had been only when a leg was set very crooked, but not where it was straight.

\* \* \*

The daughter of the plaintiff swore, that the defendant Stapleton was first sent for to take off the bandage from the plaintiff's leg; when he came he declined to do it himself, and desired the other defendant Baker might be called in to assist; when Baker came he sent for the machine that was mentioned; plaintiff offered to give Baker a guinea, but Stapleton advised him not to take it then, but said they might be paid all together when the business was done; that the third time the defendants came to the plaintiff, Baker took up the plaintiff's foot in both his hands and nodded to Stapleton, and then Stapleton took the plaintiff's leg upon his knee, and the leg gave a crack, when the plaintiff cried out to them and said, "You have broke what nature had formed"; Baker then said to the plaintiff, "You must go through the operation of extension," and Stapleton said, "We have consulted and done for the best."

\* \* \*

Another surgeon was called, who swore, that when the callous is formed to any degree, it is difficult to break it, and the callous in this case must have been formed, or it would not

have given a crack, and said extension was improper; and if the patient himself had asked him to do it, he would have declined it; and if the callous had not been hard, he would not have done it without the consent of the plaintiff; that compression was the proper way, and the instrument improper: he said the defendant Baker was eminent in his profession. . . .

\* \* \*

. . . When we consider the good character of Baker, we cannot well conceive why he acted in the manner he did; but many men very skilful in their profession have frequently acted out of the common way for the sake of trying experiments. Several of the witnesses proved that the callous was formed, and that it was proper to remove plaintiff home; that he was free from pain, and able to walk with crutches. We cannot conceive what the nature of the instrument made use of is: why did Baker put it on, when he said that plaintiff had fallen into good hands, and when plaintiff only sent for him to take off the bandage? It seems as if Mr. Baker wanted to try an experiment with this new instrument.

2dly, it is objected, that this is not the proper action, and that it ought to have been trespass vi & armis. In answer to this, it appears from the evidence of the surgeons that it was improper to disunite the callous without consent; this is the usage and law of surgeons: then it was ignorance and unskilfulness in that very particular, to do contrary to the rule of the profession, what no surgeon ought to have done; and indeed it is reasonable that a patient should be told what is about to be done to him, that he may take courage and put himself in such a situation as to enable him to undergo the operation. It was objected, this verdict and recovery cannot be pleaded in bar to an action of trespass vi & armis to be brought for the same damage; but we are clear of opinion it may be pleaded in bar. That the plaintiff ought to receive a satisfaction for the injury, seems to be admitted; but then it is said, the defendants ought to have been charged as trespassers vi & armis. The Court will not look with eagle's eyes to see whether the evidence applies exactly or not to the case, when they can see the plaintiff has obtained a verdict for such damages as he deserves, they will establish such verdict if it be possible. For any thing that appears to the Court, this was the first experiment made with this new instrument; and if it was, it was a rash action, and he who acts rashly acts ignorantly: and although the defendants in

general may be as skilful in their respective professions as any two gentlemen in England, yet the Court cannot help saying, that in this particular case they have acted ignorantly and unskilfully, contrary to the known rule and usage of surgeons.

Judgment for the plaintiff per totam Curiam.

## NOTES

### NOTE 1.

CARPENTER V. BLAKE

60 BARB. 488 (N.Y. SUP. CT. 1871),  
*reversed on other grounds*, 50 N.Y. 696 (1872)

By the Court, MULLIN, P. J.

\* \* \*

Much was said on the argument, as to the right of a surgeon to exercise his own judgment as to the mode of treatment he will adopt in the case of a wound, or of a disease which he is called upon to treat; that neither the rules prescribed by writers, nor those acted upon by other physicians or surgeons, can apply to every case, and hence latitude must be allowed for the application of remedies which the attending physician or surgeon has found to be beneficial. If this is not allowed, the argument is, that all progress in the practice of surgery or physic must cease, and the afflicted lose altogether the benefits of experience and of remedies that science furnishes for the alleviation of human suffering. It must be conceded that if a surgeon is bound, at the peril of being liable for malpractice, to follow the modes of treatment which writers and practitioners have prescribed, the patient may lose the benefits of recent improvements in the treatment of diseases, or discoveries in science, by which new remedies have been brought into use; but this danger is more apparent than real. Some standard, by which to determine the propriety of treatment, must be adopted; otherwise experience will take the place of skill, and the reckless experimentalist the place of the educated, experienced practitioner. If the case is a new one, the patient must trust to the skill and experience of the surgeon he calls; so must he if the injury or the disease is attended with injury to other parts, or other diseases have developed themselves, for which there is no established mode of treatment. But when the case is one as to which a system of treatment has been followed for a long time, there should be no departure from it, unless the surgeon who does it is prepared to take

the risk of establishing, by his success, the propriety and safety of his experiment.

The rule protects the community against reckless experiments, while it admits the adoption of new remedies and modes of treatment only when their benefits have been demonstrated, or when, from the necessity of the case, the surgeon or physician must be left to the exercise of his own skill and experience.

\* \* \*

NOTE 2.

JACKSON V. BURNHAM  
20 COLO. 532, 39 PAC. 577 (1895),  
reversing 28 PAC. 250 (1891)

GODDARD, J.

\* \* \*

In this connection . . . we notice instruction No. 16 . . . : "That, if writers on . . . treatment . . . , or practical surgeons prescribe a mode of treatment, it is incumbent on surgeons called on to treat such an ailment to conform to the system of treatment thus established, and, if they depart from it, they do so at their peril." The learned writer of the opinion of the court of appeals condemns this instruction, because it contravenes the rule that the criterion by which to judge of the correctness of a particular mode of treatment must be one universally adopted by the profession, and that the language used in the instruction may be construed to mean that a treatment prescribed by some writers or some surgeons may not be departed from without peril, and for the further reason that, if sustained, the rule announced will prohibit further progress in surgery. We do not think the language used should be construed, or that the jury could have understood it to mean, that a treatment laid down by some writers or practiced by some surgeons should control, but that it clearly conveys the idea that the mode of treatment meant is one which writers and the profession universally commend. With this construction the rule announced is correct. There must be some criterion by which to test the proper mode of treatment in a given case; and, when a particular mode of treatment is upheld by a consensus of opinion among the members of the profession, it should be followed by the ordinary practitioner; and, if a physician sees fit to experiment with some other mode, he should do so at his peril. In other words, he must be able, in the case of deleterious results, to satisfy the jury that he had reason for the faith that was in him, and

justify his experiment by some reasonable theory. . . .

\* \* \*

NOTE 3.

ALLEN V. VOJE  
114 WIS. 1, 12, 89 N.W. 924, 931 (1902)

DODGE, J.

\* \* \*

The tenth assignment of error is predicated upon certain instructions to the jury. . . .

. . . The asserted vice in the first of these appears most strongly in the following quotation: "A departure from approved methods in general use, if it injures the patient, will render him [the physician] liable, however good his intentions may have been." This is criticised because it makes the physician liable in case he adopts new methods, although improved ones, and counsel suggest that no progress in medicine is possible if physicians must adhere to ancient methods; that vaccination or the use of anti-toxin, however wise and generally helpful, would, under that doctrine, have been malpractice originally. The instruction, viewed in the light of the rest of the charge, of course, excludes the idea of liability for variations from customary practice merely in the way of increased precautions, recognized as such. Its only application, in the light of the evidence, must have been to the omission of precautions such as it was testified other physicians uniformly took, or in the deviation from such practice by making an operation or curettement under the circumstances presented. . . .

We have little doubt that, if the first case of vaccination had proved disastrous and injured the patient, the physician should have been held liable. Nor do we believe that a physician of standing and loyalty to his patients will subject them to mere experiment, the safety or virtue of which has not been established by experience of the profession, save possibly when the patient is in extremis, and fatal results substantially certain unless the experiment may succeed.

\* \* \*

NOTE 4.

FORTNER V. KOCH  
272 MICH. 273, 261 N.W. 762 (1935)

EDWARD M. SHARPE, JUSTICE

\* \* \*

It is the duty of a physician or surgeon in diagnosing a case to use due diligence in ascer-

taining all available facts and collecting data essential to a proper diagnosis. The instant case, not being an emergency and the defendant not having used such diligence in availing himself of various methods of diagnosis for discovering the nature of the ailment as are practiced by physicians and surgeons of skill and learning in the community in which he practiced, he must be held liable for the damages due to his negligence.

We recognize the fact that, if the general practice of medicine and surgery is to progress, there must be a certain amount of experimentation carried on; but such experiments must be done with the knowledge and consent of the patient or those responsible for him, and must not vary too radically from the accepted method of procedure. One who claims to be a specialist insofar as diagnosing a case is concerned must also be held to the above rule.

\* \* \*

#### NOTE 5.

##### FIorentino v. Wenger

26 App. Div.2d 693, 272 N.Y.S.2d 557 (Sup. Ct. 1966), *reversed as to defendant hospital*, 19 N.Y.2d 407, 227 N.E.2d 296 (1967)

#### MEMORANDUM BY THE COURT.

\* \* \*

It was virtually undisputed that plaintiff's decedent, her fourteen-year-old son, was caused to suffer an exsanguinating hemorrhage as the result of an operation performed upon him by the defendant physician at the defendant hospital for the purpose of correcting the decedent's scoliotic condition. The surgery performed was not the generally accepted medical treatment in the community for scoliosis, but was a procedure utilized in this country only by the defendant physician, who had first developed it five years before the operation and death in the case at bar.

It was also virtually undisputed that over the course of those five years, in at least five of the thirty-five instances in which the procedure was utilized by the defendant physician prior to the operation here involved, there had been unexpected and untoward results. Approximately one year before the operation herein, one of the defendant physician's patients had been caused to suffer an immediate paralysis when one of the screws inserted into her vertebral column to anchor the steel bar or "spinal jack" (which was in-

tended to support and hold the spine in a straight position) pierced the spinal canal and severed the spinal cord. As a result, the hospital where that operation had been performed withdrew permission for the defendant physician's use of its facilities for this type of procedure.

We are of the opinion that, under the facts and circumstances disclosed by this record, including the fact that no immediate emergency existed, the defendant physician was obligated to make a disclosure to the parents of his infant patient that the procedure he proposed was novel and unorthodox and that there were risks incident to or possible in its use. . . .

\* \* \*

## 2.

### The Genesis of "Informed Consent"

#### a.

##### Natanson v. Kline

186 Kan. 393, 350 P.2d 1093 (1960)

SCHROEDER, JUSTICE.

This is an action for malpractice against a hospital and the physician in charge of its radiology department to recover for injuries sustained as the result of radiation therapy with radioactive cobalt, alleged to have been given in an excessive amount.

The plaintiff (appellant), Irma Natanson, suffering from a cancer of the breast, had a radical left mastectomy performed on May 29, 1955. At the direction of Dr. Crumpacker, the surgeon who performed that operation, the plaintiff engaged Dr. John R. Kline, a radiologist, for radiation therapy to the site of the mastectomy and the surrounding areas.

Dr. Kline, a licensed physician and specialist in radiation therapy, was head of the radiology department at St. Francis Hospital at Wichita, Kansas. The plaintiff seeks damages for injuries claimed to have been sustained as a result of alleged acts of negligence in the administration of the cobalt radiation treatment. Dr. Kline and the hospital were named as defendants (appellees).

The case was tried to a jury which returned a verdict in favor of both defendants. The plaintiff's motion for a new trial having been denied, this appeal followed specifying various trial errors.

\* \* \*

One of the alleged grounds of negligence, concerning which there was evidence before the jury, was that Dr. Kline failed to warn the appellant the course of treatment which he undertook to administer involved great risk of bodily injury or death.

The appellant requested and the trial court refused to give the following instruction:

You are instructed that the relationship between physician and patient is a fiduciary one. The relationship requires the physician to make a full disclosure to the patient of all matters within his knowledge affecting the interests of the patient. Included within the matters which the physician must advise the patient are the nature of the proposed treatment and any hazards of the proposed treatment which are known to the physician. Every adult person has the right to determine for himself or herself whether or not he will subject his body to hazards of any particular medical treatment.

You are instructed that if you find from the evidence that defendant Kline knew that the treatment he proposed to administer to plaintiff involved hazard or danger he was under a duty to advise plaintiff of that fact and if you further find that defendant Kline did not advise plaintiff of such hazards then defendant Kline was guilty of negligence.

There was evidence from which the jury could have found that the appellant fully appreciated the danger and the risk of the radiation treatment. The appellant's husband testified:

Q: Yes, how did it happen you went there for the conference with Dr. Kline? A: We, of course, made a periodic visit to Dr. Crumpacker after the operation, and he told us that as a precautionary measure Mrs. Natanson should go to the St. Francis Hospital and take the cobalt treatment. He explained to us that the cobalt was a new therapy; that it was much more powerful than the x-ray they had used previously. He suggested we see Dr. Kline.

On cross examination he testified:

Q: Just a question or two. Mr. Natanson, when you and your wife went to see Dr. Crumpacker, did you have a discussion with him about the purpose of the irradiation? A: Yes.

Q: And, was the general objective of irradiation explained to you? A: Yes.

Q: And, that was when Mrs. Natanson was with you? A: Yes.

Q: Now, did you consult any radiologist other than Dr. Kline in determining anything about this irradiation? A: No, sir.

\* \* \*

Q: Now, I take it that it was Dr. Crumpacker's thought or suggestion at least to you that Dr. Kline be consulted? A: Yes.

Q: And, up to the time you engaged Dr. Kline, Dr. Crumpacker had been the doctor on the case? A: Yes.

There was also testimony from the appellant and her husband that Dr. Kline did not inform the appellant the treatment involved any danger whatever. The testimony of Dr. Kline, a radiologist with special training in cobalt irradiation, was that he knew he was "taking a chance" with the treatment he proposed to administer and that such treatment involved a "calculated risk." He testified there was always a danger of injury in the treatment of cancer. Insofar as the record discloses Dr. Kline did not testify that he informed the appellant the treatment involved any danger. His only testimony relevant thereto was the following:

Q: Now, tell us what transpired when you first met with the Natansons? A: I could not completely recall that meeting. It was such a long time ago.

Q: Just tell us what you can recall of it? A: I remember Mr. and Mrs. Natanson coming in to see me. I can't remember if I met them in my office or whether we were downstairs. I remember in a very vague way. I remember in a vague way that we discussed the treatment, about how long it took, the number of areas we would irradiate. I have a recollection of that. I remember we took her into the treatment room. She was marked out, measured. I believe the marking out and measurement was done by Mr. Darter. Her first treatment occurred the first day she came. I am not sure of that but I think so.

Q: Have you told us everything you recall? A: Yes.

No other evidence appears in the record concerning the subject.

The appellees argue that we are here concerned with a case where the patient consented to the treatment, but afterwards alleges that the nature and consequences of the risks of the treatment were not properly explained to her. They point out this is not an action for assault and battery, where a patient has given no consent to the treatment.

What appears to distinguish the case of the unauthorized surgery or treatment from traditional assault and battery cases is the fact that in almost all of the cases the physician is acting in relatively good faith for the benefit of the patient. While it is true that in some cases the results are not in fact beneficial to a patient, the courts have repeatedly stated that doctors are not insurers. The traditional assault and battery involves a defendant who is acting for the most part out of malice or in a manner generally con-

sidered as "anti-social." One who commits an assault and battery is not seeking to confer any benefit upon the one assaulted.

The fundamental distinction between assault and battery, on the one hand, and negligence such as would constitute malpractice, on the other, is that the former is intentional and the latter unintentional. . . .

We are here concerned with a case where the patient consented to the treatment, but alleges in a malpractice action that the nature and consequences of the risks of the treatment were not properly explained to her. This relates directly to the question whether the physician has obtained the informed consent of the patient to render the treatment administered.

The treatment of a cancer patient with radioactive cobalt is relatively new. Until the use of atomic energy appeared in this country, X ray was the type of radiation treatment used for such patients. Radioactive cobalt is manufactured by the Atomic Energy Commission in a neutron pile by bombarding the stable element of cobalt in its pure state. This makes the cobalt unstable and by reason thereof it is radioactive. The radioactive cobalt emits two homogeneous beams of pure energy called gamma rays, very close in character, which are far more powerful than the ordinary X rays. It produces no other rays to be filtered out. This makes it desirable for use in the treatment of cancer patients. The cobalt machine may be compared to a three-million volt X ray machine.

Radioactive cobalt is so powerful that the Atomic Energy Commission specifies the construction of the room in which the cobalt unit is to be placed. The walls of the room are made of concrete forty inches thick and the ceiling, also concrete, is twenty-four inches thick. The room is sunken down in a courtyard outside the hospital. A passageway off the control room about ten feet long leads to the treatment room. All controls are placed in the outer control room and, when the radiation treatment is administered to a patient, the operator in the outer room looks through a specially designed thick lead quartz glass which gives a telescopic view. A periodic report of radiation outside the room must be made to the Atomic Energy Commission in accordance with regulations. These facts were given by Dr. Kline in his testimony.

These facts are not commonly known and a patient cannot be expected to know the hazards or the danger of radiation from radioactive cobalt unless the patient is informed by a radiolo-

gist who knows the dangers of injury from cobalt irradiation. While Dr. Kline did not testify that the radiation he gave the appellant caused her injury, he did state cobalt irradiation could cause the injury which the appellant did sustain.

What is the extent of a physician's duty to confide in his patient where the physician suggests or recommends a particular method of treatment? What duty is there upon him to explain the nature and probable consequences of that treatment to the patient? To what extent should he disclose the existence and nature of the risks inherent in the treatment?

We have been cited to no Kansas cases, nor has our research disclosed any, dealing directly with the foregoing questions. A recent article by William A. Kelly published in the *Kansas Law Review* entitled "The Physician, The Patient, And The Consent" (8 *Kan. L. Rev.* 405) reviews many malpractice cases dealing with the consent of the patient, but the article fails to deal with the problem of disclosure involving on one hand the right of the patient to decide for himself and on the other a possible therapeutic ground for withholding information which may create tension by depressing or exciting the patient. . . .

The courts frequently state that the relation between the physician and his patient is a fiduciary one, and therefore the physician has an obligation to make a full and frank disclosure to the patient of all pertinent facts related to his illness. We are here concerned with a case where the physician is charged with treating the patient without consent on the ground the patient was not fully informed of the nature of the treatment or its consequences, and, therefore, any "consent" obtained was ineffective. An effort will be made to review the cases from foreign jurisdictions most nearly in point with the question presently at hand, although none may be said to be directly in point.

In 1958 the Supreme Court of Minnesota, in *Bang v. Charles T. Miller Hospital*, 251 Minn. 427, 88 N.W.2d 186, had an assault case before it, and though not alleged as a malpractice action for negligence, a new trial was granted on the ground that a fact issue was presented for the jury to determine whether the patient consented to the performance of the operation. There the patient went to a urologist because of urinary trouble and apparently consented to a cystoscopic examination and a prostate operation. He was not informed that part of the procedure of a transurethral prostatic resection would be the ty-



ing off of his sperm ducts. In the opinion the court said:

While we have no desire to hamper the medical profession in the outstanding progress it has made and continues to make in connection with the study and solution of health and disease problems, it is our opinion that a reasonable rule is that, where a physician or surgeon can ascertain in advance of an operation alternative situations and no immediate emergency exists, a patient should be informed of the alternative possibilities and given a chance to decide before the doctor proceeds with the operation. By that we mean that, in a situation such as the case before us where no immediate emergency exists, a patient should be informed before the operation that if his spermatic cords were severed it would result in his sterilization, but on the other hand if this were not done there would be a possibility of an infection which could result in serious consequences. Under such conditions the patient would at least have the opportunity of deciding whether he wanted to take the chance of a possible infection if the operation was performed in one manner or to become sterile if performed in another. 251 Minn. at pages 434, 435, 88 N.W. 2d at page 190.

A malpractice action was before the Fifth Circuit Court in *Lester v. Aetna Casualty & Surety Company*, 240 F.2d 676. The patient was given electro-shock treatments prescribed by a psychiatrist and suffered a bad result. In affirming the jury's finding the court held the patient's wife gave sufficient legal consent, and said:

The basic, the fundamental, difficulty which confronts plaintiff on this appeal is that he presents his case as though it were one of a person being deprived by another of due process of law instead of grounding it upon the well settled principles that a physician must, except in real and serious emergencies, acquaint the patient or, when the circumstances require it, some one properly acting for him, of the diagnosis and the treatment proposed, and obtain consent, thereto express or implied, and, consent obtained must proceed in accordance with proper reasonable medical standards and in the exercise of due care. . . . 240 F.2d at page 679. (Emphasis added.)

The appellees rely upon the Canadian case of *Kenny v. Lockwood* [1932], 1 D.L.R. 507, where a patient alleged the defendants falsely and recklessly, without caring whether it was true or false, and without reasonable ground for believing it to be true, represented the operation to be "simple," and that her hand "would be all right in three weeks." No evidence was presented to suggest fraud or recklessness and the plaintiff's argument proceeded mainly upon the duty which

it was said the defendants owed to the plaintiff, due to the peculiar relation set up between a surgeon and his patient. The Ontario trial judge concluded that it was the duty of the defendant doctors to "enlighten the patient's mind in a plain and reasonable way as to what her ailment was, as to what were the risks of operating promptly, what were the risks of delaying the operation, and what the risks of not operating at all. Having discharged that duty, it was their further duty to secure from the patient a decision or consent as to what course is to be followed, and if that decision or consent is not had and the surgeons operate and the operation turns out badly the surgeons are liable. Such a relationship is established between a person of special skill and knowledge and a person of no skill or knowledge upon the facts required for the making of a decision that, unless the person with the special skill and knowledge discharges the duty which he owes of placing the patient in a position to make a decision, that person, when he is employed and paid because of his special skill and knowledge, has failed to perform his duty, and that breach of duty makes him liable in damages for untoward results." (*Kenny v. Lockwood Clinic Ltd.* [1931], 4 D.L.R. 906, 907.)

The trial court found for the plaintiff but on appeal the judgment was reversed, the appellate court saying there was some testimony that the doctors had explained all details to the plaintiff, although the extracts contained in the opinion indicate that the doctor admitted to having said that the operation was not a very serious one and that he had not clearly presented the alternatives to the plaintiff. In the court's opinion it was said:

[T]he duty cast upon the surgeon was to deal honestly with the patient as to the necessity, character and importance of the operation and its probable consequences and whether success might reasonably be expected to ameliorate or remove the trouble, but that such duty does not extend to warning the patient of the dangers incident to, or possible in, any operation, nor to details calculated to frighten or distress the patient. (p. 525.)

The court concluded upon the evidence presented:

That the defendant Stoddart reasonably fulfilled the duty laid upon him arising out of the relationship of surgeon and patient, not being guilty of "negligence in word" or "economy of truth" nor of misleading the plaintiff, and so is not liable for breach of the duty. . . . (p. 526.)

In the opinion it was said the duty of a surgeon is to be honest in fact and to express his honest belief, and if he does so he ought not to be judged as if he had warranted a perfect cure nor to be found derelict in his duty on any meticulous criticism of his language.

The conclusion to be drawn from the foregoing cases is that where the physician or surgeon has affirmatively misrepresented the nature of the operation or has failed to point out the probable consequences of the course of treatment, he may be subjected to a claim of unauthorized treatment. But this does not mean that a doctor is under an obligation to describe in detail all of the possible consequences of treatment. It might be argued, as indicated by the authors of the various law review articles heretofore cited, that to make a complete disclosure of all facts, diagnoses and alternatives or possibilities which may occur to the doctor could so alarm the patient that it would, in fact, constitute bad medical practice. There is probably a privilege, on therapeutic grounds, to withhold the specific diagnosis where the disclosure of cancer or some other dread disease would seriously jeopardize the recovery of an unstable, temperamental or severely depressed patient. But in the ordinary case there would appear to be no such warrant for suppressing facts and the physician should make a substantial disclosure to the patient prior to the treatment or risk liability in tort.

Anglo-American law starts with the premise of thoroughgoing self-determination. It follows that each man is considered to be master of his own body, and he may, if he be of sound mind, expressly prohibit the performance of life-saving surgery, or other medical treatment. A doctor might well believe that an operation or form of treatment is desirable or necessary but the law does not permit him to substitute his own judgment for that of the patient by any form of artifice or deception.

The mean between the two extremes of absolute silence on the part of the physician relative to the treatment of a patient and exhaustive discussion by the physician explaining in detail all possible risks and dangers was well stated by the California District Court of Appeal in *Salgo v. Leland Stanford, Etc. Bd. Trustees*, 1957, 154 Cal. App. 2d 560, 317 P.2d 170. There the court had before it a malpractice action wherein the defendants were charged with *negligence*. The patient, his wife and son testified that the patient was not informed anything in the nature of an aortography was to be performed. Two of the

doctors contradicted this, although admitting that the details of the procedure involving injection of a radio-opaque substance into the aorta and the possible dangers therefrom were not explained. As a result of the aortography the patient was paralyzed from the waist down. The trial court gave a rather broad instruction on the duty of the physician to disclose to the patient "all the facts which mutually affect his rights and interests and of the surgical risk, hazard and danger, if any." 154 Cal. App. 2d at page 578, 317 P.2d at page 181. On appeal, the instruction was held to be overly broad, the court stating:

... A physician violates his duty to his patient and subjects himself to liability if he withholds any facts which are necessary to form the basis of an intelligent consent by the patient to the proposed treatment. Likewise the physician may not minimize the known dangers of a procedure or operation in order to induce his patient's consent. At the same time, the physician must place the welfare of his patient above all else and this very fact places him in a position in which he sometimes must choose between two alternative courses of action. One is to explain to the patient every risk attendant upon any surgical procedure or operation, no matter how remote; this may well result in alarming a patient who is already unduly apprehensive and who may as a result refuse to undertake surgery in which there is in fact minimal risk; it may also result in actually increasing the risks by reason of the physiological results of the apprehension itself. The other is to recognize that each patient presents a separate problem, that the patient's mental and emotional condition is important and in certain cases may be crucial, and that in discussing the element of risk a certain amount of discretion must be employed consistent with the full disclosure of facts necessary to an informed consent. . . .

The instruction given should be modified to inform the jury that the physician has such discretion consistent, of course, with the full disclosure of facts necessary to an informed consent. 154 Cal.App.2d at page 578, 317 P.2d at page 181.

The appellees rely upon *Hunt v. Bradshaw*, 1955, 242 N.C. 517, 88 S.E.2d 762, a North Carolina case. This was a malpractice action against a physician wherein the patient sought damages alleged to have resulted from the negligent failure of the defendant (1) to use reasonable care and diligence in the application of his knowledge and skill as a physician and surgeon, and (2) to exercise his best judgment in attempting to remove a small piece of steel from plaintiff's body. On these allegations of negligence the plaintiff contended, among other things, that the defendant advised the plaintiff

the operation was simple, whereas it was serious and involved undisclosed risks. The plaintiff's evidence was sufficient to justify a finding the operation was of a very serious nature. The court after reviewing the evidence said:

... Upon Dr. Bradshaw's advice the operation was decided upon. It is understandable the surgeon wanted to reassure the patient so that he would not go to the operating room unduly apprehensive. Failure to explain the risks involved, therefore, may be considered a mistake on the part of the surgeon, but under the facts cannot be deemed such want of ordinary care as to import liability.

Proof of what is in accord with approved surgical procedure and what constitutes the standard of care required of the surgeon in performing an operation, like the advisability of the operation itself, are matters not within the knowledge of lay witnesses but must be established by the testimony of qualified experts. . . .

Plaintiff's expert testimony is sufficient to justify the finding the injury and damage to plaintiff's hand and arm resulted from the operation. But, as in cases of ordinary negligence, the fact that injury results is not proof the act which caused it was a negligent act. The doctrine *res ipsa loquitur* does not apply in cases of this character. . . .

Of course, it seems hard to the patient in apparent good health that he should be advised to undergo an operation, and upon regaining consciousness finds that he has lost the use of an arm for the remainder of his life. Infallibility in human beings is not attainable. The law recognizes, and we think properly so, that the surgeon's hand, with its skill and training, is, after all, a human hand, guided by a human brain in a procedure in which the margin between safety and danger sometimes measures little more than the thickness of a sheet of paper.

The plaintiff's case fails because of lack of expert testimony that the defendant failed, either to exercise due care in the operation, or to use his best judgment in advising it. . . . 242 N.C. at pages 523, 524, 88 S.E. 2d at page 766.

\* \* \*

In our opinion the proper rule of law to determine whether a patient has given an intelligent consent to a proposed form of treatment by a physician was stated and applied in *Salgo v. Leland Stanford, Etc. Bd. Trustees*, supra. This rule in effect compels disclosure by the physician in order to assure that an informed consent of the patient is obtained. The duty of the physician to disclose, however, is limited to those disclosures which a reasonable medical practitioner would make under the same or similar circumstances. How the physician may best discharge

his obligation to the patient in this difficult situation involves primarily a question of medical judgment. So long as the disclosure is sufficient to assure an informed consent, the physician's choice of plausible courses should not be called into question if it appears, all circumstances considered, that the physician was motivated only by the patient's best therapeutic interests and he proceeded as competent medical men would have done in a similar situation.

Turning now to the facts in the instant case, the appellant knew she had a cancerous tumor in her left breast which was removed by a radical mastectomy. Pathological examination of the tissue removed did not disclose any spread of the cancer cells into the lymphatics beyond the cancerous tumor itself. As a precautionary measure the appellant's ovaries and fallopian tubes were removed, which likewise upon pathological examination indicated no spread of the cancer to these organs. At the time the appellant went to Dr. Kline as a patient there was no immediate emergency concerning the administration of cobalt irradiation treatment such as would excuse the physician from making a reasonable disclosure to the patient. We think upon all the facts and circumstances here presented Dr. Kline was obligated to make a reasonable disclosure to the appellant of the nature and probable consequences of the suggested or recommended cobalt irradiation treatment, and he was also obligated to make a reasonable disclosure of the dangers within his knowledge which were incident to, or possible in, the treatment he proposed to administer.

Upon the record here presented Dr. Kline made no disclosures to the appellant whatever. He was silent. This is not to say that the facts compel a verdict for the appellant. Under the rule heretofore stated, where the patient fully appreciates the danger involved, the failure of a physician in his duty to make a reasonable disclosure to the patient would have no causal relation to the injury. In such event the consent of the patient to the proposed treatment is an informed consent. The burden of proof rests throughout the trial of the case upon the patient who seeks to recover in a malpractice action for her injury.

In considering the obligation of a physician to disclose and explain to the patient in language as simple as necessary the nature of the ailment, the nature of the proposed treatment, the probability of success or of alternatives, and perhaps the risks of unfortunate results and unforeseen

conditions within the body, we do not think the administration of such an obligation, by imposing liability for malpractice if the treatment were administered without such explanation where explanation could reasonably be made, presents any insurmountable obstacles.

The appellant's requested instruction on the duty of a physician to make a disclosure to his patients was too broad. But this did not relieve the trial court of its obligation to instruct on such issue under the circumstances here presented, since the issue was raised by the pleadings. On retrial the instruction should be modified to inform the jury that a physician has such discretion, as heretofore indicated, consistent with the full disclosure of facts necessary to assure an informed consent by the patient.

On retrial of this case the first issue for the jury to determine should be whether the administration of cobalt irradiation treatment was given with the informed consent of the patient, and if it was not the physician who failed in his legal obligation is guilty of malpractice no matter how skillfully the treatment may have been administered, and the jury should determine the damages arising from the cobalt irradiation treatment. If the jury should find an informed consent was given by the patient for such treatment, the jury should next determine whether proper skill was used in administering the treatment.

\* \* \*

The judgment of the lower court is reversed with directions to grant a new trial.

b.

**Natanson v. Kline**  
**187 Kan. 186, 354 P.2d 670 (1960)**

SCHROEDER, JUSTICE.

Within the time allotted after the decision of the court herein was announced the appellees filed motions for rehearing. Thereafter, pursuant to request, leave was granted the Kansas Medical Society on May 12, 1960, to file its brief amicus curiae in support of the appellees' motions for rehearing. Finding nothing, upon consideration of the motions for rehearing and the brief of amicus curiae in support thereof, which warrants a reconsideration of the case, the motions for rehearing are denied.

Recognizing, however, that this is a case of first impression in Kansas and one establishing judicial precedent of the highest importance to the medical profession, an attempt will be

made to clarify [the] portion of the opinion concerning which counsel are apprehensive.

Perhaps in preoccupation over the legal obligation of a physician to his patient, the court has not adequately emphasized procedural aspects of the case, or reiterated fundamental doctrine in the law of negligence sufficiently to completely avoid efforts to misconstrue the opinion.

It is charged that the court has confused a malpractice suit, where negligence is an essential element, with an assault and battery case, where negligence is not an essential element, thereby giving rise to a hybrid action which is neither one of negligence nor one of assault and battery, but may be a combination of the two.

It is argued the only way the court's opinion can be justified is to say that the duty of a physician to disclose to his patient the risks and hazards of a proposed form of treatment is an absolute one, and the matter is not to be judged by such disclosures as a reasonable medical practitioner would make under the same or similar circumstances.

In support of the argument, that the court has imposed an absolute duty upon the physician, the following paragraph is isolated from context:

On retrial of this case the first issue for the jury to determine should be whether the administration of cobalt irradiation treatment was given with the informed consent of the patient, and if it was not the physician who failed in his legal obligation is guilty of malpractice no matter how skillfully the treatment may have been administered, and the jury should determine the damages arising from the cobalt irradiation treatment. If the jury should find an informed consent was given by the patient for such treatment, the jury should next determine whether proper skill was used in administering the treatment. *Natanson v. Kline*, 186 Kan. 393, 411, 350 P.2d 1093, 1107.

A casual reading of this paragraph in context would indicate that reference is there being made to the order in which the jury is to consider the issues presented on retrial of the case, and not to an enumeration of the various elements which must be established by the evidence to prove each of the issues stated.

The gravamen of the plaintiff's complaint was malpractice or the failure of the defendants to properly perform the duties which devolved upon them—a failure which resulted in the alleged injuries to the plaintiff. Thus it was incumbent upon the plaintiff to prove and establish (1) that the defendants failed to perform their duty; and (2) that the plaintiff's injuries were

the direct and proximate result of such failure.

The petition alleged that the injuries were "a direct and proximate result of the defendants' negligence and carelessness" and then set forth eight specific grounds of negligence, including:

(g) He [Dr. Kline] failed to warn plaintiff that the course of treatment which he undertook to administer involved great risk of bodily injury or death.

The answers of both defendants denied generally the allegations of asserted negligence, and in addition thereto, affirmatively pleaded that the plaintiff "assumed the risk and hazard of the treatment." Thus, at the trial the defendants were fully aware that *the informed consent of the patient to the hazards of the treatment was an issue of fact in the case*. This is true because as a defense assumption of risk is applicable only where the plaintiff is equally competent with the defendant to judge concerning the risks and hazards. See, *Taylor v. Hostetter*, 186 Kan. 788, 352 P.2d 1042, and cases cited therein. These affirmative allegations of the defendants presupposed an informed consent by the patient with full knowledge of the risks and hazards of the treatment.

The court held after reviewing the record presented on this appeal that a physician violates his duty to his patient and *subjects himself to liability for malpractice*, where no immediate emergency exists and upon facts and circumstances particularly set forth in the opinion, if he makes no disclosure of significant facts within his knowledge which are necessary to form the basis of an intelligent consent by the patient to the proposed form of treatment.

