Science as a Career Choice

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Theoretical and Empirical Studies

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Editors

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To Sam, David, and Stan—
A mature scientist; a would-be scientist; and an artist

Contents

Preface

How does an individual become a Steinmetz or an Einstein, a person who conceptualizes our world in such a way that we look at it with greater understanding? It has been easy enough in the case of a single great man to reconstruct his biography or to reformulate his life experiences and postulate the events that seemed significant for his later career choice and scientific brilliancies. Easy enough but not necessarily correct enough.

Literature about scientists has been filled with myths and romanticisms which have substituted intuition for correlation, especially when chance occurrences have coincided with stereotyped expectations. Numerous historical accounts have been based on assumptions about critical incidents and relationships which were thought significant for the development of bright, capable, and often idiosyncratic scientists prior to the launch of *Sputnik*. However, once in space, *Sputnik* had the unanticipated result of sending these mythical notions about the growth and professionalization of a scientist spiraling down to earth.

With their accomplishments in the public limelight, scientists could remain no longer faceless and outside the realm of investigation themselves. Their reputed remoteness, isolation, noncommunicativeness, and technical overspecialization, which had served to protect them against even being considered as subjects for study, had to be subordinated to the reality of demands for new scientists.

Faced with the imposing task of identifying youngsters who were potentially suitable for scientific roles, motivating them into careers, and training them for maximum effective and gratifying performances, the scientists' previous immunity to study became an academic issue. In its wake, a sizable data base was accumulated on the precursors, stages, and routes by which individuals become socialized in professional scientific behaviors, and resocialized in furtherance of their career patterns.

The current national research scene suggests that the sheer magnitude and needling impingement of contemporary social problems on everyday living will trigger a major social and behavioral research effort in the next two decades. Therefore, it seems clear that a similar mobilization of talented human resources in these fields will have to be undertaken. However, to date only a limited supply of trained social scientists is available.[1]

Yet an oversupply of trained manpower in certain professional areas as physics, chemistry, and engineering complements the undersupply in the social and behavioral sciences. In attempts to remedy this, the general trend has been to train in areas where need exists. However, this is a time-consuming endeavor. It means the training of small groups in specific areas at a given time, while highly trained professional researchers in other areas are not being absorbed in research endeavors. This strategy has been adopted because at this time it is not possible to indicate whether professionals trained in single areas have the flexibility to shift fields within certain obvious limitations as different problems demand attention. And yet such an issue has important implications for the deployment, training, and retraining of professional manpower in the years to come. If funding for manpower planning is limited it may be judicious to train fewer potentially outstanding investigators. Instead the training of those who have a capacity for easy professional mobility in several areas should be emphasized over the training of a large number of narrower specialists. However, such efforts logically must rest on empirically based knowledge of the psychological structure of scientists in these various areas, their motivations, commitments, and resources.

This volume was developed as one effort to enlarge, improve, and systematize the data base compiled on the psychology of sociologists, psychologists, anthropologist, economists, and political scientists who will be needed to undertake tomorrow's urgent research. In this book are collated a representative group of empirical studies on the career choice and performance of men and women whose professional adult work is scientific research. The many extant theoretical frameworks within which these investigations have proceeded are visible. Inevitably, viewing related studies in their proximal relationships to each other encourages a critical evaluation of the scientific status of the area.

A thorough search of the literature of the last twenty-five years, and a more superficial examination of the fifteen years preceding, turned up more than 1,000 articles on psychological facets of career choice and development in the sciences. Inevitably the search moved into studies on adolescents and their anticipatory socialization processes, as well as those on organization-bound men who were in their middle and later years, and on the gerontologically directed disengagement processes of professionals from careers in which they had been heavily committed. These were some of the topics which interested us because of their inherent content, but which proved tangential to the task as we had defined it. Selection was to be limited to articles that were directly concerned with the psychological background, development, and behavior or motivations of persons in the social, behavioral, natural, life, and physical sciences. Subjects were to be primarily associated with research careers or had to show a strong tendency to enter or to consider these careers in their choice of adult work. This meant that studies of creative adolescents were not included unless they were already considered potential scientists. Studies of the Westinghouse

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[1] Even in areas where no shortages are apparent, for example psychology, there are relatively few individuals who have research training in the social problem areas.

sample of youngsters would be included, therefore, whereas data from Project Talent would not.

For the most part studies of creative persons and of talented geniuses in allied but nonscientific fields had to be excluded. On occasion certain kinds of artists were utilized by some investigators as comparative populations, thus giving a sample of findings on other related populations. In addition, studies of the creative process were considered outside the province of this book. Examination showed that in this area particularly, intuitive and introspective data form the main data base; in all but occasional instances, studies on process were neither analytic nor were they science-discipline-specific. However an illustrative group of papers assessing the cognitive and perceptual characteristics of scientists and those bearing on what have been thought to be components of creativity or original talent have been included.

These constraints then have served to create some arbitrary delineations: the creativity literature, as noted, is included only when background or personality data are emphasized, and not when the product or process receive focus. Similarly, while a developmental focus is theoretically very homogenous to psychological analysis of the studies on personality and its role in evolution of a career choice in science, the articles on children whose creative performances, behaviors, and propensities may seemingly elaborate the developmental precursors for later scientific work but who are not committed to a career as yet, have been considered out of bounds. The prolific data on high-achieving individuals and on achievement motivation was excluded, except when directly referring to achievement in science.

Within such constraints, the body of studies to which we ultimately attended was more sizeable than that to which Walter Miles, an early pioneer in vocational development, had referred in 1932, when he pointed to the inverse relationship between the number of adult years and proportion of research devoted to these years. It was superior in quality as well as in quantity to the work on persons in science and other creative fields existing prior to 1945, to which Guilford (1950) had drawn attention. Yet the total data base was by no means unwieldy, and could be readily reviewed and collated.

In compiling a book of readings on scientists that was illustrative of the scope of the field studied, and of the strategies, populations, assessments, and analyses undertaken, our attention inevitably was oriented to a number of phenomena:

1. The areas in which some fairly "hard" and incontravertible data lie—the areas that should be definitively pushed in future research to point of confidence, if not conclusion
2. The gaps, omissions, and discrepancies in the mosaic of extant knowledge on scientists, that may have import for individual variances within the population, but which must be looked at more closely to assess their real significance
3. The fit of empirical data with current theoretical constructions in the area of occupational socialization behaviors, with an eye toward strengthening those conceptualizations that seem most consistent and explanatory

Our own desire to group the papers in this volume within a conceptual framework that is consonant with existing personality theory on the one hand, and with a theory of occupational socialization behaviors on the other, showed us how delicate and tentative such a task must be at this point. The work on research scientists has suffered from the general alienation of social and behavioral scientists from the psychology of adulthood. Moore (1969) has drawn attention to the almost paradoxical situation whereby the topic of vocational behaviors, so basic to the understanding of society's economic processes, has drawn minimal scholarly interest. There has been little empirical response to the promptings of such theorists as Erikson (1968), Parsons (1970), Merton, Reader, and Kendall (1957), Brim (1966), Birren (1960), Neugarten (1969), urging that the developmental threads linking childhood, adolescence, adulthood, and old age have to be made more explicit. One hope for this collation is that by illustration it will show not only the pitfalls but also the promise of this and like areas of work.

The selections have been grouped to highlight the variables which have been studied as possibly salient to career choice and development in science, and the mechanisms by which these variables influence choice of assumption of a scientist's role. Thus, this book is divided into the following sections:

1. Variables that have been considered formative or salient for career choice in science and the postulated interrelationships
2. Mechanisms by which these variables operate
3. Final expressions of those variables in the scientific work role, including available choices, choice points, and constraints influencing choice
4. Psychological behaviors as expressed in the performance and further development of the scientific role

This organization of articles suggested a useful theoretical framework for encompassing the comprehensive body of studies on scientists. Its appropriateness will be further tested as psychological studies on social and behavioral scientists are generated.

Our own interest in systematizing the psychological aspects of career development of scientists met a responsive chord in Orville G. Brim, Jr., former president of Russell Sage Foundation. His interest in the study of socialization made it possible for us to work closely with him in the preparation of this volume, and to have the support of Russell Sage Foundation.

The cooperation and interest of colleagues who also had done original studies in this area was gratifying. We appreciate their willingness to allow their material to be reviewed and reproduced as it conformed to our goal of presenting a representative collation of theoretical and experimental work. We benefitted particularly from the critiques of Lois-ellin Datta, Wallace B. Hall, John L. Holland, Anne Roe, Morris Stein, and Harriet Zuckerman, as the volume was in process. Both Dr. Zuckerman and Dr. Harvey Sapolsky permitted us to review bibliographies that each had developed for courses on sociological and on public policy aspects of the scientific career at Columbia University and the Massachusetts Institute of Technology respectively.

Work on this volume was initiated at the Reiss-Davis Child Study Center, and

concluded at the Department of Psychiatry, U.C.L.A. School of Medicine, our present affiliation. At these facilities, Mrs. Beryl Gordon and Mrs. Marilyn Weir carried the main burden of secretarial services, and Mrs. Mary Johnson O'Connor served as research assistant. We are grateful for their superior intelligence and meticulous task-orientation, and for the personal pride they put into their efforts.

Los Angeles B.T.E.
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PART ONE / PSYCHOLOGICAL ASPECTS OF CAREER CHOICE AND DEVELOPMENT IN THE RESEARCH SCIENTIST

Psychological Aspects of Career Choice and Development in the Research Scientist

Bernice T. Eiduson

INTRODUCTION

If the last two decades were the decades of the physical sciences, the next two will be the decades of social and behavioral sciences. It is generally agreed that current social problems loom so large, and impinge so ponderously on the lives of all of us, that only massive and concerted efforts to understand and eradicate the problems will make tomorrow's world a viable one. With this in mind, social scientists and planners have begun to confront the weighty problems involved in mobilizing the resources, expertise, and sophistication essential to social science research enterprises.

In 1969 a Special Commission (National Science Foundation, 1969), charged by the United States Congress to recommend how such a thrust might proceed, anticipated a number of geographically distributed, multidisciplinary social and behavioral science research institutes with problem-oriented missions. Realistic appraisal of the human resources available for participation highlighted the small number of trained researchers in the field, the dearth of good training facilities, and the limitations in methodological tools needed to power research efforts.

Such concerns seem analogous to those confronting the physical sciences in the period prior to Sputnik. At that time, Sputnik stimulated an ambitious and wide-ranging effort to identify, recruit, and train promising scientists to work on the new problems of space. Needs, motivations, and requisites for performance in the physical sciences were evaluated, so that potential talent could be fostered, and the conditions of work under which such talent flourishes, could be provided. Considering the enormous flow of talent into the physical and natural science professions, and the undeniable creativity of their performance, this was a highly successful effort, and one in which guidelines for a mobilization of talent in the social sciences surely reside.

But to what extent can a body of work generated mainly through studies of the natural and physical sciences contribute to the identification and recruitment problems in social and behavioral sciences? Can information gained on "hard" scientists be generalized to researchers in "softer" areas? Of course the social and behavioral scientist is not completely unknown. Some descriptive survey data have been accumulated on current geographical concentrations of psychologists, sociologists, anthropologists, their subspecialties, and their origins by birthplace or university degree (Carpenter, 1954; Clark, 1957; Harmon, 1961; Knapp, 1963; Wispé, 1969). A few detailed developmental and clinical studies have produced fragmentary insights into the psychology of some social scientists: psychologists, for example, have been described as unconventional, bohemian, and imaginative in personality, and thus distinguished from chemists (Chambers, 1964) or teachers and administrators (Cattell and Drevdahl, 1955). They have also been shown to demonstrate little affection toward their parents, and a preference, if any, for the mother over the father in childhood (Galinsky, 1962). Psychologists have been described as relatively isolated in childhood (Chambers, 1964) and rebellious toward authority during adolescence (Galinsky, 1962).

In terms of career selection, work choice for social scientists seems at least in part reflective of childhood interests (Strauss, 1965). The average social scientist elects his career at the undergraduate level, in contrast to the physical scientist who chooses his work at an earlier stage, primarily in high school (Clark, 1957). Social scientists achieve their Ph.D.'s at a later age than do physical scientists and, perhaps not unrelatedly, make their most important contributions at a later age, i.e., 35 to 39, in contrast to 30 to 34 for natural scientists (Dennis and Gruden, 1954; Lehman, 1966; Pressey, 1960).

One of the predictors of choice of work in the social sciences seems to be the significantly lower high school grades in mathematics and science subjects, in contrast to generally high scholastic performances (Harmon, 1961). In line with this finding, favorite subjects of social scientists are foreign languages, English, and social studies (Chambers, 1964).

A prominently voiced motivation of the psychological career is an early interest in behavior; however, many psychologists have been unsuccessful medical school aspirants, or have flirted with teaching and business as possible careers (Clark, 1957). Since social scientists have an urban rather than a rural background, heightened sensitivity to social problems has also been regarded as a motivating factor for professional work choice (Glenn and Weiner, 1969). Psychologists who choose a career in research report being motivated by desires for eminence and by strong feelings of professional commitment; they have outstanding conceptual ability, and in contrast to nonresearchers, are not altruistic (Wispé, 1963). Some psychologists appear to have drifted into their careers, but those who have achieved eminence have been very much advantaged by their upper middle-class origins (Wispé, 1963). In contrast to their noneminent colleagues, eminent psychologists have had a significantly higher proportion of eminent psychologists as fathers. The less eminent have worked their way through school since their parents placed a low value on an education.

These findings, while provocative, suggest the sparse and unsystematic data base that exists on the psychology of the social and behavioral scientist. Also, it is apparent that in many of these studies, comparative analyses with physical and natural scientists were thought to be the logical way to proceed. Such a strategy has presupposed that the same background, personality, and motivational variables will be found in both social and physical scientists, and that they will have the same meaning. This may or may not be the case.[1] Some variables may be characteristic of researchers in general, regardless of field, but others may be discipline-dependent. The demand characteristics of the behavioral fields in general or of specific behavioral fields may be sufficiently different from those of the physical sciences so that they possibly may call out quite different psychological characteristics in persons who work in them. Only systematic comparative studies or a tightly formulated and well-supported conceptual framework will provide compelling evidence on this problem.

A rather comprehensive overview of investigations on scientists done to date suggests that certain issues directed the development of the main body of work: (*1*) What conditions, background features, or early milieu encourage selection of a scientific career? (*2*) What personal resources are requisite, and how are these resources shaped by experiences and life events? (*3*) By what processes or mechanisms are salient experiences, attitudes, and interests focused and internalized that they finally lead to a research career? (*4*) Are there certain conditions or stimuli that precipitate the decision to become a scientist? (*5*) How does an individual become professionalized and gain the identity, values, and goals that he shares with his colleagues? (*6*) As a scientist, what options are available in terms of behaviors, activities, or roles? (*7*) Are these choices which are predictable from his own background, preference, and previous experience? (*8*) Could the level of performance achieved be predicted from earlier events or behaviors? (*9*) What is the course of the scientific career, and the influences on such a course? (*10*) What are some of the psychological components of being a scientist—i.e., the conflicts, attitudes, emotional satisfactions, and frustrations? How

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[1] M. Stein has recently brought this to my attention.

are these related to the perceptions of the scientific life?

Obviously these questions grow out of a long view of the scientific career, one which presumes notable precursors or antecedents and identifiable consequents. This is the case whether a personality or a socialization approach is used. Most of the empirical data on the process by which decisions related to careers are reached has developed within a personality model, which has given particular attention to aptitudes, talents, resources, and early environmental factors which have encouraged specific interests and experiences at the expense of others. The encouraged and rewarded activities, relationships, and motivations tend to direct an individual increasingly along avenues felicitous to the demands of a scientific career. Thus the decisions about work that are formulated at some specific point reflect skills, motivations, relationships, and activities that have been most satisfying in earlier life as they have matured and become refined, as well as the anticipation that adult work in science will provide more of the kinds of experiences and challenges that have been most rewarding.

This developmental perspective also characterizes a socialization theory orientation. Early social roles set the stage for the kinds of later experiences which are sought in one's adult socialization patterns. The social reward systems that proved most potent and durable in early relationships become modified, elaborated, and changed over time so that new knowledge, ability, and motivations are available to meet the demands of new learning and the revised goals and expectations of others. However, early learning often sets limits for future socialization patterns. In the case of taking on an adult role as scientist, precursory behaviors, knowledge, and skills that were developed earlier and perceived as useful, become recombined and refined so that they meet the levels of proficiency and sophistication required by the professional scientific context. At the same time conscious and unconscious aspects of personality which also were developed in earlier learning situations become restructured so that adult behaviors and values meet what is valued and required in a professional scientific role. Thus, continuity between precursory experiences which have shaped early roles and the later adult work roles is established.

The questions also reflect the organismic view of development, a perspective which puts the study of growth and change into the foreground, and the total individual into focus. This viewpoint seems consonant with the theoretical and empirical work on vocations, occupations, and professions—although the data bank here is so limited that prominent conceptual underpinnings are obscured (Becker and Carper, 1956; Moore, 1969; Osipow, 1968). It also shares a common base with the work on normal adults, their development and vagaries (Erikson, 1968; Grinker, 1962; Hughes, 1965). Thus far, psychological data on adults appear strikingly discontinuous and even discordant, showing little evidence of the anticipated smooth and consistent course (Goulet and Baltes, 1970) and, to a large extent, this is the case with the work on scientists, too.

In this company, the investigator on the psychology of scientists is forced to think about such problems as how behaviors move steadily toward decisions favoring science as adult work; what behaviors, activities, interests, attitudes, roles show visible change and maturity in the direction of scientific skills and preferences; what subgoals encapsulate the final goals of a professional career; what role perceptives make the idea of becoming a scientist syntonic to the personality. The developmental perspective is also evident in the two main strategies used to study scientists: the mature person functioning at the professional level, and the fledgling, the potential scientist.

SCIENTIST POPULATIONS

Scientific populations have generally been classified into two groups:

1. Professionals: Those mature, actively functioning, and, on occasion, retired scientists who have already achieved professional training leading to a Ph.D., or in some cases to a master's degree, who are employed in research settings, and used in research capacities.
2. Preprofessionals: Fledgling or would-be scientists who are in preprofessional stages, or those who are enrolled as science majors or adolescents in high school programs.

The latter have often been identified as having exceptional promise in science through the Westinghouse Science Talent programs, or the National Merit Scholarship winners, or Project Talent, which utilize extensive assessment measures to spot young people with interests and aptitudes commensurate with a scientific career. It generally has been assumed that data from these two populations would feed into each other, i.e., that studies of established scientists would provide clues as to what to look for in embryonic scientists and, in turn, that data on emerging scientists would anticipate and show in developmental or even transitory form, characteristics of more established men. However, this is an assumption that has thus far been given only preliminary test (Datta and Parloff, 1965; Taylor and Ellison, 1967). The studies on very young children who seem scientifically inclined or creative, and therefore apt to consider scientific careers, have not been part of the scientific literature. The chronological distance of such children from the phases of maturity at which they choose careers often makes an apparent correlation more spurious than real.

Some studies have analyzed scientists with nonscientist adults as controls. However, an equivalent number of studies have attempted to distinguish more creative from less creative individuals within scientific populations. In this framework, some criterion of creativity has served as predictor. The predictor variables have ranged from more objective measures such as scores on tests of originality or from measurements of outside validity drawn from findings on the individual's scientific contributions, number of patents, number of publications, or citations, to subjective estimates of the individual's creativity by peers, colleagues, and supervisors. In a few instances self-reports and self-judgments of creativity have been used. Measures of creativity have often been interchanged with measures of success. However, for the latter some additional criteria have proved useful, as honors, distinguished awards, and other indications of meritorious service.

A number of disciplines have been included under the title of research scientist. The natural or physical sciences category includes the biological scientist, sometimes called the life scientist, as well as chemists and physicists. Social and behavioral scientists include characteristically psychology, sociology, and anthropology as well as history, economics, and political science; but studies of persons in the latter three areas are rare. Mathematicians, who are not easily encompassed under the rubric of social and behavioral scientist, or natural scientist, have been studied as creative persons. Those in the fields of the arts have sometimes been used as control groups in studies of creative persons. Although arts or humanities are usually not considered to be research fields. Nor are persons in psychology or sociology who are clinical practitioners or social service workers, or persons who primarily teach considered research scientists.