In other words, on the facts and circumstances presented by the record the appellant was entitled to some explanation concerning the risks and hazards inherent in the administration of cobalt irradiation treatment which Dr. Kline proposed to administer to her. For this treatment she was Dr. Kline's patient and not the patient of Dr. Crumpacker by whom she was referred to Dr. Kline.

The appellant was entitled to a reasonable disclosure by Dr. Kline so that she could intelligently decide whether to take the cobalt irradiation treatment and assume the risks inherent therein, or in the alternative to decline this form of precautionary treatment and take a chance that the cancerous condition in her left breast had not spread beyond the lesion itself which had been removed by surgery. There was no emergency calling for immediate attention. The

appellant had recovered from the surgery. In addition to the evidence related in the opinion her husband testified:

Q: Now directing your attention to approximately the 5th or 6th day of June, 1955, I would like to have you describe for us the general apparent condition of the health of Mrs. Natanson. A: Mrs. Natanson at that particular time was very, very well. She had gone through the two operations and had made a very, very fine recovery. She was able to use her arm because of the therapy; she had almost the complete use of the left arm again. The breast had healed fully. There were actually no scars—just the one large scar but there was a thickness there. We were living a very normal life after the big scare we had.

Q: Now, directing your attention to the first week of June, 1955, I will ask you whether or not Mrs. Natanson ever recovered to the point where she was able to do her own housework? A: Yes, she had.

But contrary to the legal obligation imposed upon a physician to make a reasonable disclosure to his patient of the inherent risks and hazards of a proposed form of treatment, Dr. Kline gave the appellant no explanation whatever. He made no disclosures. He was silent. On this state of the record Dr. Kline failed in his legal duty to make a reasonable disclosure to the appellant who was his patient *as a matter of law*.

Conceivably, in a given case as indicated in the opinion, no disclosures to a patient may be justified where such practice, under given facts and circumstances, is established by expert testimony to be in accordance with that of a reasonable medical practitioner under the same or similar circumstances. But on the state of the record here presented the appellant was not required to produce expert medical testimony to show that the failure of Dr. Kline to give any explanation or make any disclosures was contrary to accepted medical practice. To hold otherwise would be a failure of the court to perform its solemn duty.

Whether or not a physician has advised his patient of the inherent risks and hazards in a proposed form of treatment is a *question of fact* concerning which lay witnesses are competent to testify, and the establishment of such fact is not dependent upon expert medical testimony. It is only when the facts concerning the actual disclosures made to the patient are ascertained, or ascertainable by the trier of the facts, that the expert testimony of medical witnesses is required to establish whether such disclosures are in accordance with those which a reasonable medical practitioner would make under the same or similar circumstances.

The question then remains whether such failure on the part of Dr. Kline to make a reasonable disclosure to the appellant was a proximate cause of her injury. As indicated in the opinion the mere fact that Dr. Kline was silent does not compel a verdict for the appellant. It was said:

... Under the rule heretofore stated, where the patient fully appreciates the danger involved, the failure of a physician in his duty to make a reasonable disclosure to the patient would have no causal relation to the injury. In such event the consent of the patient to the proposed treatment is an informed consent. The burden of proof rests throughout the trial of the case upon the patient who seeks to recover in a malpractice action for her injury. *Natanson v. Kline*, supra, 186 Kan. at page 410, 350 P.2d at page 1106.

Negligence is an essential element of malpractice, and the foregoing statement recognizes that a causal relation must be established by the patient, between the negligent act of the physician and the injury of the patient, to sustain the burden of proof where damages are sought in a malpractice action for injury. Prior to a discussion of the manner in which the court instructed the jury it was said in the opinion:

... At best it may be said, upon all the facts and circumstances presented by the record, there was evidence from which a jury could find that the proximate cause of the appellant's injury was the negligence of the defendants. On the other hand a jury, properly instructed, would be justified in finding for the appellees. *Natanson v. Kline*, supra, 186 Kan. at page 398, 350 P.2d at page 1098.

After making the foregoing statement in the opinion, discussion was directed to the instructions of the court without further specific attention to the issue of proximate cause. If, of course, the appellant would have taken the cobalt irradiation treatments even though Dr. Kline had warned her that the treatments he undertook to administer involved great risk of bodily injury or death, it could not be said that the failure of Dr. Kline to so inform the appellant was the proximate cause of her injury. While the appellant did not directly testify that she would have refused to take the proposed cobalt irradiation treatments had she been properly informed, we think the evidence presented by the record taken as a whole is sufficient and would authorize a jury to infer that, had she been properly informed, the appellant would not have taken the cobalt irradiation treatments.

Two days after the decision of this court was

announced, the Supreme Court of Missouri handed down its opinion in *Mitchell v. Robinson*, 334 S.W.2d 11, 12, on April 11, 1960, wherein the Missouri court reached the same conclusion as this court on the duty of a physician to inform his patient of the hazards of treatment. There the patient had a rather severe emotional illness but was not mentally incompetent. The treatment prescribed was "combined electro-shock and insulin subcoma therapy." A sharp conflict developed in the testimony as to whether the patient was informed of the risks of the treatment. Serious hazards incident to shock treatment were admitted, to wit: fractured bones, serious paralysis of limbs, irreversible coma and even death, and further that there were no completely reliable or successful precautions. The patient as a result of treatment went into convulsions which caused the fracture of several vertebrae and sued the physicians in a malpractice action on the ground that he was not informed of the risks inherent in the treatment. The "essentially meritorious problem" before the court was whether upon the record there was any evidence to support the jury's finding of negligence. In the opinion the court said:

In the particular circumstances of this record, considering the nature of Mitchell's illness and this rather new and radical procedure with its rather high incidence of serious and permanent injuries not connected with the illness, the doctors owed their patient in possession of his faculties the duty to inform him generally of the possible serious collateral hazards; and in the detailed circumstances there was a submissible fact issue of whether the doctors were negligent in failing to inform him of the dangers of shock therapy. [At page 19.]

As always, an effort is made by the court to present an opinion in logical sequence, so that consideration of subsequent issues is dependent upon the disposition of issues previously determined, and if opinions are analyzed in this manner misinterpretations will be minimized.

## NOTES

### NOTE 1.

WILLIAMS V. MENEHAN  
191 KAN. 6, 8, 379 P.2D 292, 294 (1963)

WERTZ, JUSTICE.

\* \* \*

... We said in the *Natanson* case [that] it is the duty of a doctor to make a reasonable disclosure to his patient. ... But this does not mean that a doctor is under an obligation to de-

scribe in detail all of the possible consequences of treatment. To make a complete disclosure of all facts, diagnoses and alternatives or possibilities which might occur to the doctor could so alarm the patient that it would, in fact, constitute bad medical practice.

... So long as the disclosure is sufficient to assure an informed consent, the physician's choice of plausible courses should not be called into question if it appears, all circumstances considered, that the physician was motivated only by the patient's best therapeutic interests and he proceeded as competent medical men would have done in a similar situation.

\* \* \*

#### NOTE 2.

AIKEN V. CLARY  
396 S.W. 2d 668 (Mo. 1965)

FINCH, JUDGE.

\* \* \*

We ... have concluded that the question of what disclosure of risks incident to proposed treatment should be made in a particular situation involves medical judgment and that expert testimony thereon should be required in malpractice cases involving that issue.

\* \* \*

The question is not what, regarding the risks involved, the juror would relate to the patient under the same or similar circumstances, or even what a reasonable *man* would relate, but what a reasonable *medical practitioner* would do. Such practitioner would consider the state of the patient's health, the condition of his heart and nervous system, his mental state, and would take into account, among other things, whether the risks involved were mere remote possibilities or something which occurred with some sort of frequency or regularity. This determination involves medical judgment as to whether disclosure of possible risks may have such an adverse effect on the patient as to jeopardize success of the proposed therapy, no matter how expertly performed.

#### NOTE 3.

HUNTER V. BROWN  
4 WASH. APP. 899, 484 P.2d 1162 (1971)

\* \* \*

JAMES, JUDGE:

Plaintiff, Mrs. Chung Hunter, claims damages suffered as a result of an allegedly unsuc-

cessful dermabrasion procedure performed by defendant, Walter S. Brown. Dr. Brown is a medical doctor whose specialty is plastic and reconstructive surgery. At the close of Mrs. Hunter's case, the trial judge sustained Dr. Brown's challenge to the sufficiency of the evidence. Mrs. Hunter appeals from the judgment of dismissal which followed. ...

\* \* \*

[W]e find that there was substantial evidence which, if believed, would tend to establish that during pregnancy Mrs. Hunter became concerned about dark spots of increased pigmentation which appeared upon her face. She consulted Dr. Brown who diagnosed her condition as chloasma. ...

Dr. Brown recommended and thereafter performed a surgical procedure known as "dermabrasion." Dermabrasion is a mechanical procedure whereby the epidermis is removed by sandpapering. Although performed under local anesthetic, the recovery period was for Mrs. Hunter a painful, prolonged and embarrassing experience. Rather than improving Mrs. Hunter's appearance, the dermabrasion resulted in increased pigmentation in her face.

\* \* \*

The evidence upon which Mrs. Hunter relies would permit findings that: Dr. Brown is a recognized expert in the treatment of chloasma by the dermabrasion process; that he knew (1) that the probability of a good result was only 50 per cent; (2) that there was a possibility of resulting hyperpigmentation—a worsening of the chloasma condition; (3) that the risk of hyperpigmentation is greater when the patient is of Oriental origin; and (4) that he considered Mrs. Hunter to be a "borderline case." Mrs. Hunter is of Korean extraction.

The evidence would further support a finding that Dr. Brown did not inform Mrs. Hunter that there was any possibility that the operation would not be successful, and that Mrs. Hunter was justified in assuming that there was no question but what the dark spots on her face would disappear.

Dr. Brown was called as an adverse witness. He testified that it was not good standard medical practice within the specialty of plastic and reconstructive surgery to inform a patient of the risk involved in a dermabrasion operation. He said:

Now, if we go into the risks involved, I would be talking the rest of the day about the risks. ...

\* \* \*



[R]isks are minimal, and they are never mentioned to a patient. . . .

\* \* \*

. . . A patient would walk out of everybody's office if you would say there is a danger of anything. This is never done.

\* \* \*

It is not good practice to frighten a patient by telling them a dozen different things that might happen as a result of dermabrasion.

Mrs. Hunter produced no evidence to contradict Dr. Brown's testimony.

Dr. Brown was not interrogated as to whether there was a recognized medical standard as to disclosure to a patient of the percentage probability of success. Mrs. Hunter presented no other testimony concerning medical standards.

\* \* \*

The trial judge felt that he was compelled to dismiss Mrs. Hunter's case because she produced no evidence of a medical standard of disclosure.

\* \* \*

The physician-patient relationship is of a fiduciary character. The inherent necessity for trust and confidence requires scrupulous good faith on the part of the physician. *Lockett v. Goodill*, 71 Wash.2d 654, 430 P.2d 589 (1967). His duty of disclosure extends beyond the realm of risks. He must disclose to his patient all material facts which reasonably should be known if his patient is to make an informed and intelligent decision. The availability of alternatives to surgery is an example of the kind of information required.

Whether the failure to disclose was willful or attributable to negligence is immaterial. The causal wrong is the violation of the fiduciary duty to disclose.

We cannot agree that the matter of informed consent must be determined on the basis of medical testimony any more than that expert testimony of the standard practice is determinative in any other case involving a fiduciary relationship. We agree with appellant that a physician's duty to disclose is not governed by the standard practice of the physicians' community, but is a duty imposed by law which governs his conduct in the same manner as others in a similar fiduciary relationship. To hold otherwise would permit the medical profession to determine its own responsibilities. . . .

*Berkey v. Anderson*, 1 Cal.App.3d 790, 805, 82 Cal.Rptr. 67, 78 (1969).

Reasons why the physician withheld facts are a matter of defense. Evidence that the patient was not uninformed or that the facts undisclosed were immaterial or that disclosure might be harmful would properly be matters of defense. It is at this juncture that evidence of medical standards of disclosure might become relevant and material.

\* \* \*

Dr. Brown testified that the "risks" in a dermabrasion procedure were minimal. And, as he also observed, the contemplated surgery was "an elective thing." There was no emergency. His patient's health was not at stake. Arguably, one of the facts which Mrs. Hunter needed for an informed and intelligent decision was the percentage probability that the contemplated surgery would improve her appearance. Whether or not Dr. Brown violated his fiduciary duty in withholding the information is a question of fact to be judged by reasonable man standards.

\* \* \*

We hold that if a patient-plaintiff presents substantial evidence that (1) his physician failed to disclose material facts reasonably necessary to form the basis of an intelligent consent, and (2) he has been injured as a result of submitting to a surgical procedure, he has made out a prima facie case.

Accordingly, we reverse and remand for a new trial.

#### NOTE 4.

DOW V. KAISER FOUNDATION  
12 CAL. APP.3D 488, 503-508, 90 CAL. RPTR.  
747, 756-759 (1970)

COMPTON, ASSOCIATE JUSTICE.

Plaintiff Dorothy Dow sued defendants Dr. Paul Harmon, Permanent Medical Group, a partnership, Southern California Permanent Medical Group, a partnership, and Kaiser Foundation Hospital, a non-profit corporation, seeking damages for injuries allegedly resulting from medical malpractice and the lack of an informed consent to lower back surgery performed by Dr. Harmon.

\* \* \*

Plaintiff presented to the jury through her pleadings and evidence [a] theory of liability designed to demonstrate that the operation was performed without her "informed" consent and that therefore she should recover for her injuries.

\* \* \*

It is well established that a doctor has a duty to inform his patient concerning contemplated medical procedure and the inherent risks therein.

...

\* \* \*

In California, contrary to the conclusion reached in *Kline*, the cause of action which arises from medical treatment based on an uninformed consent sounds in battery and not negligence.

The jury here was instructed that "Failure to obtain consent when and as required renders the physician and surgeon liable in damages for any injury *proximately resulting* from the operation or treatment. . . . The failure to disclose in all instances does not necessarily suggest a *neglect* of duty." (Emphasis added.)

Furthermore, the court instructed the jury that the doctor's duty was to make those "Disclosures which a competent medical practitioner would make under the same or similar circumstances. . . ." (Emphasis added.)

\* \* \*

A surgeon's negligence in performing an operation may be the cause of the resultant injuries but it does violence to logic, however, to say that the failure to inform a patient about certain risks is the proximate cause of those subsequent injuries. If the lack of sufficient information vitiates a consent the cause of action is the same as if no consent had ever been given. Thus a doctor who breaches his duty to inform, as we shall define it below, is liable for *all* injuries sustained by the patient whether the result of

negligence in the performance of the operation or not.

\* \* \*

Inasmuch as the patient is already well protected where the treatment itself is negligently performed and recovery on the basis of uninformed consent will generally occur where such negligence cannot be proved; we hold that in order for a patient to vitiate his voluntary consent to treatment on the basis that the doctor breached his duty of disclosure, it must be proved that the doctor *wilfully*, and *without good medical reason*, withheld material information.

Battery being an intentional tort requires in this situation proof of a higher degree of culpability than ordinary negligence in failing to inform a patient of some aspect of proposed medical treatment.

Further, the plaintiff must establish as part of his burden of proof that the information which was withheld was of such significance that, had it been disclosed, consent would not have been given.

It is our opinion that these standards comport squarely with the fiduciary nature of the doctor-patient relationship.

\* \* \*

In the final analysis the court's instructions did not accurately inform the jury on the law of uninformed consent. This error was prejudicial. Moreover, the evidence was insufficient to support the verdict in light of the requirements enunciated above.

\* \* \*

The judgment is reversed.

## B. To Promote Individual Autonomy

### 1.

#### Safeguarding the Concept of Freedom

a.

**John S. Mill  
On Liberty\***

\* \* \*

The object of this Essay is to assert one very simple principle, as entitled to govern ab-

solutely the dealings of society with the individual in the way of compulsion and control, whether the means used be physical force in the form of legal penalties, or the moral coercion of public opinion. That principle is, that the sole end for which mankind are warranted, individually or collectively, in interfering with the liberty of action of any of their number, is self-protection. That the only purpose for which power can be rightfully exercised over any member of a civilised community, against his will, is to prevent harm to others. His own good, either physical or moral, is not a sufficient warrant. He can-

\* London: John W. Parker & Son 21-23 (1859).

not rightfully be compelled to do or forbear because it will be better for him to do so, because it will make him happier, because, in the opinions of others, to do so would be wise, or even right. These are good reasons for remonstrating with him, or reasoning with him, or persuading him, or entreating him, but not for compelling him, or visiting him with any evil in case he do otherwise. To justify that, the conduct from which it is desired to deter him must be calculated to produce evil to someone else. The only part of the conduct of anyone, for which he is amenable to society, is that which concerns others. In the part which merely concerns himself, his independence is, of right, absolute. Over himself, over his own body and mind, the individual is sovereign.

It is, perhaps, hardly necessary to say that this doctrine is meant to apply only to human beings in the maturity of their faculties. We are not speaking of children, or of young persons below the age which the law may fix as that of manhood or womanhood. Those, who are still in a state to require being taken care of by others, must be protected against their own actions as well as against external injury. . . .

\* \* \*

b.

**Patrick Devlin**  
**The Enforcement of Morals\***

\* \* \*

The core of [Mill's] principle is that a man must be allowed to pursue his own good in his own way. Its opposite has come to be identified as paternalism. But an identifying mark is not a line. To secure the citadel of freedom Mill flung a line beyond which the law must not trespass. The law was not to interfere with a man unless what he did caused harm to others. What Mill included in "harm to others" was chiefly physical harm to other individuals.

Now if a man lives in society it is not simply his own concern whether or not he keeps himself physically, mentally, and morally fit. He owes in these respects a duty to others as well as to himself. Mill accepted the duty as owing to "assignable individuals," such as a man's family or his creditors. He did not see it as a debt due to society at large. The only right he allowed to society as a collective entity, i.e., to the State, and

which it might enforce by law, was the right to exact contributions to common defence and protection. "But with regard to the merely contingent, or, as it may be called, constructive injury which a person causes to society, by conduct which neither violates any specific duty to the public, nor occasions perceptible hurt to any assignable individual except himself, the inconvenience is one which society can afford to bear, for the sake of the greater good of human freedom."

Yet if apart from his assignable duties a man does not observe some standard of health and morality, society as a whole is impoverished for such a man puts less than his share into the common well-being. The enforcement of an obligation of this sort can be distinguished from paternalism. The motive of paternalism is to do good to the individual; the motive of the other is to prevent the harm that would be done to society by the weakness or vice or too many of its members. Mill did not overlook the distinction; he overrode it in the interests of individual freedom. If a man knew his own true interest and pursued it as he ought to, he would make himself as virtuous as he could and by so doing make his contribution to society's well-being. The right to exact such a contribution must be sacrificed on the altar of freedom. . . .

As Mill noted, this conception of liberty was not accepted in his own time which we now look back upon as an age of individualism triumphant. In the hundred years that have passed since then it has over and over again been decisively rejected in economic matters. Its weakness in practice is that it enables one man in a hundred to hold up indefinitely projects which would benefit the other ninety-nine. So we have laws that allow the compulsory acquisition of property. We have also social schemes that an individual is not allowed to contract out of because he cannot be excluded from the benefits of the scheme without wrecking it. Contracting out may be an expression of individuality and proceed from the pure desire for liberty, but we have come to think that it proceeds from selfishness or laziness, indifference to the common good, or a desire to get something for nothing. So we have health laws, thinking it wrong that a man should receive the benefit of modern sanitation in the town in which he lives and keep his own home as a pigsty.

In short, the great majority of our fellow citizens may be as highminded as Mill expected them to be but we have not yet got rid of the

\* London: Oxford University Press 103-105, 121-123, 132-136 (1965). Reprinted by permission.

troublesome minority who will yield only to compulsion. Perhaps in the course of several centuries, teaching and example will lift the minority to the common level and in the end it might have been better for us all if we had waited for that to happen. But social reformers are not as patient as philosophers and we have not waited.

This does not mean that necessarily we have witnessed the triumph of paternalism. We would still, I think, most of us deeply resent a law that was passed avowedly for our own good and treated us as if we were in need of care and protection. What it means is that the citadel has not been secured from attack in the way in which Mill proposed. His outer line enclosed territory which has had to be yielded, and authority has not decisively, as he hoped, been kept at bay.

\* \* \*

... What Mill demands is that we must tolerate what we know to be evil and what no one asserts to be good. He does not ask that in particular cases we should extend tolerance out of pity: he demands that we should cede it for ever as a right. Because it is evil we may protect youth from corruption by it, but save for that we must allow it to spread unhindered by the law and infect the minds of all those who are not strong enough to resist it. Why do ninety of us have to grant this licence to the other ten or, it would be truer to say, ninety-nine to the other one? Because, the answer is, we are fallible. We are all quite convinced that what we call vice is evil but we may be mistaken. Although no one asserts that it is not evil, yet we may be mistaken. True it is that if the waters of toleration are poured upon the muck, bad men will wallow in the bog; but it may be—how can we tell otherwise?—that it is only under such conditions that seed may flourish which some day some good man may bring to fruit and that otherwise the world would lose and be the poorer for it.

This is the kernel of Mill's freedom. This is why we must not suppress vice. It is not because it is not evil; Mill thought that it was. It is not because legal suppression would be futile; this argument, favoured by some of Mill's followers, is not one that he advanced. Nor because Mill thought, that in the battle between virtue and vice, virtue would be bound to triumph without the aid of the law. In some cogent passages he refuted the argument that in spite of persecution truth would always prevail against error; and if truth can be suppressed, so can error and so can vice. When all this is stripped away, the kernel

of Mill is just this—that he beseeches us to think it possible that we may be mistaken. Because of this possibility, Mill demanded almost absolute freedom for the individual to go his own way, the only function of society being to provide for him an ordered framework within which he might experiment in thought and in action secure from physical harm.

There is here, I humbly believe, a flaw in Mills' thinking which, even assuming that we accept his ideal, renders it unacceptable to the law-maker as a basis for action. It lies in the failure to distinguish sufficiently between freedom of thought and freedom of action. It may be a good thing for a man to keep an open mind about all his beliefs so that he will never claim for them absolute certainty and never dismiss entirely from his mind the thought that he may be wrong. But where there is a call for action, he must act on what he believes to be true. . . .

\* \* \*

... For better or worse the law-maker must act according to his lights and he cannot therefore accept Mill's doctrine as practicable even if as an ideal he thought it to be desirable.

But I must say for my part that I do not accept it as an ideal. I accept it as an inspiration. What Mill taught about the value of freedom of inquiry and the dangers of intolerance has placed all free men for ever in his debt. His admonitions were addressed to a society that was secure and strong and hidebound. Their repetition today is to a society much less solid. As a tract for the times, what Mill wrote was superb, but as dogma it has lost much of its appeal. For Mill's doctrine is just as dogmatic as any of those he repudiates. It is dogmatic to say that if only we were all allowed to behave just as we liked so long as we did not injure each other, the world would become a better place for all of us. There is no more evidence for this sort of Utopia than there is for the existence of Heaven and there is nothing to show that the one is any more easily attained than the other. We must not be bemused by words. If we are not entitled to call our society "free" unless we pursue freedom to an extremity that would make society intolerable for most of us, then let us stop short of the extreme and be content with some other name. The result may not be freedom unalloyed, but there are alloys which strengthen without corrupting.

\* \* \*

The second fundamental principle has to do

with the function of consent in the criminal law. Mill's doctrine should make it always a good defence because the law should be concerned only with harming another against his will. But in general, consent is no defence, though there are crimes, such as rape and larceny, in which the absence of consent is an ingredient of the offence and has to be proved by the prosecution accordingly. One example of the ordinary crimes to which consent is no defence is murder in cases of euthanasia, duelling, and suicide pacts. Another is assault; some form of masochism would perhaps today be the most likely case of a willing submission to an assault.

The conclusion which I drew from this . . . was that a breach of the criminal law was regarded as an offence not merely against the person injured but against society as a whole; and that an act done by consent, such as euthanasia, could be prohibited only as the breach of a moral principle which society required to be observed. Professor Hart says roundly that this "is simply not true." The emphasis suggests that I have overlooked the obvious. What, alas, I did not foresee was that some of the crew who sail under Mill's flag of liberty would mutiny and run paternalism up the mast. Professor Hart considers that it is paternalism and not moral principle that is the justification of the law in these matters and he is thereby enabled to accept the second principle. "The rules excluding the victim's consent as a defence to charges of murder or assault may perfectly well be explained as a piece of paternalism, designed to protect individuals against themselves."

"Mill no doubt might have protested," Professor Hart goes on in a meiosis which deserves to be commemorated. This tears the heart out of his doctrine. "His own good either physical or moral is not a sufficient warrant. He cannot rightfully be compelled to do or forbear because it will be better for him to do so, because it will make him happier, because in the opinions of others, to do so would be wise or even right."

Professor Hart suggests that Mill might have objected not quite as much to paternalism as to the enforcement of moral principle. He bases this on Mill's particularization of the three grounds on which compulsion would be wrongful. These, he says, are separate, the first two as I understand it, referring to paternalism, and the third to enforcement of morality. This seems to me a forced reading. Mill states his reasons cumulatively and not alternatively. If a man does what is wise and right, surely in Mill's view it

would make him better and happier; he would not have distinguished between them. . . .

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. . . When dealing with the exclusion of consent as a defence to murder or assault, Professor Hart uses a phrase which suggests that he might be drawing a distinction between physical and moral paternalism. He refers to "using the law to protect even a consenting victim from *bodily* harm." But I do not think that—at least in this connexion—he can mean the word "bodily" to be distinctive. It would be quite unrealistic to treat the crimes with which Professor Hart is dealing as offences against the body of the consenting party and not against morals. The most common case of a man willingly submitting to assault would, as I have suggested, be a case of masochism. To say that the law should intervene there not because of the vice but to protect the man in his own best interests from getting bodily hurt hardly seems sense. So in euthanasia. It cannot seriously be suggested that, if there were no moral principle involved, the law in a free country would tell a man when he was and when he was not to die, obtaining its mandate from its paternal interest in his body and not in his soul. Or that in euthanasia the crime lies not in the moral decision to seek death but purely in the physical and no doubt painless act that causes it.

If however there is an element of physical paternalism in the law that forbids masochism and euthanasia these crimes seem to me as good examples as any that could be selected to illustrate the difficulty in practice of distinguishing between physical and moral paternalism. Neither in principle nor in practice can a line be drawn between legislation controlling the individual's physical welfare and legislation controlling his moral welfare. . . .

The terms in which Professor Hart justifies the sort of paternalism he advocates lead to the same conclusion. There is, he says, "a general decline in the belief that individuals know their own interests best." There can be no reason to believe that if unable to perceive their own physical good unaided, they can judge of their own moral good. He continues: "Choices may be made or consent given without adequate reflection or appreciation of the consequences; or in pursuit of merely transitory desires; or in various predicaments when the judgment is likely to be clouded; or under inner psychological compulsion; or under pressure by others of a kind too subtle to be susceptible of proof in a law court."

... It is moral weakness rather than physical that leads to predicaments when the judgement is likely to be clouded and is the cause of inner psychological compulsion.

These considerations drive one to the conclusion that a distinction between moral and physical paternalism is not what Professor Hart has in mind. But the alternative hypothesis seems even more unacceptable. If it is difficult to draw a line between moral and physical paternalism, it is impossible to draw one of any significance between moral paternalism and the enforcement of the moral law. A moral law, that is, a public morality, is a necessity for paternalism, otherwise it would be impossible to arrive at a common judgement about what would be for a man's moral good. If then society compels a man to act for his own moral good, society is enforcing the moral law; and it is a distinction without a difference to say that society is acting for a man's own good and not for the enforcement of the law. ...

\* \* \*

c.

**Isaiah Berlin**  
**Two Concepts of Liberty (1958)\***

To coerce a man is to deprive him of freedom—freedom from what? Almost every moralist in human history has praised freedom. Like happiness and goodness, like nature and reality, the meaning of this term is so porous that there is little interpretation that it seems able to resist. ... I propose to examine no more than two of these senses—but those central ones, with a great deal of human history behind them and, I dare say, still to come. The first of these political senses of freedom or liberty (I shall use both words to mean the same), which I shall call the “negative” sense, is involved in the answer to the question “What is the area within which the subject—a person or group of persons—is or should be left to do or be what he wants to do or be, without interference by other persons?” The second, which I shall call the positive sense, is involved in the answer to the question “What, or who, is the source of control or interference, that can determine someone to do, or be, one thing rather than another?” The two questions are clearly different, even though the answers to them may overlap.

\* *Four Essays on Liberty*. Oxford: Clarendon Press 118–138 (1969). Reprinted by permission of the Clarendon Press, Oxford.

*The notion of “negative” freedom*

... If I am prevented by other persons from doing what I want I am to that degree unfree; and if the area within which I can do what I want is contracted by other men beyond a certain minimum, I can be described as being coerced, or, it may be, enslaved. Coercion is not, however, a term that covers every form of inability. If I say that I am unable to jump more than 10 feet in the air, or cannot read because I am blind, or cannot understand the darker pages of Hegel, it would be eccentric to say that I am to that degree enslaved or coerced. Coercion implies the deliberate interference of other human beings within the area in which I wish to act. You lack political liberty or freedom only if you are prevented from attaining your goal by human beings.

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... If my poverty were a kind of disease, which prevented me from buying bread or paying for the journey round the world, or getting my case heard, as lameness prevents me from running, this inability would not naturally be described as a lack of freedom at all, least of all political freedom. It is only because I believe that my inability to get what I want is due to the fact that other human beings have made arrangements whereby I am, whereas others are not, prevented from having enough money with which to pay for it, that I think myself a victim of coercion or slavery. In other words, this use of the term depends on a particular social and economic theory about the causes of my poverty or weakness. If my lack of means is due to my lack of mental or physical capacity, then I begin to speak of being deprived of freedom (and not simply of poverty) only if I accept the theory. If, in addition, I believe that I am being kept in want by a definite arrangement which I consider unjust or unfair, I speak of economic slavery or oppression. ...

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[T]here ought to exist a certain minimum area of personal freedom which must on no account be violated, for, if it is overstepped, the individual will find himself in an area too narrow for even that minimum development of his natural faculties which alone makes it possible to pursue, and even to conceive, the various ends which men hold good or right or sacred. It follows that a frontier must be drawn between the area of private life and that of public authority.

Where it is to be drawn is a matter of argument, indeed of haggling. Men are largely interdependent, and no man's activity is so completely private as never to obstruct the lives of others in any way. "Freedom for the pike is death for the minnows"; the liberty of some must depend on the restraint of others. Still, a practical compromise has to be found.

\* \* \*

... The answer to the question "Who governs me?" is logically distinct from the question "How far does government interfere with me?" It is in this difference that the great contrast between the two concepts of negative and positive liberty, in the end, consists.<sup>1</sup> For the "positive" sense of liberty comes to light if we try to answer the question, not "What am I free to do or be?" but "By whom am I ruled?" or "Who is to say what I am, and what I am not, to be or do?" The connexion between democracy and individual liberty is a good deal more tenuous than it seemed to many advocates of both. The desire to be governed by myself, or at any rate to participate in the process by which my life is to be controlled, may be as deep a wish as that of a free area for action, and perhaps historically older.

<sup>1</sup> "Negative liberty" is something the extent of which, in a given case, it is difficult to estimate. It might, *prima facie*, seem to depend simply on the power to choose between at any rate two alternatives. Nevertheless, not all choices are equally free, or free at all. If in a totalitarian state I betray my friend under threat of torture, perhaps even if I act from fear of losing my job, I can reasonably say that I did not act freely. Nevertheless, I did, of course, make a choice, and could, at any rate in theory, have chosen to be killed or tortured or imprisoned. The mere existence of alternatives is not, therefore, enough to make my action free (although it may be voluntary) in the normal sense of the word. The extent of my freedom seems to depend on (a) how many possibilities are open to me (although the method of counting these can never be more than impressionistic. Possibilities of action are not discrete entities like apples, which can be exhaustively enumerated); (b) how easy or difficult each of these possibilities is to actualize; (c) how important in my plan of life, given my character and circumstances, these possibilities are when compared with each other; (d) how far they are closed and opened by deliberate human acts; (e) what value not merely the agent, but the general sentiment of the society in which he lives, puts on the various possibilities. All these magnitudes must be "integrated," and a conclusion, necessarily never precise, or indisputable, drawn from this process. . . .

But it is not a desire for the same thing. So different is it, indeed, as to have led in the end to the great clash of ideologies that dominates our world. For it is this—the "positive" conception of liberty: not freedom from, but freedom to—which the adherents of the "negative" notion represent as being, at times, no better than a specious disguise for brutal tyranny.

#### *The notion of "positive" freedom*

The "positive" sense of the word "liberty" derives from the wish on the part of the individual to be his own master. I wish my life and decisions to depend on myself, not on external forces of whatever kind. I wish to be the instrument of my own, not of other men's, acts of will. I wish to be a subject, not an object; to be moved by reasons, by conscious purposes which are my own, not by causes which affect me, as it were, from outside. I wish to be somebody, not nobody; a doer—deciding, not being decided for, self-directed and not acted upon by external nature or by other men as if I were a thing, or an animal, or a slave incapable of playing a human role, that is, of conceiving goals and policies of my own and realizing them. This is at least part of what I mean when I say that I am rational, and that it is my reason that distinguishes me as a human being from the rest of the world. I wish, above all, to be conscious of myself as a thinking, willing, active being, bearing responsibility for his choices and able to explain them by reference to his own ideas and purposes. I feel free to the degree that I believe this to be true, and enslaved to the degree that I am made to realize that it is not.

The freedom which consists in being one's own master, and the freedom which consists in not being prevented from choosing as I do by other men, may, on the face of it, seem concepts at no great logical distance from each other—no more than negative and positive ways of saying the same thing. Yet the "positive" and "negative" notions of freedom developed in divergent directions until, in the end, they came into direct conflict with each other.

One way of making this clear is in terms of the independent momentum which the metaphor of self-mastery acquired. "I am my own master"; "I am slave to no man"; but may I not . . . be a slave to nature? Or to my own "unbridled" passions? Are these not so many species of the identical genus "slave"—some political or legal, others more or spiritual? Have not men had the experience of liberating themselves from spiritual



slavery, or slavery to nature, and do they not in the course of it become aware, on the one hand, of a self which dominates, and, on the other, of something in them which is brought to heel? This dominant self is then variously identified with reason, with my "higher nature," with the self which calculates and aims at what will satisfy it in the long run, with my "real," or "ideal," or "autonomous" self, or with my self "at its best"; which is then contrasted with irrational impulse, uncontrolled desires, my "lower" nature, the pursuit of immediate pleasures, my "empirical" or "heteronomous" self, swept by every gust of desire and passion, needing to be rigidly disciplined if it is ever to rise to the full height of its "real" nature. Presently the two selves may be represented as divided by an even larger gap: the real self may be conceived as something wider than the individual (as the term is normally understood), as a social "whole" of which the individual is an element or aspect: a tribe, a race, a church, a state, the great society of the living and the dead and the yet unborn. This entity is then identified as being the "true" self which, by imposing its collective, or "organic," single will upon its recalcitrant "members," achieves its own, and, therefore, their, "higher" freedom. The perils of using organic metaphors to justify the coercion of some men by others in order to raise them to a "higher" level of freedom have often been pointed out. But what gives such plausibility as it has to this kind of language is that we recognize that it is possible, and at times justifiable, to coerce men in the name of some goal (let us say, justice or public health) which they would, if they were more enlightened, themselves pursue, but do not, because they are blind or ignorant or corrupt. This renders it easy for me to conceive of myself as coercing others for their own sake, in their, not my, interest, I am then claiming that I know what they truly need better than they know it themselves. What, at most, this entails is that they would not resist me if they were rational, and as wise as I, and understood their interests as I do. But I may go on to claim a good deal more than this. I may declare that they are actually aiming at what in their benighted state they consciously resist, because there exists within them an occult entity—their latent rational will, or their "true" purpose—and that this entity, although it is belied by all that they overtly feel and do and say, is their "real" self, of which the poor empirical self in space and time may know nothing or little; and that this inner spirit is the only self that deserves

to have its wishes taken into account. Once I take this view, I am in a position to ignore the actual wishes of men or societies, to bully, oppress, torture them in the name, and on behalf, of their "real" selves, in the secure knowledge that whatever is the true goal of man (happiness, fulfilment of duty, wisdom, a just society, self-fulfilment) must be identical with his freedom—the free choice of his "true," albeit submerged and inarticulate, self.

This paradox has been often exposed. It is one thing to say that I know what is good for *X*, while he himself does not; and even to ignore his wishes for its—and his—sake; and a very different one to say that he has *eo ipso* chosen it, not indeed consciously, not as he seems in everyday life, but in his role as a rational self which his empirical self may not know—the "real" self which discerns the good, and cannot help choosing it once it is revealed. This monstrous impersonation, which consists in equating what *X* would choose if he were something he is not, or at least not yet, with what *X* actually seeks and chooses, is at the heart of all political theories of self-realization. It is one thing to say that I may be coerced for my own good which I am too blind to see; and another that if it is for my good, I am not being coerced, for I have willed it, whether I know this or not, and am free even while my poor earthly body and foolish mind bitterly reject it, and struggle against those who seek to impose it, with the greatest desperation.

This magical transformation, or sleight of hand . . . can no doubt be perpetrated just as easily with the "negative" concept of freedom, where the self that should not be interfered with is no longer the individual with his actual wishes and needs as they are normally conceived, but the "real" man within, identified with the pursuit of some ideal purpose not dreamed of by his empirical self. And, as in the case of the "positively" free self, this entity may be inflated into some super-personal entity—a state, a class, a nation, or the march of history itself, regarded as a more "real" subject of attributes than the empirical self. But the "positive" conception of freedom as self-mastery, with its suggestion of a man divided against himself, lends itself more easily to this splitting of personality into two: the transcendent, dominant controller, and the empirical bundle of desires and passions to be disciplined and brought to heel. This demonstrates (if demonstration of so obvious a truth is needed) that the conception of freedom directly derives from the view that is taken of what constitutes a self, a

person, a man. Enough manipulation with the definitions of man, and freedom can be made to mean whatever the manipulator wishes. Recent history has made it only too clear that the issue is not merely academic.

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For if the essence of men is that they are autonomous beings—authors of values, of ends in themselves, the ultimate authority of which consists precisely in the fact that they are willed freely—then nothing is worse than to treat them as if they were not autonomous, but natural objects, played on by causal influences, creatures at the mercy of external stimuli, whose choices can be manipulated by their rulers, whether by threats of force or offers of rewards. To treat men in this way is to treat them as if they were not self-determined. "Nobody may compel me to be happy in his own way," said Kant. "Paternalism is the greatest despotism imaginable." This is so because it is to treat men as if they were not free, but human material for me, the benevolent reformer, to mould in accordance with my own, not their, freely adopted purpose. This is, of course, precisely the policy that the early utilitarians recommended. Helvetius (and Bentham) believed not in resisting, but in using, men's tendency to be slaves to their passions; they wished to dangle rewards and punishments before men—the acutest possible form of heteronomy—if by this means the "slaves" might be made happier. But to manipulate men, to propel them towards goals which you—the social reformer—see, but they may not, is to deny their human essence, to treat them as objects without wills of their own, and therefore to degrade them. That is why to lie to men, or to deceive them, that is, to use them as means for my, not their own, independently conceived ends, even if it is for their own benefit, is, in effect, to treat them as sub-human, to behave as if their ends are less ultimate and sacred than my own. In the name of what can I ever be justified in forcing men to do what they have not willed or consented to? Only in the name of some value higher than themselves. But if, as Kant held, all values are the creation of men, and called values only so far as they are so, there is no value higher than the individual. Therefore to do this is to coerce men in the name of something less ultimate than themselves—to bend them to my will, or to someone else's particular craving for happiness or expediency or security or convenience. I am aiming at something desired by me or my group, to which I am

using other men as means. But this is a contradiction of what I know men to be, namely ends in themselves. All forms of tampering with human beings, getting at them, shaping them against their will to your own pattern, all thought control and conditioning, is, therefore, a denial of that in men which makes them men and their values ultimate.

## NOTES

### NOTE 1.

GINSBERG V. NEW YORK  
390 U.S. 629 (1968)

MR. JUSTICE BRENNAN delivered the opinion of the Court.

This case presents the question of the constitutionality on its face of a New York criminal obscenity statute which prohibits the sale to minors under 17 years of age of material defined to be obscene on the basis of its appeal to them whether or not it would be obscene to adults.

Appellant and his wife operate "Sam's Stationery and Luncheonette" in Bellmore, Long Island. They have a lunch counter, and, among other things, also sell magazines including some so-called "girlie" magazines. Appellant was prosecuted under two informations, each in two counts, which charged that he personally sold a 16-year-old boy two "girlie" magazines on each of two dates in October 1965, in violation of § 484-h of the New York Penal Law. He was tried before a judge without a jury in Nassau County District Court and was found guilty on both counts. . . .

\* \* \*

The "girlie" picture magazines involved in the sales here are not obscene for adults, *Redrup v. New York*, 386 U.S. 767. But § 484-h does not bar the appellant from stocking the magazines and selling them to persons 17 years of age or older, and therefore the conviction is not invalid under our decision in *Butler v. Michigan*, 352 U.S. 380.

Obscenity is not within the area of protected speech or press. *Roth v. United States*, 354 U.S. 476, 485. . . .

The New York Court of Appeals "upheld the Legislature's power to employ variable concepts of obscenity". . . . In sustaining state power to enact the law, the Court of Appeals said,

[M]aterial which is protected for distribution to adults is not necessarily constitutionally protected from re-

striction upon its dissemination to children. In other words, the concept of obscenity or of unprotected matter may vary according to the group to whom the questionable material is directed or from whom it is quarantined. Because of the State's exigent interest in preventing distribution to children of objectionable material, it can exercise its power to protect the health, safety, welfare and morals of its community by barring the distribution to children of books recognized to be suitable for adults.

Appellant's attack is not that New York was without power to draw the line at age 17. Rather, his contention is the broad proposition that the scope of the constitutional freedom of expression secured to a citizen to read or see material concerned with sex cannot be made to depend upon whether the citizen is an adult or a minor. He accordingly insists that the denial to minors under 17 of access to material condemned by § 484-h, insofar as that material is not obscene for persons 17 years of age or older, constitutes an unconstitutional deprivation of protected liberty.

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... We do not regard New York's regulation in defining obscenity on the basis of its appeal to minors under 17 as involving an invasion of such minors' constitutionally protected freedoms. Rather § 484-h simply adjusts the definition of obscenity "to social realities by permitting the appeal of this type of material to be assessed in terms of the sexual interests . . ." of such minors. *Mishkin v. New York*, 383 U.S. 502, 509; *Bookcase, Inc. v. Broderick*, *supra*, at 75, 218 N.E.2d, at 671. That the state has power to make that adjustment seems clear, for we have recognized that even where there is an invasion of protected freedoms "the power of the state to control the conduct of children reaches beyond the scope of its authority over adults. . . ." *Prince v. Massachusetts*, 321 U.S. 158, 170. . . . To sustain state power to exclude material defined as obscenity by § 484-h requires only that we be able to say that it was not irrational for the legislature to find that exposure to material condemned by the statute is harmful to minors. . . . To be sure, there is no lack of "studies" which purport to demonstrate that obscenity is or is not "a basic factor in impairing the ethical and moral development of . . . youth and a clear and present danger to the people of the state." But the growing consensus of commentators is that "while these studies all agree

that a causal link has not been demonstrated, they are equally agreed that a causal link has not been disproved either." We do not demand of legislatures "scientifically certain criteria of legislation." *Noble State Bank v. Haskell*, 219 U.S. 104, 110. We therefore cannot say that § 484-h, in defining the obscenity of material on the basis of its appeal to minors under 17, has no rational relation to the objective of safeguarding such minors from harm.

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MR. JUSTICE STEWART, concurring in the result.

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The First Amendment guarantees liberty of human expression in order to preserve in our nation what Mr. Justice Holmes called a "free trade in ideas." To that end, the Constitution protects more than just a man's freedom to say or write or publish what he wants. It secures as well the liberty of each man to decide for himself what he will read and to what he will listen. The Constitution guarantees, in short, a society of free choice. Such a society presupposes the capacity of its members to choose.

When expression occurs in a setting where the capacity to make a choice is absent, government regulation of that expression may co-exist with and even implement First Amendment guarantees. . . .

I think a state may permissibly determine that, at least in some precisely delineated areas, a child—like someone in a captive audience—is not possessed of that full capacity for individual choice which is the presupposition of First Amendment guarantees. It is only upon such a premise, I should suppose, that a state may deprive children of other rights—the right to marry, for example, or the right to vote—deprivations that would be constitutionally intolerable for adults.

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MR. JUSTICE DOUGLAS, with whom MR. JUSTICE BLACK concurs, dissenting.

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... Today the Court determines the constitutionality of New York's law regulating the sale of literature to children on the basis of the reasonableness of the law in light of the welfare of the child. If the problem of state and federal regulation of "obscenity" is in the field of sub-

stantive due process, I see no reason to limit the legislatures to protecting children alone. The "juvenile delinquents" I have known are mostly over 50 years of age. If rationality is the measure of the validity of this law, then I can see how modern Anthony Comstocks could make out a case for "protecting" many groups in our society, not merely children.

While I find the literature and movies which come to us for clearance exceedingly dull and boring, I understand how some can and do become very excited and alarmed and think that something should be done to stop the flow. It is one thing for parents and the religious organizations to be active and involved. It is quite a different matter for the state to become implicated as a censor. As I read the First Amendment, it was designed to keep the state and the hands of all state officials off the printing presses of America and off the distribution systems for all printed literature. . . .

#### NOTE 2.

##### UNITED STATES CODE TITLE 25—INDIANS (1964)

§174. The President is authorized to exercise general superintendence and care over any tribe or nation which was removed upon an exchange of territory . . . and to cause such tribe or nation to be protected, at their new residence, against all interruption or disturbance from any other tribe or nation of Indians, or from any other person or persons whatever.

§202. It shall be unlawful for any person to induce any Indian to execute any contract, deed, mortgage, or other instrument purporting to convey any land or any interest therein held by the United States in trust for such Indian, or to offer any such contract, deed, mortgage, or other instrument for record in the office of any recorder of deeds. . . .

§263. The President is authorized, whenever in his opinion the public interest may require the same, to prohibit the introduction of goods, or of any particular article, into the country belonging to any Indian tribe, and to direct all licenses to trade with such tribe to be revoked, and all applications therefore to be rejected. No trader to any other tribe shall, so long as such prohibition may continue, trade with any Indians of or for the tribe against which such prohibition is issued.

d.

#### His Holiness, Pope Pius XII The Moral Limits of Medical Research and Treatment\*

. . . You do not expect Us to discuss the medical questions which concern you. Those are your domain. . . . We wish to make Ourselves the interpreter of the moral conscience of the research worker, the specialist and the practitioner and of the man and Christian who follows the same path.

. . . A serious, competent doctor will often see with a sort of spontaneous intuition the moral legality of what he proposes to do and will act according to his conscience. But there are other instances where he does not have this security, where he may see or think he sees the contrary with certainty or where he doubts and wavers between Yes and No. In the most serious and profound matters, the man in the physician is not content with examining from a medical point of view what he can attempt and succeed in. He also wants to see his way clearly in regard to moral possibilities and obligations.

We would like to set forth briefly the *essential principles* which permit an answer to be given to this question. . . .

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[T]he basic considerations may be set out in the following form: "The medical treatment of the patient demands taking a certain step. This in itself proves its moral legality." Or else: "A certain new method hitherto neglected or little used will give possible, probable or sure results. All ethical considerations as to the licitness of this method are obsolete and should be treated as pointless."

How can anyone fail to see that in these statements truth and falsehood are intermingled? In a very large number of cases the "interests of the patient" do provide the moral justification of the doctor's conduct. Here again, the question concerns the *absolute* value of this principle. Does it prove by itself, does it make it evident that what the doctor wants to do conforms to the moral law?

In the first place it must be assumed that, as

\* 44 *Acta Apostolicae Sedis* 779-784 (1952); Rome: 3 *Proceedings of the First International Congress of Neuropathology* 713-725 (1952). (Translated from the French by the N.C.W.C. News Service). Reprinted by permission.

a private person, the doctor can take no measure or try no course of action without the consent of the patient. The doctor has no other rights or power over the patient than those which the latter gives him, explicitly or implicitly and tacitly. On his side, the patient cannot confer rights he does not possess. In this discussion the decisive point is the moral licitness of the right a patient has to dispose of himself. Here is the moral limit to the doctor's action taken with the consent of the patient.

As for the patient, he is not absolute master of himself, of his body or of his soul. He cannot, therefore, freely dispose of himself as he pleases. Even the reason for which he acts is of itself neither sufficient nor determining. The patient is bound to the immanent teleology laid down by nature. He has the right of use, limited by natural finality, of the faculties and powers of his human nature. Because he is a user and not a proprietor, he does not have unlimited power to destroy or mutilate his body and its functions. Nevertheless, by virtue of the principle of totality, by virtue of his right to use the services of his organism as a whole, the patient can allow individual parts to be destroyed or mutilated when and to the extent necessary for the good of his being as a whole. He may do so to ensure his being's existence and to avoid or, naturally, to repair serious and lasting damage which cannot otherwise be avoided or repaired.

The patient, then, has no right to involve his physical or psychic integrity in medical experiments or research when they entail serious destruction, mutilation, wounds or perils.

Moreover, in exercising his right to dispose of himself, his faculties and his organs, the individual must observe the hierarchy of the orders of values—or within a single order of values, the hierarchy of particular rights—insofar as the rules of morality demand. Thus, for example, a man cannot perform on himself or allow doctors to perform acts of a physical or somatic nature which doubtless relieve heavy physical or psychic burdens or infirmities, but which bring about at the same time permanent abolition or considerable and durable diminution of his freedom, that is, of his human personality in its typical and characteristic function. Such an act degrades a man to the level of a being reacting only to acquired reflexes or to a living automaton. The moral law does not allow such a reversal of values. Here it sets up its limits to the "medical interests of the patient."

Here is another example. In order to rid himself of repressions, inhibitions or psychic complexes man is not free to arouse in himself for therapeutic purposes each and every appetite of a sexual order which is being excited or has been excited in his being, appetites whose impure waves flood his unconscious or subconscious mind. He cannot make them the object of his thoughts and fully conscious desires with all the shocks and repercussions such a process entails. For a man and a Christian there is a law of integrity and personal purity, of self-respect, forbidding him to plunge so deeply into the world of sexual suggestions and tendencies. Here the "medical and psychotherapeutic interests of the patient" find a moral limit. It is not proved—it is, in fact, incorrect—that the pansexual method of a certain school of psychoanalysis is an indispensable integrating part of all psychotherapy which is serious and worthy of the name. It is not proved that past neglect of this method has caused grave psychic damage, errors in doctrine and application in education, in psychotherapy and still less in pastoral practice. It is not proved that it is urgent to fill this gap and to initiate all those interested in psychic questions in its key ideas and even, if necessary, in the practical application of this technique of sexuality.

We speak this way because today these assertions are too often made with apodictic assurance. Where instincts are concerned it would be better to pay more attention to indirect treatment and to the action of the conscious psyche on the whole of imaginative and affective activity. This technique avoids the deviations we have mentioned. It tends to enlighten, cure and guide; it also influences the dynamic of sexuality, on which people insist so much and which they say is to be found, or really exists, in the unconscious or subconscious.

Up to now we have spoken directly of the patient, not of the doctor. We have explained at what point the personal right of the patient to dispose of himself, his mind, his body, his faculties, organs and functions, meets a moral limit. But at the same time we have answered the question: Where does the doctor find a moral limit in research into and use of new methods and procedures in the "interests of the patient?" The limit is the same as that for the patient. It is that which is fixed by the judgment of sound reason, which is set by the demands of the natural moral law, which is deduced from the natural teleology inscribed in beings and from the

scale of values expressed by the nature of things. The limit is the same for the doctor as for the patient because, as we have already said, the doctor as a private individual disposes only of the rights given him by the patient and because the patient can give only what he himself possesses.

What we say here must be extended to the legal representatives of the person incapable of caring for himself and his affairs: children below the age of reason, the feeble-minded and the insane. These legal representatives, authorized by private decision or by public authority have no other rights over the body and life of those they represent than those people would have themselves if they were capable. And they have those rights to the same extent. They cannot, therefore, give the doctor permission to dispose of them outside those limits.

\* \* \*

#### NOTE

APPLICATION OF PRESIDENT AND DIRECTORS  
OF GEORGETOWN COLLEGE  
331 F.2d 1000 (D.C.Cir.),  
*certiorari denied*, 377 U.S. 978 (1964)

J. SKELLY WRIGHT, CIRCUIT JUDGE.

\* \* \*

Mrs. Jones was brought to the hospital by her husband for emergency care, having lost two thirds of her body's blood supply from a ruptured ulcer. She had no personal physician, and relied solely on the hospital staff. She was a total hospital responsibility. It appeared that the patient, age 25, mother of a seven-month-old child, and her husband were both Jehovah's Witnesses, the teachings of which sect, according to their interpretation, prohibited the injection of blood into the body. When death without blood became imminent, the hospital sought the advice of counsel, who applied to the District Court in the name of the hospital for permission to administer blood. Judge Tamm of the District Court denied the application, and counsel immediately applied to me, as a member of the Court of Appeals, for an appropriate writ.

I called the hospital by telephone and spoke with Dr. Westura, Chief Medical Resident, who confirmed the representations made by counsel. I thereupon proceeded with counsel to the hospital, where I spoke to Mr. Jones, the husband of the patient. He advised me that, on religious grounds, he would not approve a blood transfu-

sion for his wife. He said, however, that if the court ordered the transfusion, the responsibility was not his. I advised Mr. Jones to obtain counsel immediately. He thereupon went to the telephone and returned in 10 or 15 minutes to advise that he had taken the matter up with his church and that he had decided that he did not want counsel.

I asked permission of Mr. Jones to see his wife. This he readily granted. Prior to going into the patient's room, I again conferred with Dr. Westura and several other doctors assigned to the case. All confirmed that the patient would die without blood and that there was a better than 50 per cent chance of saving her life with it. Unanimously they strongly recommended it. I then went inside the patient's room. Her appearance confirmed the urgency which had been represented to me. I tried to communicate with her, advising her again as to what the doctors had said. The only audible reply I could hear was "Against my will." It was obvious that the woman was not in a mental condition to make a decision. I was reluctant to press her because of the seriousness of her condition and because I felt that to suggest repeatedly the imminence of death without blood might place a strain on her religious convictions. I asked her whether she would oppose the blood transfusion if the court allowed it. She indicated, as best I could make out, that it would not then be her responsibility.

\* \* \*

[I] signed the order allowing the hospital to administer such transfusions as the doctors should determine were necessary to save her life.

\* \* \*

Before proceeding with this inquiry, it may be useful to state what this case does not involve. This case does not involve a person who, for religious or other reasons, has refused to seek medical attention. It does not involve a disputed medical judgment or a dangerous or crippling operation. Nor does it involve the delicate question of saving the newborn in preference to the mother. Mrs. Jones sought medical attention and placed on the hospital the legal responsibility for her proper care. In its dilemma, not of its own making, the hospital sought judicial direction.

\* \* \*

If self-homicide is a crime, there is no exception to the law's command for those who believe the crime to be divinely ordained. The

Mormon cases in the Supreme Court establish that there is no religious exception to criminal laws, and state *obiter* the very example that a religiously inspired suicide attempt would be within the law's authority to prevent. . . . But whether attempted suicide is a crime is in doubt in some jurisdictions, including the District of Columbia.

The Gordian knot of this suicide question may be cut by the simple fact that Mrs. Jones did not want to die. Her voluntary presence in the hospital as a patient seeking medical help testified to this. Death, to Mrs. Jones, was not a religiously commanded goal, but an unwanted side effect of a religious scruple. . . . Nor are we faced with the question of whether the state should intervene to reweigh the relative values of life and death, after the individual has weighed them for himself and found life wanting. Mrs. Jones wanted to live.

A third set of considerations involved the position of the doctors and the hospital. Mrs. Jones was their responsibility to treat. The hospital doctors had the choice of administering the proper treatment or letting Mrs. Jones die in the hospital bed, thus exposing themselves, and the hospital, to the risk of civil and criminal liability in either case. It is not certain that Mrs. Jones had any authority to put the hospital and its doctors to this impossible choice. The normal principle that an adult patient directs her doctors is based on notions of commercial contract which may have less relevance to life-or-death emergencies. It is not clear just where a patient would derive her authority to command her doctor to treat her under limitations which would produce death. The patient's counsel suggests that this authority is part of constitutionally protected liberty. But neither the principle that life and liberty are inalienable rights, nor the principle of liberty of religion, provides an easy answer to the question whether the state can prevent martyrdom. Moreover, Mrs. Jones had no wish to be a martyr. And her religion merely prevented her consent to a transfusion. If the law undertook the responsibility of authorizing the transfusion without her consent, no problem would be raised with respect to her religious practice. Thus, the effect of the order was to preserve for Mrs. Jones the life she wanted without sacrifice of her religious beliefs.

The final, and compelling, reason for granting the emergency writ was that a life hung in the balance. There was no time for research and reflection. Death could have mooted the cause

in a matter of minutes, if action were not taken to preserve the *status quo*. To refuse to act, only to find later that the law required action, was a risk I was unwilling to accept. I determined to act on the side of life.

e.

**Fyodor Dostoyevsky**  
**The Brothers Karamazov (1880)\***

\* \* \*

. . . Amid the profound darkness, the iron door of the prison is suddenly opened and the old Grand Inquisitor himself slowly enters the prison with a light in his hand. He is alone and the door at once closes behind him. He stops in the doorway and gazes for a long time, for more than a minute, into his face. At last he approaches him slowly, puts the lamp on the table and says to him:

\* \* \*

. . . And look what you have done . . . in the name of freedom! I tell you man has no more agonizing anxiety than to find someone to whom he can hand over with all speed the gift of freedom with which the unhappy creature is born. But only he can gain possession of men's freedom who is able to set their conscience at ease. With the bread you were given an incontestable banner: give him bread and man will worship you, for there is nothing more incontestable than bread; but if at the same time someone besides yourself should gain possession of his conscience—oh, then he will even throw away your bread and follow him who has ensnared his conscience. You were right about that. For the mystery of human life is not only in living, but in knowing why one lives. Without a clear idea of what to live for man will not consent to live and will rather destroy himself than remain on the earth, though he were surrounded by loaves of bread. That is so, but what became of it? Instead of gaining possession of men's freedom, you gave them greater freedom than ever! Or did you forget that a tranquil mind and even death is dearer to man than the free choice in the knowledge of good and evil? There is nothing more alluring to man than this freedom of conscience, but there is nothing more tormenting, either. And instead of firm foundations for

\* Translated by David Magarshack. Baltimore: Penguin Books 293, 298-301 (1958). Translation copyright © David Magarshack, 1958. Reprinted by permission.



appeasing man's conscience once and for all, you chose everything that was exceptional, enigmatic, and vague, you chose everything that was beyond the strength of men, acting, consequently, as though you did not love them at all—you who came to give your life for them! Instead of taking possession of men's freedom you multiplied it and burdened the spiritual kingdom of man with its sufferings for ever. You wanted man's free love so that he should follow you freely, fascinated and captivated by you. Instead of the strict ancient law, man had in future to decide for himself with a free heart what is good and what is evil, having only your image before him for guidance. But did it never occur to you that he would at last reject and call in question even your image and your truth, if he were weighed down by so fearful a burden as freedom of choice? They will at last cry aloud that the truth is not in you, for it was impossible to leave them in greater confusion and suffering than you have done by leaving them with so many cares and insoluble problems. It was you yourself, therefore, who laid the foundation for the destruction of your kingdom and you ought not to blame anyone else for it. And yet, is that all that was offered to you? There are three forces, the only three forces that are able to conquer and hold captive for ever the conscience of these weak rebels for their own happiness—these forces are: miracle, mystery, and authority. You rejected all three and yourself set the example for doing so. . . . But . . . are there many like you? And could you really assume for a moment that men, too, could be equal to such a temptation? Is the nature of man such that he can reject a miracle and at the most fearful moments of life, the moments of his most fearful, fundamental, and agonizing spiritual problems, stick to the free decision of the heart? Oh, you knew that your great deed would be preserved in books, that it would go down to the end of time and the extreme ends of the earth, and you hoped that, following you, man would remain with God and ask for no miracle. But you did not know that as soon as man rejected miracle he would at once reject God as well, for what man seeks is not so much God as miracles. And since man is unable to carry on without a miracle, he will create new miracles for himself, miracles of his own, and will worship the miracle of the witch-doctor and the sorcery of the wise woman, rebel, heretic and infidel though he is a hundred times over. You did not come down from the cross when they shouted to you, mock-

ing and deriding you: "If thou be the Son of God, come down from the cross." You did not come down because, again, you did not want to enslave man by a miracle and because you hungered for a faith based on free will and not on miracles. You hungered for freely given love and not for the servile raptures of the slave before the might that has terrified him once and for all. But here, too, your judgement of men was too high, for they are slaves, though rebels by nature. Look round and judge: fifteen centuries have passed, go and have a look at them: whom have you raised up to yourself? I swear, man has been created a weaker and baser creature than you thought him to be! Can he, can he do what you did? In respecting him so greatly, you acted as though you ceased to feel any compassion for him, for you asked too much of him—you who have loved him more than yourself! Had you respected him less, you would have asked less of him, and that would have been more like love, for his burden would have been lighter. He is weak and base. What does it matter if he does rebel against our authority everywhere now and is proud of his rebellion? It is the pride of a child and of a schoolboy. They are little children rioting in class and driving out their teacher. But an end will come to the transports of the children, too. They will pay dearly for it. They will tear down the temples and drench the earth with blood. But they will realize at last, the foolish children, that although they are rebels, they are impotent rebels who are unable to keep up with their rebellion. Dissolving into foolish tears, they will admit at last that he who created them rebels must undoubtedly have meant to laugh at them. They will say so in despair, and their utterance will be a blasphemy which will make them still more unhappy, for man's nature cannot endure blasphemy and in the end will always avenge it on itself. And so, unrest, confusion, and unhappiness—this is the present lot of men after all you suffered for their freedom! Your great prophet tells in a vision and in an allegory that he saw all those who took part in the first resurrection and that there were twelve thousand of them from each tribe. But if there were so many then, they, too, were not like men, but gods. They had borne your cross, they had endured scores of years of the hungry and barren wilderness, feeding on locusts and roots—and you can indeed point with pride to those children of freedom, freely given love, and free and magnificent sacrifice in your name. But remember that there were only a few thousand of

them, and they, too, gods. But what of the rest? And why are the rest, the weak ones, to blame if they were not able to endure all that the mighty ones endured? Why is the weak soul to blame for being unable to receive gifts so terrible? Surely, you did not come only to the chosen and for the chosen? But if so, there is a mystery here and we cannot understand it. And if it is a mystery, then we, too, were entitled to preach a mystery and to teach them that it is neither the free verdict of their hearts nor love that matters, but the mystery which they must obey blindly, even against their conscience. So we have done. We have corrected your great work and have based it on *miracle, mystery, and authority*. And men rejoiced that they were once more led like sheep and that the terrible gift which had brought them so much suffering had at last been lifted from their hearts. Were we right in doing and teaching this? Tell me. Did we not love mankind when we admitted so humbly its impotence and lovingly lightened its burden and allowed men's weak nature even to sin, so long as it was with our permission? . . .

\* \* \*

## 2.

### Protecting the Status of the Subject as a Human Being

a.

**Pratt v. Davis**  
**118 Ill. App. 161, 166 (1905),**  
**affirmed, 224 Ill. 30, 79 N.E. 562 (1906)**

MR. JUSTICE BROWN delivered the opinion of the Court.

\* \* \*

[U]nder a free government at least, the free citizen's first and greatest right, which underlies all others—the right to the inviolability of his person, in other words, his right to himself—is the subject of universal acquiescence, and this right necessarily forbids a physician or surgeon, however skillful or eminent, who has been asked to examine, diagnose, advise, and prescribe (which are at least necessary first steps in treatment and care), to violate without permission the bodily integrity of his patient by a major or capital operation, placing him under an anaesthetic for that purpose, and operating on him without his consent or knowledge. . . .

\* \* \*

b.

### Alfred Gellhorn Experimental Treatment of Cancer Patients\*

\* \* \*

. . . I have little confidence in the usual interpretation of written consent as informed consent, because I believe this to be a fiction. The patient cannot be expected to understand adequately the implications of the proposed research and his consent is therefore unlikely to be informed. The written consent is important, however, because it accords to the patient the status of a person, not an experimental animal, and provides a degree of assurance that he is being considered as an end, not merely a means.

\* \* \*

c.

### Margaret Mead Research with Human Beings—A Model Derived from Anthropological Field Practice†

\* \* \*

. . . To fail to acquaint a subject of observation or experiment with what is happening—as fully as is possible within the limits of the communication system—is to that extent to denigrate him as a full human being and reduce him to the category of dependency in which he is not permitted to judge for himself. The various ethical ruses that are used—such as telling the subject he has been tricked, deluded, spied upon, or lied to immediately after the experiment is over—fail to take into account that when such a subject is debriefed, he can accept such debriefing only by some other ruse, such as in the identification with the lying experimenter or in the decision that social science is a bunch of confidence tricks and now he also knows a few. Alternately, if he cannot make use of satisfying self-protective devices, his dignity will have been abused and affronted. If he decides that being lied to or tricked is the price he must pay for some other benefit—health, education, political preferment, employment, or a graduate

\* V. Fattorusso, ed., *Biomedical Science and the Dilemma of Human Experimentation*. Paris: Council for International Organizations of Medical Sciences 41 (1967). Reprinted by permission.

† 98 *Daedalus* 361, 375 (1969). Reprinted by permission of Daedalus, Journal of the American Academy of Arts and Sciences, Boston, Massachusetts.

education in a social science—he will nevertheless invest these very benefits and those who confer them upon him with some negative effect.

## NOTES

### NOTE 1.

GRISWOLD V. CONNECTICUT  
381 U.S. 479 (1965)

Mr. JUSTICE DOUGLAS delivered the opinion of the Court.

\* \* \*

The . . . cases suggest that specific guarantees in the Bill of Rights have penumbras, formed by emanations from those guarantees that help give them life and substance. . . . Various guarantees create zones of privacy. The right of association contained in the penumbra of the First Amendment is one, as we have seen. The Third Amendment is its prohibition against the quartering of soldiers "in any house" in time of peace without the consent of the owner is another facet of that privacy. The Fourth Amendment explicitly affirms the "right of the people to be secure in their persons, houses, papers, and effects, against unreasonable searches and seizures." The Fifth Amendment in its Self-Incrimination Clause enables the citizen to create a zone of privacy which government may not force him to surrender to his detriment. The Ninth Amendment provides: "The enumeration in the Constitution, of certain rights, shall not be construed to deny or disparage others retained by the people."

The Fourth and Fifth Amendments were described in *Boyd v. United States*, 116 U.S. 616, 630, . . . as protection against all governmental invasions "of the sanctity of a man's home and the privacies of life."

\* \* \*

The present case, then, concerns a relationship lying within the zone of privacy created by several fundamental constitutional guarantees. And it concerns a law which, in forbidding the use of contraceptives rather than regulating their manufacture or sale, seeks to achieve its goals by means having a maximum destructive impact upon that relationship. . . .

We deal with a right of privacy older than the Bill of Rights—older than our political parties, older than our school system. Marriage is a coming together for better or for worse, hopefully enduring, and intimate to the degree of

being sacred. It is an association that promotes a way of life, not causes; a harmony in living, not political faiths; a bilateral loyalty, not commercial or social projects. Yet it is an association for as noble a purpose as any involved in our prior decisions.

*Reversed.*

Mr. JUSTICE GOLDBERG, whom THE CHIEF JUSTICE and Mr. JUSTICE BRENNAN join, concurring.

\* \* \*

[T]he right of privacy is a fundamental personal right, emanating "from the totality of the constitutional scheme under which we live." . . . Mr. Justice Brandeis, dissenting in *Olmstead v. United States*, 277 U.S. 438, 478, comprehensively summarized the principles underlying the Constitution's guarantees of privacy.

The protection guaranteed by the [Fourth and Fifth] amendments is much broader in scope. The makers of our Constitution undertook to secure conditions favorable to the pursuit of happiness. They recognized the significance of man's spiritual nature, of his feelings and of his intellect. They knew that only a part of the pain, pleasure and satisfaction of life are to be found in material things. They sought to protect Americans in their beliefs, their thoughts, their emotions and their sensations. They conferred, as against the government, the right to be let alone—the most comprehensive of rights and the right most valued by civilized men.

The Connecticut statutes here involved deal with a particularly important and sensitive area of privacy—that of the marital relation and the marital home. This Court recognized in *Meyer v. Nebraska*, supra, that the right "to marry, establish a home and bring up children" was an essential part of the liberty guaranteed by the Fourteenth Amendment. 262 U.S., at 399. . . . In *Pierce v. Society of Sisters*, 268 U.S. 510 . . . the Court held unconstitutional an Oregon Act which forbade parents from sending their children to private schools because such an act "unreasonably interferes with the liberty of parents and guardians to direct the upbringing and education of children under their control." 268 U.S., at 534–535. . . . As this Court said in *Prince v. Massachusetts*, 321 U.S. 158, at 166 . . . the Meyer and Pierce decisions "have respected the private realm of family life which the state cannot enter."

\* \* \*

My Brother STEWART, while characteriz-

ing the Connecticut birth control law as "an uncommonly silly law," post, at 1705, would nevertheless let it stand on the ground that it is not for the courts to "substitute their social and economic beliefs for the judgment of legislative bodies, who are elected to pass laws." Post, at 1705. Elsewhere, I have stated that "[w]hile I quite agree with Mr. Justice Brandeis that . . . 'a . . . State may . . . serve as a laboratory; and try novel social and economic experiments,' *New State Ice Co. v. Liebmann*, 285 U.S. 262 . . . (dissenting opinion), I do not believe that this includes the power to experiment with the fundamental liberties of citizens. . . ." The vice of the dissenters' views is that it would permit such experimentation by the States in the area of the fundamental personal rights of its citizens. I cannot agree that the Constitution grants such power either to the States or to the Federal Government.