Scientists have also been studied in a number of research environments such as industrial, academic, and governmental laboratories. Investigations of these organizations and of the various roles scientists assume in each, show that in some respects the milieu places unique demands on the scientist's function. In some studies these differences are highlighted; in others they are ignored, on the assumption that the differences are not significant for the question under study.

In similar vein, studies have been conducted mainly on the American scientist. However, studies of scientists in other countries as France, Germany, and India, are present though infrequent (Chandra, 1970; Raychaudhuri, 1966; Grubel and Scott, 1967; Wilson, personal communication, 1969). In these studies, the influence of findings on American populations is generally acknowledged, and may be responsible for similarities in data across national boundaries.

PREDICTOR VARIABLES

The variables predisposing an individual to science have generally been selected from the following areas:

1. Family background, life style, practices, and attitudes, variables that provide a picture of the milieu into which the scientist is born and grows up.
2. Events in childhood, particularly relating to school and academic background.
3. Variables which describe the intellectual and emotional resources of the subject, as endowment that has been shaped through interaction with environment.

The rationale determining selection of variables has grown out of the literature on personality development and on the requisites for performance in the scientific environment. In the personality literature, special attention has been paid to the data on bright, high-achieving individuals and to individuals who show promise of unusual intellectual development. The studies on the research environment have elucidated the kind of resources, skills, aptitudes, behaviors, and patterns that mark successful performance in research, on feedback denote variables of personality, cognition, and motivation that might be profitably studied. Analysis of still other variables that have been scrutinized for their predictive capacity suggests that they have been chosen out of methodologically opportunistic considerations. They are accessible, easy to isolate from context and to manipulate statistically, so that the fact that they are difficult to embrace conceptually is ignored.

Family Background and Environmental Variables

Among the demographic and cultural variables that have been examined in the search for background variables that predispose an individual toward a scientific career, are the father's occupation and educational level, socioeconomic status of the family, geographical and regional origins, religion, race, and political background. These variables embrace the intellectual, and socioeconomic status of the family and, thus, presumably provide referents to the opportunities, exposure, and value systems available to the scientist during his childhood.

The notion of "occupational inheritance" has been tested in numerous studies on scientists. In one, which used a sample of over 80,000 college freshmen, Werts (1968) found that the students tended to gravitate toward their father's career, if the latter were a professional. However, parental occupations of scientists range over many levels of skill (Visher, 1948). In the case of creative scientists, however, more fathers are professionals than would be expected by chance, as Chambers (1964) found for chemists, and Knapp (1963) for social as compared to natural scientists. Creative adolescents who have shown strong potential for science (Parloff, Datta, Kleman and Handlon, 1968) also disproportionately come from homes in which fathers are professional. Evidence, however, has not been supported in Roe's highly selective sample of natural scientists (1953). Attitudes of parents toward work are more difficult to establish when looking at their influence on sons. Yet West (1961) found no differences when the motivations toward education were studied in fathers who were blue-collar workers, as compared with those in white-collar jobs.

Astin (1963) noted that fathers who were engaged in physical activities, science, or social service chose colleges for their sons that were high producers of scientists, as contrasted to fathers who were engaged in sales businesses, law, or persuasive vocations. In quite another vein, Roe (1951) found that among highly selected scientists, those who scored highest on verbal imagery indices, came from families in which fathers were in law, clergy, or in college teaching. Thus, the role of the father's vocational level and attitudes for scientific career remains of interest, though for not very conclusive reasons.

In a closely related area, educational background of the parents, Clark (1957) found that 48 percent of eminent psychologists had professional fathers. Six to ten percent of the fathers had a Ph.D. degree, while only 10 to 20 percent of the fathers and mothers had less than high school training. Parents of adolescents who were sufficiently distinguished in scientific aptitudes to have become part of the Westinghouse Science sample, were also found to be educationally privileged, with 25 percent of the mothers having some college, 29 percent of the fathers having a B.A. degree, and 22 percent of the latter achieving an advanced degree. Educational status of the parents has been shown to be a good indication for the kind of intellectual interests encouraged in the child (Getzels and Jackson, 1961; Schaefer, 1967) and for the pull toward intellectual stimulation. More specifically, West (1961) has been able to show that the parents' level of academic performance is primarily an index of whether the child will go on to the Ph.D. level.

Evidence on class status of scientists' families supports a cluster of variables pointing to the generally middle-class background of scientists who have been studied in the past (Glenn and Weiner, 1969; Roe, 1953; Werts, 1968). Social scientists seem to be more upper class than natural scientists (Knapp, 1963). Class status seems to make some difference in the choice of a social science versus a natural science discipline when sex is also taken into consideration (Werts, 1968). Class membership is not a relevant factor in determining whether the college chosen by the subject is recognized as a high or low producer of scientists (Holland, 1957).

When psychologists have been separated into clinicians and experimentalists, it has been found that clinicians tend to come from larger cities and are more urbanized than are experimental psychologists. Since sociologists also have urban origins, awareness of urban problems has been considered a prominent factor in motivating interest in social science areas (Carpenter, 1954; Glenn and Weiner, 1969).

Such demographic features at best offer faint clues to support the association between educational and urbane sophistication of parents and scientific careers of their children. A related stimulus, however, may be their attitude and acceptance of vocational goals which demand intensive training, preparation, and financial support.

As study has turned toward parental attitudes and behaviors toward children, early interests and accomplishments, a sizable number of family-related variables have been studied, such as place in family, discipline, relations with mothers, fathers, and siblings, and sources of intellectual satisfaction. Only a few of these turn out to be of more than passing importance. By and large, families of scientists are smaller than expected in the general population; at least this was the case in the late fifties (West, 1960). In line with the rather abundant data which has established the consistent relationship between eminent persons and their firstborn status in the family (Altus, 1965; Apperly, 1939; Jones, 1931; Sutton-Smith and Rosenberg, 1970), Roe (1953) found that the eminent scientists in her study were predominantly firstborns or only children. However, in the Westinghouse sample of adolescents interested in science, these trends were not supported. Datta's (1968) analysis of the Westinghouse sample suggested that the favorable effects of ordinal position noted were attenuated when such factors as sex, size of family, and number of siblings were considered.

Schacter's (1963) hypothesis that actual or psychological "onliness" may be of greater psychological import than ordinal position is supported by the consistent finding of periods of isolation in the early life of scientists. These isolation times usually occur during preadolescence or latency, from 8 to 11 years, and make for real and imagined distance from peers and siblings (Eiduson, 1962; Roe, 1953; Super and Bachrach, 1957; West, 1960). Isolation or separation was at times stimulated by physical illness, psychological problems, or mobility of the family. However, the stimulus was not considered as significant as was the response generated in the subject by the fact of isolation —the need to develop his own personal resources, a tolerance for being alone, and for finding ways to amuse himself during these periods (Bush, 1969; Eiduson, 1962). Closely allied to periods of isolation are a number of other relationships contributing to the feeling of "onliness," as (*1*) significant incidents of father-absence through death, divorce, or vocational circumstances during formative years (Barron, 1969; Eiduson, 1962; Roe, 1960); and (*2*) the lack of strong positive ties in relationships with families (Eiduson, 1962; Roe, 1957; Terman, 1954). Comparing psychological to natural scientists (physicists and chemists, specifically), conflictual and stormy relationships were the more prevalent milieu during early childhood years. The tenuous relationships have not invariably been found in scientists or would-be scientists (Strauss and Brechbill, 1959; Weisberg and Springer, 1961) but they do appear with sufficient regularity to suggest a less than warm or intimate involvement with parent figures.

The affective and emotional tone of the home has received considerable attention, especially since parental orientation and attitudes toward the child have been singled out as critical dimensions in the kinds of occupations to which the subject then turns (Roe, 1963). For scientists, the home has generally appeared to be rejecting, although this does not deny considerable interest in the child who becomes a scientist (Strauss and Brechbill, 1959). Empirical data do not bear out the postulated significance of the parent-child relationship for choice of field (Brunkan, 1965; Hagen, 1960). The notion seems too simplistic, or imprecise, based as it is on retrospective data that tend to be overdetermined by events in intervening years. Rejection by parents also may be too global a notion, for family values, aspirations, attitudes, beliefs, and ways of thinking, can be incorporated despite negative relationships.

A less than completely gratifying emotional environment has been also perceived as a condition encouraging the initiative desired in science. Autonomy and independence seem to have been encouraged by the child's physical and/or emotional distance from father, by ordinal position, by being an only child or a psychological "only," or by the rewards and gratifications given for showing initiative. Scientists often come from homes which are individualistic and nonconformist (Strauss and Brechbill, 1959), democratic in orientation (Myer and Penfold, 1961), and where discipline is not authoritarian or overly domineering (Nichols, 1964; Weisberg and Springer, 1961). According to their own reports, creative scientists have often experienced less discipline than have noncreative scientists (Stein, 1963) and a much less strict upbringing (Dauw, 1966). Social scientists report less harmonious homes and more inconsistent discipline than do natural scientists; as adults they feel more rebellious in their feelings toward parents (Galinsky, 1962; Roe, 1957). The low levels of authoritarianism found in the studies of high achievers is also found in scientists (Rosen, Crockett and Nunn, 1969).

Identification with the mother as achiever is an equivocal finding. In the Eiduson sample (1962), mothers of scientists were identified more with achievement than were fathers, who were absent from the home to such an extent that more than half the subjects could be rated as "fatherless" by the age of 12. The lack of fathering is more prevalent than the lack of adequate mothering (Bush, 1969; Taylor, 1963). Family relations are notably lacking in warmth or closeness, and remain tenuous through life. Mothers are perceived to be insecure, frustrating, preoccupied, and not too warm or protective, and fathers, though distant, are viewed with more respect (Bush, 1969; Eiduson, 1962; Roe, 1957).

These data conform with the greater autonomy permitted scientists as children, especially those who show signs of creativity (Datta and Parloff, 1967; West, 1960). The need to experience independence, curiosity, and the need to pursue their own way have been recognized as important formative experiences by parents of alert and bright scientists (Nichols, 1964). However, some sex differences are noted here: Dauw (1966) has found that fathers, but not mothers, encourage independence in girls. And retrospectively, adult women who are scientists report that only their fathers have been strong sources of encouragement (Rossi, 1965).

Some of the same distance and aloofness characterizing relationships with authority are noted in peer relationships. Physical scientists generally show less personal interaction than do social scientists (Cattell and Drevdahl, 1962; Galinsky, 1962; Roe, 1953). Psychosexual development of the scientists altogether appears retarded in the early and adolescent phases of his life, as Terman (1954) has found in his more intensive, longitudinal studies of gifted children who have turned to science as an adult career. When relationships are not intense, as in the case of physicists as compared to clinical psychologists (Galinsky, 1962), they of course have less of the stormy and conflictful characters that psychologists report.

Despite the generally lukewarm family relationships, substantive intellectual stimulation is provided in the family setting (Eiduson, 1962; McClelland, 1962; Super and Bachrach, 1957). Thus, the childhood environment of the scientist is far from the closely knit, warm milieu that is often idealized as the soil for fostering curiosity, independence, and maturity. Perhaps it is the very absence of nurturant and security-giving family background that fosters the initiative and resourcefulness that scientists show.

Childhood Interests as Predictors of Career

In line with the importance attributed to intellectual growth and development, the development of scientific and intellectual interests has been followed rather closely. Eiduson (1962), McClelland (1962), and Schaefer (1967) have found that satisfaction from intellectual activities appears early, becomes overdetermined, and can generally be traced to parents. Reading scientific material and mechanics are early preoccupations (Walberg, 1969). In fact, scientific interests seem to be crystallized in boys by the time they are in the eighth grade, with the 10- to 14-year-old period most important for this development, few interests in natural science being stimulated for the first time beyond that age period (Tyler, 1964). Since scientific interests are found in boys who are more masculine, these interests are thought to grow possibly out of a masculine matrix. The girls tend to develop scientific interests at a later period in their development, during high school years, and those who turn to science tend, also, to be outstanding in both personality and achievement.

The adult social scientist shows some definitive interest patterns which point to possible childhood precursors (Goldschmid, 1967, Rossman, Lips, and Campbell, 1967), yet none have been traced back to childhood as yet. Campbell and Soliman (1968) show that interest patterns of female psychologists remain stable over the extended period of their adult professional life, so it is quite likely that these interests had some identifiable determinants in childhood hobbies and activities.

Academic Performance

In line with general scientific interests, Myer and Penfold (1961) note that future scientists show an interest in physical science courses in their early school years. This specificity does not appear in many studies; more characteristic is the good ability which scientists show in grade school and high school (Roe, 1957; Strauss and Brechbill, 1959; Visher, 1948). By the seventh grade, skills predictive of creativity in the twelfth grade can be identified in scientists (Chambers, 1964). In addition, the motivation for good attainment in school is present.

Pressey (1960) has found that college at an early age is a favorable index to career performance. By the college years, when vocational decisions have been made, undergraduate grades in science appear to be a good index of later performance (Platz, McClintock, and Katz, 1959). This is the case for eminent psychologists, too, who range in the top 5 percent of their classes (Clark, 1957). Taylor and Ellison (1967) find, however, that grades are only good predictors if the course work itself is very directly related to the areas in which later performance is accomplished, a finding aligned with studies on assessment and prediction of performance in professional psychology and psychiatry (Holt and Luborsky, 1958; Kelly and Fiske, 1951; Kelly and Goldberg, 1959).

Interestingly, only natural scientists have chosen their major areas of professional interest by college. Less than half of the Ph.D. sociologists in the American Sociological Association (ASA), surveyed by Glenn and Weiner (1969), were undergraduates in sociology although they had studied social science and psychology. Subject majors for eminent psychologists, too, were often in the area of humanities, as opposed to the "hard" sciences. This finding suggests that interests and aptitudes go together, for social scientists score lower in high school math and science as compared with physical science majors (Harmon, 1961, 1966). These scores are not reflective of differences in endowment, for in intelligence quotient as well as in grade-point average, social science majors rank second only to physicists, and are superior to biologists and chemists.

This trend of findings has raised the question of the ways in which university and preprofessional training institutions reinforce the interests and good aptitudes that scientists show in earlier years. Do good scientific institutions select or attract the good students, or do promising science candidates initiate the choice of schools, teachers, and opportunities that will be conducive to a fruitful career? Knapp, Greenbaum, and Wertheimer (1953), Holland (1957), and Astin (1963), find that good students and good science programs do get together; yet it is not easy to unconfound the factors that bring about this integration. Evidence suggests that students first select the most productive institutions; and then that institutions rise to the caliber of their students. Teachers who are masterful, warm, and intellectually eminent make the most impression on science students (Knapp, 1963).

Intellectual Resources and Capabilities

What intellectual aptitudes does the child bring to academic experiences? Does he show a superior basic endowment which in turn encourages problem-solving activities and attitudes, or do problem-solving activities and intellectual interests encourage talents which might otherwise remain latent?

In the early stages of work on the requisites of scientific performance, superior intelligence was thought to be a minimal requirement. However, often mature and very capable scientists, with outstanding creative abilities, were shown to display a fairly wide range of ability extending from high-average levels into genius categories on standard intelligence tests (Barron, 1969; MacKinnon and Hall, 1971). Although standard tests are known to be poorly refined at high levels with insufficient top for groups clustered at the high end, MacKinnon and Hall (1971) were able recently to report reliable Wechsler-Bellevue Intelligence Test scores on a group of more than forty industrial research scientists studied initially some years earlier. Their findings run from lows in the high average range (118) to very superior, and correlation between the Wechsler scores with a test of intellectual ability emphasizing abstract thinking (California Concept Mastery Test or CCMT) were low but positive.

The CCMT, and the Miller Analogies Test (MAT) have been used with some effectiveness to show that conceptual ability does vary within scientific populations. This is also the case in regard to tests of originality and creative thinking which developed in response to Guilford's theoretical and empirical work elaborating the structure of intellectual abilities. Results on the Guilford tests which measure such dimensions as fluidity, flexibility, and reasoning are not consistent for scientific samples (Barron, 1969; Drevdahl, 1956). Scientists were not included in the populations on which these tests were standardized, although other groups showing creative abilities were utilized.

Prior to the more systematically developed theses of Guilford regarding the structure of the intellect, investigators approached the dimensions of ability and aptitude involved in science in various ways. For example, analysis of perceptual styles, preferences for complexity and disorder, which frequently were formulated in response to tachistiscopically presented stimuli, imagery modes, and associative facilities, supplemented the assessment of more traditional verbal-mathematical-spatial skills (Barron, 1963; Mednick, 1962; Roe, 1963; Stein, 1963). In these endeavors, motivational elements in intellectual performance came under scrutiny, and in recent work these have become an even more prominent focus (Eiduson, 1962; Holt, 1970). Thus, the scientist's orientation toward intellectual stimuli, his attention to novel, unusual, nonhackneyed percepts, his desire to restructure reality once obvious recognition occurs, his interest in fantasy, and attention to sensuous and unusual sensory impressions, have been noted. It is within this framework which enhances both conceptual and perceptual factors that the role of unconscious elements and primary-process thinking in the more rational secondary-process thinking of science has been explored. In science the structure of acceptable thought process is so formalized that signs of individuality in conventional, logical, rational thinking and personalized elaboration stand out (Bush, 1969; Holt, 1970; Pine, 1959).

Enjoyment of fantasy (Eiduson, 1962), access to early recall (Weisberg and Springer, 1961), application of analytic thinking to thinking about early childhood (McClelland, 1962), tolerance for ambiguity (Frenkel-Brunswik, 1948; Bruner, 1962) are characteristic of scientists' thinking, along with verbal facility, fluidity, flexibility, and capacity for abstract and symbolic thinking (Mednick, 1962; Andrews, 1965; Drevdahl, 1956; Garwood, 1964). The loosening of intellectual controls or regression without too much anxiety or disorganization (Kris, 1951) are among the most provocative descriptors attributed to the cognitive processes that scientists display. These permit the "paranoid leaps," Kierkegaard's felicitous phrase for the original and revolutionary thinking which takes reality as its point of departure.

There is some evidence that the orientation which adult scientists display is observable during adolescence (Taylor and Ellison, 1967), and leads to unusual dedication and commitment to intellectual activities at the expense of other pursuits (Bush, 1969; Cattell and Drevdahl, 1955; Eiduson, 1962; Roe, 1957; Taylor and Ellison, 1967).

Personality and Emotional Resources

By describing scientists as adventurers, risk-takers, independent and self-sufficient producers who are autonomous in activities, enthusiastic in regard to work, dominant, and sensitive (Cattell and Drevdahl, 1955; Eiduson, 1962; McClelland, 1962; Parloff et al., 1968; Van Zelst and Kerr, 1954), investigators point to characteristics which enhance intellectual as well as emotional behaviors. There is a remarkable consistency in findings across studies, techniques of investigation, and populations. Scientists exhibit an interest in things rather than in people or personal relationships, somewhat loose controls in behavior, an acceptance of challenge, unusual drive and commitment to task, high aspirations, confidence and self-esteem. Less consensus is found among studies in regard to the scientist's level of adjustment, his anxieties, impulsiveness, and modes of reacting to aggressive and sexual drives. The latter are variables that are perhaps more difficult to assess, and are more open to interpretation. This may account for the ambiguity of data in these areas. However it is equally tenable that scientists as a group show more variance in these characteristics (Eiduson, 1962).

It is apparent, too, that the stereotypes the public tends to hold about the personality of scientists (see section on "Sequential Characteristics of Mechanisms Involved in Scientific Career: Continuities and Discontinuities") relate to the better confirmed personality characteristics. Scientists are perceived as being withdrawn, introverted, preferring distance from interpersonal involvements. These features also come out in tests (Cattell and Drevdahl, 1955; Chambers, 1964; Drevdahl, 1956; McClelland, 1962).

There has been some interesting speculation about the value to science of such personality characteristics. It has been suggested that they permit intense persistence, reduce the tendency for distractability, and provide a kind of defensiveness or paranoid thinking that resists too easy acceptance of the obvious. Less clear are the values of the impulsiveness or instability that some tests show (Cattell and Drevdahl, 1955).