The logic of the dissents would sanction federal or state legislation that seems to me even more plainly unconstitutional than the statute before us. Surely the Government, absent a showing of a compelling subordinating state interest, could not decree that all husbands and wives must be sterilized after two children have been born to them. Yet by their reasoning such an invasion of marital privacy would not be subject to constitutional challenge because, while it might be "silly," no provision of the Constitution specifically prevents the Government from curtailing the marital right to bear children and raise a family. While it may shock some of my Brethren that the Court today holds that the Constitution protects the right of marital privacy, in my view it is far more shocking to believe that the personal liberty guaranteed by the Constitution does not include protection against such totalitarian limitation of family size, which is at complete variance with our constitutional concepts. Yet, if upon a showing of a slender basis of rationality, a law outlawing voluntary birth control by married persons is valid, then, by the same reasoning, a law requiring compulsory birth control also would seem to be valid. In my view, however, both types of law would unjustifiably intrude upon rights of marital privacy which are constitutionally protected.

In a long series of cases this Court has held that where fundamental personal liberties are involved, they may not be abridged by the States simply on a showing that a regulatory statute has some rational relationship to the effectuation of a proper state purpose. "Where there is a significant encroachment upon per-

sonal liberty, the State may prevail only upon showing a subordinating interest which is compelling." *Bates v. City of Little Rock*, 361 U.S. 516, 524. . . .

\* \* \*

#### NOTE 2.

MULLER V. OREGON  
208 U.S. 412 (1908)

MR. JUSTICE BREWER delivered the opinion of the court.

On February 19, 1903, the legislature of the State of Oregon passed an act . . . the first section of which is in these words:

"SEC. 1. That no female (shall) be employed in any mechanical establishment, or factory, or laundry in this State more than ten hours during any one day. The hours of work may be so arranged as to permit the employment of females at any time so that they shall not work more than ten hours during the twenty-four hours of any one day."

\* \* \*

The single question is the constitutionality of the statute under which the defendant was convicted so far as it affects the work of a female in a laundry. . . .

\* \* \*

It is the law of Oregon that women, whether married or single, have equal contractual and personal rights with men. . . .

\* \* \*

It thus appears that, putting to one side the elective franchise, in the matter of personal and contractual rights they stand on the same plane as the other sex. Their rights in these respects can no more be infringed than the equal rights of their brothers. We held in *Lochner v. New York*, 198 U.S. 45, that a law providing that no laborer shall be required or permitted to work in a bakery more than sixty hours in a week or ten hours in a day was not as to men a legitimate exercise of the police power of the State, but an unreasonable, unnecessary and arbitrary interference with the right and liberty of the individual to contract in relation to his labor, and as such was in conflict with, and void under, the Federal Constitution. . . .

\* \* \*

It is undoubtedly true, as more than once declared by this court, that the general right to contract in relation to one's business is part of

the liberty of the individual, protected by the Fourteenth Amendment to the Federal Constitution; yet it is equally well settled that this liberty is not absolute and extending to all contracts, and that a State may, without conflicting with the provisions of the Fourteenth Amendment, restrict in many respects the individual's power of contract. . . .

That woman's physical structure and the performance of maternal functions place her at a disadvantage in the struggle for subsistence is obvious. This is especially true when the burdens of motherhood are upon her. Even when they are not, by abundant testimony of the medical fraternity continuance for a long time on her feet at work, repeating this from day to day, tends to injurious effects upon the body, and as healthy mothers are essential to vigorous offspring, the physical well-being of woman becomes an object of public interest and care in order to preserve the strength and vigor of the race.

Still again, history discloses the fact that woman has always been dependent upon man. He established his control at the outset by superior physical strength, and this control in various forms, with diminishing intensity, has continued to the present. . . . Differentiated by these matters from the other sex, she is properly placed in a class by herself, and legislation designed for her protection may be sustained, even when like legislation is not necessary for men and could not be sustained. It is impossible to close one's eyes to the fact that she still looks to her brother and depends upon him. Even though all restrictions on political, personal and contractual rights were taken away, and she stood, so far as statutes are concerned, upon an absolutely equal plane with him, it would still be true that she is so constituted that she will rest upon and look to him for protection; that her physical structure and a proper discharge of her maternal functions—having in view not merely her own health, but the well-being of the race—justify legislation to protect her from the greed as well as the passion of man. . . .

\* \* \*

NOTE 3.

IN RE GAULT  
387 U.S. 1 (1967)

Mr. JUSTICE FORTAS delivered the opinion of the Court.

This is an appeal . . . from a judgment of the Supreme Court of Arizona affirming the dismissal of a petition for a writ of habeas corpus.

. . . The petition sought the release of Gerald Francis Gault, appellants' 15-year-old son, who had been committed as a juvenile delinquent to the State Industrial School by the Juvenile Court of Gila County, Arizona. The Supreme Court of Arizona affirmed dismissal of the writ against various arguments which included an attack upon the constitutionality of the Arizona Juvenile Code because of its alleged denial of procedural due process rights to juveniles charged with being "delinquents."

\* \* \*

. . . In their jurisdictional statement and brief in this Court, appellants do not urge upon us all of the points passed upon by the Supreme Court of Arizona. They urge that we hold the Juvenile Code of Arizona invalid on its face or as applied in this case because, contrary to the Due Process Clause of the Fourteenth Amendment, the juvenile is taken from the custody of his parents and committed to a state institution pursuant to proceedings in which the Juvenile Court has virtually unlimited discretion, and in which the following basic rights are denied:

1. Notice of the charges;
2. Right to counsel;
3. Right to confrontation and cross-examination;
4. Privilege against self-incrimination;
5. Right to a transcript of the proceedings; and
6. Right to appellate review.

\* \* \*

From the inception of the juvenile court system, wide differences have been tolerated—indeed insisted upon—between the procedural rights accorded to adults and those of juveniles. In practically all jurisdictions, there are rights granted to adults which are withheld from juveniles. In addition to the specific problems involved in the present case, for example, it has been held that the juvenile is not entitled to bail, to indictment by grand jury, to a public trial or to trial by jury. It is frequent practice that rules governing the arrest and interrogation of adults by the police are not observed in the case of juveniles.

\* \* \*

The early reformers were appalled by adult procedures and penalties, and by the fact that children could be given long prison sentences and mixed in jails with hardened criminals. They were profoundly convinced that society's duty

to the child could not be confined by the concept of justice alone. They believed that society's role was not to ascertain whether the child was "guilty" or "innocent," but "What is he, how has he become what he is, and what had best be done in his interest and in the interest of the state to save him from a downward career." The child—essentially good, as they saw it—was to be made "to feel that he is the object of [the state's] care and solicitude," not that he was under arrest or on trial. The rules of criminal procedure were therefore altogether inapplicable. The apparent rigidities, technicalities, and harshness which they observed in both substantive and procedural criminal law were therefore to be discarded. The idea of crime and punishment was to be abandoned. The child was to be "treated" and "rehabilitated" and the procedures, from apprehension through institutionalization, were to be "clinical" rather than punitive.

\* \* \*

The right of the state, as *parens patriae*, to deny to the child procedural rights available to his elders was elaborated by the assertion that a child, unlike an adult, has a right "not to liberty but to custody." He can be made to atton to his parents, to go to school, etc. If his parents default in effectively performing their custodial functions—that is, if the child is "delinquent"—the state may intervene. In doing so, it does not deprive the child of any rights, because he has none. It merely provides the "custody" to which the child is entitled. On this basis, proceedings involving juveniles were described as "civil" not "criminal" and therefore not subject to the requirements which restrict the state when it seeks to deprive a person of his liberty.

Accordingly, the highest motives and most enlightened impulses led to a peculiar system for juveniles, unknown to our law in any comparable context. The constitutional and theoretical basis for this peculiar system is—to say the least—debatable. And in practice, as we remarked in the *Kent* case, *supra*, the results have not been entirely satisfactory. Juvenile Court history has again demonstrated that unbridled discretion, however benevolently motivated, is frequently a poor substitute for principle and procedure. In 1937, Dean Pound wrote: "The powers of the Star Chamber were a trifle in comparison with those of our juvenile courts. . . ." The absence of substantive standards has not necessarily meant that children receive careful, compassionate, individualized

treatment. The absence of procedural rules based upon constitutional principle has not always produced fair, efficient, and effective procedures. Departures from established principles of due process have frequently resulted not in enlightened procedure, but in arbitrariness. The Chairman of the Pennsylvania Council of Juvenile Court Judges has recently observed: "Unfortunately, loose procedures, high-handed methods and crowded court calendars, either singly or in combination, all too often, have resulted in depriving some juveniles of fundamental rights that have resulted in a denial of due process."

Failure to observe the fundamental requirements of due process has resulted in instances, which might have been avoided, of unfairness to individuals and inadequate or inaccurate findings of fact and unfortunate prescriptions of remedy. Due process of law is the primary and indispensable foundation of individual freedom. It is the basic and essential term in the social compact which defines the rights of the individual and delimits the powers which the state may exercise. . . .

It is claimed that juveniles obtain benefits from the special procedures applicable to them which more than offset the disadvantages of denial of the substance of normal due process. As we shall discuss, the observance of due process standards, intelligently and not ruthlessly administered, will not compel the States to abandon or displace any of the substantive benefits of the juvenile process.

\* \* \*

Ultimately . . . we confront the reality of that portion of the Juvenile Court process with which we deal in this case. A boy is charged with misconduct. The boy is committed to an institution where he may be restrained of liberty for years. It is of no constitutional consequence—and of limited practical meaning—that the institution to which he is committed is called an Industrial School. The fact of the matter is that, however euphemistic the title, a "receiving home" or an "industrial school" for juveniles is an institution of confinement in which the child is incarcerated for a greater or lesser time. His world becomes "a building with whitewashed walls, regimented routine and institutional hours. . . ." Instead of mother and father and sisters and brothers and friends and classmates, his world is peopled by guards, custodians, state employees, and "delinquents" confined with him for anything from waywardness to rape and homicide.

In view of this, it would be extraordinary if our Constitution did not require the procedural regularity and the exercise of care implied in the phrase "due process." Under our Constitution, the condition of being a boy does not justify a kangaroo court. . . .

## NOTE 4.

ARTHUR ALLEN LEFF  
UNCONSCIONABILITY AND THE CODE—  
THE EMPEROR'S NEW CLAUSE\*

\* \* \*

When faced with the difficulties inherent in deciding the bargaining fairness of any given transaction, the equity courts . . . leaned heavily on relatively gross classifications. In effect, they seem continually to have taken a kind of *sub rosa* judicial notice of the amount of power of certain classes of people to take care of themselves, often without too much inquiry into the actual individual bargaining situation. And it is arguable that sometimes they were wrong: not all old ladies or farmers are without defenses. Put briefly, the typical has a tendency to become stereotypical, with what may be unpleasant results even for the beneficiaries of the judicial benevolence. One can see it enshrined in the old English equity courts' jolly treatment of English seamen as members of a happy, fun-loving race (with, one supposes, a fine sense of rhythm), but certainly not to be trusted to take care of themselves. What effect, if any, this had upon the sailors is hidden behind the judicial chuckles as they protected their loyal sailor boys, but one cannot help wondering how many sailors managed to get credit at any reasonable price. In other words, the benevolent have a tendency to colonize, whether geographically or legally.

\* \* \*

d.

Geoffrey Edsall  
A Positive Approach to the Problem  
of Human Experimentation†

\* \* \*

[In 1957 there] arose the landmark case concerning the consent of a minor to donate a

\* 115 *University of Pennsylvania Law Review* 485, 556-557 (1967). © 1967. Reprinted by permission.

† 98 *Daedalus* 464-467 (1969). Reprinted by permission of Daedalus, Journal of the American Academy of Arts and Sciences, Boston, Massachusetts.

kidney for his identical twin brother, who suffered from chronic renal disease that would soon prove fatal. The question at issue was whether or not the operation to remove a kidney from the healthy brother could proceed—even with the consent of the parents and of both twins—without incurring civil or criminal liability. The judge pointed out that testimony by a psychiatrist had indicated that if the sick twin should die without the transplant, the resulting emotional disturbance could well affect the health and physical well-being of the donor twin for the rest of his life. Thus the judge found the operation was necessary "for the continued good health and future well-being of Leonard" (the healthy twin). Here we have clear reaffirmation of the principle implied in the skin graft case: If the action to be taken is at variance with traditionally or legally established procedure, it may be condoned if it is of self-interest to the person upon whom the operation is to be performed. On the other hand, the argument that the proposed action has a generous, humanistic, or idealistic basis is apparently inadmissible in court. Thus, in the eyes of the law, the individual is physically inviolable, his interests are paramount, and consent for any action that may violate the integrity of his physical being must be based upon the assumption that such action will be for his benefit.

e.

In re Brooks Estate  
32 Ill. 2d 361, 205 N.E. 2d 435 (1965)

UNDERWOOD, JUSTICE.

\* \* \*

On and sometime before May 7, 1964, Bernice Brooks was in the McNeal General Hospital, Chicago, suffering from a peptic ulcer. She was being attended by Dr. Gilbert Demange, and had informed him repeatedly during a two-year period prior thereto that her religious and medical convictions precluded her from receiving blood transfusions. Mrs. Brooks, her husband and two adult children are all members of the religious sect commonly known as Jehovah's Witnesses. Among the religious beliefs adhered to by members of this group is the principle that blood transfusions are a violation of the law of God, and that transgressors will be punished by God. . . .

Mrs. Brooks and her husband had signed a document releasing Dr. Demange and the hospital from all civil liability that might result from the failure to administer blood transfu-



sions to Mrs. Brooks. The patient was assured that there would thereafter be no further effort to persuade her to accept blood.

Notwithstanding these assurances, however, Dr. Demange, together with several assistant State's attorneys, and the attorney for the public guardian of Cook County, Illinois, appeared before the probate division of the circuit court with a petition by the public guardian requesting appointment of that officer as conservator of the person of Bernice Brooks and further requesting an order authorizing such conservator to consent to the administration of whole blood to the patient. . . . Thereafter, the conservator of the person was appointed, consented to the administration of a blood transfusion, it was accomplished and apparently successfully so, although appellants now argue that much distress resulted from transfusions due to a "circulatory overload."

\* \* \*

Appellees argue that society has an overriding interest in protecting the lives of its citizens which justifies the action here taken. . . .

\* \* \*

We believe Jefferson's fundamental concept that civil officers may intervene only when religious "principles break out into overt acts against peace and good order" has consistently prevailed. . . .

\* \* \*

. . . It seems to be clearly established that the First Amendment of the United States Constitution, as extended to the individual States by the Fourteenth Amendment to that constitution, protects the absolute right of every individual to freedom in his religious belief and the exercise thereof, subject only to the qualification that the exercise thereof may properly be limited by governmental action where such exercise endangers, clearly and presently, the public health, welfare or morals. Those cases which have sustained governmental action as against the challenge that it violated the religious guarantees of the First Amendment have found the proscribed practice to be immediately deleterious to some phase of public welfare, health or morality. The decisions which have held the conduct complained of immune from proscription involve no such public injury and no danger thereof.

Applying the constitutional guarantees and the interpretations thereof heretofore enunciated

to the facts before us we find a competent adult who has steadfastly maintained her belief that acceptance of a blood transfusion is a violation of the law of God. Knowing full well the hazards involved, she has firmly opposed acceptance of such transfusions, notifying the doctor and hospital of her convictions and desires, and executing documents releasing both the doctor and the hospital from any civil liability which might be thought to result from a failure on the part of either to administer such transfusions. No minor children are involved. No overt or affirmative act of appellants offers any clear and present danger to society—we have only a governmental agency compelling conduct offensive to appellant's religious principles. Even though we may consider appellant's beliefs unwise, foolish or ridiculous, in the absence of an overriding danger to society we may not permit interference therewith in the form of a conservatorship established in the waning hours of her life for the sole purpose of compelling her to accept medical treatment forbidden by her religious principles and previously refused by her with full knowledge of the probable consequences. In the final analysis, what has happened here involves a judicial attempt to decide what course of action is best for a particular individual, notwithstanding that individual's contrary views based upon religious convictions. Such action cannot be constitutionally countenanced.

\* \* \*

While the action of the circuit court herein was unquestionably well-meaning, and justified in the absence of decisions to the contrary, we have no recourse but to hold that it has interfered with basic constitutional rights.

Accordingly, the orders of the probate division of the circuit court of Cook County are reversed.

### 3.

#### Avoiding Fraud and Duress

- a. **Stammer v. Bd. of Regents, University of N.Y.**  
262 App. Div. 372, 29 N.Y.S. 2d 38 (1941),  
affirmed, 287 N.Y. 359, 39 N.E. 2d 913  
(1942)

PER CURIAM.

Petitioner was licensed to practice medicine in New York State on September 19, 1919.

He is now charged with violations of paragraphs (a) and (d) of subd. 2, § 1264, of the Education Law, which provide among other things that a physician's license to practice may be suspended if he is guilty of fraud and deceit in the practice of medicine or that he undertook to cure or treat a disease by a secret formula.

One Gladys Brower was suffering from a very bad cancer on the side of her face. The photographs show that it had advanced to immense size and nearly covered one side of her face. She had received both radium and surgical treatments without avail and her family had been told that there was no hope for her, and that she would live but a short time.

Petitioner had a patient named Blakeney who had told him that he had a formula that was used in the treatment of cancer. A friend of Mrs. Brower heard about Blakeney and this treatment and went to see him. Blakeney had given this formula to petitioner who was familiar with its ingredients and knew that some of them were used in treatment of cancers of this type. Petitioner also applied the treatment to portions of his own body to make certain that there would be no ill effects from it when applied to healthy tissue. Petitioner was consulted and he examined Mrs. Brower. The patient was informed that the treatment might do some good and that it could not do any harm and consented to its use. The treatment was thus applied under petitioner's directions and in due time a complete cure was effected. Blakeney was present at some of the treatments but took no part therein. The cancerous growth disappeared entirely, the sore was completely healed and there has been no reappearance. Petitioner never submitted a bill and has never received any pay whatsoever for his services, although he made something over a hundred calls. He testified that if the formula proved satisfactory he intended to write the case up for the medical profession, that the treatment was an experiment on his part and that both he and the patient knew that. He gave the contents of the formula and made no effort to conceal the same.

At least one member of the sub-committee which conducted the hearing evidenced a wholly unfair and partial attitude. This sub-committee had the duty to conduct the hearing and weigh the testimony in a manner that should have been at least quasi-judicial. Such attitude was entirely lacking in at least the one member. This doctor effected a cure when the so-called orthodox methods of treatment had failed and now he

has been punished for it. It is not fraud or deceit for one already skilled in the medical art, with the consent of the patient, to attempt new methods when all other known methods of treatment had proved futile and least of all when the patient's very life has been despaired of. Initiative and originality should not be thus effectively stifled, especially when undertaken with the patient's full knowledge and consent, and as a last resort. Under these circumstances we fail to find fraud or deceit on the part of the petitioner or that he undertook to treat or cure disease by a secret treatment and the determination is against the weight of the evidence.

The determination should be annulled and the matter remitted.

\* \* \*

b.

**John P. Dawson**

#### **Economic Duress—An Essay in Perspective\***

The boundaries of common law duress have been gradually expanding for more than a century. The processes of expansion are themselves of interest, as illustrating methods of growth in a system of case law. More important is the goal toward which this movement aims. For it is through duress and related ideas that private law has dealt most directly with problems raised by inequality in bargaining power. . . .

\* \* \*

The concept of duress which first appeared in common law sources was merely a by-product of legal controls over crime and tort. In Bracton's treatise the specific content given duress was physical assault, exerted or threatened, by means of which transfers (in particular, transfers of land) were extorted. . . . The cancellation of transfers induced by such means was a natural supplement to the sanctions then being evolved for the control of private violence.

\* \* \*

Even harder to eliminate was another restrictive formula, the requirement that the pressure or threat must be sufficient to overcome a "constant" man. The standard of "ordinary firmness" was borrowed by Bracton from the glossators, through whom it was likewise transmitted to the legal systems of continental Eu-

\* 45 *Michigan Law Review* 253-255, 261-267, 272-274, 279-280, 288-290 (1947). Reprinted by permission.

rope. . . . In American cases of the nineteenth century this formula was frequently reproduced; even in the twentieth century it has occasionally put in an appearance. . . . Its chief effect was to preserve emphasis on the misconduct of the coercing party, thus distracting attention from the specific consequences to the party coerced.

\* \* \*

Equity doctrines of undue influence had been concerned from the outset with a different type of inequality of bargaining power. They were never conceived, like common law doctrines of duress, as a corollary of the law of crime and tort. They were aimed instead at protection for the mentally or physically inadequate, whose inadequacy fell short of a total lack of legal capacity. Protection for such persons did not need to be justified through some violation, accomplished or threatened, of the law of tort or crime; indeed it was seldom that the pressure used would include any element of "wrong" as defined by damage action or criminal prosecution. It was enough that the extraction of economic gain from persons mentally or physically handicapped was condemned by prevailing standards of ethics, defined and applied by equity courts through their own independent tests.

. . . Throughout the formative period doctrines of undue influence were frequently reinforced by other protective doctrines of equity, particularly those evolved for "confidential" and "fiduciary" relationships. Close family relationships frequently provided opportunity for the exercise of "influence"; or if the parties were not related by blood or marriage, a condition of dependence by the weaker party might provide the elements of a "confidential" relationship which supplemented undue influence as ground for overhauling the transaction. . . .

It was not until the nineteenth century that serious efforts were made to explain the undue influence cases in terms of a larger objective. The objective chiefly employed soon acquired a remarkable appeal, since it coincided with main movements in nineteenth century thought. The "wrong" involved in undue influence, it was said, was the interference with another's will, which should ideally be free. The test for the existence of undue influence became the presence or absence of "free agency," whether or not the individual will have been "overpowered." From this it was easy to move to the broader thesis that, whatever the means of pressure used, "the inequity of the act consists in compelling a per-

son to do what he does not want to do." The objective defined for cases of mental or physical weakness began to seem equally appropriate for situations included in common law duress, such as threats of criminal prosecution and even duress of goods. Inspired by this new conception, the nineteenth century cases seemed to have set off in pursuit of an ideal as attractive as it was unattainable.

Even in the undue influence cases themselves, the ideal of complete freedom for the individual will was incompletely realized. In the first place, it was clear that no legal agencies would entirely eliminate the pressures that operate on the physically, mentally, or emotionally handicapped, or insulate them from all the multiplied stimuli of a complex social environment. The problem, here as elsewhere, is to select the means of pressure that are permissible and to regulate the manner in which they may be exercised. . . .

. . . It was the undue influence cases that helped most to exorcise the ghostly figure of the "constant man," who had stalked the fields of common law duress since the time of Bracton; for in the undue influence cases it was abundantly clear that the weak, the timid, the anxious and submissive were precisely the ones who should and did receive the greatest legal protection. By posing the problem of individual freedom and condemning some of the subtler forms of compulsion, the undue influence cases suggested to many courts a new approach to those other types of pressure, particularly economic pressure, which were being brought within the scope of common law duress.

Toward the end of the nineteenth century the effects of the undue influence cases were increased by the breakdown of procedural distinctions between law and equity. So long as doctrines of undue influence could be confined to "equity" cases, there remained at least a procedural distinction which preserved the purity of common law doctrine. But in the late nineteenth century and increasingly in the twentieth, undue influence was made available in law actions, both by way of defense and by way of affirmative action for restitution.

Accompanying the breakdown of divisions between law and equity there came an expansion of the content of undue influence to include various cases of psychological pressure without extreme disparities in mental or physical condition of the parties. Precise definition of the elements of undue influence had never been undertaken, indeed, had been carefully avoided. But it was

clear that the doctrine was being extended to numerous peripheral situations, just as common law duress was being pushed out beyond the typical cases on which attention had been mainly focussed. This widening range of application obscured still further the boundaries between undue influence and other types of coercion.

More rapid growth was prevented, however, by a basic contradiction in the concepts of "freedom" which were now at work. On the one hand, doctrines of undue influence were attempting to "free" the individual by regulating the pressures that restricted individual choice; on the other hand, theories of economic individualism aimed at an entirely different kind of freedom, a freedom of the "market" from external regulation. . . . From this point of view, where urgent need or special disadvantage compelled agreement to the terms proposed, these circumstances must be disregarded since they differed only in degree from the basic conditions which governed the exchange of goods and services throughout society.

To resolve this major dilemma more perspective was needed than was supplied by the materials of individual cases or the techniques of refined distinction in a system of case law. Indeed it appears that the insight gained through a close analysis of the undue influence cases helped to postpone the needed perspective, through directing attention toward a false issue. Lacking a general theory to explain the results desired, the courts approached the undue influence problem through analysis of the "will" which undue influence destroyed. It is true that in some of the more extreme cases the condition of the person "unduly" influenced might almost be described as one of complete subjection, with a "substitution" of the will of another. But even in the more extreme cases, it was usually inaccurate to say that the transfer or agreement did not result from an exercise of volition, however narrow the range within which volition could operate. In the cases of less extreme disparity, the search for evidence that the "will" had been destroyed confused the reasoning of the undue influence cases. More important than this, it left a legacy in the broader field of duress and helped to confirm the impression that relief for any type of pressure depends on a showing of complete absence of consent. This impression had filtered down from the cases of threats of physical violence, the original source of duress doctrines. Even in this type of case, courts had been slow to realize that the instances of more extreme pressure were precisely those in which the consent expressed was *more* real; the more unpleasant the alternative,

the more real the consent to a course which would avoid it.

\* \* \*

In the meantime another body of doctrine had taken shape out of the haze which surrounds the earlier activities of the English Chancery. This doctrine, giving relief to expectant heirs, promised at first no major contribution. It aimed at a narrow objective, the protection of a landed aristocracy against its own improvidence. . . .

[The] very vagueness of Chancery doctrines . . . helped greatly in their extension to related situations. The process of extension was easiest in those cases where bargain transactions were made with persons who were physically and mentally weak. Such elements of personal disability occasionally appeared in the reversioner cases themselves, and in such cases the relations between doctrines as to sales of reversions and "undue influence" were particularly close. The contribution of the reversioner cases is more directly traceable in other situations where disparity in worldly experience was coupled with extreme poverty or pressing economic need. The English cases, in considerable variety, which awarded cancellation, could spell out no reason more precise than "fraud," "surprise," or the "unconscionable" character of the bargain. Taken together they constituted an important departure from the class bias which first inspired the reversioner cases. They also established a source, from which American courts were to draw, for a broad doctrine that inadequacy of consideration would justify cancellation when coupled with extreme disparity in knowledge, experience, or economic and social position.

\* \* \*

[O]ne should not neglect the special rules developed in equity for the so-called "confidential" and "fiduciary" relationships. Throughout this large and important group it is clear that ordinary processes of bargaining are considered inappropriate, because of unusual reliance on personal honesty and good faith that exists in fact or that is thought necessary for performance of "fiduciary" functions. In enforcing such expectations of honesty and good faith, the technique commonly used is the one already encountered in the English reversioner cases, a shift in the burden of proof. The person securing an advantage from a confidential or fiduciary relationship is required to prove not only his own full disclosure of all the relevant facts, but the fairness of the transaction as a whole. The attempt to

frame substantive doctrines in terms of purely procedural handicap, together with the murky language employed in the equity cases, has disguised the main policies that are at work. Though discussion is largely in terms of false motivation ("fraud" and associated ideas), the effect is a standard of equivalence, that applies in a wide variety of personal relationships and to many types of legal transactions.

In addition there survived some broader and still vaguer doctrines of equity which admitted mere inadequacy of consideration as ground for cancellation. The language used was characteristic. After denying that equity possesses its own independent tests of adequacy of price, it was usual to add that the result was otherwise where the inadequacy was so great as to "shock the conscience." In such cases the inadequacy was said to raise a presumption of "fraud" (often expressed as a conclusive presumption), so that relief could be rested on an ancient and respectable ground of equity jurisdiction, rather than on the doubtful ground of inadequacy as such. . . .

The preceding survey was intended to suggest that the modern American law of duress reflects the convergence of several lines of growth, originally moving from sources quite distinct. The symptom of this convergence has been an increasing interplay and transfer of ideas. Its result has certainly not been a coherent body of doctrine, unified around some central proposition: on the contrary, the conflict and confusion in results of decided cases seem greater than ever before. This conflict and confusion must be attributed in part to the fact that the processes of growth are still continuing and the effects of earlier history are not yet dissipated. In part, however, they are due to the complex issues of ethics and economic policy that constantly intrude themselves and on which courts, like other agencies of organized society, must take a positive stand.

\* \* \*

c.

**Friedrich Kessler**  
**Contracts of Adhesion—Some Thoughts**  
**about Freedom of Contract\***

The development of large scale enterprise with its mass production and mass distribution

made a new type of contract inevitable—the standardized mass contract. A standardized contract, once its contents have been formulated by a business firm, is used in every bargain dealing with the same product or service. The individuality of the parties which so frequently gave color to the old type contract has disappeared. The stereotyped contract of today reflects the impersonality of the market. It has reached its greatest perfection in the different types of contracts used on the various exchanges. Once the usefulness of these contracts was discovered and perfected in the transportation, insurance, and banking business, their use spread into all other fields of large scale enterprise, into international as well as national trade, and into labor relations. It is to be noted that uniformity of terms of contracts typically recurring in a business enterprise is an important factor in the exact calculation of risks. Risks which are difficult to calculate can be excluded altogether. Unforeseeable contingencies affecting performance, such as strikes, fire, and transportation difficulties can be taken care of. The standard clauses in insurance policies are the most striking illustrations of successful attempts on the part of business enterprises to select and control risks assumed under a contract. The insurance business probably deserves credit also for having first realized the full importance of the so-called "juridical risk," the danger that a court or jury may be swayed by "irrational factors" to decide against a powerful defendant. Ingenious clauses have been the result. Once their practical utility was proven, they were made use of in other lines of business. It is highly probable that the desire to avoid juridical risks has been a motivating factor in the widespread use of warranty clauses in the machine industry limiting the common law remedies of the buyer to breach of an implied warranty of quality and particularly excluding his right to claim damages. . . .

The use of standard contracts has, however, another aspect which has become increasingly important. Standard contracts are typically used by enterprises with strong bargaining power. The weaker party, in need of the goods or services, is frequently not in a position to shop around for better terms, either because the author of the standard contract has a monopoly (natural or artificial) or because all competitors use the same clauses. His contractual intention is but a subjection more or less voluntary to terms dictated by the stronger party, terms whose consequences are often understood only in a vague way, if at all. Thus, standardized contracts are frequently

\* 43 *Columbia Law Review* 629, 631-633, 640-642 (1943). Reprinted by permission.

contracts of adhesion; they are *à prendre ou à laisser*. . .

And yet the tremendous economic importance of contracts of adhesion is hardly reflected in the great texts on contracts or in the Restatement. As a matter of fact, the term "contract of adhesion" or a similar symbol has not even found general recognition in our legal vocabulary. This will not do any harm if we remain fully aware that the use of the word "contract" does not commit us to an indiscriminate extension of the ordinary contract rules to all contracts. But apparently the realization of the deepgoing antinomies in the structure of our system of contracts is too painful an experience to be permitted to rise to the full level of our consciousness. Consequently, courts have made great efforts to protect the weaker contracting party and still keep "the elementary rules" of the law of contracts intact. As a result, our common law of standardized contracts is highly contradictory and confusing, and the potentialities inherent in the common law system for coping with contracts of adhesion have not been fully developed. The law of insurance contracts furnishes excellent illustrations. Handicapped by the axiom that courts can only interpret but cannot make contracts for the parties, courts had to rely heavily on their prerogative of interpretation to protect a policy holder. To be sure many courts have shown a remarkable skill in reaching "just" decisions by construing ambiguous clauses against their author even in cases where there was no ambiguity. Still, this roundabout method has its disadvantages. . .

\* \* \*

The individualism of our rules of contract law, of which freedom of contract is the most powerful symbol, is closely tied up with the ethics of free enterprise capitalism and the ideals of justice of a mobile society of small enterprisers, individual merchants and independent craftsmen. This society believed that individual and cooperative action left unrestrained in family, church and market would not lessen the freedom and dignity of man but would secure the highest possible social justice. It was firmly convinced of a natural law according to which the individual serving his own interest was also serving the interest of the community. Profits can be earned only by supplying consumable commodities. Freedom of competition will prevent profits from rising unduly. The play of the market if left to itself must therefore maximize net satisfactions.

Justice within this framework has a very definite meaning. It means freedom of property and of contract, of profit making and of trade. Freedom of contract thus receives its moral justification. The "prestabilized harmony" of a social system based on freedom of enterprise and perfect competition sees to it that the "private autonomy" of contracting parties will be kept within bounds and will work out to the benefit of the whole.

With the decline of the free enterprise system due to the innate trend of competitive capitalism towards monopoly, the meaning of contract has changed radically. Society, when granting freedom of contract, does not guarantee that all members of the community will be able to make use of it to the same extent. On the contrary, the law, by protecting the unequal distribution of property, does nothing to prevent freedom of contract from becoming a one-sided privilege. Society, by proclaiming freedom of contract, guarantees that it will not interfere with the exercise of power by contract. Freedom of contract enables enterprisers to legislate by contract and, what is even more important, to legislate in a substantially authoritarian manner without using the appearance of authoritarian forms. Standard contracts in particular could thus become effective instruments in the hands of powerful industrial and commercial overlords enabling them to impose a new feudal order of their own making upon a vast host of vassals. This spectacle is all the more fascinating since not more than a hundred years ago contract ideology had been successfully used to break down the last vestiges of a patriarchal and benevolent feudal order in the field of master and servant. Thus the return back from contract to status which we experience today was greatly facilitated by the fact that the belief in freedom of contract has remained one of the firmest axioms in the whole fabric of the social philosophy of our culture.

\* \* \*

In the happy days of free enterprise capitalism the belief that contracting is law making had largely emotional importance. Law making by contract was no threat to the harmony of the democratic system. On the contrary it reaffirmed it. The courts, therefore, representing the community as a whole, could remain neutral in the name of freedom of contract. The deterioration of the social order into the pluralistic society of our days with its powerful pressure groups was needed to make the wisdom of the contract

theory of the natural law philosophers meaningful to us. The prevailing dogma, on the other hand, insisting that contract is *only* a set of operative facts, helps to preserve the illusion that the "law" will protect the public against any abuse of freedom of contract. This will not be the case so long as we fail to realize that freedom of contract must mean different things for different types of contracts. Its meaning must change with the social importance of the type of contract and with the degree of monopoly enjoyed by the author of the standardized contract.

## NOTES

## NOTE 1.

NEW YORK GENERAL OBLIGATIONS LAW (1964)  
 AGREEMENTS EXEMPTING LESSORS  
 FROM LIABILITY FOR NEGLIGENCE VOID  
 AND UNENFORCEABLE

§ 5-321. Every covenant, agreement or understanding in or in connection with or collateral to any lease of real property exempting the lessor from liability for damages for injuries to person or property caused by or resulting from the negligence of the lessor, his agents, servants or employees, in the operation or maintenance of the demised premises or the real property containing the demised premises shall be deemed to be void as against public policy and wholly unenforceable.

## NOTE 2.

FEDERAL COMMUNICATIONS COMMISSION  
 ADVERTISEMENT OF CIGARETTES\*

\* \* \*

The Commission's previous action [with regard to the advertisement of cigarettes] was designed to carry out the Congressional policy embodied in the 1965 [Cigarette Labeling and Advertising] Act of not, in effect, barring cigarette advertisements and at the same time promoting intensive smoker-education during the life of the Act.