Are the characteristics found in mature scientists also found in embryonic scientists? Parloff et al. (1968) have found that more creative adolescents are like more creative adults, and differentiated from less creative adolescents and adults, in many characteristics, such as assertiveness, self-confidence, and autonomy. However, there is not continuity in all personality characteristics between creative adolescents and creative adults. For example, in this study the creative adolescent showed self-discipline, was reasonably circumspect in his dealings with others without detriment to his capacity to be independent in thinking and to integrate his ideas in novel ways. This characteristic was less notable in the creative adult. Although the significance of this finding for creative performance is ambiguous, the caution against assuming that the same traits will identify and characterize creative persons as they develop chronologically is clear: those characteristics which differentiate more and less creative adult groups cannot be applied indiscriminately for the purpose of identifying the potentially creative adolescent.

Data are equivocal regarding the expression of aggression and sexual drives: Knapp (1963) found physical scientists less aggressive than the general population while McClelland (1962), reviewing results of many investigations, concluded that scientists are more disturbed by aggression, especially in the interpersonal areas, and thus turn away to "safer" things and objects. The jealousy, competitiveness, hostility, and intracommunity feuds suggest that a great deal of aggression is expressed toward colleagues (Eiduson, 1962; Hagstrom, 1965; Merton, 1957, 1969). Undoubtedly the constraints provided by the rules and regulations of science encourage this open expression. For some men, the ethos of science merges and becomes one with their superegos; for other men, personal and professional conflicts are more discomforting.

Obviously certain personality and emotional characteristics are more advantageous for scientists than are others. On the basis of data to date it is extremely difficult to determine whether more adaptive resources have emerged slowly through the events and experiences of the scientist's life so that they have become characteristics which the work situation capitalizes upon or exploits; or whether the demands of scientific environment stress certain characteristics which among many others are part of the scientist's emotional makeup. The methodological difficulties of trying to chart the development of traits over time has been pointed out very perceptively by Kagan (1968), Coulet and Baltes (1970), and Kagan and Moss (1962). Some traits of personality are notoriously vagarious over a person's lifespan. Certain features can be identified early and reliably charted so that individual base lines can be established; other features, however, are notably unpredictable. They may not be manifest in every developmental phase, may be transitory, or may appear in phenotypes which are unrecognizable. Our ability to trace tendencies or characteristics of scientists to earlier manifestations may have to await a more sophisticated understanding of the stability and consistency of personality and intellectual traits over time.

MECHANISMS OR PROCESSES LEADING TO A SCIENTIFIC CAREER

By what processes or mechanisms are the important variables that predispose an individual to science integrated along the developmental course so that choice of a scientific career results? Thus far, understanding of a scientific work choice has been mainly dependent on the theoretical understanding of vocational choice in general. Uniquely professional, or science-specific aspects have emerged to some small degree.

Two theorists have moved science into the broader concept of occupational choice: Roe (1963) representing a need-theory approach and Holland (1966) in a factor-theory framework. Roe has given primacy in her system to early childrearing modes, the responses to childhood needs which in turn lead to preferences for certain satisfactions. The range of vocations from which an individual chooses his adult work depends on his general orientation toward providing service to others, a derivative of attitudes derived from parental relationships; the level of work he chooses depends on genetic structure and endowments. Vocational occupations are divided into six groups, from which prediction is made, once the childrearing attitudes of the home are known. The lack of success of this model in early empirical tests (Brunkan, 1965; Hagen, 1960) led Roe to a more elaborate formulation in which childhood interests, aptitudes, personality, and special environmental events were also contributing factors. Theoretically, the extent to which each of these factors moves the individual toward a selection of science as a career can be mathematically expressed, but this has not as yet been undertaken (Roe, 1968).

Holland (1966) has worked within a factor-trait approach. He organizes individuals into six personality types—realistic, intellectual, social, conventional, scientific, and artistic. Also, six model environments are formulated. Careers and behaviors are postulated through sets of axioms, laws, and hypotheses that predict the interaction of different types of persons in different environments. The internal relations of the theory rely also on a few constructs, such as homogeneity, consistency, and congruency. From this theory a comprehensive classificatory system of environments and personalities emerge, which permits organization of individuals along career lines and other dimensions of their lives.

The development of a personality type, in Holland's schema, involves the preference for certain activities which in turn encourages the development of competencies in those areas. In the case of the scientist or investigative personality type, the special heredity and experience of the investigative person lead to a preference for activities that entail the observational, symbolic, systematic, creative investigation of physical, biological, and cultural phenomena; and to an aversion to social, repetitive, and persuasive activities. These behavioral tendencies lead in turn to an acquisition of scientific competencies and to a deficit in persuasive competencies.

This pattern of preferences and competencies creates a scientific personality type which is predisposed to exhibit the following behaviors: achievements have an investigative quality; perceives self as scholarly, intellectually self-confident, having mathematical and scientific ability, and lacking in leadership ability; perceives the world in complex, abstract, independent, original terms with ability to integrate diverse stimuli; values science; susceptible to abstract, theoretical, and analytic influences, and least sensitive to materialistic and social influences; prefers occupations and occupational roles that facilitate his preferred activities and competencies and that minimize his aversions and incompetencies. Therefore, a scientific personality prefers investigative occupations and the role of the researcher or investigator. He avoids enterprising occupations and persuasive roles; copes with others in intellectual, analytic, indirect ways; and is characterized as analytical, rational, independent, radical, introverted, curious, and critical.

This theoretical position may be conceived as describing a way of organizing information about oneself, and focusing on the cognitive aspects of decisions involved in work choice. Other theorists have also seen choice as an information-processing task. Its developmental aspects, for example, are evident in the early work of Ginzberg, Ginsburg, Herma, and Axelrad (1951), who stated that choice becomes more settled as fantasies are increasingly restructured in the direction of reality. Super (1953), in a very comprehensive model of the occupational choice, saw that the family directly influenced the child through the direct information and attitudes it provided, and indirectly influenced the child through its vocational models. In addition to the tangible resources and opportunities it provided, the family also opened up to the potential scientist supportive information. Some of the system-analytic approaches also attempt to work toward a comprehensive theory of information input: Rogers (1968), e.g., notes that the inputs of the family, society, and genetic endowment are each important cognitive sources. Ellis and Tyler (1967) see the work situation contributing input about such realities as the rewards, feasibility of accomplishments, scope of activities.

This focus on the cognitive and decision-making aspects of choice represents a shift from the more classic developmental framework in which vocational roles were seen either as representing an expression of personality and motivational needs or as crystallization of specific aspects of identification (Darley and Hagenah, 1958; Forer, 1953; Golann, 1963). The latter was the rationale for studies of the similarities between the occupational choices of father and son. While father's occupation has in the past shown itself to be an anchoring point for the son (Caplow, 1958), as have class membership and economic opportunity, this may be in the process of becoming an outdated phenomenon. Even in science, change seems to be occurring so rapidly that "psychological inheritance" is no longer tenable; neither opportunities, professional structures, nor conditions of work are sufficiently similar to those of the past to be relevant (Dubos, 1961).

In the psychoanalytic framework, identification has been only one of the mechanisms by which remnants of earlier psychosexual relationship and conflicts become expressed. Interestingly, psychoanalysis itself has not given particular attention to work behaviors, assuming that any one of a number of behaviors could encapsulate aspects of identity; work was implicated only if occupational choice was an apt reflector of psychosexual development. Bordin, Nachmann, and Segal (1963) have attempted to demonstrate that various impulse-defense configurations and the superego manifestations, different for distinct personality types, predispose to certain professional choices. So far the only scientifically relevant professional studied by this group in line with this hypothesis is the social worker, who is service rather than research oriented, and therefore probably quite different from the scientist.

No conceptual model for the development of a professional career has as yet grown out of the empirical work on scientists. However, the studies of salient variables suggest that once intellectual requisites are assured, rewarded, and reinforced in early experiences, interests, personality and motivational factors become important. Further, as children move chronologically into experiences which provide exposure to scientific research or toward suitable counterparts, the number of situations which can serve as valid predictors increases.

Scientists as Perceived Role Models

As adolescents entertain the notion of entering science, they have many fantasies about what science and scientists are like. At times this reflects the notions that the public holds about scientists as a group. At other times, the images of youngsters who entertain the idea of becoming a scientist are more personalized and express their anxieties. These more or less correspond to the images that scientists hold and share as a group, and which permits their behavior, values, and attitudes to be predicted.

Perceptions of teen-agers toward scientists were first studied when efforts were being made to recruit promising talent among them. The often quoted study of Mead and Métraux (1957) pointed to many negative components in the teen-agers' images of scientists. In another study (Remmers and Radler, 1957), adolescents saw scientists as being differently motivated from other persons, difficult to talk to, overinvolved in their own work at the expense of consideration of others. The predominance of an intellectual, rational, and studious bent at the expense of warmer interpersonal reactions was a feature that was also noted when Bendig and Hountras (1958) and Braun (1962) studied college students. Apparently neither did direct experience with scientists (Marks and Webb, 1969) nor the ways in which the mass media presented scientists (Kerster and Hirsch, 1958) change these negative images. Krippner (1963) noted that after the concerted effort to depict the scientist in a more realistic and more favorable way, few parents encouraged scientific careers for even very able children. Thus teachers who are themselves quite impressed with the scientist's prestige are stronger influences than are parents in directing adolescents toward a scientific career. Incidentally, the stereotype of scientists shares some of the noncompassionate features attributed to academic types (Currie, Finney, Hirshi and Selvin, 1966; Demos and Belok, 1963).

When imagery in young people is studied in developmental perspective, there seems to be little difference between the young and the older teen-agers who are interested in science. However, as science moves into professional training and roles, the more powerful and positive imagery appears. Scientists as a group seem to be caught up in the same stereotypes that the public holds about them, and, in fact, the researchers seem to have been drawn into science by some of these same fantasies and stereotypes. For example, they see themselves as intellectuals, as discoverers of new worlds—worlds they not only create but which they then proceed to live in. Their work is propelled primarily, they think, by pressing inner drives, so that the majority scorn "impure" motivations, such as the desire for recognition, or exhibitionism, or personal aggrandizement, or pragmatic reward unless these characteristics are inescapable concomitants of devotion to the search for truth (Eiduson, 1962). Happiness and fulfillment rest primarily in their satisfactions at work, with routine drudgery and administrative problems played down as interferences. In fact, for these men, rigor, persistence, and discipline have all become institutionalized in their morality code as values in themselves, and therefore the nine-to-five gentleman scientist is looked down upon as the laggard who is bound to be unproductive.

There is evidence, however, that differences in the way science is being practiced today are accompanied by certain differences in the identifications that scientists have with other scientists. An example of this changing trend is the researcher's shying away from identification with the "great but maladjusted" or "eccentric" scientist. Reverence for forefathers whose outstanding minds were sometimes housed in very peculiar and odd personalities still exists, and yet the newer scientists seem consciously to be dissociating themselves from peculiar and difficult associates or students, knowing full well that they may be thus shunting themselves off from some very creative workers in their own laboratory. These men nowadays prefer to depend for progress on well-organized, smooth-running, large-scale operations, whose stability demands the minimum of interpersonal relationships, especially disturbed ones.

Time of Choice

Among the more studied aspects of career choice is the time at which the choice is made and its reversibility. This is of interest particularly since some social-learning theorists (Becker, 1964; Becker and Carper, 1956) maintain that choice per se does not occur, for the decision is never a conscious or specific one; instead movement toward a career emerges gradually through a constant narrowing down of interests, skills, and preferences during childhood and youth. Whether choice is, in fact, a conscious decision, most scientists can point to a time at which they decided to pursue a scientific career. Natural scientists tend to decide upon careers prior to college (Strauss, 1965; Tyler, 1964). In the Strauss and Rainwater (1962) sample of the members of the American Chemical Society, sixty-four percent said that they chose to be chemists in high school or before. Early commitment may be encouraged by research experience, or by high school counselors who are aware of the long period of specialized training involved (Hollender 1967, Chambers 1964). A commitment to science occurs so seldom after high school for boys that it can be said that movement after the high school period is only in the direction away from science (Forrest, 1961).

By contrast, social scientists seldom commit themselves to research in sociology or psychology before their undergraduate years (Tiedeman and O'Hara, 1963; Holland, 1963). This is also the time when the professional and academic career is decided upon (Strauss, 1965). Roe's (1953) eminent psychologists decided on their careers later than did the anthropologists. However, this does not mean that other options had not been entertained. In fact, in Clark's (1957) sample of eminent psychologists, a number of other possible options had been entertained such as careers in medicine, teaching, and business. Nobel Prize winners in science also decided on scientific careers early (Zuckerman, 1967).

Once into a scientific career there is a small group that tends to reverse its decision (Tiedeman and O'Hara, 1963). Werts (1967), examining the conditions conducive to staying with a career, finds that there is a greater likelihood that young men will remain if the particular career is one in which there are peers from the same socioeconomic group, if the choice is compatible with the father's occupation, and if career performance has been successful. Surprisingly, a fairly high percentage (50 percent) of the National Merit Scholarship winners leave science between senior year in high school and the end of their junior year in college (Forrest, 1961).

Thus the mechanisms involved in scientific career choice are vaguely described by ascription of roles played by various factors: as genetic resources, endowment, influence of parents and significant others, information about the career, as well as the more unconscious fit of needs, personality and emotional needs for gratification of certain kinds of activities, commitments, and values. Because the decision about work has been considered to be revealing of one's identification and self-concept, it has been thought that vocational decision could be subjected to the same kinds of analysis as are other important decisions in a person's life, like choice of a marital partner. As yet the degree to which vocational decision embodies personality and the way such decisions evolve over time remains ambiguous. Also, insufficient evidence about antecedents and consequents makes it impossible to identify continuities and discontinuities, or critical periods in the process. Further, the difficulties in achieving such complex information as demanded by developmental theories has moved some conceptualizers to seek more manageable formulations, such as stressing only certain kinds of activities and preferences, or competencies and skills that distinguish various kinds of work. The assumption here is that while these preferences and competencies are shaped by genetics, resources, training, family interests, and environment, it is not necessary to elucidate the relative contribution of each in order to make some predictive statements about work choice. Thus, process is dealt with more simplistically.

PSYCHOLOGICAL ASPECTS OF PROFESSIONAL ROLE BEHAVIORS

Once a career choice is made and appropriate training and preparation for the professional role accomplished, what career behaviors are evident? The scientific career can be studied along a number of dimensions, e.g., the kind of roles, assumed activities engaged in, the distribution of time and effort, styles of work, interactions with colleagues, supervisory relationships, eminence achieved, emotional satisfactions and frustrations. Many of these dimensions have proved to be subject to measurement and analysis, while others remain on a descriptive level. While studies of the scientific career have been useful in documenting the psychological behaviors involved as the scientist advances in his career, they have been less attuned to the question of how career behaviors reflect or are predictable from experiences. The latter is a particularly provocative question in the light of the control of scientific activities by the rules and regulations of science, professional mechanisms, and the institutions and context in which work is performed.

A number of questions have directed the study of psychological aspects of scientific career development to date: (*1*) What options are available to the scientist as he enters on his career and what factors influence his choice? (*2*) Can certain generic phases of the scientific career be identified? (*3*) In what ways do experiences during earlier phases of the career influence later phases? (*4*) How does development of the career relate to a man's productivity and creativity, and to the rewards he receives? (5) Are subgroups and subcultures identifiable within science? (6) What are the satisfactions in a scientific career? The frustrations? The emotional component and conflicts? (7) Since intensive investment in work is so distinctive for the scientist, can he turn work off? What other activities involve him?

Choice of Discipline

Studies comparing scientists in different disciplines have suffered from methodological weaknesses. It has been difficult to take account of function or role across subject areas. Also, studies of persons who are in preprofessional training, rather than already on the job often offer little insight into attitudes which are useful in professional activities. Demographic data on chemists, physicists, psychologists, and sociologists are now available through professional organizations, but data such as region of origin, socioeconomic status, or specialty areas are not regularly collected. If Carpenter's (1954) work is a good sample, we should expect rather considerable differences in background and present cultural data among disciplines and specialization areas. He found that experimental psychologists are drawn from quite different geographical origins, and different socioeconomic groups than are clinical psychologists. They are also attracted to schools of different sizes.

There are some provocative clues suggesting that some of the discipline-related differences may reflect cognitive differences, or differences in aptitude. Ph.D.s in social sciences are more number oriented and science oriented; non-Ph.D.s are more social service directed. Pelz and Andrews (1966) found that physical scientists who had achieved Ph.D.s performed as predicted on creativity tests, when motivational conditions were accounted for; in the case of non-Ph.D.s there was no correlation between creativity scores and performance.

The family backgrounds of natural and social scientists seem to differ in the small body of data available. Chemists come from individualistic families, more rejecting and less affectionate families, than do psychologists (Chambers, 1964). Galinsky (1962) showed that physicists formed fewer intimate relationships with family members but were intellectually stimulated at home. Psychologists, on the other hand, were more involved with their families, more curious about interpersonal relationships, had warmer relationships with their mothers but reported having more rigid and harsher discipline and, in general, more conflicts. The latter findings are supported by Roe's (1957) data. In objective personality tests chemists were showed to be more dominant and showed stronger initiative, while psychologists were more bohemian, introverted, and impulsive (Chambers, 1964). The academic situation and motivations toward school seem to have played a greater role in the natural scientist's life than in the social scientist's.

Professional Career Activities

When the actual workday of the scientist is analyzed, there is considerable discrepancy in what he does and the ideal of pure research activity which has been his pre-role anticipation. Surveys by professional scientific organizations have documented that work load is usually distributed among a variety of research, administrative, formal and informal teaching, supervisory, advisory, and consultant activities. Differences among scientists in the scientific tasks assumed are attributable to the nature of the research organization, some to level of professional experience, and still others to preferred work styles and to willingness to assume responsibility.

A number of activities and involvements have been described and studied in order to show the way the scientific career tends to evolve, and the ways the scientist decides to invest himself. As a result of this work, certain career lines become evident. Certain stages which are invariable are distinguishable from those that are more fluid, and certain critical periods for mobility within career paths can be elucidated. In addition, relationships between scientific performance, achievement, and career course have been suggested.

The distribution of time and effort among activities is one of the most readily assessed. When productivity is measured by publication rate, or number of patents produced, the periods of maximum output are directly related to effort devoted to research (Andrews, 1965).

As other activities such as administrative duties or teaching take precedence, productivity is lessened (Eiduson, 1966; Dragstedt, 1962). However, full-time research on highly technical activities is not necessarily an optimum condition for creative output. Andrews (1965), evaluating performance in eleven research and development laboratories, has found that full-time technical work is less productive than part time, regardless of the type of milieu. For some disciplines and for some experience levels, the spending of three-quarters or one-half of the scientist's time in research is correlated with optimum productivity. For example, supervisory science Ph.D.s did better research if they alternated it with administrative duties and did less teaching; engineers did better research when they also were engaged in teaching.

In longitudinal studies of academic natural scientists who were all originally devoting 75 percent or more of their time to research, Eiduson (1966) found that the subjects, in a five-year period, actually moved into five career patterns. Only 25 percent of the groups remained researchers primarily; better than one-half of the group interspersed research with duties other than research and development administration; 15 percent were full-time administrators; 10 percent left for industry. Productivity, as measured in academically valued goals, was directly affected.

Trends in changes of professional activity show low positive correlation with age, academic rank, and performance level. Administration is generally regarded as an academic plum for outstanding, upwardly mobile young men. In addition, there are certain ages when the options for change in major activity are most readily available, and certain ages when the possibility of mobility is sharply reduced (Eiduson, 1970). Further, moving out of research and into teaching or administration is easier than re-entry. The rapidity of scientific developments in most areas makes it difficult for a person to maintain sufficient sensitivity to advances in his field to be able to return without being penalized for his absence (Price, 1963).

Vertical and horizontal mobility in a scientific career can be measured and the factors influencing mobility evaluated. Scientists spend their careers at only a few institutions. Reinterviewing scientists studied twenty-five years previously, Roe found that one quarter of the sixty-four men had been completely stable during this time; a smaller percentage showed more than occasional mobility (Roe, 1965). Similarly, except for the period immediately following receipt of the Ph.D., forty scientists followed for a ten-year period generally remained with one university (Eiduson, 1970).

In studying the factors influencing mobility, sociologists have been impressed with the importance of regional factors and prestige of the Ph.D.-granting institution. A greater proportion of new doctorates than would be expected by chance stay within both the same region and the same prestige level as their doctoral schools (Hargens, 1969). Hargens and Hagstrom (1967) also showed that the prestige of the Ph.D.-granting university influences later institutional affiliation. Even when productivity of the individual is controlled, recruitment into top ranked graduate institutions is the important factor for what later happens to a person. The academic stratification system is such that a greater number of scientists than expected by chance are inbred into their own doctoral institutions.

Turning to vertical mobility, Mercer and Pearson (1962b) hypothesized that a large number of factors affect ascendency. Some factors extrinsic to the institution such as age, amount of income, lack of tenure, and academic rank prove to influence ascent rate; while surprisingly, the kind of work done, the type of institution, and job satisfaction are not as influential.

Another dimension of the research career which shows individual variation is style of research. Researchers have spoken of their preferences for certain kinds of research activities such as initiating the idea, formulating the problem, developing methodology. To some extent, preference for theoretical or experimental approaches is relevant here. In a more elaborate formulation, Gough and Woodworth (1960) described eight types of scientists who could be identified according to their strengths and preferences in research roles. Interestingly, they found that stylistic research types such as the methodologist, zealot, innovator, which were obtained on the basis of Q-sorts related to activities, could be correlated with personality as measured on the California Personality Inventory. This suggests that the personality differences, shown by Goldschmid (1967) to be correlated with choice of major field, may also be instrumental for the scientific niches into which a scientist moves.

The pressure on scientists to extend themselves into other kinds of work often comes as a result of success (Dragstedt, 1962; Eiduson, 1966). In addition, new activities open up that have appeal to persons who are not eager to stay at the laboratory bench, or who feel that they can be more effective in other things. For example, scientists function as science administrators in government laboratories, or cut across fields and translate their background to applied areas (Uyeki and Cliffe, 1963; Rubenstein, 1968). In the mid-60's their roles as advisors and consultants in Washington gained new importance, with scientists on the President's Science Advisory Committee holding some of the most influential posts (Storer, 1966; Eiduson, 1966). The elaboration of these roles deviates from the traditional concept of what scientists can and should do. As yet, only preliminary data are available on the skills that scientists need to employ to succeed in these activities (Eiduson, 1966). It has been noted that merely assuming different roles often raises the question of what values assume priority, and where loyalties primarily lie. Such conflicts seem to be natural concomitants of activities which involve professional affiliations, specialties, contexts or milieus, and organizational components (Glaser, 1963; Merton, 1960). By assuming scientific administrative posts in cross-disciplinary fields, one finds additional conflicts arise from trying to balance off various backgrounds, skills, and training as these become transposed to new tasks.

One of the other patent issues mirrored in one study of scientists in advisory and consultant roles in Washington, is that creative scientists are neither by ability nor personality nor by their research experience particularly adept at translating what they do and know to other arenas (Eiduson, 1966). Conflicts arise when roles are executed differently from the way scientists traditionally work and when personal involvements are at issue. In like vein, what scientists can do well is not always apparent and realistically anticipated by the people who want to use their expertise.

As the "Megabuck era" has changed research from an individual to a largely group enterprise, collaborative arrangements have been studied to see the effects of the enlarged effort on performance. Certain scientists, e.g., those who are older, are more resistant to group work than are others (Shilling, Bernard, and Tyson, 1964). Group research efforts prove most effective when a person's tenure is low enough so that he is interested in becoming a pioneer and high enough so that his interests are not too specific (Pelz, 1967). While it is generally agreed that ideas come to single minds (Cottrell, 1962), even solitary workers are challenged through contacts with colleagues. Especially fruitful are the number of scientific contacts a person has outside the laboratory (Shilling, Bernard, and Tyson, 1964).

The number of projects worked on simultaneously (Pelz, 1967), the perception of the project investigator by the other participants and the participating behaviors between members (Collins, 1970) are also factors effecting creativity. A curvilinear relationship has been found to exist between the responsibility for a project and efficiency as a scientific researcher, thus pointing to the importance of participatory relationships (Collins, 1970). The effective scientist is found to interact with colleagues (Pelz, 1967) and spends as much as a third of his time talking to colleagues (Hagstrom, 1965). Strong association among colleagues serves not only to concentrate energies but also results in greater productivity and more recognition than for the individual who works alone (Price, 1963). Following up this "invisible college hypothesis," Crane (1969), using a sociometric analysis technique, has found that the networks of relationship which develop between individuals who work together in a single area bring scientists into more intense, intimate relationships with each other. They form the kind of bonds that characterize, in Riesman's (1958) felicitous phrase, "true colleagues."

Productivity

How successfully do scientists meet professional goals? To establish modes of evaluating performance considerable effort has been devoted to finding reliable, valid, and useful measures of productivity, determining base rates for specific groups, and studying the factors influencing deviances from these rates.

Objective criteria for performance have been determined in ways that are consonant to science and to the ultimate critics, the scientist's colleagues. Numbers of publications, citations, patents, and inventions have been useful "hard" indices, as have to some lesser extent honors, awards, and election to honorary societies. Such measures have been made as sensitive as possible to discriminate among contributions of a relatively small proportion of the scientific community. The findings which show that, in psychology, a small minority produces one-third to one-half of total publications, and that 50 percent of professional psychologists (in the period from 1887 to 1951) produced 15 percent or less of the contribution in the field emphasize the need for measures which are relatively finely calibrated (Dennis, 1954).

Until the development of the Science Citation Index in 1964, productivity was measured mainly in terms of the number of publications produced. Publication rate was consonant with criteria for performance in the academic world, but its shortcomings, such as lack of comparative measures to establish quality and scope of different kinds of publications, were well known. The ability to establish usefulness of a work to colleagues by citation rate refined this measure, since citations were shown to have a high face validity (Bayer and Folger, 1966). Quality and quantity of publications were shown to be highly correlated (Cole and Cole, 1967; Price, 1965; Skager, Schultz, and Klein, 1965). However, quantity was shown to be more related to intellectual achievement in the home situation, while quality was more related to academic aptitude (Skager, Schultz, and Klein, 1965). Even more refined models of evaluating performance have been developed recently, e.g., the model suggested for sociological works by Glenn and Villemez (1970). These models, which differentiate the kind of publication, as well as determining to some extent its originality, have provided useful criteria by which productivity of department or members of faculty could be compared. They also constitute improvement over the more subjective ratings of originality, scholarliness, and significance (Lehman, 1960b).

However, the more judgmental and impressionistic assessment served as base lines in the pioneer work on rates and periods of scientific productivity. Lehman's work (1960a, 1960b, 1962) indicated that peak periods of creativity occurred relatively early in a professional career, when scientists were in their late twenties and early thirties. Also, the peak ages were to some extent discipline-specific, i.e., mathematicians who were "burned out" if they had no notable discovery by age twenty-one; physicists peaked between the ages of twenty-five to twenty-nine, and the social scientists found maximum stride in the late thirties and early forties.

Together with this determination of peak periods based on historical data, Lehman also postulated a relatively short range of productivity in the scientific career. However, using similarly retrospective analysis, Dennis (1954, 1956) found that half the output of physical scientists who lived in the years from 1800 to 1900 was produced after age 50. Roe's empirical work on eminent men (1957, 1963) supported these latter findings, as did Eiduson's (1966); in both samples productivity being maintained fairly evenly over time until the age of 60, when reduction was noted. There have been a variety of explanations proferred for Lehman's findings. Bromley (1967), pointing to experimental work suggesting the differential effects of age in quantity and quality of intellectual output, notes that Lehman failed to emphasize the likely effects of the reduction with age in problem-solving ability and spread of intellectual process.

There is general agreement that in the case of psychologists, peak periods occur in the late thirties and early forties (Dennis and Gruden, 1954). Pressey (1960) has speculated that this later flowering results from the later career decision points (Clark, 1957) and from the longer preprofessional training experiences (Kelly and Fiske, 1951). Yet all psychologists do not get degrees late; Clark (1957) found that the most eminent psychologists get degrees earlier than the less eminent. Chronological age and professional age are, of course, highly positively correlated (Lyons, 1968; Eiduson, 1966).

Studies show that scientists are aware of the evidence that has shown that most creative years for the natural scientist are the twenties and thirties (Eiduson, 1970). The lore about the breakthroughs in early years of the scientific career has been reinforced recently, particularly as the field of biochemical genetics with its specifications of the genetic code of life has become an extremely exciting area. The model for the genetic code was the product of Watson and some colleagues when he was about twenty-five years old (Watson, 1970). This has led to questions about the impact of early discoveries on one's subsequent professional career, especially since it is almost impossible to make second discoveries of unusual significance (Reif and Strauss, 1965). The association of youthfulness and originality has implicated the intellectual development of the talented person, the ability to solve problems in an original way when one is free from the known or doctrinaire, and when cognition is flexible, spontaneous, and when one can take the big risks (see section on "Cognitive Resources").

Also, this motivation for breakthrough, the search for the original solution, was important in the imagery which encourages persons to go into science in the first place. Scientists have identified in their professional images with the discoverers of new knowledge and have seen science as the field that offers them the opportunity to assume this role (Eiduson, 1962). In putting a value or a premium on original cognitive activity and on unusual perceptions, science was perceived as providing the kinds of satisfactions they had known in their previous intellectual endeavors.

With this strong pull toward the novel, the different, the original, and with the knowledge that this was associated with youthfulness, it can be anticipated that aging might be a major conflict for men in science. It seems inevitable that getting older would lead to soul-searching, some rationalization, and the developing of new mythologies for "older scientists." Roe (1965), Cook and Hazzard (1965), and Eiduson (1970) provide some data on personal reactions and behaviors as individuals in science grow older, and some responses of institutions which search for innovative ways of utilizing older scientists. Especially since the research environment has been shown to have the power to facilitate productivity on the one hand or hinder it on the other (see section on "Career Activities"), the work context has undertaken responsibility to see what it can do to attenuate some of the biological changes affecting intellectual performance.

Achievement of Eminence

Both tangible awards, such as the receipt of the esteemed Nobel Prize, honorary degrees, chairs, and some less tangible but equally important evidences of respect and recognition by peers and colleagues form powerful elements in the scientific reward system. An eminent scientist is usually thought to be one who has made the kind of great discovery or breakthrough that has brought recognition and respect from colleagues. However, the reward system does not operate invariably in a rational way. Its laws are not completely explicit, nor do the formal mechanisms that are spelled out encompass the contingencies and conditions or the "informal" arrangements that also affect the achievement of eminence.

The intricacies relating to success have stimulated the study of the psychological characteristics and scientific circumstances of scientists who achieve eminence as compared to those who do not. In studying "eminents," Zuckerman (1967), Roe (1953), Wispé (1963), MacKinnon (1965), and Clark (1957) used subjects who had been nominated for the major awards or had been ranked by colleagues as eminent on the basis of their contributions in their respective fields. In intellect, eminent psychologists seem to have high ratings for research and conceptual ability and a very strong professional commitment, as compared to less eminent psychologists (Wispé, 1963). Unlike their less eminent confreres, however, they are not particularly motivated by altruism or humanitarian motives. They appear to get started in careers earlier, have an earlier taste of the research laboratory (Zuckerman, 1967) and, generally, have come under the influence of parents, teachers, and family friends who encouraged and gratified their intellectual drives. Careers in science are even distinctive in other ways: they are longer, i.e., the years during which active publication is achieved are extended, and they move into major universities at an earlier age (Zuckerman, 1967).

The disproportionate number of eminent scientists at certain major universities has led to the question of how much the environment in which one works contributes to the eminence that a man receives. Crane (1965) has documented the fact that scientists who are at major universities are likely to be more productive than those at minor schools and to receive more recognition. Further, this tendency is inbred and has its starting point early in the scientific career (Hargens, 1969). Crane (1969) has found that graduates of major universities find more opportunities available at the time of graduation. However, professional opportunities seem also affected by social-class origins, with scientists who have lower-class backgrounds less likely to get positions at major universities than their middle-class colleagues. Scientists graduating from major universities have more abundant and more profitable work opportunities throughout the course of their careers.

Additionally, the prestige of the department in which one works is, along with the quality of work, a prominent factor in the recognition received (Cole and Cole, 1967). Since quality of work is more highly rewarded than quantity, the less creative scientists are siphoned off.

Can scientists manipulate the recognition they receive? It is generally accepted that such a phenomenon as "sciencemanship" exists in science, and that "visibility" can to some extent be promoted (Cole and Cole, 1968). This presents an interesting ethical problem for scientists. Ideally, merit should be the basis of reward, but they have become aware that salesmanship and a business ethic are often at play in the reward system (Eiduson, 1962). Further, as Merton has postulated (1968) and Cole documents (1970), the "Matthew Effect" is a powerful mechanism. That is, those persons who receive recognition continue to do so, the process of cumulative advantage operating in science.

Once eminence is achieved, career activities seem inevitably changed. For example, greater demands for lectures, for advisory and organizational commitments, for traveling entice the individual out of research and out of the activity which led to his eminence initially (Straus and Radel, 1969; Zuckerman, 1967). Even patterns of collaboration are affected. Nobel Prize winners engage in more collaboration at every stage of activity, even though collaboration stresses are greater after the award and sometimes collapse (Zuckerman, 1967). These changes in collaborative behavior patterns change in work practice and role activity after the reward is achieved. Productivity itself is often reduced, especially in the case of persons who are made eminent by the Nobel Prize, as compared to persons who were already eminent.

Alternate Career Patterns

Although the outlines of scientific career patterns remain sketchy and uninvestigated in many areas, it must be noted that what data have emerged represent the situation for the male, Caucasian, American scientist. Few studies exist on women, blacks, or foreigners—the minority groups in the sciences. Some of these groups, e.g., the women, are not numerically in the minority at every stage of career development: there are more female sociologists than male in graduate schools, but their attrition rate is high so that fewer women emerge as Ph.D.'s. In general, however, career development in both the social and behavioral sciences, as well as in the natural sciences, has a different course for these minorities, and thus their career structure is frequently called deviant. Basic to the observable differences in number, and in attainment, is the lack of social approval and encouragement of a career choice in science, the lack of good professional models, as well as the limited opportunities for work, for equal conditions of work, and equal rewards. Thus, the conditions for entering careers and for developing along professionally accepted lines are discriminatory.

Studies which have elaborated the nature of the uneven practices faced by minority groups have focused on development once in science. There has been very little work on the earlier familial and societal constraints which have mitigated against the entrance of certain populations into science, or even the consideration of it as a viable field for work. Inferences about this can be drawn, of course, from the skewed representation of certain classes and membership groups, as compared to others. "Hard" data on this matter exist mainly in absentia.

As one moves into the question of level of professional work attained, there is a beginning documentation of the one-sidedness of practice. Science has, for example, been considered a masculine profession, Mme. Curie and Gerta Cori notwithstanding. In fact, so consistently has science been identified as a masculine field that women who enter and develop competencies have been thought to be worthy of special investigation (Helson, 1965, 1967a, 1967b, 1971).

As the academic communities have begun to examine their employment practices, they have routinely noted the very small proportion of women faculty members, especially in science, as compared to those in the humanities area. Even in sociology, which attracts a high female graduate population (Rossi, 1965; 1970), the numbers of female faculty members are disproportionately low. In addition, academic status and the rank achieved are lower. Rossi points to the self-perpetuating mechanism at work in these situations: with so few models who attain the rank that men do, female graduate students are inevitably "turned off."

The picture is not particularly different in industry, especially in the natural sciences and engineering fields (Perrucci, 1970) or in other fields of endeavor (Bachtold and Werner, 1970; Epstein, 1970). Differentials in income, rates of advancement, opportunities for flexible work schedules, etc., are more customary than rare.

In tracing the genesis of these attitudes, well-known historical biases are found contemporaneously. For example, differences between men and women in mathematical and scientific abilities, though as yet without basis in fact, are attitudinally sex-linked. Thus, girls interested in these areas are discouraged more by their mothers (with whom they must identify in many other respects), while fathers apparently are less disapproving (Rossi, 1965). While data showing genuine sex differences in abilities demanded by science are weak, another line of study suggests that able men and women in any profession are remarkably similar in aptitude, interests, and personality. Helson's (1967b, 1971) and Bachtold and Werner's (1970) findings show that creative men and women in scientific professions are more like each other than they are to noncreative men or to noncreative women respectively. In personality and cognitive factors, significant differences occur between creative women and noncreative women (girls studied were in both science and humanities areas). Retrospective reports of early experiences also show some of the family factors and interests supporting these differences. Interestingly, when siblings of creative women are studied, the latter are shown to be more similar in cognitive development to their brothers than to their sisters. Scores on the California Personality Inventory support the greater number of common denominators between successful women and successful men, as compared to successful adult women and female college students.

Work on career practices in regard to black scientists are also just beginning to document the differences in career patterns (Wispé, Ash, Awkard, Hicks, Hoffman and Porter, 1969). Educational training which prepares the black for a science career is different and unequal. No studies exist on career lines of other racial minorities, or of psychological minorities such as homosexuals, or of national minorities such as the foreign-born, but investigations undoubtedly will be extended to cover the groups. Of interest in this latter regard is Grubel and Scott's (1967) isolated study of economists which points to the superior attainment in both position and financial remuneration for those men who received Ph.D.s from foreign universities. Although this singular finding may be a function of a specific historical period or a specifically qualified group, more elaborate work on the "brain-drain" group of scientists may validate this result (Wilson, personal communication, 1969).

In a rather free sense, the "new" scientists, the highly politicized young people coming into science, may also be called a minority. Do these people share the same motivations, interests, and drives as do the professionally older group? Some of the anxieties about their motivations, and the effect of these motivations on performance, are voiced by the older scientists, who have, in fact, labeled these young people as "gentlemen scientists" (Eiduson, 1962). Their persistence and dedication, and their commitment has been brought into question. Yet, their impact on the more formal scientific professional organizations is already in evidence (cf. American Association for the Advancement of Science Committee of Young Scientists; Scientists and Public Policy Sessions, etc.)

ROLE CONFLICTS AND ALTERNATIVE ROLES

Conflicts in Role Activity

In any professional career in which the professional has the intense investment, the dedicated, long-term commitment, and the great zeal that scientists have for their work, one would suspect that the work situation is fraught with heightened tension and conflict on the one hand, and thrills, enthusiasm, and deep satisfactions on the other. So much of the man is tied up in his work that many of the anxieties he has about himself, as well as about his performance at work, get expressed (Eiduson, 1962).

The emotional behaviors of scientists, their anxieties, conflicts, and other psychological reactions, seem to have become of particular interest because scientific activity itself is regarded as rational, objective, and logical; by extension, these same characteristics are projected upon the scientists. Empirical study, however, has shown that the same characteristics do not necessarily describe the attitudes and behaviors of the men who work within the scientific method. Like other areas of work, research has its moments of excitement, thrill, elatedness, depression, unhappiness, and anxiety.

Hagstrom (1965) has shown that scientists do develop a variety of mechanisms which ideally help keep performance at an even keel and make exchanges with colleagues smooth. There are referees, standard ways of communicating results, and recognizing excellence. However, a great deal of conflict, particularly of a competitive nature, is generated because the rewards for discovery and outstanding performance are so highly valued. In addition, evaluation of scientific performance is not invariably fixed. Many ambiguities about significance exist, priorities become all-important, and the single individual's contribution to discovery in this age of group research is often difficult to assess.

The literature on emotional reactions has heavily, and perhaps unduly, focused on such feelings as rivalry, competitiveness, jealousy, envy, and ambivalence toward colleagues. Undoubtedly, this emphasis has mushroomed because the values of science lean so heavily on personal disinterest, universalism, and selflessness (Barber, 1961; Merton, 1957b; West, 1960). The reactions of the man who is invested in work, his lability, narcissism, feelings of omnipotence, seem incongruent to the rational, logical, orderly pursuit of a research problem.

Reif (1961) vividly describes scientific competitiveness through showing the efforts generated to maximize and even promote success, and the depression and feelings of impotence that result when competition has not been successfully met. Often, under competitive circumstances, or when the man himself is driven mercilessly, the exchange of information is guarded, and privacy is overprotected. The information overflow and the enormous growth in number of scientists have undoubtedly intensified the need to accelerate the drive to succeed (Price, 1963). However, Merton (1957a), relying on the perspective of history, convincingly shows that scientists have always been threatened at the thought of "being scooped." And he notes further, that without the investment that such attitudes reflect, there is question whether the drive for achievement and accomplishment would be so sustained or so fruitful.