... It required that a broadcast licensee presenting cigarette commercials—which convey "... any number of reasons why it appears desirable to smoke ..."—must "... devote a significant amount of time to informing the listeners of the other side of the matter—that however enjoyable smoking may be, it repre-

sents a habit which may cause or contribute to the earlier death of the user."

\* \* \*

... As stated in the 1967 Report to Congress on the Health Consequences of Smoking by the Department of Health, Education, and Welfare:

In the 3½ years since the publication of [the 1964] report, an unprecedented amount of pertinent research has been completed, continued, or initiated in this country and abroad under the sponsorship of governments, universities, industry groups, and other entities. This research has been reviewed and no evidence has been revealed which brings into question the conclusions of the 1964 report. On the contrary, the research studies published since 1964 have strengthened those conclusions and have extended in some important respects our knowledge of the health consequences of smoking.

The present state of knowledge of these health consequences can, in the judgment of the Public Health Service, be summarized as follows:

1. Cigarette smokers have substantially higher rates of death and disability than their non-smoking counterparts in the population. This means that cigarette smokers tend to die at earlier ages and experience more days of disability than comparable non-smokers.

2. A substantial portion of earlier deaths and excess disability would not have occurred if those affected had never smoked.

3. If it were not for cigarette smoking, practically none of the earlier deaths from lung cancer would have occurred; nor a substantial portion of the earlier deaths from chronic bronchopulmonary diseases (commonly diagnosed as chronic bronchitis or pulmonary emphysema or both); nor a portion of the earlier deaths of cardiovascular origin. Excess disability from chronic pulmonary and cardiovascular diseases would also be less.

4. Cessation or appreciable reduction of cigarette smoking could delay or avert a substantial portion of deaths which occur from lung cancer, a substantial portion of the earlier deaths and excess disability from chronic bronchopulmonary diseases, and a portion of the earlier deaths and excess disability of cardiovascular origin.

\* \* \*

[P]resentation of commercials promoting the use of cigarettes is inconsistent with the obligation imposed upon broadcasters to operate in the public interest. One of the foremost facets of the public interest standard is public health, as the Court pointed out in *Banzhaf v. F.C.C.* ... We are here faced with a most serious, unique danger to public health "authenticated by official and Congressional action. ..." It would thus ap-

\* Notice of Proposed Rule Making, 34 *Federal Register* 1959 (1969).



pear wholly at odds with the public interest for broadcasters to present advertising promoting the consumption of the product posing this unique danger—a danger measured in terms of an epidemic of deaths and disabilities.

The commercials do promote the use of cigarettes. As we developed in our 1967 document, that is understandably their purpose. We also note that in its 1968 report to Congress, the Federal Trade Commission concluded:

In 1964 and again in 1967, the Commission found that three principal themes dominate cigarette advertising. These are that (1) smoking and particularly the taste derived from it are satisfying; (2) smoking is associated with that which is desirable or even good; and (3) it is an activity relatively free of hazard.

A review of specimen 1967 and early 1968 advertising, obtained through the Commission's continuous monitoring program and also directly from cigarette advertisers, reveals that these three themes, the "satisfaction" theme, the "associative" theme, and the "assuaging of anxiety" (relative to the danger of cigarette smoking) theme, continue to dominate.

There is no question but that cigarette commercials have significant impact. Here we note initially that the broadcast industry is the recipient of more than 75 percent of the advertising dollar of cigarette manufacturers, in the amount of \$244.4 million in 1967. This expenditure, when measured in terms of "exposures" on television to members of the broadcast audience (i.e., the number of cigarette commercials times the estimated program audience), resulted in 13.3 billion exposures in January, 1968 alone. Finally, we note that the commercials reach children to a very significant extent. . . .

\* \* \*

This brings up a most important consideration—that of voluntary industry action to eliminate cigarette commercials. We specifically listed this possibility in our 1967 decision. We again stress it, and indeed regard it as a threshold matter—ahead of any final consideration of the issue by either the Commission or Congress. The broadcast industry does not accept the advertising of hard liquor. . . . Why, then, should this same industry accept cigarette commercials in the face of the public health findings. . . .

\* \* \*

The proposed rule would simply provide that after a certain date, broadcast licensees shall not present cigarette advertising. . . .

The proposed rule does not affect the presentation of broadcast material concerning cigarette smoking in any other form, such as in newscasts, documentaries, roundtable discussions, etc. Licensees might adjudge that there is a controversial issue to be discussed or explored, and here we refer to all facets of the matter (including the issue of this notice, a ban on radio and TV advertising). They, of course, might well conclude that the anti-smoking messages, which contribute to an informed public in this critical area, should continue unabated, with the cigarette manufacturer afforded the opportunity to present his side in newscasts, documentaries, roundtable discussions, and other formats. . . .

\* \* \*

NOTE 3.

WILLIAMS V. WALKER-THOMAS FURNITURE CO.  
350 F.2d 445 (D.C.Cir. 1965)

J. SKELLY WRIGHT, CIRCUIT JUDGE:

\* \* \*

[W]e hold that where the element of unconscionability is present at the time a contract is made, the contract should not be enforced.

Unconscionability has generally been recognized to include an absence of meaningful choice on the part of one of the parties together with contract terms which are unreasonably favorable to the other party. Whether a meaningful choice is present in a particular case can only be determined by consideration of all the circumstances surrounding the transaction. In many cases the meaningfulness of the choice is negated by a gross inequality of bargaining power. The manner in which the contract was entered is also relevant to this consideration. Did each party to the contract, considering his obvious education or lack of it, have a reasonable opportunity to understand the terms of the contract, or were the important terms hidden in a maze of fine print and minimized by deceptive sales practices? Ordinarily, one who signs an agreement with full knowledge of its terms might be held to assume the risk that he has entered a one-sided bargain. But when a party of little bargaining power, and hence little real choice, signs a commercially unreasonable contract with little or no knowledge of its terms, it is hardly likely that his consent, or even an objective manifestation of his consent, was ever given to all the terms. In such a case the usual rule that the terms of the agreement are not to be questioned should

be abandoned and the court should consider whether the terms of the contract are so unfair that enforcement should be withheld.

\* \* \*

d.

**Miranda v. Arizona**  
**384 U.S. 436 (1966)**

MR. CHIEF JUSTICE WARREN delivered the opinion of the Court.

The cases before us raise questions which go to the roots of our concepts of American criminal jurisprudence: the restraints society must observe consistent with the Federal Constitution in prosecuting individuals for crime. More specifically, we deal with the admissibility of statements obtained from an individual who is subjected to custodial police interrogation and the necessity for procedures which assure that the individual is accorded his privilege under the Fifth Amendment to the Constitution not to be compelled to incriminate himself.

\* \* \*

Today . . . there can be no doubt that the Fifth Amendment privilege is available outside of criminal court proceedings and serves to protect persons in all settings in which their freedom of action is curtailed in any significant way from being compelled to incriminate themselves. We have concluded that without proper safeguards the process of in-custody interrogation of persons suspected or accused of crime contains inherently compelling pressures which work to undermine the individual's will to resist and to compel him to speak where he would not otherwise do so freely. In order to combat these pressures and to permit a full opportunity to exercise the privilege against self-incrimination, the accused must be adequately and effectively apprised of his rights and the exercise of those rights must be fully honored.

\* \* \*

At the outset, if a person in custody is to be subjected to interrogation, he must first be informed in clear and unequivocal terms that he has the right to remain silent. For those unaware of the privilege, the warning is needed simply to make them aware of it—the threshold requirement for an intelligent decision as to its exercise. More important, such a warning is an absolute prerequisite in overcoming the inherent pressures

of the interrogation atmosphere. It is not just the subnormal or woefully ignorant who succumb to an interrogator's imprecations, whether implied or expressly stated, that the interrogation will continue until a confession is obtained or that silence in the face of accusation is itself damning and will bode ill when presented to a jury. Further, the warning will show the individual that his interrogators are prepared to recognize his privilege should he choose to exercise it.

The Fifth Amendment privilege is so fundamental to our system of constitutional rule and the expedient of giving an adequate warning as to the availability of the privilege so simple, we will not pause to inquire in individual cases whether the defendant was aware of his rights without a warning being given. Assessments of the knowledge the defendant possessed, based on information as to his age, education, intelligence, or prior contact with authorities, can never be more than speculation; a warning is a clearcut fact. More important, whatever the background of the person interrogated, a warning at the time of the interrogation is indispensable to overcome its pressures and to insure that the individual knows he is free to exercise the privilege at that point in time.

The warning of the right to remain silent must be accompanied by the explanation that anything said can and will be used against the individual in court. This warning is needed in order to make him aware not only of the privilege, but also of the consequences of foregoing it. It is only through an awareness of these consequences that there can be any assurance of real understanding and intelligent exercise of the privilege. Moreover, this warning may serve to make the individual more acutely aware that he is faced with a phase of the adversary system—that he is not in the presence of persons acting solely in his interest.

The circumstances surrounding in-custody interrogation can operate very quickly to overbear the will of one merely made aware of his privilege by his interrogators. Therefore, the right to have counsel present at the interrogation is indispensable to the protection of the Fifth Amendment privilege under the system we delineate today. Our aim is to assure that the individual's right to choose between silence and speech remains unfettered throughout the interrogation process. A once-stated warning, delivered by those who will conduct the interrogation, cannot itself suffice to that end among those

who most require knowledge of their rights. . . . [T]he need for counsel to protect the Fifth Amendment privilege comprehends not merely a right to consult with counsel prior to questioning, but also to have counsel present during any questioning if the defendant so desires.

\* \* \*

An individual need not make a pre-interrogation request for a lawyer. While such request affirmatively secures his right to have one, his failure to ask for a lawyer does not constitute a waiver. . . .

\* \* \*

In order fully to apprise a person interrogated of the extent of his rights under this system then, it is necessary to warn him not only that he

has the right to consult with an attorney, but also that if he is indigent a lawyer will be appointed to represent him. Without this additional warning, the admonition of the right to consult with counsel would often be understood as meaning only that he can consult with a lawyer if he has one or has the funds to obtain one. . . .

\* \* \*

If the interrogation continues without the presence of an attorney and a statement is taken, a heavy burden rests on the government to demonstrate that the defendant knowingly and intelligently waived his privilege against self-incrimination and his right to retained or appointed counsel. . . .

\* \* \*

## C. To Encourage Rational Decisionmaking

### 1.

#### Allowing the Subject to Know

##### a.

#### **Halushka v. University of Saskatchewan 52 W.W.R. 608 (Sask. 1965)**

HALL, J. A.—The appellants, Wyant and Merriman, were medical practitioners employed by the appellant, University of Saskatchewan. The appellant, Wyant, was professor of anaesthesia and chief of the department of anaesthetics at the University Hospital. The appellant, Merriman, was director of the cardio-pulmonary laboratory. As part of their duties in the employ of the appellant, University of Saskatchewan, the appellants, Wyant and Merriman, conducted and carried out medical research projects, some of which involved the comparative study of anaesthetics. When anaesthetics were administered, the subjects were obtained from the employment office.

The respondent, a student at the University of Saskatchewan, had attended summer school in 1961. On August 21, 1961, he went to the employment office to find a job. At the employment office he was advised that there were no jobs available but that he could earn \$50 by being the subject of a test at the University Hospital. The

respondent said that he was told that the test would last a couple of hours and that it was a "safe test and there was nothing to worry about."

The respondent reported to the anaesthesia department at the University Hospital and there saw the appellant, Wyant. The conversation which ensued concerning the proposed test was related by the respondent as follows:

Dr. Wyant explained to me that a new drug was to be tried out on the Wednesday following. He told me that electrodes would be put in my both arms, legs and head and that he assured me that it was a perfectly safe test, it had been conducted many times before. He told me that I was not to eat anything on Wednesday morning and I was to report at approximately nine o'clock, then he said it would take about an hour to hook me up and the test itself would last approximately two hours, after the time I would be given fifty dollars, pardon me, I would be allowed to sleep first, fed and then given fifty dollars and driven home on the same day.

The appellant, Wyant, also told the respondent that an incision would be made in his left arm and that a catheter or tube would be inserted into his vein.

The respondent agreed to undergo the test and was asked by the appellant, Wyant, to sign a form of consent. This form, entered as Ex.D.1, reads as follows:

Intensive Care  
460-57-2

Halushka, Walter  
72756 Jan 2 '40 MR.  
Dr. Nanson

*Consent for Tests on Volunteers*

I, Walter Halushka, age 21 of 236-3rd Street, Saskatoon hereby state that I have volunteered for tests upon my person for the purpose of study of

*Heart & Blood Circulation Response under General Anaesthesia*

The tests to be undertaken in connection with this study have been explained to me and I understand fully what is proposed to be done. I agree of my own free will to submit to these tests, and in consideration of the remuneration hereafter set forth, I do release the chief investigators,

*Drs. G. M. Wyant and J. E. Merriman*, their associates, technicians, and each thereof, other personnel involved in these studies, the University Hospital Board, and the University of Saskatchewan from all responsibility and claims whatsoever, for any untoward effects or accidents due to or arising out of said tests, either directly or indirectly.

I understand that I shall receive a remuneration of \$50.00 for one test

Witness my hand and seal.

[Sgd.] WALTER HALUSHKA

[Sgd.] IRIS ZAECHTOWSKI (Witness)

Date: Aug. 22/61

The respondent described the circumstances surrounding the signing of D.I, saying:

He then gave me a consent form, I skimmed through it and picked out the word "accident" on the consent form and asked Doctor Wyant what accidents were referred to, and he gave me an example of me falling down the stairs at home after the test and then trying to sue the University Hospital as a result. Being assured that any accident that would happen to me would be at home and not in the hospital I signed the form.

The test contemplated was known as "The Heart and Blood Circulation Response under General Anaesthesia," and was to be conducted jointly by the appellants, Wyant and Merriman, using a new anaesthetic agent known commercially as "Fluoromar." This agent had not been

previously used or tested by the appellants in any way.

The respondent returned to the University Hospital on August 23, 1961, to undergo the test. The procedure followed was that which had been described to the respondent and expected by him, with the exception that the catheter, after being inserted in the vein in the respondent's arm, was advanced towards his heart. When the catheter reached the vicinity of the heart, the respondent felt some discomfort. The anaesthetic agent was then administered to him. The time was then 11:32 A.M. Eventually the catheter tip was advanced through the various heart chambers out into the pulmonary artery where it was positioned.

The appellants, Wyant and Merriman, intended to have the respondent reach medium depth of surgical anaesthesia. However, an endotracheal tube which had been inserted to assist the respondent in breathing caused some coughing. In the opinion of the appellant, Wyant, the coughing indicated that the respondent was in the upper half of light anaesthesia—on the verge of waking up. At 12:16 P.M., therefore, the concentration of the mixture of the anaesthetic was increased. The respondent then descended into deeper surgical anaesthesia.

At about 12:20 P.M. there were changes in the respondent's cardiac rhythm which suggested to the appellants, Wyant and Merriman, that the level of the anaesthetic was too deep. The amount of anaesthetic was then decreased, or lightened.

At 12:25 P.M. the respondent suffered a complete cardiac arrest.

The appellants, Wyant and Merriman, and their assistants took immediate steps to resuscitate the respondent's heart by manual massage. To reach the heart, an incision was made from the breastbone to the line of the armpit and two of the ribs were pulled apart. A vasopressor was administered as well as urea, a drug used to combat swelling of the brain. After one minute and 30 seconds the respondent's heart began to function again.

The respondent was unconscious for a period of four days. He remained in the University Hospital as a patient until discharged 10 days later. On the day before he was discharged, the respondent was given \$50 by the appellant, Wyant. At that time the respondent asked the appellant, Wyant, if that was all he was going to get for all he went through. The appellant said that \$50 was all that they had bargained for but that

he could give a larger sum in return for a complete release executed by the respondent's mother or elder sister.

As a result of the experiment, the appellants concluded that as an anaesthetic agent "Fluoromar" had too narrow a margin of safety and it was withdrawn from clinical use in the University Hospital.

The respondent brought action against the appellants, basing his claim for damages on two grounds, namely, trespass to the person and negligence. The action came on for trial before Balfour, J., sitting with a jury. The respondent called Dr. Mark Baltzan, whose testimony regarding medical practice and whose expert opinion were supplemented by, and in some areas confirmed by, questions and answers from the examinations-for-discovery of the appellants, Wyant and Merriman.

The medical evidence established that the use of any anaesthetic agent involves a certain amount of risk and should be accompanied by care and caution. In general medical practice the risk involved in the use of an anaesthetic agent is balanced against the threat to life presented by the ailment to be treated. It is standard procedure to obtain a medical history of the patient and in some cases to conduct a complete physical examination before administering a general anaesthetic. The medical history is for the most part obtained by interrogating the patient himself. The taking of a medical history usually involves investigation of the functioning of certain of the organic systems. Included are questions primarily related to the heart, to ascertain whether the patient has had any specific heart disease, such as high blood pressure or rheumatic fever, in the past.

In the instant case the appellants, Wyant and Merriman, admit that the cardiac arrest would not have occurred if the respondent had not undergone the test, the arrest being caused by the anaesthetic agent used. Dr. Baltzan was of the opinion that the test itself had been well conducted. He also gave his opinion that the insertion of a catheter into the heart is not a dangerous procedure.

If a patient does not die immediately from cardiac arrest, the damage which might ensue can vary in degree from none at all to eventual death with all intermediate degrees possible. Brain damage is the usual cause of death and most of the intermediate damage occurring will be to the brain. The brain cells can be damaged either permanently or temporarily. The portion

of the brain most susceptible to damage under these circumstances is that which controls the highest functions, that is, the thinking functions as contrasted to the lowest or automatic functions. Major damage is objective as the patient is totally oblivious to his surroundings. Minor degrees of damage are more subjective as they are confined to emotional and intellectual attributes and are difficult to detect clinically. Dr. Baltzan had examined the respondent prior to the trial and could find no abnormality but he stated that he knew of no equipment available today which would necessarily and unequivocally determine whether there had been minor brain damage.

In Dr. Baltzan's opinion a certain amount of pain would be associated with the incision necessary for the open massage of the heart and expected general discomfort at the site of the incision for a month or two. The respondent himself testified that he experienced a considerable amount of pain in the chest area and that a portion of his left arm was numb for approximately six weeks.

\* \* \*

The respondent returned to the university in the fall of 1961. He testified that he became very tired every day and that he had to rest for about three hours before doing his homework. Although this condition did gradually improve, the respondent said that he was never able to complete his homework because of it. The respondent failed in six or seven subjects that year. He said he could not think or concentrate on problems as he had before. He therefore did not try to continue with his university course.

At the time of the trial the respondent was employed as an electrician at Thompson, Man., earning \$376 per month. He stated that it was difficult for him to think or concentrate and that he could not understand instructions given to him in the course of the employment unless they were given very slowly.

The appellants, at the close of the respondent's case, moved a non-suit. The motion was denied by the trial judge. The questions then put to the jury and the answers to them were as follows:

1. Q: Did the plaintiff consent to the performance of the test made by the defendant doctors? A: No.
2. Q: If the answer to Question 1 is no, did the defendant doctors commit a trespass in the performance of the test? A: Yes.
3. Q: Were the defendant doctors or either of

them negligent in the performance of the test? A: Yes.

4. Q: If the answer to Question 3 is yes, in what respect was there negligence? A: (1) Lack of full explanation to the plaintiff of the test at the time of the so-called consent. (2) Failure to acquire medical history of the plaintiff and to perform a physical examination of the plaintiff. (3) Lack of liaison between the two defendant doctors throughout this test.

5. Q: If the answer to Question 2 or Question 3 is yes, then at what amount do you assess the plaintiff's damage? A: \$22,500.00.

From these findings and the judgment thereon the appellants appeal on the grounds:

1. That the learned trial judge erred in refusing to withdraw the plaintiff's claim from the jury on the ground that there was no evidence upon which the jury could find liability against the defendants or either of them.

2. That the learned trial judge misdirected the jury in respect of the consent which had been signed by the plaintiff and further erred in instructing them that this was a case of a doctor and patient relationship whereas he should have charged the jury that it was a contractual relationship.

3. That the findings of the jury on all the questions submitted to them were perverse.

\* \* \*

The main issue before the jury concerning the respondent's claim of trespass to the person was that of consent. The attachment of the electrodes, the administration of anaesthetic and the insertion of the catheter were each an intentional application of force to the person of the respondent. When taken as a whole they certainly constitute a trespass which would be actionable unless done with consent: See *Parmley v. Parmley and Yule* [1945] SCR 635, reversing (*sub nom. Yule v. Parmley*) [1945] 1 WWR 405, 61 BCR 116. The appellants rely upon Ex.D.1 and the conduct of the respondent as evidence of consent.

In ordinary medical practice the consent given by a patient to a physician or surgeon, to be effective, must be an "informed" consent freely given. It is the duty of the physician to give a fair and reasonable explanation of the proposed treatment including the probable effect and any special or unusual risks. . . .

\* \* \*

It was on the basis of the ordinary physician-patient relationship that the learned trial judge charged the jury on the matter of consent. In dealing with this part of the case he said:

In the circumstances of this case I will say that before signing such a document the plaintiff was entitled to a reasonably clear explanation of the proposed test and of the natural and expected results from it.

In my opinion, the duty imposed upon those engaged in medical research, as were the appellants, Wyant and Merriman, to those who offer themselves as subjects for experimentation, as the respondent did here, is at least as great as, if not greater than, the duty owed by the ordinary physician or surgeon to his patient. There can be no exceptions to the ordinary requirements of disclosure in the case of research as there may well be in ordinary medical practice. The researcher does not have to balance the probable effect of lack of treatment against the risk involved in the treatment itself. The example of risks being properly hidden from a patient when it is important that he should not worry can have no application in the field of research. The subject of medical experimentation is entitled to a full and frank disclosure of all the facts, probabilities and opinions which a reasonable man might be expected to consider before giving his consent. The respondent necessarily had to rely upon the special skill, knowledge and experience of the appellants, who were, in my opinion, placed in the fiduciary position described by Lord Shaw in *Nocton v. Ashburton* (Lord) [1914] AC 932, 83 LJ Ch 784, when he said at p. 969:

Once . . . the relation of parties has been so placed, it becomes manifest that the liability of an adviser upon whom rests the duty of doing things or making statements by which the other is guided or upon which that other justly relies can and does arise irrespective of whether the information and advice given have been tendered innocently or with a fraudulent intent.

And at p. 972:

. . . [O]nce the relations of parties have been ascertained to be those in which a duty is laid upon one person of giving information or advice to another upon which that other is entitled to rely as the basis of a transaction, responsibility for error amounting to misrepresentation in any statement made will attach to the adviser or informer, although the information and advice have been given not fraudulently but in good faith.

Although the appellant, Wyant, informed the respondent that a "new drug" was to be tried out, he did not inform him that the new drug was, in fact, an anaesthetic of which he had no previous knowledge, nor that there was risk involved with the use of an anaesthetic. Inas-

much as no test had been previously conducted using the anaesthetic agent "Fluoromar" to the knowledge of the appellants, the statement made to the respondent that it was a safe test which had been conducted many times before, when considered in the light of the medical evidence describing the characteristics of anaesthetic agents generally, was incorrect and was in reality a non-disclosure.

The respondent was not informed that the catheter would be advanced to and through his heart but was admittedly given to understand that it would be merely inserted in the vein in his arm. While it may be correct to say that the advancement of the catheter to the heart was not in itself dangerous and did not cause or contribute to the cause of the cardiac arrest, it was a circumstance which, if known, might very well have prompted the respondent to withhold his consent. The undisclosed or misrepresented facts need not concern matters which directly cause the ultimate damage if they are of a nature which might influence the judgment upon which the consent is based.

The explanation of Ex.D.1 given by the appellant, Wyant, to the respondent could be misleading and could well serve to distract the respondent from a proper appraisal of his position.

In view of the foregoing, there was no misdirection on the question of consent of which the appellants can complain and there was evidence upon which the jury could find that the respondent gave no effectual consent or release to the appellants. The appellants cannot, therefore, succeed on their first three grounds of appeal in so far as they relate to the respondent's claim of trespass.

\* \* \*

## NOTES

### NOTE 1.

#### FOOD AND DRUG ADMINISTRATION CONSENT FOR USE OF INVESTIGATIONAL NEW DRUGS (IND) ON HUMANS—STATEMENT OF POLICY\*

(a) Section 505 (i) of the act provides that regulations on use of investigational new drugs on humans shall impose the condition that investigators "obtain the consent of such human beings or their representatives, except where they deem it not feasible or, in their professional judgment, contrary to the best interest of such human beings."

\* 32 *Federal Register* 8753 (1967); see 21 *Code of Federal Regulations* §130.37 (1971).

(b) this means that the consent of such humans (or the consent of their representatives) to whom investigational drugs are administered primarily for the accumulation of scientific knowledge, for such purposes as studying drug behavior, body processes, or the course of a disease, must be obtained in all cases and, in all but exceptional cases, the consent of patients under treatment with investigational drugs or the consent of their representatives must be obtained.

(c) "Under treatment" applies when the administration of the investigational drug for diagnostic, therapeutic, or other purpose involves medical judgment, taking into account the individual circumstances pertaining to the patient to whom the investigational drug is to be administered.

(d) "Exceptional cases" as used in paragraph (b) of this section are those relatively rare cases in which it is not feasible to obtain the patient's consent or the consent of his representative, or in which, as a matter of professional judgment exercised in the best interest of a particular patient under the investigator's care, it would be contrary to that patient's welfare to obtain his consent.

(e) "Patient" means the person under treatment.

(f) "Not feasible" is limited to cases wherein the investigator is not capable of obtaining consent because of inability to communicate with the patient or his representative; for example, the patient is in a coma or is otherwise incapable of giving consent, his representative cannot be reached, and it is imperative to administer the drug without delay.

(g) "Contrary to the best interests of such human beings" applies when the communication of information to obtain consent would seriously affect the patient's well-being and the physician has exercised a professional judgment that under the particular circumstances of this patient's case, the patient's best interests would suffer if consent were sought.

(h) "Consent" means that the person involved has legal capacity to give consent, is so situated as to be able to exercise free power of choice, and is provided with a fair explanation of pertinent information concerning the investigational drug, and/or his possible use as a control, as to enable him to make a decision on his willingness to receive said investigational drug. This latter element means that before the acceptance of an affirmative decision by such person the investigator should carefully consider and make known to him (taking into consideration



such person's well-being and his ability to understand) the nature, expected duration, and purpose of the administration of said investigational drug; the method and means by which it is to be administered; the hazards involved; the existence of alternative forms of therapy, if any; and the beneficial effects upon his health or person that may possibly come from the administration of the investigational drug.

When consent is necessary under the rules set forth in this section, the consent of persons receiving an investigational new drug in Phase 1 and Phase 2 investigations (or their representatives) shall be in writing. When consent is necessary under such rules in Phase 3 investigations, it is the responsibility of investigators, taking into consideration the physical and mental state of the patient, to decide when it is necessary or preferable to obtain consent in other than written form. When such written consent is not obtained, the investigator must obtain oral consent and record that fact in the medical record of the person receiving the drug.

# NOTES

## NOTE 1.

WATSON v. CLUTTS

262 N.C. 153, 159, 136 S.E. 2d 617, 621 (1964)

HIGGINS, JUSTICE.

\* \* \*

Difficulty arises in attempting to state any hard and fast rule as to the extent of the disclosure required. The doctor's primary duty is to do what is best for the patient. Any conflict between this duty and that of a frightening disclosure ordinarily should be resolved in favor of the primary duty. And yet, the consent of the patient or of someone duly authorized to consent for him, except in emergencies, is required before the operation is undertaken. The surgeon should disclose danger of which he has knowledge and the patient does not—but should have—in order to determine whether to consent to the risk.

\* \* \*

## NOTE 2.

PUTENSEN v. CLAY ADAMS, INC.

12 CAL. APP.3d 1062, 1084, 91 CAL.

RPTR. 319, 333 (1970)

MOLINARI, ASSOCIATE JUSTICE.

\* \* \*

Plaintiff requested that the jury be instructed that a doctor-patient relationship is one fidu-

ciary in nature and a doctor accordingly must reveal all pertinent information to his patient. Such request was refused. Plaintiff urges such refusal was error since there was evidence that Dr. Paley failed to apprise plaintiff of the risks involved in the heart catheterization.

It is the duty of a doctor to properly explain a contemplated procedure or operation to his patient in a manner which the patient can reasonably comprehend in order for the patient to give his informed or knowledgeable consent to the procedure or operation. Here, the evidence does not show that Dr. Paley withheld any information from plaintiff. Although plaintiff testified that Dr. Paley failed to explain the operation and its risks, she also testified that she had specifically requested not to be told about the intricacies of heart catheterization. Moreover, plaintiff, prior to the operation, looked into heart catheterization and stated she was aware of what it involved. . . . Under the circumstances of this case it may not be said that plaintiff's consent to the procedure was not an informed one. Rather, the evidence was such that Dr. Paley's attempts at explanation were prevented by plaintiff's insistence on remaining ignorant of the risks involved and that Dr. Paley acceded to this request in the exercise of his discretion on the basis that an explanation of any risk involved might result in actually increasing the risks by reason of the psychological results of the apprehension itself. Accordingly, we perceive no error in refusing the requested instruction.

\* \* \*

b.

Alexander M. Capron

The Law of Genetic Therapy\*

\* \* \*

[A] distinction is sometimes drawn between the kind of consent needed from a patient in treatment as against that required of a subject in research. Some have argued, however, that there is no basis for a distinction between therapy and experimentation. A. C. Ivy has written that

Even after the therapy of a disease is discovered, its application to the patient remains, in part, experimental. Because of the physiological variations in the response of different patients to the same therapy, the therapy of disease is, and will always be, an ex-

\* M. Hamilton, ed.: *The New Genetics and the Future of Man*. Grand Rapids, Michigan: Eerdmans Publishing Co. (1972). © 1971. Reprinted by permission.

perimental aspect of medicine. [T]he patient is always to some extent an experimental subject of the physician. . . .

On the theory that the medical profession agrees with Ivy that the distinction is bogus, Jay Katz has carried this argument one step further. He finds in physicians' collective "reluctance to examine" the ethics of medical experimentation a "conscious or unconscious realization that any resolution of the problems posed by human experimentation cannot be limited to research settings, but instead has far-reaching consequences for medical practice."

While there is certainly at least a grain of truth in the position that all treatment is experimentation, I do not agree that there is no distinction between the conditions for consent in the two settings. However, I take a rather heretical view of which situation sets a higher standard. As suggested by the quotation from Katz, it is usually assumed that "more" consent is needed from experimental subjects than from patients; thus, physicians fear that the standards developed for subjects will be extended to their patients, for whom physicians have traditionally been allowed to make many decisions on "best interests" grounds.

Yet if we look at these contexts carefully and focus on the psychological stance of the patient or subject, it seems to me that the standard approach has it backwards. Higher requirements for informed consent should be imposed in therapy than in investigation, particularly when an element of honest experimentation is joined with the therapy. The "normal volunteer" solicited for an experiment is in a good position to consider the physical, psychological and monetary risks and benefits to him in consenting to participate. How much harder that is for the patient to whom an experimental technique is offered during a course of treatment. The man proposing the experiment is one to whom the patient may be deeply indebted (emotionally as well as financially) for past care and on whom he is probably dependent for his future well-being; the procedure may be offered, despite its unknown qualities, because more conventional modalities have proved ineffective. Even when a successful, but slow, recovery is being made, patients offered new therapy often have eyes only for its novelty and not for its risks. To paraphrase an observation of Dr. Francis D. Moore:

People in this country have been weaned on newspaper accounts of exciting new cures. Particularly in

the field of [genetics], patients are pressing their doctors to be the subjects of innovation.

How many of those of us who heard Dr. Anderson describe the process of virus-instigated changes in genes would volunteer to have that procedure performed on ourselves tomorrow? Yet what would our responses be if we suffered from a rare genetic anomaly? The answer was given by Dr. Anderson's example of the German girls who are being treated for argininemia: "Many parents . . . would not only allow [previously untried genetic therapy], they would urge its use." Certainly, part of the difference in response of subjects and patient-subjects is based on the obvious difference in the potential benefits which may be derived from participation. But that would not entirely explain the far greater favorable response in the second situation. And while, as I argued previously, we may not wish in any particular instance to override the consent given by a patient whose strong desire for treatment causes him to overrate the benefits and underestimate the risks of a research technique, I believe we should nevertheless decide as a general rule to set higher requirements for consent and to impose additional safeguards on experimentation-with-therapy lest investigators (even unwittingly) expose "consenting" patient-subjects to unreasonable risks.

The disinterested weighing of risks and benefits suggested here is not intended to imply that patients usually ought to be excluded as subjects; in some research it may be necessary to have persons manifesting a disease or defect in order to study that condition. Yet this is not always the case. For example, the ova for research on *in vitro* human development need not be obtained from infertile women; these women merely provide a more compliant subject population for the necessary laproscopies (surgery on the egg follicles) than normal women would.

\* \* \*

C.

#### **Daedalus—NIH Working Party Ethical Aspects of Experimentation on Human Subjects\***

MOORE: It does not help the surgeon one whit to have talked to the patient because the

\* Sponsored by *Daedalus* and the Department of Health, Education, and Welfare. Public Health Service, National Institutes of Health. Proceedings of Working Party #3 (December 5, 1966). Reprinted by permission of *Daedalus*, Journal of the American Academy of Arts and Sciences, Boston, Massachusetts.

patient has no concept of the hazard involved in taking out a small piece of liver. Let me ask you, what is the hazard to the patient in taking out a small piece of liver?

KATZ: I would turn to you for the answer.

MOORE: In other words, if you cannot even answer this question, what real good is consent doing—this is an informed consent. The hazard of taking out a small piece of liver is very peculiar; it is that of a bile leak. How could the patient possibly know that? In fact, the surgeon probably doesn't know that himself.

KATZ: Could you translate what we ought to communicate to the patient about this into ordinary language?

MOORE: Well, I like your question because my answer is a categorical, "It's ridiculous." By telling that to a patient and somehow assuaging yourself, all you are doing is making yourself feel better. There has been no ethical transaction. It is a vacuum because the patient does not know anything about the hazard of taking out a bit of his liver. All you have done is to make yourself feel better because you have talked to him and he has signed something.

KATZ: What are the hazards?

MOORE: I have just told you.

KATZ: What does it mean?

MOORE: In effect, . . . [it] is a tiny hazard, but it is the hazard and it is there. All I am saying is that you have made a brushstroke, but there is nothing in the brush, nothing. You have stroked yourself, and you have stroked the patient, by having this little talk with him, but there is no advantage. Ethically the problem has been unchanged by saying to the patient, "Mr. Jones, we are planning to take your gall bladder out in the morning, do you mind terribly if we take a bit of liver out for my old friend Gus?"

KATZ: But let me carry this a step further. You have to tell him about the hazards. . . .

MOORE: Well you say, what's the hazard? and I say there have not been a great many done so I cannot give you a significant figure. I suppose if this were done in a thousand patients, one or two might have quite a sore belly for a few days. That is about all you could say. All I am trying to say is that here the patient's assumed confidence in his doctor is being taken advantage of. So where are you? You have not moved. You are still on home base.