Scientists are aware, of course, of the payoff for outstanding achievement. In addition to the direct contribution to new knowledge, a basic motivation which is often taken for granted and goes unvoiced, is the rapidity of rise in science. Much of their future roles as scientists turns on discovery. Honors, awards, appointments, advisory posts, mobility, friendships, editorial and organization involvements are correlated with recognition and visibility (Cole and Cole, 1968; Crane, 1969; Glenn and Weiner, 1969; Straus and Radel, 1969).

The zest for being the great discoverer of new knowledge is part of the scientist's identity with his creative forebearers; yet their great performance becomes the ideal for making the scientist feel inadequate to his own task (Glaser, 1964). Scientists adapt with varying degrees of success to such pressures. The scientist can be greatly motivated to maintain creativity, and yet this is not invariably accessible to all, not only because of human limitations, but also because of the growing specialization in science, the short half-life of ideas, and the rapidity with which instrumentation and equipment become outdated (Reif and Strauss, 1965).

Notwithstanding the significance of the research milieu or the institutions of science for contributing to the scientist's plight, the psychology of the individual scientist plays heavily on what he does and how he reacts. Kubie (1953) has pointed this out lucidly, noting the individual differences in the ways that scientists respond to tensions within the scientific situation and the individual pressures which arise to influence career growth. Upon clinical analysis, these reactions often appear to be related to unconscious neurotic factors traceable to childhood fixations and conflicts. Relationships with supervisors in the laboratory rearouse conflicts with authority figures of the past, e.g., fathers, mothers, or siblings. Attitudes and unresolved relationship difficulties are readily transposed and projected to colleagues and peers. Overinvestment in work, or inhibition, contains earlier unresolved sexual and aggressive components (McClelland, 1962). Even choice of scientific problems may have neurotic determinants as well as reality based elements (Kubie, 1953; Roe, 1953, 1957).

Although the conflicts and personal problems found among scientists are not different from those found in other populations, they are differentiated in two ways: (*1*) the scientist's overcommitment to work gives them expression primarily in that situation, and thus the severe intensity of hostilities, jealousies, competitions at work; and (*2*) because the scientist's sense of personal identity is derived from his being a scientist, the tensions and conflicts in this area affect his self-perception and his self-attitudes.

Role of the Research Environment on Creativity

It is known that what an individual does is also affected by the conditions under which he performs. Studies of the scientific research organization have suggested that some environmental situations and work arrangements can stifle spontaneity and original productivity, while others are more conducive to original work. This has encouraged the search for factors in organizational arrangements which tend to influence scientific performance.

Studying more than thirteen hundred scientists representing different levels of professional skill at eleven research and development laboratories in government, industrial, and academic contexts, Pelz and Andrews (1966) found that the most creative environment was not totally without anxiety-producing elements. Using the phrase, "creative tensions," Pelz and Andrews expressed the notion that achievement was high under conditions that seemed, on the surface, rather incongruous, for they included both sources of stability, confidence, or security, and disrupting tensions. These tensions existed in regard to amount of time available for research activities, extent of interaction with colleagues, group climate, distribution of effort among projects, participatory role in formulating research goals, and degree of autonomy in coordinating activities. Interestingly, the optimal situation was not identical for all scientists. Maturity, experience and technical role influenced the scientist's response to certain conditions. Andrews (1965), analyzing a subset of these data, found that scientists who were involved in restructuring knowledge responded favorably to having some control over the communication, activities, and manipulation involved in their work. Their performance did not correlate positively with creative aptitudes unless these conditions were met.

Freedom of the environment has generally been considered necessary for innovativeness. Classically the academic milieu was thought to be most conducive to this freedom; however, Ben-David (1960) and Gilfillan (1935) have suggested that this relationship is a spurious one, and that more marginal academic departments provide a greater spur to creative work. An analysis of work in social science areas has borne out this thesis (Gordon and Marquis, 1966). In identifying the factors accounting for greater innovation in marginal settings, Gordon and Marquis found that merely maximizing freedom was not a sufficient inducement to creative work. Only when there is impetus to innovate and the impetus occurs in organizations where there is visibility of the consequences of the research, does freedom serve as a powerful device. It is for this reason that often marginal institutions, such as hospitals and clinics, produce extremely imaginative and creative research; the administrator who must justify a research program encourages products of unusual merit.

Working in organizations which have their own goals, and which, in turn, are structured into subunits, each of which has specific goals, often generates conflict for the scientist. The values and interests of science are not always identical with, nor even compatible with, the orientation of his organization or some of the subgroups within the organization. Thus, the value of publications, the lines of authority and supervision, the modes of work, the career lines, and the networks of communication may be different for the scientist, his unit, and the overall organization. In a narrow way, the frequently discussed distinction between applied and basic research goals embodies such differences (Kornhauser, 1962).

Glaser (1963) has used Merton's terms "locals" and "cosmopolitans" (1957b) to suggest two dimensions of orientation of the scientist who works in a research context within an institution. Glaser suggested that these two dimensions can actually exist within the same individual and are not necessarily conflictful, as Merton indicated. If the scientist is highly motivated, he seems to be directed to various activities and goals at different times and places, depending on the organizational structure in which he works. If the organizational goals embrace the professional goals of the individual, as well as the institutional goals of science, then obviously less conflict is generated in the scientist, who can perform within this dual orientation.

Activities of Scientists External to Professional Work

Studies of career behavior offer an opportunity to validate the question of whether looking at the scientist's activities away from the desk provides evidence of (*1*) the kinds of outside activities and relationships in which he invests himself; (2) the degree to which he is otherwise occupied; and (3) the emotional gratifications or tensions and conflicts other activities provide. These, then, can be compared to bench behaviors and attitudes.

Undoubtedly, one good index of the relative importance of non-scientific activities in the life of a creative scientist is the amount of comparative data obtained in the two areas: "non-bench" activities run a very poor second. Investigators must have felt that the essence of the creative individual does not reside or cannot fruitfully be studied in non-work behaviors. Interestingly, however, a few aspects of outside behavior have provoked more than sporadic interest, viz., religious behaviors and political attitudes and practices. It has been hypothesized that these areas of behavior may serve in feedback to influence scientific behaviors—thus, their significance.

The studies on religion evolve from the age-old question as to the compatibility of the philosophy of scientific inquiry and allegiances to authoritative dogma. The early Lehman and Witty study (1931) is characteristic of the simplistic nose-counting effort to establish the nature and extent of the affiliation of science with formal religion. Lehman and Schriver's (1968) recent effort is more sophisticated, attempting to distinguish a level of identification with religion through behavior-based interview data.

A much more lively issue is the extent to which scientists are politically involved, particularly since the whole question of the scientist's responsibility for the implications of his work has been revitalized. The political orientation of the scientist in various disciplines has long been of interest since scientists as a vocational group have been sufficiently stable to provide a useful measure of the role of demographic and familial characteristics on political orientation (Palmore, 1962; Turner, Spaulding and McClintock, 1963). However, since scientists have had an important role in political decision-making (Eiduson, 1966; Hirsch, 1968; Storer, 1966), issues differentiating the scope and purpose of basic and applied research are not so easily dismissed. In fact, a rash of inquiry into the attitudes of scientists on a host of contemporary political questions has been generated (see e.g., Hughes, 1965; Ladd, 1970; Nichols, 1969; Selvin and Hagstrom, 1965; Teich, 1969). In addition, the impact of the professional role and the resources scientists as a group can bring to political problems has begun to be investigated (Barber, 1965; Beer and Lewis, 1965; Ezrahi, 1969; Gamson, 1968; Morison, 1969).

Perhaps the discussion of the dichotomy between the societal consequences of scientific activity and scientific activity itself will also lead to more research on the role of scientific work in the life of the mature scientist.

To date, this area has scarcely been explored, suggesting that the hiatus between the pursuits and culture of the humanists and scientists is as wide as Snow (1959) has described so vividly. Once the role of work in the life of the mature, well-endowed, well-functioning scientific individual is better defined, perhaps the interplay between career development and outside interests and relationships will be documented with some of the thoroughness that now is found in the studies of the early aspects of career development and professional identification in medicine. The studies of Becker (1962, 1964) and Becker, Greer, Hughes, and Strauss (1961) have been particularly useful in showing how the development of some professional careers capitalize on personal and social interests and experiences that occur during adolescence.

Undoubtedly leisure time and personal relationship areas will come to the foreground as scientific practices and expectancies of performance change as they appear to be doing at the present time (Eiduson, 1962; 1966). The aging and retiring scientist also offers a unique opportunity for exploring the aging process and geriatric development of the well-endowed person who has invested heavily in the professional aspects of his life (Kerr, Newman and Sadewic, 1949; Pressey, 1955).

CONCLUDING REMARKS

The questions asked in the studies on the psychology of persons going into science are important ones. On their answers rest policy decisions about the identification and utilization of human resources. Their answers also reflect some pressing academic issues: the roles played by early predisposition, events, and traits in the formation of the identity of the professional adult; the nature of the socialization processes which lead toward a career; the consistencies and vagaries of aspects of psychological growth and development; the developmental course of life styles and of their maintenance in the highly endowed and well-functioning individual.

Data that have thus far emerged on the scientific career are dangerously seductive. Results obtained by diverse orientations, methods, and investigators are remarkably convergent. It is tempting to regard the findings with scientific closure, denying the splintery substructure, and the thinly glossed-over contingencies on which they rest. There is power, but also pitfall, in consensually-validated information.

A critical look at the work as a body indicates that most studies are at best descriptive, a few, comparative, and very few, predictive. Further, even the descriptive studies produce a picture of the scientist and the factors influencing movement toward career and work performance that is a composite, unevenly drawn from a variety of studies which have made quite different assumptions in selecting their original populations and their strategies.

Thinking about the process of socialization toward a professional career is not as yet very sophisticated. This is evident, for example, in the choice of variables initially selected for study, and in the homage investigators have paid to those factors which appear to be salient. Their theoretical importance has sometimes been difficult to interpret which is, in part, a reflection of ambiguous and uninspired theoretical underpinnings. Even a circumscribed historical look at variables appearing to be significant for scientific career development show a number which were catapulted into remarkable, but temporary, importance. By and large, investigations to date have offered little hard data, but have generated suggestions for future work which might obviate recognized shortcomings in studies conducted so far. For example, one has merely to list the populations employed in studies on scientists —the fledgling scientist, the mature well-functioning scientist, the creative versus non-creative scientist, the scientists in academia versus industry, the physical scientist (subgrouped by discipline usually) versus the social and behavioral scientist (similarly subgrouped)—to become sensitive to the need for some methodological refinements. If all these groups can tell us something about the development and performance of "scientists," each must be sufficiently studied so that findings then can be meaningfully inter-related.

Similarly, longitudinal strategies could contribute much to career developmental issues. The course of developmental factors which appear to be important predispositions for science have not been charted as yet, although this would appear to be a requisite for understanding how professional socialization behaviors develop.

In addition, the significance of context for the scientist's role must be systematically explored so that comparative studies of the psychology of the scientists in different work settings can be meaningfully understood. If different fields of science have different "demand" characteristics and call out different behaviors, these must be made explicit.

Also, the data sources used in scientific studies—retrospective data, current data, and projected data—have not been carefully related to each other. Therefore, we are left not knowing whether one can scientifically transpose or combine data collected from the various time frames. Further, the old myths of the scientist as a unique individual, somehow motivated by dynamics different from those which characterize others seems somehow to have been unconsciously accepted by those persons investigating the psychology of scientists. How else could one explain the assumption underlying most studies—i.e., that whatever is learned about scientists characterizes scientists rather than professional persons, or high achieving persons, or well-endowed individuals who pursue intellectual careers?

For all these limitations, the work on career and scientific interest remains compelling. It is one of the few areas in which the "normal" well-functioning adult male has been approached and studied with imaginative techniques and hypotheses. Since strategies for getting "handles" on scientists have been so wide-ranging, data have accumulated about the scientist himself, his background, talents, events and experiences, role behaviors, current performance, and about the scientific environment in which he operates. Studies of vocational prediction have shown that the more specifically the work context can be described, the better the chances for prediction of success of performance.

With this in mind, it seems appropriate to refine our studies by better measurements of both the scientist and his environment. In the style suggested by Dave (1963) and Wolf (1963), catalogs of scientific performance skills and resources which are at a premium in the scientist's environment could serve as clues for developing hypotheses about psychological variables in the early environment, background, and history of the scientist which are likely to be good predictors of future interest in science. Once the relevance of these precursory and predisposing variables is made explicit, their development and transformation as the individual matures can be noted; and the factors effecting development, evaluated.

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PART TWO / VARIABLES INFLUENCING CAREER CHOICE IN SCIENCE

A. Child's Social Matrix

Introduction

Examination of early history and family background of fledgling scientists rests on the assumptions that (*1*) certain conditions in the family and home milieu are conducive to building interests, attitudes, values, and predispositions that would be felicitous to a scientific career; and (*2*) that these can be agreed upon and identified at various early stages of individual development. These admittedly speculative statements have encouraged the approaches described in this section.

The articles by Datta and West explore the saliency of single variables that have been implicated in the literature as conducive for a later choice of scientific career. They look for distribution patterns in scientific populations which would support their postulated significance. By contrast, the latter two articles are postdictive, i.e., using scientist and nonscientist populations, a large number of biographical items are examined to see if any discriminate between populations.

Datta's selection of birth order follows previous studies on the high frequency of firstborns among eminent persons and high achievers. A disproportionately high percentage of eminent men have been firstborn or only children, Rhodes scholars (Apperly, 1939; Jones, 1931), or have attained unusual achievements (Altus, 1965; Harris, 1965; Krinsky, 1963; Sampson, 1965). A number of studies support these data for research scientists too (Roe, 1953). However, the question has been raised as to whether being firstborn or having the physical and psychological characteristics of "onliness" is the salient feature (Schacter, 1963). Using a subset of high school seniors from the Westinghouse Science Talent Search applicants, Datta explores whether or not the hypothesis derived from studies of adults—that potential scientific creativity is associated with being firstborn—is also true of adolescents. Her results do not support the hypothesis. However, she did note an inhibiting influence on the younger son who was more isolated. It appears that for a considerable range of family size, sibling sex, sibling separation, and ordinal position combinations, any "favorable" effects of one ordinal position appear to be as susceptible to attenuation by other influences as any "unfavorable" effects of another ordinal position.

In a similar vein, West has studied frequency of birth order in a sizeable sample of adult scientists in various research organizations. His work shows that the frequency of first, fifth, and sixth orders of birth are bound to be enhanced, and frequencies of the second, third, and fourth orders depressed, relative to chance expectations. Also, comparison of the sample distribution in number of siblings with the distribution expected from the general population demonstrates a rapid decrease of the ratio of observed to expected frequency with increasing size of sibship. These latter data would thus support the hypothesis that either relative isolation in childhood is a prerequisite for a successful pursuit of research or the hypothesis that mothers of scientists tend to have fewer children than other women.

Although such studies suggest that even patent variables are not necessarily explanatory of later scientific work choice, the biographical approach to the identification of scientific talent does seem promising when applied to a variety of situations including different laboratories, age groups, and fields of specialization. Taylor and Ellison describe one of the most sophisticated attempts to arrive at a biographical information sheet or questionnaire whose items are discriminating for various criterion measures of successful performance and accomplishment in science. Terman's well-known article is included here because it was an early effort in this direction. The longitudinal approach in this study permitted the followup of approximately 800 male members of a gifted group who had been selected in childhood for study on the basis of an intelligence test. Thirty years after the study was initiated, the vocational history of the subjects permitted Terman to examine retrospectively the items of information that might differentiate those subjects who became scientists from those who did not, as well as various subgroups of the scientists. The study is important for two reasons: (*1*) There was no attempt in 1921 to select subjects on the basis of variables which differentiated between scientists and nonscientists; the aim of this undertaking was much broader and more general, i.e., its aim was to identify the characteristics of children who were rated in the top 1 percent of their respective ages in general intelligence. (*2*) Also, the examination of more than 500 biographical items provided a relatively poor yield of background variables that were discriminating for scientist and nonscientist groups. As Terman points out, many of the variables which did not yield significant differences closely resemble those variables which met statistical test, thus forcing caution in the interpretation of results.

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Birth Order and Potential Scientific Creativity

Lois-ellin Datta

The data discussed in this paper were collected during the initial phase of a study on the development of potentially creative scientists. The primary question during this phase was whether variables such as personality traits, early experiences with peers and parents, and certain demographic factors which had been reported either to characterize unusually eminent men or to distinguish more and less eminent persons would also be found as early as the senior year of high school to differentiate young men of high potential creativity in science from those who demonstrated less potential creativity in science; that is, whether hypotheses developed on the basis of adult data would apply to fledgling scientists.

Many investigators have assumed that it is reasonable to expect and to find a relationship between a particular ordinal position in the family and eminence, primarily because the role assigned to the first born has been thought to influence the formation of personality characteristics relevant to unusual attainment (Altus, 1966; Harris, 1965; Krinsky, 1963; Sampson, 1965). Yet the impact of birth order on personality would have to be extraordinarily powerful to contribute significantly to the prediction of attainment without regard to capacities or to the environmental conditions to which the individual was and is exposed. Despite this objection, evidence in favor of a possible association between scientific eminence and birth order was presented consistently in early studies; recent evidence is less convincing. One possible difference is cultural change but another is inherent in the nature of the studies themselves.

In the earlier work, a predominance of first born or only children was reliably found in such samples as men starred in *American Men of Science* (Bello, 1954; Cattell and Brimhall, 1921; Galton, 1874; Roe, 1952; Visher, 1947). The proportion of first borns in these samples was not compared with the proportion among noneminent men in the same profession; thus these reports do not unequivocally demonstrate a relation between scientific eminence and birth order in the sense of covariation (Datta, 1966). As Schachter (1963:757) has pointed out, the association between birth order and eminence as shown in these reports may be a methodological artifact, ". . . simply a reflection of the fact that scholars, eminent or not, derive from a college population in which first borns are in marked surplus."

The evidence from the recent studies comparing eminent and less eminent men is inconsistent. It has been reported that first borns (as

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compared to all later borns) are not overrepresented among more creative industrial research chemists (Stein, cited in Harris, 1965:254) and eminent psychologists (Chambers, 1964); that highly creative architects tend to be first borns rather than only children or later borns (Craik, 1961); and that first-sons-but-not-oldest children and middle children are overrepresented among more creative chemists (Chambers, 1964). In these studies, however, comparisons are based on simple proportions for more and less creative men rather than on the discrepancy between observed and expected frequencies computed for family size distributions, so variations in family size could obscure otherwise significant tendencies or exaggerate otherwise slight differences. Consider, for example, the report that the eminent chemist was more likely than the control chemist to be a middle child. Larger families for eminent chemists would favor the obtained difference since the probability of being the middle child increases as family size increases while the probability of being the oldest child or the youngest child decreases.

The study reported here attempted to determine whether the original hypothesis that there are particular advantages associated with the position of being first born, which was based primarily on adult data, applies to young scientists when adequate control groups and adjustments for family size are used. A second objective was to test whether other patterns of family structure are differentially associated with creativity.

METHOD

Subjects

More attention has been paid in the literature to problems in the selection of creative subjects than to problems in the selection of control subjects, although the nature of the control group has varied most and thus is critical in studies comparing more and less creative individuals. Too homogeneous a sample may fail to reveal differences that would appear in a group encompassing a greater range of creativity. Greater heterogeneity, on the other hand, may be associated with variables other than creativity which are themselves related to the factors under study. The degree to which differences appearing in such heterogeneous groups are attributable to creativity rather than to the correlated other variables is often difficult to estimate.

The population selected for study was Westinghouse Science Talent Search applicants. This competition is aimed at discovering, ". . . boys and girls whose scientific skill, talent and ability indicate potential creative originality. . . ." The primary reason for choosing the Talent Search group was the availability of a product criterion of creativity. Such a criterion would appear to have more in common with the criteria by which adult samples of creative scientists have been identified than would (a) the largely unvalidated "creativity" tests available or (b) the other measures such as teacher and peer ratings which have been used in studies of creativity in adolescents.

The original experimental design involved variation of creativity and scientific aptitude and required at least four groups of young scientists: high aptitude, high creativity; high aptitude, low creativity; low aptitude, high creativity; low aptitude, low creativity. A pilot study indicated that there were no low aptitude, high creative subjects in a randomly selected Talent Search sample. We then attempted to obtain a group of high aptitude students that was as heterogeneous as possible with regard to early scientific creativity without also having correlated differences in interest in science, scientific knowledge, and general scholastic attainment.

The sample of young scientists was selected from high school seniors who competed in the 1963 Westinghouse Science Talent Search (STS). Of the more than 2500 male applicants, the 573 young men scoring above the 80th percentile on a science aptitude test (SCAT) devised by Science Service, Inc. were selected for further study. Only male applicants were considered since there were too few high aptitude female students for the study contemplated.

Each of the subjects had submitted to the Talent Search a report describing his independently conducted research project. This project was initiated by the student and conducted by him over as long a period as he felt necessary, ranging from a few weeks to three years. The projects were scored for "creativity and potential creativity" by the STS judges.

The standards for this judging have been refined over the period of 22 years since the competition was initiated. The judges were selected on the basis of their acknowledged eminence in one of the following fields: biology, chemistry, mathematics, medicine, psychology, psychiatry, and physics. Each student's project was rated independently by pairs of judges expert in the area of science concerned. The judges assigned each project a letter grade ranging from A, "Unusually original," through D, "Pedestrian," and E, "A poor essay; rehash of material read with little evidence that it was absorbed; thinking obviously not straight." A 14.0 (A) to 1.0 (E−) scale was used to obtain numerical equivalents of the judges' ratings.

Although some of the projects were judged to be of adult professional caliber, the majority of the ratings indicate potential creativity as demonstrated by the way in which the student went about the work and by the originality and value of the project in terms of information and equipment available to high school seniors. We will thus refer to the "potential scientific creativity" ratings of the projects.

On the basis of the project ratings, the sample was divided into three groups to represent different levels of potential scientific creativity. Group I, High Potential Creativity, includes 112 subjects whose project scores range from 14.0 to 9.5 (A to B−); Group II, Moderate Potential Creativity, includes 137 subjects whose project scores range from 9.0 to 6.5 (C+ to C−); and Group III, Low Potential Creativity, includes 287 subjects with project scores of 6.0 and lower (D+ to E−). Since the selection procedures were not consistent with those of the Science Talent Search, the group will be referred to as the Young Scientist sample (YS).

Comparisons (a) between the selected sample and a sample of noneligible male applicants and (b) among groups I, II, and III indicate that the sample includes students at the high and low extremes of the distribution of creativity ratings for the population of male STS applicants, without correlated variance in science aptitude and verbal and mathematical achievement.

(a) Examination of the project ratings of a sample of young men who scored below the 80th percentile on the SCAT indicated that lowering the criterion of eligibility for the YS sample would increase the number of subjects in YS III without a proportional increase in the number of subjects in YS I, thus reducing the science aptitude scores of the less creative subjects to significantly below the scores of the more creative subjects. None of a sample of students with SCAT scores in the lowest 20th percentile and few students in a sample of applicants with SCAT scores in the intermediate ranges submitted projects judged as high in potential creativity. The mean project rating of the 287 high aptitude, low creativity subjects (3.5) is not significantly different from the mean project rating (3.6) of the sample of 227 noneligible subjects. The YS sample thus includes virtually all of the "high creative" subjects and a group of 287 subjects whose creativity ratings were as low as those of the noneligible sample.

(b) In Table 1 are given the mean scores and p values for the F ratios of the project ratings, the SCAT, and of the Scholastic Aptitude Test—Verbal (SAT-V) and the Scholastic Aptitude Test—Mathematical (SAT-M) administered by the Educational Testing Service for Groups I, II and III. Analysis of variance comparisons showed that there are no significant differences among the groups on the measures of scientific and scholastic apti-

Table 1. Scholastic Aptitude and Scientific Aptitude Test Scores of 536 Male High School Seniors of High (I), Middle (II), and Low (III) Potential Scientific Creativity

Measure	*Mean scores* *I*	*II*	*III*	*p*
Scientific aptitude (SCAT)	67.6	68.0	65.6	n.s.
Verbal aptitude (SAT-V)	686.8	685.6	674.4	n.s.
Mathematical aptitude (SAT-M)	721.9	722.7	721.2	n.s.
Potential scientific creativity rating	10.9	7.3	3.5	.001

tude and that the project ratings differ significantly ($F=169.9$, $p<.001$). Students in the three groups are similar in scientific aptitude, in verbal and mathematical ability and indicated equal interest in becoming scientists. They differ reliably, and, in terms of the judges' descriptions, meaningfully on demonstrated potential for scientific creativity.

Procedure

A four hour battery of personality, attitude, and social history questionnaires was administered by mail at the time of high school graduation to each of the subjects. Complete returns were received from 95% of YS I, 92% of YS II and 94% of YS III subjects. The birth order information was obtained from the social history questionnaire.

This report will not attempt to include a full survey of the social background characteristics but will mention a few findings to help describe the YS sample. The students' average age at high school graduation was 17.7 years. On the basis of the Scholastic Aptitude Test scores (Verbal and Mathematical), the YS sample is clearly of high intelligence; the SAT-V and SAT-M means shown in Table 1 are above the 99th and 97th percentiles for male high school seniors, respectively (Duggan, 1962). More than 53% of the fathers are in the professions, a significant departure from the 12% found in the general population. Parents of the YS sample are highly educated; about 25% of the mothers held college degrees, as contrasted with 5% of the women in the general population, and about 29% of the fathers hold bachelor's degrees, with an additional 22% recipients of advanced degrees, about five to six times the national average. An estimate of the father's socioeconomic status (SES) based on the Hollingshead-Redlich Two-Factor Index (Hollingshead & Redlich, 1958) indicates that the total YS sample comes disproportionately from the higher socioeconomic groups: a total of 59% were classified as SES I and II as contrasted with 8% found in a New Haven sample. Of the total YS group, only 12% came from homes broken by death, divorce, or separation, somewhat lower than national averages. There are no meaningful differences among the three subgroups on any of these variables nor is family size or number of first borns related to socioeconomic status.

Kennedy (1961) has urged that high aptitude, high attainment students be studied if we are ". . . better to understand the characteristics which combine into scientific genius." While the YS group is clearly a high aptitude, high attainment sample, at least two questions may be raised concerning its use in a study of birth order effects.

Homogeneity. The YS group is homogeneous for both scholastic aptitude and socioeconomic status. Homogeneity in these respects does not necessarily mean insensitivity to birth order effects. Nicols (1962) has shown that the association of birth order with scholastic aptitude is greatest in homogeneous samples of high achievement subjects: the National Merit Scholarship test scores were higher for first borns than for later borns among the NMS winners but not among the samples of students scoring in the middle and lower thirds of the test distribution. The association of birth order with motivation and achievement also appears greatest among children from high income families; Levinson (1963) found that only among children from upper and middle class families did first borns have significantly higher reading achievement scores than later borns, and Rosen (1959) has reported similar results for achievement motivation. If creativity is affected by factors similar to those associated with achievement motivation or scholastic attainment, it is reasonable to expect birth order effects in the YS sample.

Generalizations. The second question concerns generalizations from self-selected samples. There is no adequate information available on birth order for an unselected sample of STS applicants or for high school seniors in general. Although there are considerable methodological problems in comparisons of birth order data, the proportion of first borns in the YS sample is similar to those reported for a nationally representative sample of undergraduate and graduate college students (Schachter, 1963).

RESULTS

In Table 2, the number of first born subjects for each family size is given for the three groups. To test the hypothesis that the surplus of first borns is greatest among young scientists rated high in potential scientific creativity, (1) the expected frequency of first borns was computed by assuming that the proportion of subjects in each ordinal position would be equal for a given family size in an unselected group, e.g., that 25% of the students from four-child families would be first born, 25% would be second born, and so on. As shown in Table 2, with only children and subjects who are twins and/or have twin sibs excluded from the analysis, the χ^2 values for expected versus observed proportions of first borns are significant at the .01 level within all groups; (2) the number of "expected" and of "surplus" first borns was next compared among the three groups. For example, of the 57 observed first borns in YS I, 38 were "expected" on the basis of the family size distributions and 19 were "surplus," while of the 69 observed first borns in YS II, 47 were "expected" and 22 were "surplus." The χ^2 comparisons of the "expected" and "surplus" first borns in YS I versus YS II, YS II versus YS III, and YS I versus YS III indicate that the differences among groups in the surplus of first borns are not statistically reliable. Among students of high scholastic ability, first borns appear to be overrepresented among young men of unusual early scientific attainment, but not to any greater extent than among young men of lesser achievement. In this sample, overrepresentation of first borns does not increase as potential scientific creativity increases.

The possibility was next considered that although being chronologically first born was not in any simple way related to early scientific attainment, other patterns of family structure might be associated with potential creativity. Three variables suggested as critical in the literature (Apperly, 1939; Chambers, 1964; Craik, 1961; Eiduson, 1962; Faris, 1940; Roe, 1952) were explored: other ordinal positions (various combinations of only, first-son-and-first-born, first-son-but-not-first-born, only son, middle child and youngest child); sibling separation (1, 2, 3, 4, 5 and 6 or more years from next oldest sib); and sex of next oldest sibling. These variables are not significantly related to early scientific attainment; none of the χ^2 comparing the categories listed above for YS I, II, and III reaches the .05 level.

Scholastic achievement, personality characteristics, and parental expectations have been found, however, to be affected complexly by interactions among ordinal positions, sibling separation, sibling sex, and family size (Lasko, 1954; Levinson, 1963; Rosen, 1961; Toman, 1961). To test the possibility that similar interactions influence potential scientific creativity, a four-way harmonic analysis of variance

Table 2. Number of First Born Subjects by Family Size Among 536 Male High School Seniors Rated as High (I), Middle (II), or Low (III) in Potential Scientific Creativity

Rating	*I*		*II*		*III*	
Family size	*First and later borns*	*First borns only*	*First and later borns*	*First borns only*	*First and later borns*	*First borns only*
1	13	–	14	–	47	–
2	52	35	59	35	117	76
3	24	12	37	23	59	34
4	13	7	13	5	31	15
5	3	2	6	3	10	4
6	2	1	1	1	5	4
7	0	0	0	0	5	1
8	0	0	1	1	0	0
9	0	0	1	1	1	0
Twins	5	–	5	–	12	–
N	112		137		287	
Observed first borns		57		69		134
Expected first borns		38		47		90
χ^2		9.5*		10.3*		21.5*

* With d.f. = 1, a χ^2 of 6.6 is required for significance at the .01 level of confidence.

Table 3. Potential Scientific Creativity Rating Means, Standard Deviations and Ns by the Subjects' Ordinal Position (Older, Younger), Separation from Next Oldest Sibling (Close, Distant), Family Size (Small, Larger), and Sibling Sex (Male, Female)

Subject is		*Mean*		*S.D.*		*N*	
O, C, S, M		5.7		3.2		42	
O, C, S, F		6.5		3.4		59	
O, C, L, M		5.6		2.8		58	
O, C, L, F		5.8		3.3		46	
	O, C		6.0		3.2		205
O, D, S, M		5.6		3.5		24	
O, D, S, F		6.2		3.9		21	
O, D, L, M		6.5		3.0		10	
O, D, L, F		9.2		2.4		5	
	O, D		6.3		3.5		60
Y, C, S, M		6.1		2.4		31	
Y, C, S, F		7.2		3.2		26	
Y, C, L, M		6.5		3.3		44	
Y, C, L, F		5.8		3.2		32	
	Y, C		6.4		3.1		133
Y, D, S, M		5.2		3.4		13	
Y, D, S, F		5.1		3.0		12	
Y, D, L, M		4.5		2.3		16	
Y, D, L, F		5.5		2.4		11	
	Y, D		5.0		2.7		52

was computed for the project scores. In Table 3 are given the cell frequencies, means and standard deviations of this analysis. As shown in Table 4, there are no significant main effects for family size (two-child versus multi-child); separation (less than five years from next oldest sib versus more than five years); position (older versus younger in relation to next oldest sib); and sex of the next oldest sibling. The only significant interaction is between position and separation ($F=9.7$, $p<.05$); exact comparisons indicated that the project scores of subjects who are distant younger brothers are significantly lower than those of subjects who are distant older brothers, close older brothers, and close younger brothers. The mean project scores of the last three groups do not differ significantly, at the .05 level, although the creativity ratings of subjects who are older brothers with close younger sibs tend to be lower than those of the other two groups.

Parallel analyses have been computed for the family structure data for a replication sample of male applicants scoring above the 80th SCAT percentile of the 1965 Science Talent Search. The only significant *F* ratio is for the interaction of position with separation ($F=5.7$, d.f.=1,308; $p<.05$). This independent replication of the A x C interaction suggests that the one significant *F* of the 15 *F* ratios shown in Table 4 may be a reliable, rather than a chance phenomenon.

Table 4. Summary of Analysis of Variance of Potential Scientific Creativity by Subjects' Ordinal Position, Separation From Next Oldest Sibling, Family Size, and Sibling Sex

Source of Variation	*d.f.*	*Mean square*	*F*
A (Ordinal position)	1	34.5	3.4
B (Sibling sex)	1	29.7	2.9
C (Separation)	1	3.5	.3
D (Family size)	1	2.1	.2
AxB	1	11.2	1.1
AxC	1	97.2	9.7*
AxD	1	23.2	2.3
BxC	1	7.4	.7
BxD	1	.5	.0
CxD	1	29.7	2.9
AxBxC	1	5.4	.5
AxBxD	1	7.4	.7
AxCxD	1	21.5	2.1
BxCxD	1	29.7	2.9
AxBxCxD	1	.5	.0
Within cells	434	10.0	–

* With 1, ∞ d.f., *F*s of 3.84 and 6.63 are required for significance at the .05 and .01 levels of confidence, respectively.

As would be expected, the A × C interaction is significant in the analysis of the combined 1963 and 1965 data ($F=12.5$; $p<.01$, d.f.=1,758). This analysis also yielded a significant position by separation by family size interaction ($F=5.7$, $p<.05$): the A × C interaction is greater in larger families than in two-child families. While it seems reasonable that the impact of birth order might be smaller in two-child families than in larger families, this interaction is dependent on an extremely high mean in the smallest cell (older, distant, female, large; N=8). Further studies may indicate that size of family and the combination "close, older" are related to early achievement; the most reliable positive finding in the 1963 and 1965 samples is the lower creativity ratings of the "distant, younger" students.

Considering possible factors in the lower attainment of the distant younger sons, we note that in the YS sample, general intelligence as measured by the SAT-V and SAT-M is not related to the potential scientific creativity ratings, and there is agreement in the literature that among adults matched for education and field of interest, intelligence is not related to level of creativity. A number of motivational characteristics have been found, however, to be associated with creativity in adults and adolescents; the two primary dimensions seem to be a high level of achievement motivation, particularly for independent achievement, and emotional involvement with ideas rather than with people. Rosen (1961:582) has reported that youngest children from middle class families with more than two children have lower achievement motivation scores than do first borns: he writes that, "In large families, the youngest child is frequently indulged, overprotected and may in general be exposed to few of the socialization influences associated with the development of high achievement motivation." Rosen did not investigate the effect of sibling separation; however, a child born considerably after the next oldest sibling, even in a two-child family, might be similarly "indulged." Analyses are in process to test the hypothesis that the distant younger subjects in the YS sample are lower in achievement motivation, are less willing to compete, have a higher need for affection and affiliation, and have experienced more overprotection and love, and less punishment and rejection than did subjects in the other groups.

DISCUSSION

The hypothesis that potential scientific creativity is associated with being first born has not been supported. The study does suggest that there may be an inhibiting influence on the younger son who is more isolated; yet, for a considerable range of family size, sibling sex, sibling separation and ordinal position combinations, any "favorable" effects of one ordinal position appear to be as susceptible to attenuation by other influences as any "unfavorable" effects of another ordinal position. Birth order, considered as simple ordinal position, does not appear to be a very powerful factor within the YS sample. It is possible that significant effects would appear if a wider range of ability were sampled, e.g., a range from students who fail science courses to STS winners. As noted previously, considerable caution would have to be exercised in regard to correlated factors such as socioeconomic status and scholastic ability, and it is also possible that birth order effects would not be found in such a sample.

These results may also offer inferential support for Schachter's conclusion that the birth order effect shown in earlier studies of eminent men is a methodological artifact. Such support would have to take into account two questions: the sensitivity of the criterion of potential scientific creativity and the relation of early recognition to a criterion based on evaluation of the individual's life work.

1. The potential scientific creativity ratings are not insensitive to other variables; the groups differ on a large number of personality and social history characteristics and a complex birth order effect was demonstrated. It seems likely that the failure to find a significant relation between simple ordinal position and early attainment indicates the weakness of the birth order effect when appropriate controls are used, rather than a Type II error. This interpretation would also be consistent with the variability of the recent studies of more and less creative adults.
2. The relation of early attainment to eminence is not known for the criterion

used. If, however, eminence is considered as being on a continuum, the relationships obtaining at the upper end of the continuum might also be expected to obtain among individuals of lesser absolute achievement when the ranges of achievement are in both instances known to be adequate for demonstrating relationships to variables other than birth order. Studies relating family structure and achievement during college and maturity would offer more conclusive support for the methodological artifact interpretation of the "eminent first born" phenomenon; such longitudinal studies are planned for this sample.

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Sibling Configurations of Scientists

S. Stewart West

For several decades it has been known that samples of gifted children[2] or eminent men[3] show frequencies of first-born persons larger than chance expectation from their distributions in number of siblings. However, little attention has been given to other orders of birth or to the shape of the distribution in number of siblings and its relation to expectation from the general population. At least one large sample of eminent scientists[4] has been described without computation of expected frequencies. Samples of psychotics[5] and of neurotic patients[6] are available for comparing failure in social adjustment with outstanding success.

This report describes the sibling configurations of a sample of persons engaged in research, the criterion of selection being occupation only, without regard to eminence. (The term "scientist" will be used to mean any person engaged in the production of new knowledge, either in basic research or in applied research or development, including research engineers.) The purpose of the report was to obtain clues to the relationship between early socialization in the parental family and choice of research as an occupation. The data were obtained as part of a larger investigation of research organizations by means of questionnaires distributed to the entire research personnel of each of five industrial laboratories and seven science departments of a large midwestern university (Table 1).

Comparison of sites in respect to distribution of respondents in birth order and to distribution in number of sibs by means of chi-square test showed no significant difference between the academic site (Site I) and any one of the industrial sites (II–VI). No two of the industrial sites differed significantly in distribution in size of family, although either II or IV differed from either III or VI as to distribution in birth order (at the .05 level of chi square). It was therefore not appropriate to treat the sites separately, and their distributions were combined. Table 2 shows the distribution of the total sample in sibship size and birth order; Table 3, the distribution in mother's birth cohort and father's occupational class according to the Kinsey scale.[7]

Expected frequencies of birth order were computed from the distribution in number of sibs by assuming an equiprobable distribution among the possible positions in a sibship of given size, according to the method of Greenwood and Yule.[8] Table 4A compares fractional deviations of observed frequencies from expected values so obtained (only children being excluded) for the distribution in Table 2 and for seven other distributions cited in the first paragraph of this paper. Three of the samples were not large enough to make their deviations significant by chi-square test, but their positions in the sequence of Table 4 may provide grounds for useful conjectures. The data from

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[2] L. M. Terman *et al.*, *Genetic Studies of Genius*, Vol. I: *The Mental and Physical Traits of a Thousand Gifted Children* (Stanford, Calif.: Stanford University Press, 1925).

[3] J. McK. Cattell and D. K. Brimhall, *American Men of Science* (3d ed.; Garrison, N.Y.: Science Press, 1921; H. Ellis, *A Study of British Genius* (Boston: Houghton Mifflin Co., 1926); Anne Roe, "A Psychological Study of Eminent Psychologists and Anthropologists, and a Comparison with Biological and Physical Scientists," *Psychological Monographs*, Vol. LXVII, No. 2, Whole No. 352 (1953). See also the summary and discussion by H. E. Jones in *Manual of Child Psychology*, ed. Leonard Carmichael (1st ed.; New York: John Wiley & Sons, 1946), pp. 607–8.