\* \* \*

RUTSTEIN: I feel that on this matter of informed consent, the majority of patients, re-

gardless of educational level, probably will not understand what you are talking about. That is true. I think you have a responsibility. This does not mean that you should try to do something to a patient without explaining to him what you are going to do. You should explain to him not only what you are going to do, but what the hazards are. I think the hazards for a liver biopsy are a little more specific than Dr. Moore would imply at the moment. In any event, you ought to tell the patient the story insofar as he is able to understand it. On the other hand, I do not set much store by informed consent as an important part of this problem, because I think that any doctor who is a good doctor and who has the confidence of his patient can get his patient to do practically anything he wants. So I do not think this really has much meaning. I think the problem is to make sure that the person who is talking to the patient is honest and objective. . . .

\* \* \*

Informed consent, however you look at it, has so many liabilities. I still think the doctor ought to tell a patient, as part of the job of being the patient's doctor, what the risks are. But I do not set very much store by this as a protection for the patient. Not if the doctor is also an investigator. Even though you do all of this mumble-jumble—and I still think it is fine to do it, I am in favor of doing it—but I do not think it means anything. I agree with Dr. Moore. I do not think it means anything in terms of making medical research more ethical in the long run.

\* \* \*

. . . I think the patient needs other controls. I do not think [this explanation is] a control. It is a good thing to do, but I do not think it is a control.

\* \* \*

BLUMGART: What you say is true but it is only an element. The patient when asked about this might come back to the doctor and say, "Is this necessary to take a piece of my liver out?" And the doctor says, "No." "Well then, I do not want to have it taken out if it is not necessary." That might also be an answer.

\* \* \*

RUTSTEIN: If I may try to answer your [comment] indirectly. I think you [must distinguish between] the question of being informed [and] that of consent because you get shocking situations. [I]n New York, they actually in-

jected living cancer cells into human beings without telling them anything about it. [T]he need for informing the patient [in such a situation] is evident. People feel very strongly that these individuals should have been told that they were being given living cancer cells. . . .

FREUND: The investigator may [perhaps] be ashamed to explain and therefore he refrains. I do not think he could bring himself to say, "My friend Gus wants a piece of liver."

KATZ: Dr. Rutstein just made a very important distinction. The concept of informed consent may have to be studied from two aspects: what needs to be disclosed and what importance to place on consent. [W]hatever [the] reasons which eventually will make the patient agree, the physician has a duty and obligation to inform [the patient] that he wants to take out a piece of liver. I think it is good for the patient, and it is good for the physician.

MOORE: I agree with that. [Moreover, from] a legal point of view, it is . . . essential for the physician to protect himself.

d.

### Oliver Cope Breast Cancer—Has the Time Come for a Less Mutilating Treatment?\*

In May, 1958, a physician called and asked me to come immediately to his office to see a patient with a lump in her breast. He had arranged for her to enter the hospital the following day under the care of a well-known cancer surgeon, but at lunchtime she had dismissed him and was now asking for me. Would I please take care of her?

\* \* \*

There was, indeed, a lump in one of her breasts which felt as if it were malignant. Not knowing why she had dismissed the other surgeon, I was wary in what I said. I began by reminding her that I was not sure about the nature of the lump, and that as the first step she should have a biopsy to establish its identity. I told her that we did not need to go beyond a biopsy until we had a chance to consult with each other. She agreed.

When I saw her alone at the hospital the next day, she said, "I expect you are surprised that I have asked you to care for me. I first no-

ticed the lump in my breast several months ago. It has been slowly increasing in size. I had decided that I would do nothing about it and accept the consequences. But, recently my arthritis flared up and I had to see my physician. I did not tell him about the lump, but he, of course, found it. As I expected, he insisted that I be operated upon, and the surgeon he first chose for me unequivocally advised that I have my breast removed.

"It may seem strange to you, but I have a horror of losing my breast. I am 62, my husband is dead, and I have no thought of marrying again. However, I am still horrified by the thought of losing my breast, and I asked for you because I thought you might help me find a way to keep it."

I examined her again. The mass was large. Above the breast, under the fold of her pectoral muscles, was a smaller lump consistent with spread of cancer into the nearby lymph nodes of the axilla. Together, these two masses meant that the growth was already advanced, and, according to the criteria of the day, even radical surgery would probably only delay, not cure, the disease.

At this point, I recalled that two years before another woman in her sixties had consulted me about a lump in her breast. The lump had been a small one with a good outlook if treated by the traditional surgical operation. She had refused, however, to have her breast removed. In view of her refusal, I had asked Dr. Laurence Robbins, chief of the radiology department at Massachusetts General Hospital, to treat her by radiation. She was well and free of evidence of any residual cancer now at the time of this current patient.

I told my [patient] that I would ask Dr. Robbins to treat her with radiation if the lump proved to be malignant, as indeed it did. I also followed my promise to her to remove only the lump and not the breast. Her physician was very upset when I did not do the traditional mastectomy, and her son-in-law, also a physician, was outraged at my neglect.

After radiation of the breast and adjacent areas, the secondary lump melted away, and my patient was remarkably well for the next six years. Then suddenly she felt poorly, lost strength and weight, and died within a month at the age of 68. An autopsy revealed that no cancer cells were within the breast or the lymph nodes in the axilla, showing that the radiation had been successful in these irradiated areas. Cancer cells, however, were widely distributed

\* 54 *Radcliffe Quarterly* 6-11 (No. 4, 1970). Reprinted by permission of Maryel Finney Hartung Locke, editor.

throughout her body, having escaped from the area of the breast either before or during the treatment.

\* \* \*

With the knowledge which we now possess in 1970, how should cancer of the breast be treated? . . . I favor a considerable change in attitude toward treatment. Unfortunately, there are still large gaps in our knowledge which must be filled in before we will have solid, accountable treatments. For example, until the forces giving rise to cancerous tumors are identified, we will not have a vaccination or inoculation such as we now possess for poliomyelitis or a drug with the specificity that penicillin has against the streptococcus. There is room for much optimism on these points, however, since a number of investigators are pursuing leads which may prove successful in the foreseeable future. But we cannot wait for this more complete knowledge. How should we treat tumors at present?

Until physicians know more about the origin of cancer, there is room for disagreement as to what constitutes the best treatment. With this incomplete knowledge in mind, I suggest the following:

Women should know that there are alternatives. There is room now for them to have a say. They don't need to be railroaded into having their breast removed.

The physician needs to know the full diagnosis, including every bit of knowledge that can be obtained. Diagnostic procedures such as roentgenographic mammograms are helpful in directing the physician's attention.

A biopsy is essential. Under special circumstances, a needle biopsy may suffice, but since the needle offers the pathologist so little tissue to study, an open biopsy is much to be preferred. The surgeon and pathologist should examine not only the type of cell involved and the rate of proliferation, but also the cell distribution. Can cells be seen entering the blood vessels of the tumor itself? Are they confined to the tumor, or are they spreading widely through the lymph spaces? These important details cannot be identified on frozen sections. The tissue has to be properly prepared for study, which takes at least 24 hours, and then time is needed for study and consultation. The patient has the right to ask of her surgeon that he delay decision regarding definitive therapy until these questions have been answered and their meaning discussed with her. It is true that biopsy theoretically carries a risk of disseminating cancer cells

and that on this basis the breast should be removed immediately after the biopsy. But it has been shown that this danger of biopsy is slight and shrinks to unimportance when compared with the knowledge gained by waiting for the pathologist's more seasoned study.

Regarding the definitive therapy, it is to be remembered that surgery still has its good points. For those tumors of sluggish growth, it is probably the best treatment, since slowly growing types of cells are not so sensitive to radiation as are the rapidly proliferating cells.

Another argument in favor of operation is that some patients prefer psychologically to be done with the problem. The removal of the breast with the tumor in it seems to some to have eliminated the problem—out-of-sight, out-of-mind. This is, of course, a short view. In this sophisticated world, women know well that a cancer can catch up with them later. Also, some women are not aware, psychologically, of what the loss of the breast may mean to them later on, either as a mutilation or as a daily reminder that they once had cancer.

The advantages of non-surgical treatment, making use of modern radiation, are obvious. First, the two-to-three-day pause between biopsy and definitive decision enables the doctors to be sure the tumor is really malignant. From time to time, haste to get rid of the tumor has led pathologist and surgeon to believe it malignant, whereas in retrospect it was benign. A frozen section isn't good enough in the borderline cases, and unnecessary mastectomies are sometimes carried out by the overly anxious surgeon, fearful of neglecting his patient.

Second, if there is blood vessel invasion, any therapy may well be too late. Therapy should not be withheld, however, since the cells in the blood stream may not have taken root in distant organs. Nothing will have been sacrificed. If cells have taken root, palliative treatment is still indicated, and radiation is a more reasonable, less destructive palliation than surgery.

The third point is that modern radiation theoretically offers a better chance of eradicating all of the cancer than surgery. The radical mastectomy removes only the primary tumor and the nodes in the axilla. Radiation can also destroy primary tumor and axillary nodes, and in addition it may be able to eradicate cells in the internal chest nodes.

It is essential to realize that radiation, like surgery, is not without hazard. Radiation is a powerful tool and can burn healthy organs if not properly directed. The radiotherapist has to be

watchful that his beam does not injure the lung or the spinal cord. The patient also must realize that during the course of the radiation there may be discomfort in swallowing as the rays hit the esophagus. Cough may also develop from the radiation's hitting the borders of the lungs. It is usually slight and transient. Some fibrosis of the breast is also to be expected. Usually minimal, it goes unnoticed by most patients; occasionally considerable, it can be alarming until its nature is understood.

A disadvantage of the radiation program is that it takes time, at least six weeks and sometimes a good deal longer. The radiation has to cover the middle of the chest on both sides, and if the esophagus feels a little hot and painful, the therapy has to be slowed up or delayed. The length of the time required, however, is not theoretically disadvantageous, since more of the cells are given the chance to come into the phase of mitosis when they are theoretically more sensitive to the radiation.

Finally, there is the psychological advantage to the woman of keeping her breast. The breast is part of woman's beauty; the art of our civilization tells us this. Woman's breasts are also a part of her sexuality. It is she who builds the infant and nourishes it after it is born. The breast is inherently part of the survival of the race. What is so strange is that the surgeon has been so slow to realize how woman feels about her breasts. The only adequate explanation for his lack of feeling is that the problem of mutilation is too much for him to manage. Only when mutilation is put to him in terms of an analogy—the loss of his masculinity—does he react to it.

Woman has been willing to put up with a mastectomy when she was told there was no other way to rid her of the tumor. Now that there is a feasible alternative, the efforts of medicine should be directed toward improving the non-mutilating therapy. She has a right to demand this of the profession.

#### NOTE

JON R. WALTZ AND THOMAS W. SCHEUNEMAN  
INFORMED CONSENT TO THERAPY\*

\* \* \*

The informed consent concept, clearly enough, separates into two elements: informa-

tion and consent. A two-fold duty is imposed: the physician must disclose certain information about collateral risks, and he must not proceed without consent to the risks which were, or should have been, disclosed. The cases have been concerned primarily with the duty of information disclosure but the scope and content of both duties must be analyzed since each poses separate and difficult legal puzzles. Ideally, a patient will be informed of all possible collateral risks of contemplated therapy and his consent to confronting them will then insulate his physician from liability if any described risk materializes. The broad problems for analysis are the necessary and the acceptable limitations on the realization of this ideal.

The "information" element of informed consent concerns the scope of the physician's duty to disclose collateral risks. This duty becomes relevant when the patient neither knows nor could be expected to know of particular collateral risks. There is no need to disclose risks that either ought to be known by everyone or that are in fact known to the patient because of prior experience with the therapy in question.

\* \* \*

A physician probably need not disclose every risk which *could* be disclosed, if only because of the time required to disclose every remote risk. Less than "total" disclosure will satisfy the law's demands, but how much less?

\* \* \*

The question of what risks of therapy should be disclosed to the patient must take two interests into account. The first interest is the patient's desire and right, at least under other than life-preserving conditions, to make his own decision whether to undergo a particular therapy. The law, to serve this interest, must assess the significance of a risk in terms of the potential effect of knowledge of it on the patient's decision. The law must, as a matter of policy, set the level of effect which will be deemed significant. The traditional legal litmus for measuring the significance of information in decision-making is "materiality." Since the patient's interest in making an informed decision is paramount, the first principle is that all material risks should be disclosed to him. The content of the standard of materiality of risks will therefore determine the scope of the duty to disclose. Materiality of risk is thus the first issue in the duty to disclose.

The second interest, sometimes at odds with the first, is the physician's desire to withhold

\* 64 *Northwestern University Law Review*, 628, 637-640, 643-645 (1969). Reprinted by special permission of the Northwestern University Law Review. Copyright © 1970.

information about risks the disclosure of which may have a harmful effect on the well-being of the patient. . . .

Materiality is the keystone of the physician's duty to disclose. The first task, then, is to determine what risks are material and, therefore, to be disclosed in the absence of a privilege to withhold. While the cases have not clearly articulated standards of materiality, two have been proposed by commentators. It has been suggested, at one extreme, that only risks which would cause the patient to forego the therapy need be disclosed. At the other extreme, it has been argued that any risk which might have any influence, however slight, on the patient's decision to accept a therapy should be disclosed.

The first standard is myopic. It does not take into account the fact that although a single risk of a given magnitude may not cause a patient to forego a therapy, two or more such risks in combination might have that result. A standard of materiality limited to risks which, in isolation, would cause a patient to refuse a therapy deprives the patient of the opportunity to contemplate possible combinations of risks. Conversely, a rule inflexibly dictating disclosure of all risks, however unlikely they are to affect a patient's judgment, is unrealistic. We must reach middle ground.

The materiality of a risk must be determined in the first instance by the physician. Since he must first know how much impact a risk must have on the patient's judgment before its disclosure is dictated, the issue should be approached from the physician's point of view. The ideal rule would require that a risk be disclosed when the patient would attach importance to it, alone or in combination with others, in making his decision whether or not to consent to the therapy in question. But a physician obviously cannot be required to know the inner workings of his patient's mind. He can, however, employ his general experience with people. He can be required to exercise a sense of how the average, reasonable man would probably react. Additionally, he will know, or can reasonably be required to know, his particular patient's background, present circumstances and prognosis. In resolving the materiality issue, the physician—and the courts—can apply the standard of the reasonable man who finds himself in the position of the patient.

A risk is thus material when a reasonable person, in what the physician knows or should know to be the patient's position, would be

likely to attach significance to the risk or cluster of risks in deciding whether or not to undergo the proposed therapy.

\* \* \*

Consent . . . connotes the dual elements of awareness and assent. To establish consent to a risk, it must be shown both that the patient was aware of the risk and that he assented to encounter it. The hard question involves the kind of evidence that will be admitted to establish these elements.

Preliminarily, it is obvious that a risk must have been understandably communicated before the element of awareness can be established. Communication involves the manner in which the physician must disclose risks—the vocabulary he must adopt and the degree of elaboration in which he must engage. While the cases rightly indicate that technical language will not ordinarily suffice to disclose a risk to an untutored layman, they are unclear as to what more is required. To require the physician, absolutely, to use language which his patient will in fact understand calls, once again, for clairvoyance. Furthermore, any such requirement would be logically untenable within the concepts of semantics. One can only communicate in terms which, based on experience and perceptions of the recipient's capacity, one believes he will understand. Translated to the legal framework, the physician should be required to disclose risks in such terms as a reasonable man would believe the patient would understand. The disclosure should elaborate, in the same sort of terms, the nature and severity of the risk and the likelihood of its occurrence.

Some courts apparently take the approach that only the subjective state of mind of the patient should be considered in establishing the elements of awareness and assent. Under this approach, a physician could proceed only where the patient subjectively understood a risk and subjectively intended to manifest his assent to it. One difficulty with this view is that the patient's testimony, undeniably admissible at trial, in fact controls the issue of consent. And the trial lawyer's healthy cynicism tells him that a claimant's testimony is sometimes susceptible to modification based upon hindsight. Another difficulty is that it leaves no room for reasonable communication or interpretation mistakes by the physician; he assumes the risk of incorrectly concluding that the patient in fact understood and assented to the risks communicated. As the



entire history of contract law attests, legal relationships based on communication cannot practicably be made to depend on the vagaries of the parties' subjective intent.

\* \* \*

## 2.

### Helping the Subject to Decide

a.

#### **Russell v. Harwick** **166 So. 2d 904 (Fla. 1964)**

CARROLL, JUDGE.

The appellee Charlotte Harwick fell and broke her hip. Following an operation and treatment by the appellant Dr. Lyle Russell, an orthopedic surgeon, Mrs. Harwick sued Dr. Russell and others charging negligence and trespass to the person. . . . A jury trial resulted in a verdict against Dr. Russell. . . . Dr. Russell has appealed.

The complaint alleged that Dr. Russell was negligent in failing to inform the plaintiffs as to the alternate methods of surgical treatment he contemplated, and in proceeding without the informed consent of the plaintiffs; that had they been informed of such alternative procedures they would not have consented to the procedure utilized. . . .

\* \* \*

Mrs. Harwick was treated by Dr. Harris, an internist, and Dr. Russell, the orthopedic surgeon. Dr. Harris was a friend of the plaintiffs and had rendered professional service to them in the past. They engaged him to take charge of the case. His training fitted him to supervise the medication, but not the surgical processes. There was an issue and conflicting evidence on whether the plaintiffs were informed as to the nature of the operative processes contemplated and recommended by the surgeon, and their risks, dangers, and outcome probabilities. A jury question was presented as to whether the consent given for surgery by Mrs. Harwick was an informed consent.

The doctor attempted as a first process to reduce the fracture by manipulation to be secured with the use of a nail. Unable to obtain the proper positional result in that process, Dr. Russell proceeded with an alternative process

in which he performed an operation of severing the head of the femur and substituting therefor a metal (Austin Moore) prosthesis. This operation amounted to major surgery, and one which was calculated to result in some change in the length of the leg. The plaintiffs stated that had they known this was the intended or contemplated procedure they would have insisted on an orthopedic consultation, and would not have been in favor of the process.

. . . There was also an issue and conflicting evidence from which the jury could have found for or against the defendant doctor as to the need or propriety of the process undertaken in view of the lack of emergency and the circumstances of the injury as then present.

\* \* \*

Basically, this appeal amounts to a contention of want of evidence or of sufficient evidence on the material issues. We find no such deficiency in the record, and our examination of the record leads to the conclusion that the verdict was justified and had adequate evidentiary support.

\* \* \*

*Affirmed.*

DUVAL, ASSOCIATE JUDGE (dissenting).

I can not agree with the conclusions reached in the majority opinion.

\* \* \*

Dr. Russell was personally retained by Mrs. Harwick on the evening of her accident for the purpose of caring for her condition. After examining the x-rays he informed her it would be necessary for him to operate in order to repair her hip, to which she consented.

Mrs. Harwick had sustained a complicated fracture of the neck of the femur with damage to the head. All the medical testimony agreed there were two generally accepted procedures to repair such a break. One was to try and manually reduce the fracture and fasten it with a nail or pin. The other was to remove the head of the femur and substitute a metallic prosthesis. All medical witnesses testifying agreed that if a correct alignment could not be accomplished that nailing could not be used.

In this case Dr. Russell first attempted to manually reduce the fracture and failed. If he had succeeded he would have opened the hip, drilled a hole up through the femur into the

head of the femur, inserted a pin, closed the incision completing the operation. After being unable to obtain a manual reduction of the fracture he proceeded with the alternative method of removing the head of the femur and replacing it with a metallic prosthesis.

There was a difference of opinion between the doctors testifying as to what action they would have taken after manual reduction failed, a failure quite common in this type of fracture. Several testified they would have cut open the hip capsule and attempted to reduce the fracture manually and, if successful, drill and place a pin through the top of the head of the femur down into the femur. Others testified that they would have opened the capsule, but instead of attempting to drill and pin through the top of the head of the femur would remove the head and replace it with a metallic prosthesis, using the same procedure followed by Dr. Russell.

All the medical testimony recognized both alternative procedures of repair, after manual reduction failure, to be recognized and approved methods of treatment and would leave the procedure selected to the judgment of the individual doctor performing the operation. Each of the procedures described entailed complicated and major surgery.

The fact that certain physicians would have proceeded in a different manner than the defendant is not a basis for malpractice. The Supreme Court of Florida has held that the courts cannot hold a defendant in a malpractice suit to the theory of one physician to the exclusion of the contrary opinion by another physician, if the treatment used is approved by a respectable minority of the medical practice. The treatment afforded by Dr. Russell was approved by the majority of physicians testifying in this case and was not disapproved by any.

\* \* \*

Mrs. Harwick's third count alleged that the defendants were liable on a theory of trespass to the person for the reason that in adopting the surgical procedure utilized they failed to inform the plaintiff of the alternative procedures available and the potential consequences involved in the repair of the hip operation.

Liability for failure to fully inform a patient as to all the alternative procedures available and the potential consequences in an operation is relatively new in Florida. . . .

\* \* \*

The question is whether or not Dr. Russell was legally required, under the circumstances of this case, to detail to Mrs. Harwick all of the procedures involved and did such a failure cause him to be liable under the theory of operating without the informed consent of the patient.

\* \* \*

This is not a case where the doctor misrepresented any material facts or used false misrepresentation in an attempt to gain the consent of his patient to operate.

In reviewing this record it is found that in addition to the alternative methods of repair discussed, there were other recognized and acceptable methods as well as a great number of individual techniques used in the performance of these operative procedures. In addition, the record discloses that there are many different types of nails or pins used and a number of different metallic prosthesis available (at least thirty) for use. Would it, in order to have the informed consent of the patient, have been necessary to detail each of the many methods, techniques and appliances which could be used? Would such information assist a patient in making an intelligent choice? I think not.

\* \* \*

In this case Mrs. Harwick personally retained Dr. Russell's services for the purpose of repairing her hip. Dr. Russell advised her of the necessity to operate in order to accomplish her wishes. She was advised of the necessity of the operation thirty-six (36) hours prior to the surgery. During the elapsed time she spoke with Dr. Harris; she had her husband by her side for most of the day; she was alert and knew what the situation was. She testified that she relied on Dr. Russell to do whatever was necessary to relieve her condition. She did not request him or anyone to advise her of what specific method of repair was going to be attempted. Mrs. Harwick was not limited in any way in making inquiry concerning the details of the surgery. It was her prerogative to rely on the professional judgment of the Defendant without making further inquiry, which she did.

\* \* \*

There are no cases, that I have been able to find, which require a physician having consent, not gained by misrepresentation, to describe to his patient each technique or procedure available before proceeding with a required and

necessary operation. If a patient must have an operation, it is not required that a physician describe all of the gory details and further disturb the patient. The patient in most cases has no desire to have the physician describe all of the details. However, if the patient wishes to have such a description, it would be his duty to inquire and the physician's duty to give reasonable and prudent answers.

The issue of informed consent should not have been submitted to the jury.

\* \* \*

## NOTES

### NOTE 1.

MICHAEL JUSTIN MYERS  
INFORMED CONSENT IN MEDICAL MALPRACTICE\*

\* \* \*

It is submitted that the law's strong predisposition toward personal control over bodily invasions has been violated by the current judicial treatment of informed consent. That predisposition could be more effectively served if the following standard were adopted:

A physician is under an obligation (1) to make a full disclosure of all known material risks in a proposed operation or course of treatment except for those risks of which the patient is likely to know or (2) to prove the reasonableness of any lesser disclosure or the immateriality of the undisclosed risk.

\* \* \*

Where there is no problem of consent, and where harm results from an operation, the doctor need only compensate the patient for negligence. Since the patient bears the entire risk of nonnegligent injury and is concerned with his own interests as no other person can be, it is only fair to allow him to choose between accepting or rejecting the hazards of a proposed treatment even if his choice is irrational.

If a squeamish individual would rather not know about the potential hazards of treatment, he may delegate control to his physician through a conscious, knowing waiver. The physician presumably desires the best for his patient. But the doctor who proceeds under the doctor-knows-best theory without securing a deliberate waiver from his patient and without disclosing collateral hazards substitutes his judgment about the de-

sirability of undergoing a risk for that of his patient. Such substitution is inconsistent with the law's respect for the patient's control over his own body. . . .

It is objectionably paternalistic for a physician to justify nondisclosure of risks on the ground that otherwise "both the patient and [physician] would back out." To tolerate such argument is to subordinate the patient's control of his body to his physician's absolute discretion.

\* \* \*

### NOTE 2.

HENRY K. BEECHER  
CONSENT IN CLINICAL EXPERIMENTATION—  
MYTH AND REALITY\*

\* \* \*

There is the disturbing and widespread myth that "codes" (all of which emphasize, above all else, consent) will provide some kind of security. While there is value, doubtless, to be gained from their examination as guides to the thinking of others on the subject, the reality is that any rigid adherence to codes can provide a dangerous trap: no two situations are alike; it is impossible to spell out all contingencies in codes. When an accident occurs, in the course of experimentation, it will be easy for the prosecution to show failure to comply fully, and an endless vista of legal actions opens up. It is a curious thing that lawyers for even the greatest institutions are much more likely, in my experience, to cripple themselves and their institutions with inevitably imperfect codes than are the investigators involved, who usually understand the pitfalls represented by the codes. Security rests with the *responsible* investigator who will refer difficult decisions to his peers.

Most codes dealing with human experimentation start out with the bland assumption that consent is ours for the asking. This is a myth. The reality is that informed consent is often exceedingly difficult or impossible to obtain in any complete sense. The difficulties inherent in this complex situation are no excuse for giving up the effort: informed consent is a goal toward which we strive. . . . Imperfect as our attempts to get informed consent may be, an important reality nevertheless invariably emerges from such effort: The patient involved then knows he is to be the subject of an experiment—too often not

\* 55 *California Law Review* 1396, 1407-1410 (1967). Copyright, © 1967, California Law Review, Inc. Reprinted by permission.

\* 195 *Journal of the American Medical Association* 34-35 (1966). Reprinted by permission.

otherwise the case—and, knowing, can reject the opportunity if he chooses to do so.

\* \* \*

A different kind of myth is that propounded by some critics; namely, that if the investigator says he got consent all is well. . . . Far more dependable evidence of right or wrong is to be found in examination of the given investigation itself. It is clear that many published studies never should have been undertaken in the first place.

\* \* \*

A particularly pernicious myth is the one that depends on the view that ends justify means. A study is ethical or not at its inception. It does not become ethical merely because it turned up valuable data. Sometimes such a view is rationalized by the investigator as having produced the most good for the most people. This is blatant statism. Whoever gave the investigator the god-like right of choosing martyrs?

Is the patient, then, without hope for honest, responsible care? Not at all. His great safeguard in experimentation as in therapy is the presence of the skillful, informed, intelligent, honest, responsible, compassionate physician. And one hopes and believes these are in the majority.

b.

**Ralph J. Alfid**  
**Informed Consent—**  
**A Study of Patient Reaction\***

I have questioned a number of physicians concerning their reactions to obtaining an informed consent from the patient who is about to undergo a diagnostic or therapeutic procedure. Their reactions vary from (1) "I always inform my patients of what is coming, and the possible complications," to (2) "I never do it; it's a waste of time," to most frequently, (3) "If I give my patients a comprehensive explanation of what is to be done and what possible complications might ensue, the result would be the wholesale refusal of patients to undergo the procedure." For many years, I advocated the latter.

\* \* \*

. . . When this statistical study of informed consent was begun, it was expected to prove that

patients would indeed refuse angiography after they were informed of its possible complications. Much to my surprise it proved the opposite.

Two forms were used in this study. The second form was prepared because of the objections of my colleagues to the content and construction of the first form. . . . The first form was presented to 132 patients before angiography.

**FORM I**

DEPARTMENT OF HOSPITAL RADIOLOGY—  
SPECIAL PROCEDURE SECTION  
INFORMED CONSENT FOR ARTERIOGRAPHY  
AND PHLEBOGRAPHY

Dear Patient,

Your doctor has referred you to us for an angiogram (a study of your blood vessels). We would like to inform you of what we are going to do and of possible complications that might result from this procedure.

A small tube (catheter) will be introduced into one or several of your blood vessels. Later we will inject a dye which is opaque to x-rays. This will enable us to see blood vessels in your body which may be diseased. The catheter will be introduced into an artery either in your groin or at times in your arm, just above the elbow. This is done either by puncturing the artery with a needle, or through the means of minor surgery. The study is done under local anesthesia.

Although the possibility of clotting the vessel used is small, it does happen occasionally. In addition, it is possible that an artery or arteries feeding an organ could also be clotted. In either of these circumstances, it may be necessary to perform surgery to remove the clot or to treat you with certain medications which may dissolve the clot. I am sure you realize that although the risk is very small, clotting the blood supply to an organ can result in the loss of that organ and, remotely, in the loss of life. The latter is true of other complications but is just as rare.

During the procedure, it is possible that dye (contrast medium) might result in an adverse reaction causing hives, shortness of breath, extremely low blood pressure, and, rarely, temporary or permanent paralysis.

Upon removing the catheter, it is occasionally difficult to stop bleeding; a small lump (hematoma) may form at the point where the catheter was introduced into the blood vessel. These generally subside in several days; however, large hematomas may have to be treated

\* 216 *Journal of the American Medical Association* 1325-1329 (1971). Reprinted by permission.

with surgical evacuation and the hole in the artery sewed shut. More rarely, but another possibility, a small tear may develop in the blood vessel resulting in what we call pseudoaneurysm, which acts like a bulge in a weakened rubber tire. This may also require corrective surgery.

There are still more unusual complications which we could mention, but because they are so rare it would be impractical to list them. If you desire, these will be discussed with you.

Our overall "serious" complications rate is approximately 1 in 500 cases. Your chances of being injured in an auto accident in the United States during 1969 were 1 in 100.

Unfortunately, this information may have alarmed you, but I believe it to be in your best interests to know what is involved.

Sincerely,

---

This form was presented to patients by the residents or nursing staff of our department. In general, no explanation was given until the form was read and completed. Anxious patients and patients who refused on the basis of the information given were approached by members of the professional staff to allay apprehension and to dissuade those who had refused the procedure. Form I most frequently was given to the patients about an hour before the angiogram and before any premedication had been administered.

Initial objections to this form by colleagues were: (1) too strongly worded; (2) too weakly worded; (3) did not provide actual mortality statistics and therefore no reference point of ultimate risks; (4) did not state that the referring physician had weighed the risks vs the advantages, and that the latter were much greater than the former; (5) objections to inclusion of the questionnaire within the form. Some felt that the questions should be appended on the last page. Because of the objections and questions concerning the above, a revised form was substituted and presented to the next 100 patients on whom angiograms were performed.

## FORM II

### CONSENT FOR ANGIOGRAPHY

Dear Patient,

Your doctor has referred you for an angiogram, which is a study of your blood vessels. This is one of the most accurate studies we can make concerning the condition of your blood

vessels. As with all medical procedures, it carries some risks, about which we think you should be informed. Your doctor is aware of these risks and has determined that the benefit in diagnostic information which may be obtained from the arteriogram outweighs the potential risk of the procedure.

In this procedure, a small tube (catheter) is introduced into one or several of your blood vessels. Through this tube, a solution will be injected which will enable us to see your blood vessels on x-rays. This tube is introduced into a blood vessel, either in your arm or your groin, by means of minor surgery under local anesthesia.

Patients, understandably, wonder what complications can occur from this procedure. It does involve some minor surgery and it does involve entering the body and the blood stream. The usual complications which we would consider relatively minor, but nevertheless can be distressing to patients, are accumulations of blood in the tissues where the catheter has been introduced (hematoma) or a small outpouching of the artery at the site where it was entered by the catheter. There are less frequent complications which we consider more serious, which might lead to serious damage or to loss of an organ. Surgery may be required to correct the complication.

Very rarely, complications from the procedure have resulted in death. This has occurred four times in the 6,500 angiograms we have performed.

Our overall serious complications rate is approximately one in 500 angiograms.

It would be impractical, and probably misleading to the average person, to describe here in detail all of the complications which might possibly result from this procedure. If you would like more detailed information, we will be glad to discuss it with you.

Sincerely yours,

---

An attempt was made to give this form to each patient as early as possible either on the night or day before the angiogram. This was partially successful but a number of patients received the forms shortly before the angiograms because they had not been prescheduled, and angiography had been requested only a few hours before. In one emergency situation no attempt was made to obtain an informed consent.

\* \* \*

TABLE 1  
Responses of 132 Patients to Questionnaire for Informed Consent Form 1\*

Questions	Yes	No	Not Checked	Incom- pletely Checked
1. Did you appreciate receiving this information?	107	12	6	7
2. Has this information disturbed you?	46	69	6	11
3. Do you desire further information regarding very rare complications?	8	109	6	9
4. Has this information caused you to change your mind as to whether to go through with this procedure?	5	113	6	8
5. Would you have preferred that we withheld information concerning possible complications?	21	103	6	2

\* One hundred thirty patients consented; 5 patients refused consent, but after personal discussion with physician, 3 consented, leaving 2 patients who ultimately refused. Six patients checked none of questions but gave consent. Remaining disparity is due to incomplete checking.

The straightforward and perhaps even harsh statements of the possible complications of angiography were accepted and desired by the majority of patients. Eighty percent of patients responding to form 1 and 89 percent of patients responding to form 2 answered question 1 affirmatively (the context of both questions is essentially the same).

In responding to form 1, question 2, approximately 35 percent of the patients were disturbed by the information; nevertheless all but two consented. This question was modified and included in section 4C of form 2, where 27 percent of patients stated that they were made less comfortable by this information. In both forms only a few patients asked for more information concerning the complications. The incidence of these questions in form 2 is twice that in form 1 and may be related to the fact that the content of form 2 was generally felt less likely to cause apprehension.

Questions 1 and 5 in form 1 are similar but expressed differently. At first glance there seems to be considerable disparity, but comparing the incompletely checked portion of Table 1, this disparity is greatly lessened. I have interpreted this to mean that seven patients who did not complete question 1 did not wish to express their distaste for this information, but when it came to question 5, they felt it easier to say what they wanted.

In all, 228 of 232 patients consented to an-

giography after a straightforward disclosure of possible complications. Four refused the procedure; their reasons for refusal will be commented upon.

\* \* \*

Approximately 2 percent of the patients in this study refused angiography on the basis of the consent form itself. It must be remembered that this is a diagnostic procedure and it is possible that no additional diagnostic information might be gained from it. This is in contrast to the necessity for a therapeutic procedure such as cholecystectomy or appendectomy when the patient's plight would probably produce a higher percentage of consent; probably few would refuse.

The four patients who refused this procedure cannot be placed in any specific category. One patient expressed hostility. There is the hypothetical question of whether this patient would have brought suit if a complication had occurred during angiography without informed consent. Another patient who refused seemed to be a reasonable individual; yet when he weighed the risks vs the advantages and benefits of the angiogram, he elected not to have the angiogram. Still another patient refused on the basis of fear of complications. . . . The form had been presented to the patient at a time when we knew that only a pulmonary angiogram was requested. Upon examining the patient and his chart, as

TABLE 2  
Responses of 100 Patients to Questionnaire for Informed Consent Form 2\*

Questions	Yes	No	None Checked	Incom- pletely Checked
1. Do you regard the above information as useful?	89	3	4	11
2. Do you think all patients should receive the above information?	74	12	4	8
3. Do you desire further information regarding specifics of possible medical complications of this procedure?	15	73	4	6
4. Has this information caused you to change your mind as to whether to go through with this procedure?			4	3
(a) Makes me more comfortable going ahead with it, 34.				
(b) Did not affect me one way or the other, 30.				
(c) Makes me less comfortable going ahead with the procedure, 27.				
(d) Has caused me to decide not to go ahead with the procedure, 2.				