[4] S. S. Visher, *Scientists Starred 1903–1943 in "American Men of Science"* (Baltimore: Johns Hopkins Press, 1947), p. 537.

[5] Benjamin Malzberg, *Social and Biological Aspects of Mental Disease* (Utica, N.Y.: State Hospitals Press, 1940), pp. 265, 270.

[6] Alan Norton "Incidence of Neurosis Related to Maternal Age and Birth Order," *British Journal of Social Medicine*, VI (1952), 253–258.

[7] A. C. Kinsey, W. B. Pomeroy, and C. E. Martin, *Sexual Behavior in the Human Male* (Philadelphia: W. B. Saunders Co., 1948), pp. 77–79.

[8] M. Greenwood, Jr., and G. Udney Yule, "On the Determination of Size of Family and of the Distribution of Characters in Order of Birth from Samples Taken through Members of the Sibships," *Journal of the Royal Statistical Society*, LXXVII (1913–14), 179–97.

Table 1. Characteristics of Sites

	Site						
	I	*II*	*III*	*IV*	*V*	*VI*	*Total*
Type of research[*]	R	A, D	R, A, D	A, D	D	D	
Percentage of persons with doctorate	97.7	11.8	43.4	3.7	2.5	–	34.7
Total questionnaires distributed	238	60	218	213	129	70	928
Total questionnaires completed	178	54	212	189	118	62	813
In mathematics, physics, or chemistry	80	54	118	189	118	62	621
In biological science	38	–	94	–	–	–	132
In social science	60	–	–	–	–	–	60

[*] R = basic research; A = applied research; D = development.

Table 2. Order of Birth and Size of Sibship

Size of Sibship	*No. respondents in stated order of birth*							*Total*
	1	*2*	*3*	*4*	*5*	*6*	*7 or more*	
1	125	–	–	–	–	–	–	125
2	149	113	–	–	–	–	–	262
3	67	47	63	–	–	–	–	177
4	41	29	24	22	–	–	–	116
5	14	10	12	9	15	–	–	60
6	5	3	4	4	5	7	–	28
7	2	3	1	3	2	4	2	17
8	–	2	1	4	3	3	1	14
9	–	1	1	–	1	2	–	5
10 or more	–	1	2	2	–	1	3	9
Total	403	209	108	44	26	17	6	813
Expected	366.3	241.3	110.3	51.3	22.3	10.3	11.2	813

Table 3. Mother's Birth Cohort and Father's Occupational Class

Mother born	*No. respondents with father in stated Kinsey class*								*Total*
	8 or 9	*7*	*6*	*5*	*F*[*]	*4*	*3*	*2*	*Unknown*	
1916–20	–	–	–	–	–	2	–	–	–	2
1911–15	–	3	3	6	1	9	3	–	–	25
1906–10	–	11	15	26	5	21	13	1	2	94
1901–05	3	10	29	32	9	16	12	3	5	119
1896–1900	1	38	34	54	5	35	25	5	13	210
1891–95	4	15	24	28	6	13	12	2	5	109
1886–90	1	11	18	21	13	15	5	2	6	92
1881–85	–	10	13	19	6	8	4	1	7	68
1876–80	–	4	12	13	8	5	1	–	–	43
1871–75	–	3	5	11	4	1	–	–	1	25
1866–70	–	2	4	5	2	2	–	–	1	16
1846–65	–	2	1	2	2	3	–	–	–	10
Total	9	109	158	217	61	130	75	14	40	813

[*] Class F contains all farmers, normally included in classes 4 and 5.

Table 4. Comparison with Samples of Other Investigators

A. Deviations from Equiprobability of All Birth Orders (Only Children Excluded)

*Source**	*Population*	*Sample Size*	*No. children per mother*	*Percentage deviation from expected value for stated birth order* 1	2	3	*Level of significance*
Roe	Scientists, upper 1 per cent	64	2.09	54	−17	−70	.05
Cattell	Scientists, upper 25 per cent	855	3.21	40	−17	−20	.001
Visher	Scientists, upper 25 per cent	824	2.81	21	−20	−19	.001
West	Scientists, total	813	2.22	15	−13	−2	.05
Malzberg	Manic-depressives	498	4.05	16	−6	−1	n.s.
Terman	Gifted children	574	2.27	12	−7	−8	n.s.
Malzberg	Schizophrenics	549	3.54	−1	−1	5	n.s.
Norton	Neurotics	2,500	3.15	−8	−3	−5	.05

B. Deviations from Equiprobability of Second and Higher Birth Orders (Only Children Excluded)

*Source**	*Percentage deviation from expected value for stated birth order* 1	2	3	4	5	6
Cattell	66	−3	−6	−6	21	9
West	29	−3	6	−7	17	—

* For sources see nn. 2–6 in text.

Terman's sample of gifted children suggest, for example, that intelligence as measured by the Stanford-Binet scale is not a sufficient (or perhaps even necessary) condition for success in research, a conjecture supported by Roe's tests[9] of eminent physicists and biologists.

Frequency of first-born persons is enhanced and frequencies of second-born and third-born are depressed in each of the first six samples. Fractional deviations at higher birth orders were estimated by combining the four samples of scientists, which can overlap only slightly, the sum of deviations being divided by the sum of expected values at each birth order to obtain the data of Table 5. Deviations are positive for Orders 1, 5, and 6, and

Table 5. Deviations in Total of Four Samples of Scientists*

Birth order	*No. observed*	*No. expected*	*No.*	*Deviation percentage of expected no.*	*Percentage of sample*
1	1,134	956.0	178.0	18.6	7.0
2	573	688.0	−115.0	−16.7	−4.5
3	341	405.0	−64.0	−15.8	−2.5
4	203	230.6	−27.6	−12.0	−1.1
5	144	128.3	15.7	12.2	0.6
6	85	73.1	11.9	16.3	0.5
7	41	38.3	2.7	7.0	0.1
8 or more	35	36.7	−1.7	−4.5	−0.1

* $N = 2{,}556$.

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[9] Anne Roe, "A Psychological Study of Physical Scientists," *Genetic Psychology Monographs*, XLIII (1951), 231.

negative for Orders 2, 3, and 4. The absolute deviations at Orders 5 and 6 are small, but probably indicate real enhancements.

Alternatively, one may assume that the expected number of persons in each order of birth for sibships of size *N* is the average of the numbers in the second and higher orders, summing as before over all values of *N*. This is equivalent to an expectation that the first, and only the first, order will show a frequency different from chance. When equiprobability is thus assumed only for the second and higher orders, the Cattell and West samples show fractional deviations (Table 4B) from which one may infer that the fifth position is also assoiated with a larger than average probability of choosing research as an occupation.

Table 6 shows that probability of inclusion in the sample is not appreciably dependent on the sex distribution of an individual's siblings. The frequencies of the various proportions of male siblings are consistent with binomial distribution at equal probabilities for male and female. $P(\chi^2) = .85$ for one to five siblings, inclusive. Older siblings are likewise distributed randomly in sex, with $P(\chi^2) = .47$.

To explain the enhancement of frequency of first-born persons, the "isolation" hypothesis of Faris is still attractive. Faris points out that, in order to develop into a competent scientist, one must have an organized and consistent childhood, pursue knowledge rationally and without accepting customary formulas of behavior, and be confident of success where the greatest authorities have failed.[10] These things are presumably possible only as results of minimal interaction with siblings (as in the case of the first-born), with parents, and with peers.

If isolation from siblings is important after the first few years of life, the frequency of only children should be enhanced more than is that of first-born children of multiple sibships. One way of ascertaining whether such an enhancement does in fact occur is to determine the distribution function relating number of mothers to number of children in the completed family on the basis of the sample data for families of two or more children and, from this, to predict an expected number of one-child families which can be compared with the number in the sample. As a first approximation, we shall, neglect the small birth-order effect and assume that, except possibly for only children, the probability that a given child will become a scientist is independent both of birth order and of sibship size. When this probability is small, say of the order of .01, the distribution of mothers is closely approximated by dividing each frequency of sibship size in Table 2 by the number of members of the sibship.

No satisfactory mathematical model for such a distribution appears to have been developed by demographers. It is somewhat like a Poisson distribution, but frequency does not decrease rapidly enough with sibship size to make it Poissonian. Brass[11] proposes the truncated negative binomial as a distribution function, but its parameters are often indeterminate in practice, and it fits both our data and census data rather poorly. However, the cumulative lognormal distribution[12] is an excellent representation of observed cumulative distributions of mothers with respect to number of children in the completed family when class intervals are taken as bounded by half-integral values of number of children. This can readily be verified by plotting distributions from census data on logarithmic probability paper. What is needed here is a descriptive formula, and we shall not consider theoretical implications.

The computed distribution of mothers according to number of children is well fitted by a cumulative lognormal distribution with mean 0.63 and standard deviation 0.55 (in units of natural logarithm of number of children). Computing from the fitted distribution of mothers to the distribution of children on the assumption of small and constant probability for all children of all sibship sizes, one obtains the fitted distribution shown in Table 7, which is not significantly different from the observed distribution ($P[\chi^2] = .58$). The lognormal function, therefore, gives an excellent fit by Fisher's criterion. That it has also the same shape as distributions found in census data can be seen

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[10] R. E. L. Faris, "Sociological Causes of Genius," *American Sociological Review*, V (1940), 689–99.

[11] W. Brass, "The Distribution of Births in Human Populations," *Population Studies*, XII (1958), 51–72.

[12] For mathematical background see J. Aitchison and J. A. C. Brown, *The Lognormal Distribution* (Cambridge: Cambridge University Press, 1957).

Table 6. Sex of Siblings[*]

No. Siblings	*No. respondents with stated no. male siblings*							*Total*
	0	*1*	*2*	*3*	*4*	*5*	*6 or more*	
0	125	–	–	–	–	–	–	125
1	132	130	–	–	–	–	–	262
2	38	90	49	–	–	–	–	177
3	14	40	47	15	–	–	–	116
4	6	16	27	11	–	–	–	60
5	2	7	8	7	4	–	–	28
6 or more	–	4	8	10	8	8	7	45
Total	317	287	139	43	12	8	7	813

[*] Total male siblings = 828; total female siblings = 846.

Table 7. Number of Sibs

No. S of siblings	*No. respondents*			*Observed/ expected*	$2e-(S/4)^{3/2}$
	Observed	*By lognormal fit*	*Expected*		
0	125	125	64	1.95	2.00
1	262	262	138	1.90	1.78
2	177	189	129	1.37	1.40
3	116	107	107	1.08	1.05
4	60	57	80	0.75	0.74
5	28	29	63	0.44	0.49
6	17	15	54	0.31	0.32
7	14	9	51	0.27	0.20
8 or more	14	20	127	0.11	0.12
Total	813	813	813		

by comparison with the distribution of ever married white women who had completed high school and were forty-five to forty-nine years old in 1950, the two being almost identical. In this sense, the frequency of one-child sibships is consistent with the rest of the distribution.

From the bivariate distribution of respondents in mother's birth cohort and father's occupational class (Table 3) and tabulations of census data given by Grabill, Kiser, and Whelpton,[13] a sufficiently good approximation to the expected distribution of mothers can be constructed as follows: Mean number of children ever born per white woman of the United States, married once and husband present, is available for each of the several occupational classes from the censuses of 1910, 1940, and 1950. From these data, mean number of children among women with essentially completed families is known for each occupational class of each of six cohorts (five-year ranges of woman's year of birth). Values for other cohorts can be estimated by interpolation and extrapolation of curves of mean number of children as a function of mean year of cohort. . . .

It was then assumed that the distributions of mothers in total cohorts constituted a one-parameter group, definable by mean number of children only, and, further, that the distributions of subdivisions of cohorts by occupational class were members of the same group as those of total cohorts. For each number of children in the completed family, percentage of mothers having this number was plotted as a function of mean number of children per married woman. Corresponding to the value of mean

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[13] W. H. Grabill, C. V. Kiser, and P. K. Whelpton. *The Fertility of American Women* (New York: John Wiley & Sons, 1958), pp. 46, 132, 321.

number of children estimated as in the preceding paragraph for each cell of Table 3, values of percentage were obtained from these curves for each cell of that table. Averaging percentage of mothers in Table 3 for each number of children then yields an expected distribution of mothers, from which the expected distribution of respondents in Table 7 is obtained by multiplying each frequency of mothers by the corresponding number of children. This indirect procedure is necessitated by the nature of the available data, and any errors thus introduced will be those of smoothing only, being almost certainly small in comparison to sampling deviations in the data to which comparison is made.

The mean of this expected distribution is 3.14 children per mother, whereas the mean computed from the sample on the assumption of equiprobability is only 2.22 children per mother. The ratio of observed to expected frequency decreases monotonically with increasing number S of sibs and can be fairly well approximated by a function $2e^{-(S/4)3/2}$ (Table 7). That the same relation holds for all mothers' cohorts and for all fathers' occupational classes is indicated by Table 8, in which are listed mean numbers of children per mother estimated from the general population (*a*) on the assumption of small, constant probability, (*b*) on the assumption of an exponential decrease of probability as in Table 7, and (*c*) computed from the sample on the assumption of constant probability. (Constant probability here means probability of choosing research as an occupation, which is the same for all orders of birth and all sizes of sibship.) Even the larger differences between (*b*) and (*c*) are not statistically significant in terms of comparison between the observed distribution of respondents in sibship size and the expected distribution derived on the assumption of exponential decrease. The same probability function was used in all computations of type (*b*).

The preceding discussion suggests two alternative hypotheses: (1) that scientists are produced chiefly by mothers of a specific kind, who have a tendency to have fewer children

Table 8. Number of Children per Mother, by Mother's Birth Cohort and by Father's Class

		Mean no. children per mother From general population, assuming		
	No. respondents	*Equiprobability*	*Exponential decrease of probability*	*From sample, assuming equiprobability*
Mother born:				
1911–20	27	2.68	2.07	2.22
1906–10	94	2.73	2.09	2.12
1901–05	119	2.80	2.10	2.08
1896–1900	210	2.90	2.13	2.12
1891–95	109	3.10	2.20	2.35
1886–90	92	3.52	2.33	2.30
1881–85	68	3.53	2.34	2.35
1876–80	43	3.93	2.44	2.44
1846–75	51	4.30	2.59	2.72
Total	813	3.14	2.22	2.22
Father's class:				
7, 8, or 9	118	2.59	2.04	2.03
6	158	2.88	2.13	2.12
5	217	2.64	2.06	2.13
Farmers	61	5.34	2.92	3.13
4	130	3.46	2.32	2.33
3	75	3.62	2.38	2.31
2	14	3.82	2.43	2.18
Total	773			

than normal, or (2) that interaction among siblings inhibits the development of the scientific personality. An exponential decrease of probability with sibship size could as readily result from the first situation as from the second. If the sample has been derived by equiprobable selection, the distribution of mothers is as if the probability of bearing the $(N + 1)$st child, having had the Nth, were about 70 per cent of the corresponding probability in the general population, for $N \geqq 2$.

Recent data of Schachter do not seem to accord with hypothesis (2), in that, in addition to indicating that greater anxiety is aroused in first-born children than in those later born by a given situation, the data show that need for affiliation under stress is stronger in the first-born than in the later born and decreases with increasing sibship size.[14] The first of these findings is compatible with Roe's observation of a high level of free anxiety in eminent physicists and biologists, but the second conflicts with their relative lack of social interests. However, although Schachter's subjects were female and most scientists are male, the discrepancy seems more likely to be due to alternative means for reducing anxiety than to sex.

If some degree of early isolation is a prerequisite for producing a scientist, then the probability that a given child will become a scientist should vary inversely with sibship size, but so many circumstances affect the interaction of siblings that theoretical construction of the discrete probability function of which the exponential form is an approximate envelope becomes very difficult. Extrinsic variables, such as availability of money for education or current attitude about scientists, appear to be unimportant since the same function applies to all classes and to all cohorts within the limits of sampling variation. They may, however, enter into the selection by birth order, since the oldest child will tend to have first call on limited resources for education. In addition the effectiveness of interaction between siblings is likely to depend on differences in their ages. Interaction may focus on some sibs and leave others relatively unaffected, as Bales, Strodtbeck, Mills, and Roseborough found in small groups.[15] Somewhat more can still be done with demographic variables, but, for the most part, investigation must proceed in the area of the dynamics of family life.

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[14] Stanley Schachter, *The Psychology of Affiliation* (Stanford, Calif.: Stanford University Press, 1959).

[15] R. F. Bales, F. L. Strodtbeck, T. M. Mills, and M. E. Roseborough, "Channels of Communication in Small Groups," *American Sociological Review* XVI (1951), 461–68.

Biographical Predictors of Scientific Performance

Calvin W. Taylor and Robert L. Ellison

This paper will present a summary of research on the use of biographical information to predict various criterion measures of successful performance and accomplishments in science (*1*). In our studies of the relationship of biographical information to success in science, over 2,000 scientists have filled out one of our 300-item multiple choice questionnaires. The majority of this work has been conducted in conjunction with (2) the National Aeronautics and Space Administration (NASA).

The term "biographical information" is open to some possible misinterpretation when applied to the measuring instrument, the Biographical Inventory, (hereafter called the BI), which has been used in these studies. The BI contains a wide variety of questions about childhood activities, experiences, sources of derived satisfactions and dissatisfactions, descriptions of the subject's parents, academic experiences, attitudes and interests, value preferences, and self-descriptions and evaluations. The items thus encompass a wide variety of information and are not limited to a narrow definition of what could be included within the rubric of biographical information. By using such a broad approach, one potentially can attempt to measure not only previous life history experiences and past environmental ef-

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fects on a person, but also to assess the outcome or manifestation of the hereditary environment combination as it is personified in the individuals studied.

The intent in these studies was to exploit the biographical approach and thus determine and more fully understand the experiences, backgrounds, opinions, self-images, and attitudes which would aid in differentiating the highly productive and creative scientists from those who were less productive and creative. When the biographical characteristics, experiences, and self-descriptions were identified, the practical goal was to utilize these characteristics in developing an easily administered and scored biographical inventory which would aid in the identification of scientific talent at the college level. Hopefully, the inventory could be rewritten for the early high school level and used as a vocational guidance instrument, so that high school students who had scientific potential could be encouraged to further their development.

When this study was initiated in 1959, biographical information was considered to be one of the most promising means of identifying creative scientific talent. Previous research from a variety of investigators had indicated that biographical information was a potentially promising technique for the identification of creative scientific talent, although no one had made a definitive attempt to exploit this potential (*3, 4*). The approach had, however, demonstrated its usefulness in a variety of other settings for predictive purposes; for example, identifying successful salesmen, predicting college success, identifying leadership ability in the Army after World War II, and others.

Two studies were especially useful in laying the foundation for the later use of the biographical approach in the studies of NASA scientists. These were by Ellison (*5*), who tried out a large number of biographical items, and by Taylor, Smith, Ghiselin, and Ellison (*6*), who conducted an intensive citerion study and later administered a series of predictors including a biographical inventory. In both studies the initial validities found between the empirically keyed biographical scores and the corresponding criterion were extremely high, (.70 to .94). No cross validation was attempted in either of these two preliminary studies because of the relatively small sample size but the best items from both studies were identified and retained for future use in the NASA project. However, a priori scoring keys for the biographical responses worked very well on other criteria with statistically significant results. For example, the same score predicted the patents criterion with a biserial correlation of .35 in one of the subsamples in which only 15 percent of the sample had one or more patents. Despite the complexity of the criterion problem in measuring success in science, this result indicates that there is some common ground among various criterion measures and that a biographical score, even when constructed at a different research center, can overlap a significant portion of that common ground.

The same procedure was followed to further analyze the data. Briefly the total sample of 300 scientists was split into two subsamples of 148 and 152, and an item alternative analysis was carried out for each sample in a double cross-validation design. The results from the item analysis at the second center were generally not quite as high as those obtained at the first center, although a cross validity coefficient of .60 was obtained in predicting publications. The average cross validity coefficient for predicting the creativity criterion on the two subsamples at the second research center was .48. A comparison of this correlation of .48 with the correlation of .47 obtained by using the keys from the form A study indicates a high degree of stability in the biographical keys. This result also indicated that it would probably not be necessary to construct separate keys for each NASA research installation. Moreover, it provided some evidence that the same biographical key would give generally the same results in predicting creativity in different fields of specialization.