\* Ninety-eight patients consented, two refused. One patient refused after further discussion with physician. No attempt was made to discuss second patient's refusal, as there was only questionable indication for procedure. Four patients checked none of questions but gave consent.

we do normally before an angiogram, we found questionable indications for a pulmonary angiogram and made no effort to change this patient's mind.

The following paragraph was included in both of the informed consent forms.

It would be impractical and probably misleading to the average person to describe here in detail all the complications which might possibly result from this procedure. If you would like more detailed information we would be glad to discuss it with you.

We felt it necessary to add this paragraph for three reasons. First, we felt that to list all of the known complications which we have thus far encountered would simply increase apprehension. Secondly, [we felt] that, although rare, new complications are reported now and then, and that the form itself would be invalid should one of these very rare complications occur. Third, it would be impossible to list all possible complications. . . .

\* \* \*

. . . We believe that we have proven that the majority of patients not only have a right to know but want to know what possible complications may be expected from any given procedure. The concern that informing a patient of

possible complications will result in his refusal of the procedure is now outmoded.

#### NOTE

ELEANOR S. GLASS  
RESTRUCTURING INFORMED CONSENT—  
LEGAL THERAPY FOR THE DOCTOR-PATIENT  
RELATIONSHIP\*

\* \* \*

In assessing whether the doctor acted reasonably, courts should adopt a patient's, or layman's, standard of care. The jury would decide whether the doctor disclosed enough information for the reasonable patient to make an intelligent decision. The jury should not undertake a subjective inquiry into what the individual patient actually understood or whether he acted intelligently. Presumably the plaintiff will present evidence regarding material facts that were not disclosed. The jury's task is to determine whether the information actually withheld would have been relevant for the jury members themselves, for their judgment is by definition that of the reasonable patient. They make this deter-

\* 79 *Yale Law Journal* 1533, 1559-1562 (1970). Reprinted by permission of the Yale Law Journal Company and Fred B. Rothman & Company.



mination in the light of the knowledge about a given procedure which is available to the medical profession. Thus whether a piece of undisclosed information would have been relevant for their own decision is the only question which the jury need resolve, unless the doctor claims that the patient was for some reason "unreasonable." If the jurors find the information irrelevant, the doctor acted reasonably in withholding it. If they find it relevant, the doctor acted unreasonably and will be held liable for failure to obtain informed consent.

\* \* \*

In addition to a different standard of care, the restructured law of informed consent also should have at its center formal rules of disclosure stipulating the minimum amount of information that a reasonable patient must be told before his consent is requested. The doctor ought to initiate discussion with his patient on the following substantive topics: the diagnosis; the physician's choice of treatment; the physician's experience with this treatment; the methods to be used; the risks involved, major and collateral; expected pain and discomfort; the benefits of this treatment; alternatives to this treatment; prognosis. Any omission from this list would constitute a *prima facie* violation of the physician's duty to disclose and liability would ensue. The physician must at least mention basic facts within each category.

Determinations about the relevance of collateral methods, risks, pains or alternatives are within the province of the jury. Expert testimony should be received only to inform the jury about specific treatments. No inquiry should be permitted into the "practice" of professionals in disclosing these procedures. Under a restructured law of informed consent, the job of the medical professional will be the job of

the traditional expert witness—to inform the jury about facts, not to establish the rule which governs the interpretation of facts.

These substantive requirements should not deter doctors from expressing their uncertainties about the course of treatment. Doctors should convey their uncertainties as clearly as possible, so that the patient will not rely on vague probabilities as facts. . . .

In administering the substantive requirements, the court should be prepared to look beyond *pro forma* compliance. At the request of the plaintiffs, it should allow evidence that the patient was told information that apparently satisfied the requirements but was informed in a manner which did not allow him to understand—and to question—the analysis imparted by the doctor. The court could allow an inquiry into whether the doctor asked for questions from the patient about what he had disclosed, and whether he indicated a willingness to continue the discussion at the patient's request.

The patient should also be allowed the right to waive the doctor's compliance with the substantive requirements. But the law must provide safeguards against the doctor's abuse of this waiver—an abuse which would subvert the restructured law of informed consent. The problem, of course, is how to determine whether the patient's waiver is based on a genuine understanding that he is giving up his right to be informed. Two safeguards should be required, at least. The doctor should give an explicit statement indicating what the waiver entails and that he is willing to continue the discussion, before he accepts the waiver. A third party, preferably friend or relation, should also be present to corroborate, in so far as possible, that the waiver was given voluntarily and knowingly.

\* \* \*

## D.

## To Protect the Experimental Process

## 1.

**Increasing Investigator-Subject  
Communication**

**Paul Ramsey**  
**The Patient as Person—**  
**Explorations in Medical Ethics\***

\* \* \*

[A]ny human being is more than a patient or experimental subject; he is a *personal* subject—every bit as much a man as the physician-investigator. Fidelity is between man and man in these procedures. Consent expresses or establishes this relationship, and the requirement of consent sustains it. Fidelity is the bond between consenting man and consenting man in these procedures. The principle of an informed consent is the cardinal *canon of loyalty* joining men together in medical practice and investigation. In this requirement, faithfulness among men—the faithfulness that is normative for all the covenants or moral bonds of life with life—gains specification for the primary relations peculiar to medical practice.

Consent as a canon of loyalty can best be exhibited by a paraphrase of Reinhold Niebuhr's celebrated defense of democracy on both positive and negative grounds: "Man's capacity for justice makes democracy possible; man's propensity to injustice makes democracy necessary." Man's capacity to become joint adventurers in a common cause makes the consensual relation possible; man's propensity to overreach his joint adventurer even in a good cause makes consent necessary. In medical experimentation the common cause of the consensual relation is the advancement of medicine and benefit to others. In therapy and in diagnostic or therapeutic investigations, the common cause is some benefit to the patient himself; but this is still a joint venture in which patient and physician can say and ideally should both say, "I cure."

Therefore, I suggest that men's capacity to become joint adventurers in a common cause makes possible a consent to enter the relation of patient to physician or of subject to investigator. This means that *partnership* is a better

term than *contract* in conceptualizing the relation between patient and physician or between subject and investigator. The fact that these pairs of people are joint adventurers is evident from the fact that consent is a continuing and a repeatable requirement. We can legitimately appeal to permissions presumably granted by or implied in the original contract only to the extent that these are not incompatible with the demands of an ongoing partnership sustained by an actual or implied *present* consent and terminable by any present or future dissent from it. For this to be at all a human enterprise—a covenantal relation between the man who performs these procedures and the man who is patient in them—the latter must make a reasonably free and an adequately informed consent. Ideally, he must be constantly engaged in doing so. This is basic to the cooperative enterprise in which he is one partner.

At the same time, just as Lincoln said concerning political covenants among men that "no man is good enough to govern another without his consent," so there is also this same negative warrant for the requirement of consent in the relation between those who perform and those who are the patients in medical procedures. No man is good enough to experiment upon another without his consent. . . .

\* \* \*

[E]xperimentation involving human subjects should be undertaken only when an informed consent has been secured. There are enormous problems, of course, in knowing how to subsume cases under this moral regulation expressive of respect for the man who is the subject in medical investigations no less than in applying this same moral regulation expressive of the meaning of medical care. What is and what is not a mature and informed consent is a preciously subtle thing to determine. Then there are questions about how to apply this rule arising from those sorts of medical research in which the patient's knowing enough to give an informed consent may alter the findings sought; and there is debate about whether the use of prisoners or medical students in medical experimentation, or paying the participants, would not put them under too much duress for them to be said to consent freely even if fully

\* New Haven: Yale University Press 5-11 (1970). Copyright © 1970 by Yale University. Reprinted by permission.

informed. Despite these ambiguities, however, to obtain an understanding consent is a minimum obligation of a common enterprise and in a practice in which men are committed to men in definable respects. The *faithfulness-claims* which every man, simply by being a man, places upon the researcher are the morally relevant considerations. This is the ground of the consent-rule in medical practice, though obviously medical practice has also its consequence-features.

Indeed, precisely because there are unknown future benefits and precisely because the results of the experimentation may be believed to be so important as to be overriding, this rule governing medical experimentation upon human beings is needed to ensure that for the sake of those consequences no man shall be degraded and treated as a thing or as an animal in order that good may come of it. In this age of research medicine it is not only that medical benefits are attained by research but also that a man rises to the top in medicine by the success and significance of his research. The likelihood that a researcher would make a mistake in departing from a generally valuable rule of medical practice because he is biased toward the research benefits of permitting an "exception" is exceedingly great. In such a seriously important moral matter, this should be enough to rebut a policy of being open to future possible exceptions to this canon of medical ethics. On grounds of the *faithfulness-claims* alone, we must surely say that future experience will provide no morally significant exception to the requirement of an informed consent—although doubtless we may learn a great deal more about the meaning of this particular canon of loyalty, and how to apply it in new situations with greater sensitivity and refinement—or we may learn more and more how to practice violations of it.

Doubtless medical men will always be learning more and more about the specific meaning which the requirement of an informed consent has in practice. Or they could learn more and more how to violate or avoid this requirement. But they are not likely to learn that it more and more does not govern the ethical practice of medicine. It is, of course, impossible to demonstrate that there could be *no* exceptions to this requirement. But with regard to unforeseeable future possibilities or apparently unique situations that medicine may face, there is this rule-assuring, principle-strengthening, and practice-upholding rule to be added to the requirement of an informed consent. *In the grave moral*

*matters of life and death, of maiming or curing, of the violation of persons or their bodily integrity, a physician or experimenter is more liable to make an error in moral judgment if he adopts a policy of holding himself open to the possibility that there may be significant, future permissions to ignore the principle of consent than he is if he holds this requirement of an informed consent always relevant and applicable.* If so, he ought as a practical matter to regard the consent-principle as closed to further morally significant alteration or exception. In this way he braces himself to respect the personal subject while he treats him as patient or tries procedures on him as an experimental subject for the good of mankind.

The researcher knows that his judgment will generally be biased by the fact that he strongly desires one of the consequences (the rapid completion of his research for the good of mankind) which he could hope to attain by breaking or avoiding the requirement of an informed consent. This, too, should strengthen adherence in practice to the principle of consent. If every doer loves his deed more than it ought to be loved, so every researcher his research—and, of course, its promise of future benefits for mankind. The investigator should strive, as Aristotle suggested, to hit the mean of moral virtue or excellence by "leaning against" the excess or the defect to which he knows himself, individually or professionally, and mankind generally in a scientific age, to be especially inclined. To assume otherwise would be to assume an equally serene rationality on the part of men in all moral matters. It would be to assume that a man is as able to sustain good moral judgment and to make a proper choice with a strong interest in results obtainable by violating the requirement of an informed consent as he would be if he had no such interest.

Thus the principle of consent is a canon of loyalty expressive of the *faithfulness-claims* of persons in medical care and investigation. Let us grant that we cannot theoretically rule out the possibility that there can be exceptions to this requirement in the future. This, at least, is conceivable in extreme examples. It is not logically impossible. Still this is a rule of the highest human loyalty that ought not in practice to be held open to significant future revision. To say this concerning the there and then of some future moral judgment would mean here and now to weaken the protection of coadventurers from violation and self-violation

in the common cause of medical care and the advancement of medical science. The material and spiritual pressures upon investigators in this age of research medicine, the collective bias in the direction of successful research, the propensities of the scientific mind toward the consequences alone are all good reasons—even if they are not all good moral reasons—for strengthening the requirement of an informed consent. This helps to protect coadventurers in the cause of medicine from harm and from harmfulness. This is the edification to be found in the thought that man's propensity to overreach a joint adventurer even in a good cause makes consent necessary.

This negative aspect of the ethics of medical research is essential even if only because the constraints of the consent-requirement serve constantly to drive our minds back to the positive meaning or warrant for this principle in the man who is the patient and the man who performs these procedures. An informed consent alone exhibits and establishes medical practice and investigation as a voluntary association of free men in a common cause. The negative constraint of the consent-requirement serves its positive meaning. It directs our attention always upon the man who is the patient in all medical procedures and a partner in all investigations, and away from that celebrated "nonpatient," the future of medical science. Thus consent lies at the heart of medical care as a joint adventure between patient and doctor. It lies at the heart of man's continuing search for cures to all man's diseases as a great human adventure that is carried forward jointly by the investigator and his subjects. Stripped of the requirement of a reasonably free and an adequately informed consent, experimentation and medicine itself would speedily become inhumane.

\* \* \*

## 2.

### Diminishing Unfavorable Public Reaction

**Oscar M. Ruebhausen and Orville G. Brim, Jr.**  
**Privacy and Behavioral Research\***

\* \* \*

There is no doubt as to the community reaction to the administration, even in the name

\* 65 *Columbia Law Review* 1184, 1192-94 (1965). Reprinted by permission.

of research, of live cancer cells to unwitting patients. Nor should we expect that the community will be any more tolerant of behavioral research that subjects non-consenting persons to the risk of injurious, though non-fatal, after-effects. Indeed, community sensitivity as to what is reasonable, or tolerable, is not limited to situations where physical or psychic injury may be involved.

While neither the most representative nor serious intrusion, a well-known example of privacy invasion in the field of behavioral research is the so-called "jury bugging" experiment conducted by the University of Chicago. Financed by the Ford Foundation, this was a scientific inquiry conceived and carried out with the best of professional motivation and skill. Although the consent, in advance, of the court and of opposing counsel was obtained, the surreptitious probing of the individual and institutional privacy of the members of the jury shocked the community when the experiment became public knowledge in October, 1955. . . .

\* \* \*

Another example where neither physical injury nor emotional trauma is necessarily involved is found in personality testing. It requires no Cassandra to predict lawsuits by parents, and a spate of restrictive legislation, if those who administer these tests in schools—even for the most legitimate of scientific purposes—do not show a sensitive appreciation for both individual and group claims to a private personality.

The lesson is plain. Unless the advances of science are used with discrimination by scientists engaged in behavioral research—as well as by other professions, by industry and by government—the constructive and productive uses of these advances may be drastically and unnecessarily restricted by a fearful community.

\* \* \*

## NOTES

### NOTE 1.

GWYNN NETTLER  
TEST BURNING IN TEXAS\*

By a 5-1 vote the governing board of the Houston Independent School District, one of the largest in the nation, in June 1959 ordered burned the answer sheets to six sociopsychological tests.

\* 14 *American Psychologist* 682-83 (1959). Reprinted by permission.

metrics administered to some 5,000 ninth graders. Four of these instruments were taken from a pilot study of the National Talent Project to be administered by the University of Pittsburgh and the American Institute for Research in 1960; the remaining instruments were added by local psychologists interested in forecasting the realization of talent and in the assessment of psychological health.

\* \* \*

The Houston test burning came as a result of few telephone calls (no one knows how many) from parents complaining, at the outset, to two of the seven trustees concerning the content and purport of the tests. The metropolitan press was alerted and published stories in advance of the school board meeting promising a ruckus. . . .

According to newspaper accounts parents were objecting to having their children respond to such items as:

I enjoy soaking in the bathtub.

A girl who gets into trouble on a date has no one to blame but herself.

If you don't drink in our gang, they make you feel like a sissy.

Sometimes I tell dirty jokes when I would rather not.

Dad always seems too busy to pal around with me.

\* \* \*

It seems advisable that future large-scale testing programs be preceded by a public "warm up" explaining to as broad a segment of the public as possible the purposes and methods of such research. For example, effort spent in the education of PTAs and boards of education in advance of such surveys may prevent such loss as Houston has suffered.

Psychologists are behaving "ethnocentrically" in assuming that their ethic is shared by the people they study. The statement of "Ethical Standards of Psychologists" carried in the June issue of the *American Psychologist* holds:

As a scientist, the psychologist believes that society will be best served when he investigates where his judgment indicates investigation is needed. . . .

The psychologist in the practice of his profession shows sensible regard for the social codes and moral expectations of the community in which he works. . . .

When the student of behavior works in a xenophobic and individualistic community, he cannot assume that his scientifically honorable intentions will be considered morally justifiable by those whom he seeks to help. Even though the scientist says, in effect, "I am studying you, and asking you these questions, for your own good," his subject may respond, "It is part of my 'good' that you desist from your intrusion of my privacy."

\* \* \*

#### NOTE 2.

LEONARD D. ERON AND LEOPOLD O. WALDER  
TEST BURNING II\*

This is a report on public reaction to a community-wide psychological research program. . . .

\* \* \*

The research has been conducted by staff members in the research department of the Rip Van Winkle Foundation, an organization which sponsors a group practice of medicine providing comprehensive medical care on both a pre-paid and fee-for-service basis. . . . This organization . . . was founded in an attempt to provide the population of a relatively poor rural community with high quality medical care at a cost that at least 90 percent of the people could afford. Since 1954 there has been a continuous mental health unit including psychologists, psychiatrists, and psychiatric social workers integrated into the comprehensive medical program. Demand for mental health services which are provided to both children and adults has steadily increased since inception of the service. . . .

The research program sponsored by the foundation started in 1955 and has been concerned with mental health in rural areas. The content selected for one study was the development of aggression in children. For purposes of this research it was decided to study all third grade children in their classrooms and to interview the parents of each child. . . .

\* \* \*

. . . At this time plans were completed for our final data collection year in which we were to see all third grade children and parents in the county. A meeting was held with the county school administrators and our plans completely

\* 16 *American Psychologist* 237 (1961). Reprinted by permission.

outlined to them. Because of the good response we had been getting up until then, the school personnel suggested that it was not necessary to send home notices with the children. They considered our testing program like any other routine school procedure.

\* \* \*

. . . The testing of the children was completed without incident. Simultaneously and subsequently parents were contacted for interviews. A day after letters were sent out to the Chatham area, the supervising principal of that district phoned the director of the research and said she received a number of calls from irate parents concerning the letter from the Mental Health Research Center about their child in school. The parents reported that they had engaged a lawyer to see what action could be taken to prevent this study from going any further. The principal felt that the words "Mental Health" in our letterhead had scared them and that we had made a tactical error in not sending the initial contact letters out on school letterheads over the signature of the individual principals. Thus to the parents in the succeeding four school districts, letters went out on school letterheads over the supervising principal's signature. The letter remained the same with some minor changes in pronouns. After that there were only two direct complaints made to a supervising principal.

In the meantime, however, the situation in the Chatham district had blown up. The parents demanded a public meeting with the school authorities and the research team. At the meeting, held on a snowy evening, only 18 parents showed up. Questions were asked primarily by two mothers, one of whom was secretary to the aforementioned lawyer. . . . The questions had to do primarily with what authority the school could allow an outside agency to come in and test their children, who was behind the study, who financed it, who was making money out of it. There were vague references to personal questions being asked of the children and parents but the wording of these purported questions could not be ascertained at the meeting by either the school authorities or the researchers. We were frank and candid in our replies and offered to show the procedures which we had with us to the parents who were present. After describing each of the children's tests and giving sample items, one of the speakers then started to read the parents' questionnaire, item by item;

after three pages, the audience lost interest. No one asked to look at the materials afterwards. Many of the parents did come up to us and say that the fuss was all much ado about nothing.

\* \* \*

However, a week later during a routine meeting of the Chatham Board of Education, without warning the opposition appeared in force. . . . The board was unprepared for the uninterrupted barrage of questions. The report of the meeting which made the front pages the next day made the school board look foolish and the researchers sinister. We were accused of asking the third graders such questions as "How often do you have sexual relations?" "Do you get suicidal thoughts?" "Do you prefer your mother to your father?" "Is your father a tyrant?" etc. Needless to say, none of these questions had ever been asked in our survey procedures, either with the children or the parents. The matter of confidentiality, invasion of privacy, lack of parental permission, etc. were all brought up. The penny candy which we gave the children as prizes after each session was alleged to be a bribe. One mother said: "I give my children strict training never to take candy from strange men." The president of the board asked for and received a motion to suspend any further testing until a subcommittee of the board could investigate the whole matter and make a recommendation.

\* \* \*

From that point on, publicity became more favorable. The subcommittee of the Chatham Board of Education drew up a recommendation to continue wholehearted support of the research effort with the proviso that, in the future, permission from the parents should be obtained before testing the children in school. This recommendation was then presented at a well-advertised board meeting at which no one of the objectors appeared and it was unanimously approved by the board after no opposition from the floor. A petition against continuation of the research purportedly signed by many parents was not presented. A very fine statement was made at this meeting by the executive director of the foundation, supporting the research and its place in a community-oriented organization. . . .

\* \* \*

What has been the effect of this entire con-

troverly on the data gathering? Actually, the ruckus did not blow up until the period of data collection from the children was in its last week. Data from only three children were destroyed in compliance with the wishes of their parents. The effect on interview acceptance by the parents is really unassessable. The controversy exploded midway in our 6-month field period and extended until the end. However, there did not seem to be a lessening of cooperation on the part of parents; in fact, the publicity may have helped. For the total sample of 875 third graders, we successfully interviewed at least one parent in 83 percent of the cases and both parents in 75 percent. Thus we feel confident we had the support of the majority of parents.

\* \* \*

. . . Despite our careful preparation and groundwork and our indefatigable efforts in public relations and our striving to be candid at all times, there was a lot of misunderstanding about just what we were doing. This is perhaps inevitable when psychological concepts which have restricted meanings to psychologists have to be communicated to the lay public in an open meeting. The words are in English and carry the extra freight of both conventional and personal meaning to each individual. . . . How many . . . times such terms were used blithely by us and misinterpreted by our audiences is uncertain. This is illustrated by the following quotation from a newspaper account of one board meeting:

Some parents want another meeting with the research director. Others charge that this would do no good. "All he would do," one said, "would be to stand up there and turn on the charm and give us a bunch of vague and meaningless terms about norms and statistics and such, and none of us would find out anything."

\* \* \*

#### NOTE 3.

ALEXANDER M. KIDD  
LIMITS OF THE RIGHT OF A PERSON TO  
CONSENT TO EXPERIMENTATION ON HIMSELF\*

\* \* \*

[I]t may be that, on the maxim "*De minimis non curat lex*"—"The law does not regard trifles"—an extra drop of blood to build up a control group for a research study, or the use of

tissue that has been properly severed would not be condemned by the court. But the medical profession should consider carefully whether, as a matter of good public relations, nothing should be done to a patient except for his benefit, and whether he should not be used either directly or indirectly as a guinea pig without his consent. A doctor who has the reputation of experimenting with his patients is avoided by those who know that reputation.

\* \* \*

### 3.

#### Removing Civil and Criminal Liability

##### a.

**Barnett v. Bachrach**  
34 A. 2d 626 (D.C. Mun. Ct. App. 1943)

\* \* \*

CAYTON, ASSOCIATE JUDGE.

Defendant engaged Dr. Joseph Harris to treat his wife during her period of pregnancy. There was evidence that in the course of such treatment she complained of certain pains in her lower right abdomen, and of nausea. Dr. Harris made a diagnosis of tubal pregnancy. He called in the plaintiff, a surgeon, for consultation. Plaintiff took the history and made a pelvic examination, in the presence of Dr. Harris. He found a mass the size of a small orange in the right ovarian region; from that and other symptoms he made a diagnosis of a tubal or extra-uterine pregnancy, confirming the diagnosis of Dr. Harris. Plaintiff recommended an immediate operation.

Plaintiff's testimony was taken by deposition and is not too clear as to precisely what authority he received. On direct examination he said, "[A] diagnosis of extra-uterine pregnancy was made. . . . I discussed the importance of immediate operation with the patient in the presence of Dr. Harris. *Permission for operation was granted and the patient was operated on by me.*" In answer to one question on cross examination he said: "I was engaged to perform an abdominal operation and do whatever was necessary to cure the patient. I was not engaged to do an appendectomy per se." But to the next question his answer was: "Mrs. Lillian Barnett engaged me to perform an operation on her to remove a possible tubal pregnancy, however an acute appendix was found instead and removed." These answers can fairly be taken to mean that the only

\* 117 *Science* 211, 212 (1953). Reprinted by permission.



express authority he received was to remove the tubal pregnancy. If plaintiff had more specific authority the evidence does not show it.

The operation was performed under anaesthesia, and upon opening the abdomen, plaintiff found that his and Dr. Harris' original diagnosis of tubal pregnancy was mistaken and that a normal pregnancy was present in the uterus. Also he found a very unusual condition in that the patient, instead of having one uterus, had a double uterus. Also he found a very acute appendix. Deciding that this had caused patient's abdominal pains, he removed the appendix. He testified that she made an uneventful recovery and that he and Dr. Harris were satisfied that the source of her trouble had been removed.

Defendant's wife contended that as a result of the operation she had difficulty in getting about the house and going up and down stairs and was confined to bed for a "considerable" length of time. There is no denial that she gave birth in the usual course to a normal child in a normal way.

Defendant resisted the surgeon's claim for his fee on the sole ground that the appendix had been removed without the consent of himself or wife. He has disavowed any charge of malpractice, any claim of negligence, either in the original diagnosis or in the operation itself. He rests his defense entirely upon the claim that the operation, having gone further than was authorized, constituted a trespass or assault upon his wife.

\* \* \*

The question for our decision is whether an emergency existed so as to justify the removal of the appendix without the express consent of the patient. To answer the question we must look at the picture through the eyes of the surgeon. The patient lay before him on the operating table, her abdomen laid open, and unconscious from the anaesthetic. Her pregnancy was a normal one in the uterus and not tubal as he and Dr. Harris had thought it was. She did reveal a very unusual structural condition in the form of a double uterus. More immediately important, he beheld a very acute appendix with all its potentially dangerous consequences.

What was the surgeon to do? Should he have left her on the operating table, her abdomen exposed, and gone in search of her husband to obtain express authority to remove the appendix? Should he have closed the incision on the inflamed appendix and subjected the patient, pregnant as she was, to the danger of a general

spread of the poison in her system, or to the alternative danger and shock of a second, independent operation to remove the appendix? Or should he have done what his professional judgment dictated and proceed to remove the offending organ, regarded as it is as a mere appendage serving no useful physiological function and causing only trouble, suffering, and oftentimes death?

Defendant does not say that the plaintiff used bad judgment, or that the operation was not dictated by sound surgical procedure, or that it was a failure. He says only that it was unauthorized, and makes no real showing of resulting injury or damage. . . .

This case is not one where a patient was rendered barren; on the contrary, her foetus was not disturbed and she achieved motherhood in a normal manner. Nor was she crippled or otherwise mutilated; on the contrary the operation was a success, and she is forever relieved from the fear and danger of appendicitis.

And yet we are asked to deny the plaintiff's fee because he comes into court unable to show express authority for the excision he made. It seems to us that to adopt that view would be granting poor reward indeed for faithful professional service. Moreover this would require us to shut our minds and eyes, as judges, to "truths that all others can see and understand."

To accept this view, we would have to deny that it was an emergency and declare a rule which would tend to make every surgeon litigation-conscious instead of duty-conscious as he stands, scalpel in hand, over his unconscious patient. This we decline to do. We hold the law to be that in case of emergency a surgeon may lawfully perform, and it is his duty to perform, such operation as good surgery demands even when it means extending the operation further than was originally contemplated; and that for so doing he is neither to be held in damages, or denied recovery of his fee.

The law should encourage self-reliant surgeons to whom patients may safely entrust their bodies, and not men who may be tempted to shirk from duty for fear of a law suit. The law does not insist that a surgeon shall perform every operation according to plans and specifications approved in advance by the patient, and carefully tucked away in his office-safe for courtroom purposes.

We do not attempt to mark off the line which will define the type of emergency which will create implied consent in every case; that is

a question for the jury, or, as here, for the judge who sat as trier of the facts. Here we hold only that on the showing made, authority was born of the emergency, and conferred upon the surgeon the legal right to proceed as he did.

*Affirmed*

#### NOTE

MILTON OPPENHEIM  
INFORMED CONSENT TO MEDICAL TREATMENT\*

\* \* \*

It is submitted that the public interest requires that the physician be permitted to exercise his discretion in good faith, knowing that the

\* 11 *Cleveland-Marshall Law Review* 249, 251, 261-263 (1962). Reprinted by permission.

physician-patient relation is one of fiduciary requirements of trust and confidence. This is important, for if one is to explain to the patient every risk attendant upon surgical or therapeutic procedures, no matter how remote, it may well result in unduly alarming the patient, who is already apprehensive, fearful, and dejected.

\* \* \*

The absurdity of the trend towards a "more informed patient" is evident in the attempts of physicians to comply, even where compliance is not in conformance with good medical practice. This required "informed" consent may create delay, apprehension, and restrictions on the use of new techniques that will impair the progress of medicine. It is questionable whether the "average prudent man" will understand and comprehend the following examples of informed consent forms used by a prominent neurosurgeon in his practice:

#### CONSENT AND OPERATIVE PERMIT FOR CAESAREAN GANGLION OPERATION

1. The side of the face (operation) may be numb.
2. There may be a loss of sensation on the cornea of the eye on the side operated, with the possibility of ulceration leading to possible loss of the eye.
3. There may be impaired hearing on the operated side.
4. There may be residual weakness in chewing movements on the operated side.
5. There may be weakness of the muscle of facial expression on the operated side.
6. There may be residual pain on the operated side.
7. There may be weakness of the body—hemiplegia—on the operated side of the body.

The above has been read and explained to me, and I accept responsibility for these or any other complications which may arise or result during or following this surgical procedure, which is performed at my request.

WITNESS: \_\_\_\_\_

\_\_\_\_\_  
(Patient's Signature)

\* \* \*

#### CONSENT AND OPERATIVE PERMIT

PATIENT \_\_\_\_\_ AGE \_\_\_\_\_ PLACE \_\_\_\_\_

DATE \_\_\_\_\_ TIME \_\_\_\_\_ A.M.  
P.M.

1. I hereby authorize Dr. \_\_\_\_\_ and whomever he may designate as his assistants to perform upon \_\_\_\_\_

(State name of person or "myself")

the following operation: "THYROIDECTOMY" that is,  
(State procedure(s) to be performed)

"SURGICAL REMOVAL OF THYROID GLAND—SUBTOTAL" and if any unforeseen condition arises in the course of  
(State full explanation of procedure)

this operation calling in his judgment for procedures in addition to or different from these now contemplated, I further request and authorize him to do whatever he deems advisable and necessary in the circumstances.

2. The clinical outcome in my case is directly in proportion to the nature of the pathology, that is, the condition revealed, disclosed, or discovered by the procedure or procedures. The nature, purpose, and risk of the operation and procedures and possible alternative methods of treatment, possibility of complications have been fully explained to me. I acknowledge that no guarantee or assurance has been made as to the results that may be obtained. Further, I consent to the disposal of any tissue which may be removed.
3. My condition therefore, may:
  - (a) Be improved;
  - (b) Remain stationary; or
  - (c) Become aggravated with respect to "*Weakness or Hoarseness of Voice: Prominence of Eyes may persist: Calcium Metabolism may be disturbed, with resulting muscle weakness.*"
4. I consent, authorize and request the administration of such anesthetic or anesthetics as is deemed suitable by the physician-anesthetist who shall be chosen by the surgeon. It is the understanding of the undersigned that the physician-anesthetist will have full charge of the administration and maintenance of the anesthesia, and that this is an independent function from the surgery.

The above has been read and explained to me and I accept responsibility for these or any other complications which may arise or result during or following the above procedure, which is to be performed at my request.

Signature of Patient \_\_\_\_\_

Signature of Patient's  
husband or wife \_\_\_\_\_

When patient is a minor or incompetent to give consent:

Signature of Person Authorized to consent for the patient. \_\_\_\_\_

WITNESS: \_\_\_\_\_

RELATIONSHIP TO PATIENT \_\_\_\_\_

\* \* \*

b.

**Bang v. Charles T. Miller Hospital  
251 Minn. 427, 88 N.W.2d 186 (1958)**

FRANK T. GALLAGHER, JUSTICE.

\* \* \*

This was an action for damages for alleged assault or unauthorized operation by the defendant on his patient, Helmer Bang, referred to herein as plaintiff. The latter contends that the question as to whether he expressly or impliedly consented to the operating procedures involved was one of fact for the jury. At the close of plaintiffs' case, the defendant moved for a direct verdict upon the grounds that plaintiffs had failed to prove any actionable negligence or any cause of action against him. This motion was treated by the trial court as a motion for dismissal on the merits, which motion was granted. . . .

\* \* \*

[P]laintiff began having urinary trouble

in 1951 to 1952. He consulted a doctor in his home town of Austin, Minnesota, who sent him to the hospital for a cystoscopic examination which was made by two local doctors in Austin. Plaintiff testified that they informed him of an enlargement of the prostate gland and bladder soreness and recommended either Rochester or defendant in St. Paul as places he could go to have some tissue removed from the gland to overcome the trouble.

\* \* \*

The important question for determination of the matter presently before us is whether the evidence presented a fact question for the jury as to whether plaintiff consented to the severance of his spermatic cords when he submitted to the operation. Defendant testified on cross-examination under the rules that he did not tell plaintiff at the time of the office visit, April 6, that any examination defendant had made or was going to make had anything to do with the spermatic

cord, nor did he recall explaining to his patient what a prostate gland operation involved. He also said that plaintiff's life was in no immediate danger because of his condition on that day.

\* \* \*

On the following day the operation was performed. When defendant was asked as to the procedure used, he replied:

A: [T]he cysto-urethroscopic examination was made; following that I went over to the head of the table and talked to Mr. Bang, told him what the findings were, and that in my opinion the transurethral prostatic resection should be done and I had his consent that we proceed with that operation.

Q: Did you at that time as I understand it now ask him for his consent? A: Yes.

\* \* \*

Q: Did you inform Helmer Bang that in his specific case of a prostate gland resection that you intended to sever the spermatic cords of him as a part of the operation or that it was necessary to do so in his specific case? A: I did not inform Helmer Bang that as a part of the prostate gland resection it would be necessary to sever the spermatic cords of his.

\* \* \*

Q: Did you also answer, severance of the spermatic cords—bilateral was section [vas section] and ligation—is a routine part of this procedure in all cases of patients the age of Mr. Bang? A: Yes.

\* \* \*

The patient recalled the start of the operation and, when questioned on direct examination, stated:

Q: Did he at any time before the operation begin tell you that he was going to cut your spermatic cords? A: No.

Q: Did he at any time before the operation begin tell you that it was necessary to cut your cords? A: No.

When questioned as to whether he had any conversation with the defendant at the operating table or during the entire period when he was in the operating room, plaintiff replied that with the exception of a morning greeting "and stuff like that" nothing was said to him with reference to the operation.

When being questioned on cross-examination with reference to consent, the plaintiff was asked:

Q: And you certainly did consent, didn't you, Mr. Bang, to Dr. Foley doing anything to correct your trouble which in his medical knowledge he felt should be corrected? A: Not anything he wanted to, no.

Q: Did you put any limitation on his job as a surgeon? A: No.

Q: When he said, if I find anything that needs correction I will do it at the same time and you said that was all right, that was all of the conversation there was, wasn't it? A: That was all of the conversation there was.

It is plaintiff's claim that he thought he was discussing his bladder because he understood from his Austin physicians something about burning out the ulcers if there were any ulcers in there. He admitted, however, that defendant said nothing to him about ulcers. Plaintiff also admitted that he did not expect to tell the doctor what to do; that he had faith in him; and that he did not expect to tell him how to perform the operation. He said that he expected the doctor would operate to do what was necessary to right and cure his condition. He testified that he did not ask the doctor what he intended to do and left it up to him to do the right thing.

It is our opinion that under the record here the question as to whether plaintiff consented to the severance of his spermatic cords was a fact question for the jury and that it was error for the trial court to dismiss the action.

\* \* \*

While we have no desire to hamper the medical profession in the outstanding progress it has made and continues to make in connection with the study and solution of health and disease problems, it is our opinion that a reasonable rule is that, where a physician or surgeon can ascertain in advance of an operation alternative situations and no immediate emergency exists, a patient should be informed of the alternative possibilities and given a chance to decide before the doctor proceeds with the operation. By that we mean that, in a situation such as the case before us where no immediate emergency exists, a patient should be informed before the operation that if his spermatic cords were severed it would result in his sterilization, but on the other hand if this were not done there would be a possibility of an infection which could result in serious consequences. Under such conditions the patient would at least have the opportunity of deciding whether he wanted to take the chance of a pos-

sible infection if the operation was performed in one manner or to become sterile if performed in another.

Reversed and a new trial granted.

C.

**Marcus L. Plant**  
**An Analysis of "Informed Consent"**\*

\* \* \*

The legal wrong conventionally called "battery" or "assault and battery" consists of an unpermitted touching of the person of another; by definition a touching is not tortious if there has been consent to it by the one touched. Consent can be rendered nugatory under some circumstances. One of these circumstances is inducement of the consent by a certain kind of misrepresentation. Not every misrepresentation will have this vitiating effect. In order to negate consent the misrepresentation must relate to the nature and character of the touching. If it does, the touching is tortious (a battery) because it is no longer with the consent of the one touched. Misrepresentation that does not relate to the nature and character of the touching but merely concerns some collateral matter does not have the fatal effect. . . .

\* \* \*

. . . In *Bang*, plaintiff thought he was going to be touched in a certain way (operation on his prostate gland, possibly surgery on the bladder) but was subjected to a touching of a substantially different character (severance of spermatic cords). In *Mitchell*, plaintiff thought he was going to be touched in a certain way (insulin injection). He was touched in *exactly* that way, but there was a harmful result arising from a collateral risk he had not been warned about. The fundamental point is not what name we give to the two categories of cases as long as what we call them depicts two basically different wrongs which call for quite different treatment by the courts and quite different self-protective steps to be taken by the physician. It is fatal to clear understanding to intermingle the two under some broad heading such as "malpractice" or to state that both involve "informed consent." For purposes

of this discussion I will refer to the first type of case (*Bang*) as a "battery" and to the second type of case (*Mitchell*) as "medical negligence."

\* \* \*

It is clear in the battery cases that a patient has virtually an absolute right to be free from touchings of a substantially different nature and character from those to which he has consented. It is the patient's prerogative to accept medical treatment or to take his chances of living without it. . . .

\* \* \*

*Wall v. Brim* presents a variation of the general theme involving a failure to disclose. Plaintiff underwent a procedure involving an incision in the neck just under the back of the ear for removal of a cyst. When it was finished she had suffered a serious injury which caused considerable facial disfigurement with disability of her mouth, tongue and eyes. Plaintiff had been told by the surgeon before surgery that it was a "very simple operation" which would not take more than five or ten minutes and the cyst could be pulled out "like hulling a pea out of a pod." After the incision was made, the surgeon discovered that the cyst was deeply embedded and in close proximity to the facial nerve. He continued with the operation without any disclosure of these facts to plaintiff who was fully conscious, the operation being performed under a local anesthetic. Plaintiff sued on a negligence theory. The jury verdict was for plaintiff and from judgment thereon defendant appealed. The Fifth Circuit Court of Appeals held that there was insufficient evidence to establish negligence because under the applicable law (Georgia) expert evidence was required and had not been adduced. However, the evidence suggested what appeared to be an operation without the consent of plaintiff, and the case was remanded for trial and development on that theory. The court's reasoning was that the surgeon, having previously described the operation as a simple one and then having discovered that it was a complicated and different one, had a duty so to advise plaintiff, particularly as she was conscious at the time. While his failure to do so would not support a negligence action, it could support an action for unpermitted operation.

\* \* \*

\* 36 *Fordham Law Review* 639, 648-655 (1968). © 1968 by Fordham University Press, Lincoln Center, New York, N.Y. Reprinted by permission of copyright holder.

When the case involves no substantial misunderstanding of the nature and character of the touching, but plaintiff claims he was not fully or correctly informed as to collateral hazards attendant upon the procedure, the judicial approach is quite different from that found in battery cases. Here defendant-physician's obligation and plaintiff-patient's corresponding right is less certain in nature, more flexible in character and subject to considerable variation. While it is often stated as a general proposition that the patient has the right to be advised of collateral hazards and the physician has the duty so to advise him, most cases have recognized, starting with *Salgo*, that this obligation is not rigid and cannot be prescribed with specificity. It is only a part of the broad obligation of the physician to use reasonable care, but as any sophisticated person knows, the elasticity in that concept is more than negligible.

\* \* \*

One consideration often mentioned is whether the case is an emergency requiring immediate treatment. This aspect usually appears in a negative fashion; i.e., in buttressing the conclusion that defendant owed a duty to disclose collateral hazards, the court emphasizes that no emergency made it impractical to perform the duty. For example, in *Bowers v. Talmage*, the claim was that parents had not been warned of a hazard to their nine-year-old child from an arteriogram, an exploratory surgical process. The procedure was considered dangerous since three percent of the cases had injurious results. It caused partial paralysis of the boy. In holding that it was error to direct a verdict for defendant, the court emphasized that there was no emergency.

\* \* \*

A second factor, and perhaps the one most frequently referred to by courts in delineating the physician's duty, is the danger of alarming the patient or causing other adverse psychological effects on him. . . .

\* \* \*

A third factor that influences the decision as to whether there is a duty to disclose collateral dangers is the likelihood that the danger will materialize. The greater the frequency of injury from it, the greater the obligation of the physician to mention it and vice-versa. . . .

\* \* \*

d.

### **Gramm v. Boener 56 Ind. 497 (1877)**

WORDEN, J.—This was an action by the appellee, against the appellant, to recover damages for alleged negligence and unskillfulness on the part of the defendant, in the performance of his undertaking, as a surgeon, to set a broken arm and a broken leg of the plaintiff, whereby the plaintiff lost the use of his arm, and his leg became crooked, deformed and permanently lame.

Trial by jury; verdict and judgment for the plaintiff . . .

\* \* \*

It seems to us to be the duty of a surgeon, when called upon to perform some surgical operation, to advise against it, if, in his opinion, it is unnecessary, unreasonable, or will result injuriously to the patient. The patient is entitled to the benefit of his judgment, whether asked for or not. If the surgeon, when called upon, should proceed to the performance of the operation, without expressing any opinion as to its necessity or propriety, the patient would have a right to presume, that, in the opinion of the surgeon, the operation was proper.

But if a surgeon, when thus called upon, advises the patient, who is of mature years and of sound mind, that the operation is unnecessary and improper, in short advises against the performance, and the patient still insists upon the performance of the operation, in compliance with which the surgeon performs it, we do not see upon what principle the surgeon can be held responsible to the patient for damages, on the ground that the operation was improper and injurious. In such case, the patient relies upon his own judgment, and not upon that of the surgeon, as to the propriety of the operation; and he can not complain of an operation performed at his own instance and upon his own judgment, and not upon that of the surgeon. The maxim, *volenti non fit injuria*, we think, well applies to such a case. The principle is quite analogous to that which prevents a recovery for injuries consequent upon unskillful or negligent treatment by a physician, if the plaintiff's own negligence directly contributed to them.

There is evidence in the record tending to show that the plaintiff, who was a married man and may be supposed to have been of mature years, repeatedly desired to have his arm re-broken, when the defendant visited him. He said positively that he wanted it re-broken. The defen-

dant advised against it. He told them it would be of no use; that it had better be left alone, and that they ought not to think of it. In short, there is enough in the evidence, if the jury believed it, to justify them in finding that the arm was re-broken at the sole instance of the plaintiff, and against the advice of the defendant.

\* \* \*

The judgment below is reversed . . . and the cause remanded, for a new trial.

### NOTES

#### NOTE 1.

CHAMPS V. STONE  
58 N.E. 2d 803 (1944)

ROSS, PRESIDING JUDGE.

This is an appeal on questions of law from a judgment of the Court of Common Pleas of Hamilton county, in favor of the defendant, entered upon a verdict which such court instructed after the opening statement of counsel for plaintiff and his stipulation as to what his evidence would show.

MR. SCHEAR: If the court please and ladies and gentlemen of the jury, the action here is one for personal injuries. . . .

\* \* \*

I believe the evidence will show to you ladies and gentlemen of the jury that . . . the doctor was so grossly intoxicated and under the influence of alcohol that he could (not) comprehend to carry through a proper treatment. In other words, he was physically unfit to administer any aid to a patient.

\* \* \*

THE COURT: . . . It is agreed in this case as follows: That the plaintiff went to the defendant for the purpose of having a blood test made by the defendant physician; that the defendant at said time was grossly intoxicated; that the plaintiff saw that the defendant was grossly intoxicated and refused to be treated by him; that the defendant insisted upon treating the plaintiff and the plaintiff submitted to being treated by him, and that the plaintiff claims that as a result of the treatment, which was improper and not according to what an ordinary physician—that by reason of not exercising the skill a physician should exercise he was damaged.

\* \* \*

On the opening statement, the defendant moves for a directed verdict and I grant that motion on the ground that if I myself take the chance of being

treated by someone whom I ought not to take the chance of being treated by, [in] other words, if I go to a doctor who is drunk and I know he is drunk and let him treat me because he insists on treatment (treating) me I take the consequences thereof and that is my own fault, in plain language, and I am guilty of negligence in law which contributes to a proximate cause of an injury. That is the reason why I direct you all to sign the verdict. . . .

The questions presented here are: Whether such conduct on the part of the plaintiff constituted contributory negligence as a matter of law, or an assumption of the risk presented by the physician's condition.

Contributory negligence is such an act or omission on the part of the plaintiff, constituting a failure to exercise the care which ordinarily prudent persons are accustomed to employ for their own safety, which, concurring or cooperating with the negligent act of the defendant is a proximate cause or occasion of the injury of which the plaintiff complains. . . .

\* \* \*

It is claimed the intoxicated condition of defendant was the proximate cause of plaintiff's injuries. Such condition was apparent to plaintiff before he permitted the defendant to inject the needle in his arm. Had the plaintiff the right to rely upon the skill of defendant when grossly intoxicated? Plaintiff must have concluded that, although defendant was in such condition, he could still administer treatment without harmful results to him. Could reasonable minds differ on the conclusion that reasonably prudent persons would not permit a physician to act under such circumstances?

It would seem that any reasonable person would naturally ask: Why did you let the physician proceed? Why did you not at once leave his office? You certainly saw he was in no condition to use a needle upon you.

It must have been apparent to any reasonable person that the physician in such a condition might even select the wrong drug or inject an overdose. . . .

The only possible argument against the conclusion that plaintiff was either guilty of contributory negligence or assumed the risk of the physician's evident incapacity to properly perform his duties is that, under any such circumstances, a patient has the right to rely upon the professional representation of ability contained in the fact that the physician holds himself out as such and insists upon his ability to properly practice his profession.



Such implied ability and even definitely professed ability certainly may not be accepted by a patient in the presence of "gross intoxication" of which plaintiff is fully aware. Ordinary care for one's safety would impel the ordinary careful and prudent person to seriously doubt the physician's ability and cause the patient to refuse treatment.

The judgment of the trial court is affirmed.

\* \* \*

MATTHEWS, JUDGE (dissenting).

Manifestly, the plaintiff concluded that the defendant was not so drunk as to preclude him from safely performing the injection by means of a hypodermic needle. The defendant, by holding himself out as a physician, represented that he had the required training and skill, and, at the time of this treatment, represented that he was in such physical and mental condition as to enable him to apply his training and skill. He occupied the dominant position in relation of trust and confidence. I do not think we are justified in holding that the patient was negligent as a matter of law in relying on the physician's assurance that he was in condition to safely use a hypodermic needle.

It is my opinion that the opening statement of counsel was a sufficient statement of a cause for submission to the jury, and that the court erred in instructing a verdict for the defendant upon such statement.

#### NOTE 2.

FOWLER V. HARPER AND FLEMING JAMES, JR.  
THE LAW OF TORTS\*

The term assumption of risk has led to no little confusion because it is used to refer to at least two different concepts, which largely overlap, have a common cultural background, and often produce the same legal result. But these concepts are nevertheless quite distinct rules involving slightly different policies and different conditions for their application. (1) In its primary sense the plaintiff's assumption of a risk is only the counterpart of the defendant's lack of duty to protect the plaintiff from that risk. In such a case plaintiff may not recover for his injury even though he was quite reasonable in encountering the risk that caused it. *Volenti non fit injuria*. (2) A plaintiff may also be said to as-

sume a risk created by defendant's breach of duty towards him, when he deliberately chooses to encounter that risk. In such a case, except possibly in master and servant cases, plaintiff will be barred from recovery only if he was unreasonable in encountering the risk under the circumstances. This is a form of contributory negligence. Hereafter we shall call this "assumption of risk in a secondary sense."

\* \* \*

It is sometimes said that knowledge or comprehension of the risk by plaintiff is the watchword of assumption of risk. In many types of situations this is true; in others it is not. Unless the limitations which should be put on such a statement are fully appreciated, it may be very misleading. There may be assumption of a specific risk of which the plaintiff is completely ignorant. On the other hand the plaintiff does not assume, in the primary sense, many risks which he knows and fully appreciates. Thus the borrower of a chattel or the licensee on land takes the risk of dangers that he does not and cannot know about. And the rescuer or traveler on the highway, for instance, does not assume the most open and obvious risks (though he may be negligent in encountering them under the circumstances of any given case). The key to the problem lies in the relationship between the parties, and the duty owed by defendant to plaintiff under all the circumstances. It is only where (1) defendant knows of the danger, or (2) is under a duty to plaintiff to use care to discover the danger, but (in either event) will fully discharge his duty to plaintiff by complete disclosure of the danger, that plaintiff's knowledge and comprehension of the risk will spell assumption of risk in the primary sense. The commonest examples of this are cases of invitor and invitee on real property and (formerly, at least) master and servant. In these situations plaintiff is viewed as having no right to enter into or remain within the relationship, but only to be apprised of its risks so he can choose intelligently whether to encounter them. And if the risks are such that he who runs may read them, defendant owes no further duty with respect to them. Here, indeed, comprehension of the risk is the watchword of the doctrine.

In such a situation at least *actual* comprehension of a risk by a plaintiff means that if he voluntarily encounters it, he assumes it. But actual comprehension implies more than knowledge of the defect that constitutes the danger. It

\* Boston: Little, Brown and Co. 1162, 1168-1170, 1173-1174 (1956). Reprinted by permission.

also includes an appreciation and an understanding of the dangers that lurk in the defect and result in the injury, and it is usually a jury question whether there was such appreciation in fact.

A different and more difficult question concerns a defendant's duty with respect to defects which are obvious and visible or to conditions pregnant with risks which most men would appreciate, but which this plaintiff does not see or does not comprehend. . . . There an attempt was made to examine the extent to which his own shortcomings would be considered in evaluating the plaintiff's conduct as negligent or not. Now our problem is the different one of deciding the extent to which certain defendants may assume a minimum of knowledge and perceptiveness on the part of others and act on that assumption. For example, the owner of a baseball park owes no duty to warn the experienced spectator of the dangers of foul balls in the unscreened part of the bleachers; and an analogous situation is presented by hockey. But does the proprietor of either sporting event owe such a duty to the uninitiated? Except for a few situations the rule does not seem clear and opinions are too often clouded with talk about presumptions. It seems fairly safe to say, however, that there are at least some situations whose dangers are so obvious, so customary, and so commonly known that a defendant need give no warning of them. Here again a plaintiff may assume a risk that he does not in fact comprehend. Yet by no means all the dangers which would be obvious to the attentive or appreciated by the experienced are thus assumed. Whether they are depends upon the kind of relationship, the character of the place, the likelihood that attention will be distracted, the customary behavior of people who frequent the place, the likelihood that inexperienced, young, or handicapped people will be there, and the like. These are of course the same factors which are to be considered on the issue of defendant's duty, for, as we have seen, the issue is the same.

\* \* \*

We have said that the voluntary character of the association between plaintiff and defendant is the gist of the defense. This needs further elucidation. Plaintiff may voluntarily encounter a risk in one sense, yet not assume it. An example is the case of the traveler who chooses the more dangerous but shorter route on the highway. On the other hand plaintiff may assume a risk which in a very real sense he does not voluntarily encounter, as where a fireman in the

line of duty enters a factory which is dark and full of dangerous machinery, or where a tenant leases premises in a dangerous state of disrepair because no other quarters are available. The key is to be found in the character of the relationship between the parties and their respective duties in the light of it. The plaintiff takes a risk *voluntarily* (within the meaning of the present rule) where the defendant has a right to face him with the dilemma of "take it or leave it"—in other words, where defendant is under no duty to make the conditions of their association any safer than they appear to be. In such a case it does not matter that plaintiff is coerced to assume the risk by some force not emanating from defendant, such as poverty, dearth of living quarters, or a sense of moral responsibility. If, on the other hand, defendant is not privileged to put plaintiff to the choice of taking or leaving a danger, the mere posing of the dilemma takes away the voluntary character of any assumption there may be of the risk.

\* \* \*

#### 4.

#### Encouraging Self-Scrutiny by the Investigator

##### a.

#### Paul Freund Legal Frameworks for Human Experimentation\*

\* \* \*

Throughout this discussion of legal frameworks the criterion of consent has emerged in one guise or another. It may sound for investigators a jarring note, as if a *prima facie* assault were proposed, which was made lawful by the victim's consent. The concept of participation may have greater semantic appeal, suggesting as it does a common enterprise in which the various parties share. Nevertheless the concept of agreement in one form or another seems inescapable, as an earnest of the law's concern for voluntarism in private hazardous undertakings that, in fact, serve public purposes.

The concept of consent has been much derided as unrealistic and artificial, and of course it

\* 98 *Daedalus* 322-324 (1969). Reprinted by permission of *Daedalus*, Journal of the American Academy of Arts and Sciences, Boston, Massachusetts.

embraces a range of responses that differ in their degree of autonomy and understanding. The psychological constraints or compulsions that operate on a seriously ill patient are different from those that affect a person attracted to an experiment through an advertisement. Nevertheless a requirement of "voluntary, informed consent" does have values beyond the symbolic one of respect for individual autonomy and personality. It is far from the be-all and end-all of legal and ethical safeguards, but it is a valuable ultimate check, reminding one of Keynes' rationale for the gold standard: that it is a safeguard in case the managers of the currency should all go mad at once.

Not the least of the functions of consent is its reflexive effect on the management of the experiment itself. To analyze an experiment in terms of risks and benefits to particular groups by way of presentation for consent is a salutary procedure for self-scrutiny by the investigator—like the preparation of a registration statement by a corporation issuing securities.

An example in the field of medical experimentation can be taken from the tests of magnesium as a remedy for a serious form of nutritional deficiency in infants, which is fatal in perhaps 20 per cent of the cases and for which no effective treatment had been found. Magnesium appeared to give good results, but had not been subjected to a controlled experiment on human infants. Assume that such an experiment is, in fact, considered. What is the role of consent? Four possibilities suggest themselves. First, no consent might be required, in the view that a physician could conscientiously employ or decline to employ the drug in the interest of sound patient care. Secondly, consent might be thought irrelevant for an opposite reason, that good practice would call for use of the drug even without a controlled trial, in view of the seriousness of the illness, the drug's promise, the lack of an alternative, and no indication of deleterious side effects. The drug might thus be accepted, like aspirin or digitalis, on a basis lacking in scientific rigor. Assuming the experiment is to be tried, two further alternatives are open. The patients' parents, after an explanation, might be asked to consent to the use of the drug or to its non-use. It seems likely that it would be more difficult to secure the latter consent than the former. Finally, the parents might be asked to consent to the inclusion of their children in a randomly designed experiment.

One additional element of consent might enter into the calculus—namely, termination of

consent. How should a physician measure his responsibility to continue the experiment for the sake of scientific rigor against preliminary indications during the experiment that the drug is effective and safe? There is an inescapable element of choice, judgment, or will in every inductive experiment; there is no "logical" stopping point so long as the hypothesis tested has not (yet) been disproved. Consequently scientific rigor is at best an imprecise canon, to be weighed along with economy of time, effort, and other pragmatic judgments concerning appropriate termination. The making of this kind of analysis does not, to be sure, depend on a requirement of consent, but that requirement will make more vivid to the investigator the options that he must weigh in order to be candid with himself and his patient.

#### NOTE

HEDIN V. MINNEAPOLIS  
MEDICAL AND SURGICAL INSTITUTE  
62 MINN. 146, 64 N.W. 158 (1895)

\* \* \*

... The plaintiff, an illiterate man, badly injured in an accident, and physically a wreck, consulted with the physician and surgeon in charge of a medical and surgical institute or hospital as to his condition and the probability of a recovery. After an examination by the surgeons, he was positively assured, if he told the truth as to what was said (and the jury found that he did), that he could be cured, and by treatment at the institute could and would be made sound and well. Considering the circumstances, and the relations of the parties, there was something more in defendants' statements than the mere expression of his opinion upon a matter of conjecture and uncertainty. It amounted to a representation that plaintiff's physical condition was such as to insure a complete recovery. The doctor, especially trained in the art of healing, having superior learning and knowledge, assured plaintiff that he could be restored to health. That the plaintiff believed him is easily imagined; for a much stronger and more learned man would have readily believed the same thing. The doctor, with his skill and ability, should be able to approximate to the truth when giving his opinion as to what can be done with injuries of one year's standing, and he should always be able to speak with certainty before he undertakes to assert positively that a cure can be effected. If he cannot speak with certainty, let him express a doubt.

If he speaks without any knowledge of the truth or falsity of a statement that he can cure, and does not believe the statement true, or if he has no knowledge of the truth or falsity of such a statement, but represents it as true, of his own knowledge, it is to be inferred that he intended to deceive. The deception being designed in either case, and injury having followed from reliance upon the statements, an action for deceit will lie.

\* \* \*

b.

**Jon R. Waltz and Thomas W. Scheuneman**  
**Informed Consent to Therapy\***

\* \* \*

In the case of risks unknown to the patient, a distinction must be drawn between risks that a physician *could* disclose and those that he *should* disclose. The cases and commentators have focused on the second facet, without emphasizing that the class of risks which *should* be disclosed is limited to those which *could* be disclosed. Blurred analysis of the disclosure duty has resulted.

A simple proposition provides a starting point for analysis of risks a physician could disclose: it is impossible to disclose something of which one is unaware. This point is not made in the cases because most informed consent cases have involved collateral risks attaching to widely-used therapies; thus the courts have probably been correct in assuming that the physician-defendants actually knew of them. If a risk is not actually known to the physician, however, it is inappropriate to speak solely of a duty to disclose. Instead, a second dimension of the class of risks that a physician could disclose appears: the physician's duty to have known of a risk so that he could have disclosed it.

The duty to know of a risk has two branches: the duty to learn of risks known to others in the profession, and the duty to investigate to discover whether there are risks unknown to others in the profession. When a risk is known to some part of the profession but not to the treating physician, the question is whether he should be legally held to a standard of equal knowledge. Here, familiar notions of a physician's duty provide a relatively easy solution. A physician is duty-bound to have the knowledge

of a reasonably well-trained and knowledgeable physician practicing under like circumstances. If his knowledge of collateral risks does not meet this standard, liability will be based on that shortcoming, not on failure to disclose. Although the distinction as to source of liability is usually unimportant, it becomes crucial if the physician was not required to know of a risk, and was therefore not required to make disclosure.

The logical first step to liability for failure to disclose a collateral risk is the demonstration that the defendant doctor either knew or, because of the state of knowledge in the medical profession, should have known of the particular risk. It is fair to impute this breadth of knowledge to the modern-day physician—medical knowledge is highly accessible. It is, therefore, reasonable to say that the class of collateral risks that could be disclosed should extend this far. Beyond this point, however, the situation does not present an orthodox problem of informed consent since the duty to disclose particular risks must presuppose either actual or imputed knowledge. Risks that were unknown to the medical profession pose different problems which will arise most frequently in cases of innovative therapy. By "innovative" we mean a therapeutic procedure which is not one customarily used by the profession in treating a particular condition. The distinction drawn is between "customary" and "innovative" therapy. We use the term "innovative" rather than "experimental" because the distinction is basically one of frequency of use and because the term "experimental" is freighted with irrelevant overtones.

The duty to know what a reasonable physician under like circumstances would know can only extend to risks of which some segment of the profession is aware. Beyond that point the problem becomes one of initial discovery. Although no reported informed consent case has involved an undiscovered risk, the problem must be analyzed because of its obvious significance for therapeutic innovation.

The problem of risk-discovery poses the two-pronged question whether existing medical knowledge was sufficient or whether further investigation should have been made to explore the possibility of unknown additional risks. The standard of reasonable medical practice will provide the answer.

In the case of a customary therapy, the reasonableness of the level of knowledge of collateral risks will already have been determined. In the case of innovative therapy, however, a major issue is whether enough has been discovered

\* 64 *Northwestern University Law Review* 628, 630-635 (1969). Reprinted by special permission of the *Northwestern University Law Review*. Copyright © 1970.

about collateral risks to justify the conclusion that the innovation accords with reasonable medical practice. In the area that lies beyond the physician's knowledge about an innovative therapy, then, the duty to discover risks for the purpose of disclosure involves the same problem as does the question whether enough is known about the procedure's collateral risks to justify its use. The answer to both puzzles depends on whether a practitioner in the position of the innovator could reasonably conclude that enough was known about the collateral risks of the proposed therapy to warrant its use, or that, on the other hand, he should have postponed its use in order to investigate the possibility of previously unknown risks.

If a known risk of an innovative therapy is disclosed and subsequently materializes, the effect of a patient's consent will depend on the propriety of using the procedure. If use of the innovative therapy constituted reasonable medical practice under the circumstances, consent to disclosed collateral risks should relieve the innovator of liability. The result, however, should be different if use of the therapy was itself unreasonable. We accept the principle that a patient's consent to a therapy and its collateral risks will not relieve the physician of liability for negligence in performing the therapy. The same principle should apply in a case where use of an innovative therapy is negligent in light of knowledge of its collateral risks. In both cases the physician has breached his duty of care to the patient in the manner of treatment, and consent to a disclosed collateral risk becomes irrelevant.

Inevitably, innovative therapy will also raise the problem of consent to unknown risks. Two principal problems are posed. Should there be disclosure of the possibility of unknown risks? What will be the effect of the patient's consent to the possibility of unknown risks if one in fact materializes?

As to the first inquiry, it seems axiomatic that in all cases of optional innovative therapy the possibility of unknown risks should be disclosed, since this will bear heavily on the patient's decision whether to consent to use of the therapy. However, the effect of consent to unknown risks which later materialize is somewhat more troublesome. It could be maintained that a blanket agreement to confront the possibility of unknown risks will constitute consent to any unknown danger that may later materialize. The difficulty with this proposition, however, is that it does not accord with orthodox concepts of consent. The consent concept has traditionally

involved two elements: awareness and assent. Just as one cannot disclose something he does not know, so one cannot assent to something of which he is unaware. It may therefore seem both anomalous and unfair to suggest that agreement to accept the unknown constitutes knowing, binding consent.

The threshold question must again be whether the decision to employ an innovative therapy was reasonable in light of the physician's level of knowledge concerning it. Where use of an innovative technique is unreasonable because it prompts too many unanswered questions, introduction of a concept of consent to unknown risks assumes that advance consent relieves an innovator of liability for negligence in going forward with a therapy. No such concept obtains in cases of customary therapy, and none should apply in cases of innovative treatment. Consent is simply irrelevant. If a physician acted improperly by going ahead with an innovative technique as to which there were too many unplumbed questions involving its potential risks, liability will flow from the physician's unreasonable conduct. If, on the other hand, he acted reasonably in going forward on the basis of existing knowledge, the patient's consent even to the possibility of unanticipated risks is again irrelevant since the physician had no legal duty to disclose risks about which he neither knew nor should have known, and for that reason alone he is immune from liability. Maintaining the traditional meaning of consent in this context permits the question of liability for use of an innovative procedure to be resolved on a proper basis.<sup>22</sup>

<sup>22</sup> It could be argued that the concept of consent should be expanded to provide for "consent to unknown risks," thus holding a patient who consents in the face of disclosure of the possibility of unknown risks to have consented to any unknown risk that may later materialize. The difficulty with this redefinition of the consent concept is that it would do away with the distinction between innovative procedures the use of which constitutes due care in light of available knowledge and those whose use does not constitute due care. In either case, the patient would be held to have consented to the particular unknown risk, whether or not it was consistent with reasonable medical practice to employ the therapeutic procedure. If this course were adopted, the only limits to the effectiveness of consent would be set by criminal sanctions, where use of an innovative therapy would constitute a crime. It is usually held that consent to a criminal act is ineffective. . . . The traditional consent concept permits the law to stop short of this relatively crude position and assess effectiveness of consent to unknown risks on a more particularized basis.

## NOTE

FRANCIS D. MOORE  
THERAPEUTIC INNOVATION—  
ETHICAL BOUNDARIES IN THE INITIAL  
CLINICAL TRIALS OF NEW DRUGS AND  
SURGICAL PROCEDURES\*

\* \* \*

[An] aspect of "informed consent" that is so limiting in its application to therapeutic innovation (as indeed it is also in experimental in-

vestigation of any sort) is the obvious fact that there is no means of becoming informed other than by the experiment itself, even if there is a desire to give consent. The very fact that the procedure has not previously been carried out in man indicates that the scientist himself lacks the critical information required for informed consent. If the doctor knew the most likely outcome of the procedure, such information could only have come from previous experience, and in that event the patient would hardly be at risk.

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## E.

## To Increase Society's Awareness

William J. Curran  
Professional Controls—  
Internal and External†

\* \* \*

We now have four transplant programs in [Boston] with a large number of patients awaiting transplants in each one of the hospitals, and we do not yet have a developed public program of donation. We need a public program. We need public acceptance. We need an administrative mechanism for getting the donation programs off the ground. Legislation is only the start. Legislation is only the opportunity to move; it is not movement in itself. To date, not a single cadaver organ transplant has been made in this country as a result of a personal donation before death. All have been next-of-kin donations. Individual personal donation still isn't off the ground. If it doesn't proceed, if individual voluntary donation prior to death does not begin to occur, then the Dukeminier type of proposal [that organs and tissues be considered authorized for use, unless specifically denied or disallowed by the decedent prior to death, or by his next-of-kin thereafter] will receive a great deal more support. I think there will also be more support for authorizing a medical examiner to donate organs, at least those that he perhaps would have to destroy anyway, or at least sever, in his procedures.

As a result of this very fine piece of legisla-

\* 98 *Daedalus* 502, 510 (1969). Reprinted by permission of *Daedalus*, Journal of the American Academy of Arts and Sciences, Boston, Massachusetts.

† 169 *Annals of the New York Academy of Sciences* 555 (1970). Reprinted by permission.

tion, the Uniform Anatomical Gift Act, we can have a national effort in the field of donations, via a voluntary program. Both the Federal Government's reluctance to impose rules or regulations on conduct, as expressed by Dr. Confrey, and the general attitude that those of us working in the medical-legal field have tried to adopt, suggest that voluntariness is the preferred approach. We have encouraged it with regard to matters of consent as part of our professional feelings, and we want to extend it to other programs.

\* \* \*

## NOTES

## NOTE 1.

WILLIAM W. FRYE  
COMMUNITY INFORMATION PROGRAM  
FOR TISSUE AND ORGAN DONATION\*

\* \* \*

Kidney transplantation now leads the field of successes in total organ transplants. The first successful kidney transplantation ever performed in the world was accomplished in Boston in 1954. Since that time, research in all aspects of organ and tissue transplantation has made possible the transfer of a number of organs. The kidney, cornea of the eye, and skin are now being transplanted routinely to restore health of the patient, due to the generous support of normal organs and tissues by healthy donors. . . . Today, heart, liver, lung, and pancreas transplantation procedures are now in the early stage of development, and we face a similar situation that is even

\* 169 *Annals of the New York Academy of Sciences* 560-563 (1970). Reprinted by permission.

more critical. Two major problems exist: one that must be solved by the scientist and the physician; and the other, by procurement of organs that must come from an enlightened public willing to donate their organs and tissues. There are also legal, ethical, and moral problems that must be solved. We are now at the stage where the public is becoming increasingly aware of today's scientific and medical accomplishments, the potential benefits, and the extraordinary contributions that can and need to be made by individuals willing to donate their tissues and organs to help restore others to health.

The need, at the present time, for organs for transplantation is unlimited. Approximately three-fourths of the deaths in the United States result from kidney, liver, and heart disease. Thousands of patients have benefited from the use of transplants of cornea of the eye to restore vision; the use of transplanted skin in the treatment of deep burns caused by fire or scalding water; and the more recent successes with kidney transplantation. The availability of kidneys from living donors or the availability of livers, hearts, and kidneys immediately after death is severely limited because the pressing need for such organs is not generally known. . . . The public must be kept informed, and the public attitudes must be revised to keep pace with medical advances in transplantation. Individuals must be convinced of the great benefits obtained from organs if they can be removed promptly after death. . . .

For more than a year, the United Health Foundations Inc. has had a special committee investigating what function a private voluntary organization might have in the tissue and organ transplant program. . . . [T]he UHF Board of Directors, at its annual meeting in December 1968, authorized the staff to develop a National Transplant Information Center. The main function of the center will be to develop a national voluntary program relating to organ transplantation and tissue utilization. The program will combine the development of national and local services, public and professional education, community-related research and demonstrations, and legislative information.

\* \* \*

[A] function of the National Transplant Information Center will be to develop, for the general public, information about who can be a donor, what the cost of a transplant is, what kinds

of tissues are needed, where information can be obtained in any given community, and many other questions. These will be made available through community agencies, medical organizations, and other concerned groups.

\* \* \*

#### NOTE 2.

JOHN M. PRUTTING  
NEW YORK'S NEW UNIFORM ANATOMICAL  
GIFT ACT—WHAT IT MEANS\*

Many physicians have been reluctant in the past to ask the next-of-kin's permission for organ transplantation. In some measure this may have been caused by uncertainty over the legal implications as well as by fear of hostility and criticism from their patients' families. Valid excuses should be extremely rare now that the Uniform Anatomical Gift Act has clarified matters and established legally acceptable procedures. Hospitals and physicians in New York State no longer have cause to hesitate about undertaking this privileged responsibility. The transplantation of many organs is increasingly successful, and the physician or hospital not making a sincere effort to obtain these is remiss in the performance of basic medical responsibilities.

In short, the time is here for all of us to acquaint our patients with the need for donating organs and tissues by inviting them to fill out the new donor cards. Once the benefits have been clearly explained, an informed public will more readily grant permission for these procedures that can bring life to others after the donor's death. The importance of contributing toward the conquest of disease should be stressed. And families should be made to understand the dignity and care with which organs are removed, just as in a surgical operation, with respect for the body and its preservation. With implementation of the Uniform Anatomical Gift Act, there is no longer any excuse for letting inertia, apathy or unawareness deprive the needy of organs and tissues which can now be readily and legally available. By the same token, it becomes our collective obligation to overcome with logic the objections of the emotional or superstitious who would deny these great inherent benefits to their fellow human beings.

\* 1 *New York State Journal of Medicine* 2359, 2361 (1970). Copyright by the Medical Society of the State of New York. Reprinted by permission.