The official rating scores which were already available at the research center were, without exception, not as predictable as the other criterion measures, evidently because of the construction of the rating forms and the manner in which these ratings were obtained. In addition, in the inventory, there was undoubtedly some implicit item selection by the investigators to emphasize the prediction of the creativity criterion in comparison with other criteria. However, most of these other criteria were predicted with cross validity coefficients that were statistically significant. The item analysis revealed that items involving professional self confidence were most valid, followed by items measuring independence and autonomy.

THE FORM C STUDY OF THE BI

A form C of the BI was constructed in which the best items from the previous studies were used together with additional new items. This form was administered to approximately 800 scientists at a third NASA research center. In contrast to the other two centers visited, there was no existing rating procedure for the evaluation of the scientific personnel. Promotions were handled by letters of recommendations and personal conferences. Thus, the criterion measures collected at this center may have been influenced by this comparative lack of rating experience. The only measures collected besides data on publications, patents, and Government Service level were scale ratings of the following: quantity of work, skill in getting along with people, creativity, and an overall evaluation.

The procedures followed in the third study were the same as in the previous studies. After the total sample was divided into various organizational subsamples, average cross validity coefficients for the creativity criterion ranged from .41 on the total sample to .49 for one organizational subgroup. The other rating criteria were predicted at a somewhat lower level in the mid 30's. The average cross validity coefficients for the publications criterion was .62, and for the GS level criterion the average cross validity was .70.

The keys that were developed on the basis of the form A study yielded an average cross validity of .40 in predicting the creativity criterion, again indicating a high degree of stability for the biographical scores.

Some of the characteristics of creative people revealed by the BI studies provide a brief portrait of the creative scientist. Consistently the best subscore in the inventory for identifying creative scientific talent was "Professional Self Confidence." The scientists who scored high have confidence in themselves to perform at a very high level. Although not necessarily so, they are often confident of themselves in other spheres of activity. They are also very independent, which is a trait found to be relevant in almost every study of creativity. They tend to use themselves as a focal point for evaluation and are not swayed by the general consensus. They are also, as one might expect, intellectually oriented, a trait that developed relatively early in adolescence. The more successful scientists also have a high degree of dedication to their work, often to the exclusion of other hobbies, interests, and even family activities. They set very high levels of aspiration for themselves which they expect to achieve in the future.

Table 1 presents five of the biographical items and shows both the percentage responding and the correlations with the supervisors' ratings of creativity. The first two items provide information about the self-concept of the scientists as they described their own ability to do research and their reaction to a situation in which they responded whether or not they would publish their research results if such publication interfered with the desires of their supervisors. It will be noticed that those who said they would publish were rated slightly higher on creativity and that 75 percent of the sample said they would cooperate with the supervisor.

The other three items provide information about self-reported academic achievements and the age at graduation from high school. It will be noted that the relationships of these latter three items to the creativity criterion are relatively low. These findings correspond generally with those presented by Wolfle and by Hoyt (9). In our view, heavy emphasis should not be placed on academic achievement unless the particular organization in question has determined that academic achievement does have a demonstrated relationship to the work being accomplished. The last item is of some interest as those scientists who graduated from high school at an earlier age tended to be rated as more creative than those who graduated later, evidently indicating early intellectual achievement is a positive indicator of later performance in research.

FOLLOW-UP STUDIES OF THE BI

All previous studies of the BI were concurrent studies as opposed to follow-up studies

Table I. Examples of biographical items, the percentage responding, and biserial correlations of the item alternatives, with supervisors' ratings of creativity (sample size, 1000).

Biographical items	*Percentage responding*	*Correlation with creativity criterion*
1) What do you consider to be your capacity or ability to succeed in research?		
A. Superior	10	.38*
B. Above average	44	.19*
C. About average	42	−.29*
D. Slightly below average	3	−.25*
E. Does not apply	1	.00
2) Assume you are in a situation in which the following two alternate courses of action arise. Which *one* of the two would you be most likely to do?		
A. Finish my research through the stage of publishing it.	25	.18*
B. Cooperate with my supervisor by doing what he wanted me to do next.	75	−.18*
3) About what percentage of the students in your class did you surpass academically when you graduated from high school?		
A. 99%	12	.17*
B. 90%	35	.06
C. 80%	28	−.05
D. 60%	17	−.12*
E. 50% or less	8	.05
4) About what percentage of students in your class did you surpass academically when you graduated from college?		
A. 99%	5	.26*
B. 90%	27	.21*
C. 60%	37	−.10†
D. 40%	7	.02
E. Don't know	24	−.19*
5) How old were you when you graduated from high school?		
A. 15 or younger	2	.21†
B. 16	14	.15*
C. 17	48	.04
D. 18	31	−.10†
E. 19 or older	5	−.20*

* Significant at the .01 level.
† Significant at the .05 level.

where the accuracy of the BI predictions could be checked for validity over an extended period. After the completion of the form C study, a series of follow-up studies were initiated using form C-1 of the BI. This form was identical to form C except that the instructions used were modified slightly to make the administration procedure of the inventory more comparable to the actual hiring situation. This form was administered to 622 scientists as they reported for work at several NASA centers. About a year later criterion data were obtained on the performance of the newly hired research professionals.

Because of the widely dispersed geographical locations of the NASA centers, a psychologist did not assist in the collection of data. This may have affected the criterion data, since the study procedures and rationale could not be explained as thoroughly as in previous studies. But the data analyses were similar. The keys developed on the basis of previous research yielded cross validities ranging from .05 to .44, depending on the participating research center. When an item analysis was conducted on the data, it was found that only a small improvement occurred in the magnitude of the validities. In other words, this result indicates that the keys developed on the concurrent studies were almost as effective as the specially developed keys in the follow-up studies.

While these results were not of the same order of magnitude as those obtained in the concurrent studies, the investigators consider the limitations inherent in the criterion data to be responsible. This in turn has at least three facets: many of the criterion ratings appeared to be affected by a likeability factor; the scientists had been working on the job for a limited period of time so that an accurate assessment of their performance may have been difficult; and finally, the inventories and the criterion data were collected by mail, which may have reduced their accuracy.

Further research will be needed to resolve this question. The results that have been presented previously as well as those presented in this report indicate that with better criterion data more satisfactory results would have been obtained.

THE EARLY IDENTIFICATION OF TALENT

As mentioned previously, a long-term goal of this research has been to develop an instrument that was both appropriate and valid for younger age groups. Some research has already been carried out on this question (*10*). The National Science Foundation Summer Science Program for high school and college students provides a unique opportunity for research. In some of the programs the students actually participate full time in research activities and thus some relevant criterion measures could be obtained. In these studies the BI was modified for younger age groups. Since some items had to be rewritten and the scoring was based upon data from mature scientists, we expected the revised BI not to work very well, if at all, under the circumstances. A further problem was that we now sought predictive (short range follow-up) validities rather than concurrent validities. The results indicated that the BI was among the best predictors of creativity in research and could be applied to markedly different age groups.

We thought that the initial BI constructed for administration to NASA scientists would probably be more appropriate for college seniors than for high school students, since college seniors more closely resemble the adult samples on which the BI was developed. Some data have been obtained on this latter issue already, although the criteria were not as directly pertinent as those used in the study of high school students (*11*). These studies evaluated fellowship selection information at the University of Utah, where the research committee awards approximately 40 graduate fellowships per year in all fields. Selection has been based largely on grade point average and letters of recommendation. For research purposes, a modified biographical inventory was administered to a number of seniors and graduate students who applied for fellowships. A year later their graduate student performances, including their research potential, were rated. Again the BI scores proved to be among the most valid predictors of these criteria. In one case, a BI score by itself predicted the next year's performance of the graduate students more effectively than did the collective judgments of the official fellowship committee which based its decisions on a full folder of application materials from each fellowship candidate.

These studies on student groups demonstrate the potential contribution of biographical information to the early identification of creative scientific talent.

PRODUCTIVITY OR CREATIVITY OR BOTH?

Among the many unanswered questions in creativity research is the question of whether the same factors are involved in predicting at different ages and at different levels of creative performance. Our biographical research generally indicates that the same items that predict creativity in adult scientists predict creativity in high school students. In addition, the same characteristics of successful scientists obtained from biographical studies of NASA research professionals correspond to the findings of those studies that have included or even focused solely upon highly eminent scientists (*3, 12*). Thus, it is apparent that many of the same characteristics are involved in predicting different levels of creative performance. This problem could be approached directly by processing data from the NASA studies, first by subdividing the samples according to creative performance or level of education and then analyzing the subsamples separately. However, in our earlier studies of Air Force scientists (*6*), the number of years of education was unrelated to 12 of the 14 criterion factors, and to none of the creativity criteria. It was only related to the number of professional societies to which a person belonged and whether he was efficient in completing his paper work (a factor called "productivity in written work").

A related question concerns how well do biographical characteristics predict productivity in science as opposed to creativity. Although some biographical items have differential validities in predicting productivity and creativity, the majority of the items are related to both criteria. These findings agree with the rational point of view presented by Bloom (*12*) and others, that without a certain minimum amount of productivity, for example, publications and quantity of work completed, there is a low probability of creative achievement. We have been somewhat more successful in predicting supervisors' evaluations of creativity than of productivity, but as mentioned previously, the major focus of these studies has been upon creativity criteria, so that biographical items were formulated with creativity more than productivity in mind. It is evident that considerable additional research needs to be conducted before the above questions can be adequately answered.

Because of the consistently promising and positive results in studies where biographical information has been used, there has been an increasing use of psychological tests of this type. One outcome has been the initiation of a research project in a large pharmaceutical company (Richardson-Merrell, Inc.) which was based upon the previous NASA effort. In this study (*13*) a biographical inventory (constructed by Taylor and Ellison for research use in industry) was administered to a large number of scientists and information was obtained on their creativity and general scientific competence. Each participating scientist was rated by his supervisor, his peers, and, in some cases, his subordinates. Results of this study showed that the empirically constructed keys yielded cross validity predictions of the criteria in the .30's and .40's. Also, since the biographical items used in this study contained many of the same items that were used in the NASA studies, it was possible to score the test protocols of the pharmaceutical scientists with the NASA-derived keys. The results were significant and have important implications. It was found that a combination of the NASA creativity keys, when applied to the biographical information responses of the pharmaceutical scientists, yielded validities with the creativity criterion in the high .30's.

These findings indicate that the results of the NASA studies may be applied not only to different age groups but also to diverse samples of scientists (in this case, the physical and biological sciences and both a government agency and private industry). This study, therefore, suggests that biographical information may be employed to identify scientific talent found in a variety of environments.

Chambers (*14*) studied the personality and biographical factors of mature scientists who are highly creative in research work and those of scientists who are much less creative. In addition to significant differences between creative scientists and their comparative control groups on several personality variables, he also found significant differences for 16 biographical items. He was thus able to present a biographical and personality profile of those highly creative scientists and those not so creative.

W. A. Owens and his associates (*15*) have made several studies of engineers and scientists relating biographical data to creativity, professional interests, and research competence. In another study, Albright and Glennon (*16*) found that biographical information could discriminate between supervisory and research-oriented scientists at all levels of a laboratory organization. Also, Smith, Albright, and Glennon (*17*) demonstrated the value of the personal history technique in the prediction of scientific competence and creativity within a highly select group of research scientists.

In a recent study by McDermid (*18*) of the technical and engineering personnel of the Hammond Organ Company, it was found that only biographical data proved to be significant as predictors of both supervisory and peer ratings of creative performance.

The June 1965 national research conference on the use of biographical information, chaired by E. R. Henry (*19*) and supported by the Richardson Foundation, produced a consensus of the 16 participants that in professional and other complex fields, the biographical approach is at least as good and is usually better than other techniques for predicting job performance.

SUMMARY

The biographical approach to the identification of scientific talent has shown significant results in a variety of situations which included different laboratories, fields of specialization, and age groups. Much remains to be accomplished, however. The biographical approach needs to be validated in other organizational settings employing relevant criteria. Although this kind of research is being initiated, a number of studies are needed to define the advantages and limitations. The use of biographical information to identify the creative and other talents of executives, composers, administrators, and artists has been largely unexplored. Furthermore, the meaning of the biographical items has not been correlated with existing psychological theory and knowledge. All evidence to date indicates that the investigation of biographical information and its relationship to various criteria of performance and other psychological measures is a rapidly expanding area of investigation which will make further contributions to the identification of talent in a variety of fields.

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Scientists and Nonscientists in a Group of 800 Gifted Men

Lewis M. Terman

SOURCES OF DATA USED

The subjects in this study were the approximately 800 male members of a gifted group who were selected in childhood on the basis of an intelligence test and whose careers have been followed for 30 years. As children all had made intelligence scores that rated them in the top 1 per cent for their respective ages. Four field studies of the group have been made: (*a*) at the time most of them were selected in 1921–22; (*b*) in 1927–28; (*c*) in 1939–40; and (*d*) in 1950–51 (2, 7, 8). On each of these occasions field assistants administered a variety of tests in addition to tests of intelligence, and collected extensive case history information from the subjects and from their parents and teachers. Apart from the field studies the group has been followed up from time to time by sending a General Information Blank to be filled out by each subject and a Home Information Blank to be filled out by a parent or other relative. By these means almost continuous contact has been maintained with all but a few of the subjects, including slightly more than 800 males and 600 females. Of the entire group (both sexes) only some 30 subjects have been lost track of. The 800 males here reported upon do not include any who were deceased before 1940.

As this group is the only one of its kind that has been studied so intensively over so long a period of time (and at a total cost of a quarter of a million dollars), it seemed desirable to sift all the data collected to 1951 in order to find what items of information might differentiate between those who became scientists and those who did not, and between various subgroups of the scientists. This search has been limited to males because only a small number of the gifted women became scientists.

CLASSIFICATION OF SUBJECTS

In the autumn of 1951 the educational and vocational records of all the male subjects were examined as a basis for classifying them into the following groups for comparative study.

> 1. *Physical Science Research* ($N = 51$). This group, designated PSR, is composed of men who were doing or had done basic research in any field of physical science. It includes 18 engineers, 17 chemists, 9 physicists, 3 geologists, 2 astronomers, and 1 each in mathematics and oceanography. Thirty-two of the group had taken the degree of Ph.D. or Sc.D. . . .
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> 2. *Engineers* ($N = 104$). The E group as now constituted includes 27 men who are doing or have done applied research and 77 who are practicing engineers with no research record. Fields of engineering most frequently represented are mechanical, 25; civil, 24; chemical, 19; electrical, 14; architectural, 7; aeronautical, 5; six other fields, 10. . . .
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> 3. *Medical-Biological* ($N = 61$). The M-B group includes 26 who have done medical or biological research and 35 doctors of medicine who are engaged chiefly in medical practice. Fields of research represented by the research group include anatomy, biochemistry, fisheries, general biology, criminology, microbiology, Oriental diseases, pharmacology, physiology, psychiatry, public health, and surgery. The 35 practitioners include (according to their own designations) 12 general practitioners, 9 surgeons, 3 internists, 2 ophthalmologists, 2 psychiatrists, 2 radiologists, an anesthetist, a gynecologist, a pediatrician, a laboratory director, and a medical director of a state-wide medical group-insurance serv-

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ice. Degrees held by the total M-B group of 61 are as follows: M.D., 48; Ph.D., 11; M.A. plus three years, 1; A.B. plus three years, 1. . . .

4. *Physical or Biological Science, Nonresearch* ($N = 68$). This group, designated PBS, is composed of men who as undergraduates majored in a physical or biological science but who later in many cases turned to other fields of work. Their undergraduate majors classify as follows: chemistry, 15; engineering, 14; mathematics, 10; physics, 6; forestry, 5; and 1 to 3 each in astronomy, botany, dentistry, entomology, pharmacy, premedicine, and zoology. All are college graduates except 8 who had three or more years of college but took no degree. About half of the group had one to three years of graduate work. Four graduated from Annapolis and 2 from West Point. Eleven of the group are teachers of science (5 in high schools, 4 in junior colleges, and 2 in colleges). Some 30 of the others are or have been engaged in work more or less related to their science major; among these are a few who have at one time or another been employed as engineers or chemists and several who have become sales managers, superintendents, or manufacturers. However, some of the PBS group have entered vocations that make little or no use of their science training. . . .

5. *Social Scientists* ($N = 149$). The SS group is composed of men who majored in one of the social sciences but have not, to any appreciable extent, engaged in research. The most frequent undergraduate majors were economics and political science, with smaller numbers in business, history, psychology, and anthropology. Fifteen of the SS group left college without taking a degree. A majority of those who graduated had one to three years of graduate work. The greatest number of graduate degrees were in business administration, with smaller numbers in economics, history, political science, psychology, education, and journalism. The group includes some 110 whose postcollege careers have been confined chiefly to business. . . .

6. *Lawyers* ($N = 83$). A large majority of the L group are engaged in general practice, though several have specialized in such fields as corporation law, motion picture law. petroleum law, taxation, etc. . . . Although most of the L group rate high in professional sucess, 4 or 5 are not engaged in any kind of legal work.

7. *The Humanities Group* ($N = 95$). The H group is composed of men who as under graduates majored in a field of the humanities. The majors most frequent were English, 30; philosophy, 15; languages, 14; art, 11; and music, 7. The remaining 18 majors were scattered among five different fields, including architecture, education, prelaw, journalism, and theology. Twenty of the group left college after completing the junior year. Of the 75 who graduated, 12 continued to the Ph.D. (or Th.D.), 20 to the M.A., and 5 took other professional degrees. Fourteen others completed from one to three years of graduate work without taking a graduate degree. . . .

The noncollege group (hereinafter designated as NC) is composed of 177 men who did not attend college or attended less than three years. Slightly more than a third of the group (69) discontinued their schooling with high school or had less than a year of additional business or technical training; 67 had one year of college and/or additional specialized professional or technical training, and 41 had 2 years of college. Some of the latter group also had further training along special lines. By recent vocation they classify as follows: in business occupations, 50. . . . Thirty are in skilled trades or retail business and 16 in clerical or sales work. Ten are in the entertainment field, 6 are officers in police or fire department, 4 are farmers, 4 are authors or journalists, and 4 are Army or Navy officers. . . .

SUMMARY OF RESULTS

In all, some 500 items of information obtained for male subjects were studied for group differences. In bringing together the outstanding difference trends among our compared groups, especially differences between the science groups (PSR, E, and M-B) on the one hand and the nonscience groups (SS, L, and H) on the other, . . . results show, . . . that on a large proportion of the variables the first three groups contrast markedly with the last three.[1] Percentages that tend to run high

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[1] One serious limitation of this research is the heterogeneity of some of the individual groups, particularly groups PBS, SS, and H. The most homogeneous with respect to kind of work its members are doing is group L, though here too there are differences in years of schooling, and in degree and kind of specialization. Group PSR is homogeneous in the sense that all its members have done basic research in physical science or engineering, but they are scattered among a variety of fields. Although the members of group E are engaged in several different kinds of

for the former are usually low for the latter, and vice versa; this despite the fact that there are some significant differences within each trio of groups. The PBS men, who as undergraduates were science majors but in a majority of cases later shifted to other fields, often show up in the graphs somewhere between the first three groups and the last three.

For purpose of summary the differences fall roughly into five categories relating respectively to (*a*) family background, (*b*) abilities evidenced, (*c*) vocational interest scores, (*d*) social adjustment, and (*e*) occupational success and life satisfactions. For the more important variables under each of these categories the groups ranking very high and those ranking very low (in percentage frequencies) will be indicated. As a rule only the differences that are statistically significant will be cited.

Family Background

Eight variables in this category that yielded significant differences will be noted. For convenience in referring to them they are here listed and numbered.

1. Both parents were native-born.
2. The father's education included college graduation or more.
3. The father as of 1928 was in the professional class.
4. The father's areas of interests included two or more.
5. The mother's areas of interests included two or more.
6. The father has held (as of 1922) one or more positions of honor or trust.
7. The father has held (as of 1922) a religious position of honor or trust.
8. The subject (as of 1940) had two or more sibs, living or dead.

Group PSR is highest of all the groups on variables 1, 2, and 3, and is lowest on 8. Group E is lowest on variables 2 and 5, and high on none. Group M-B is highest on variable 6, is tied for highest on variable 8, and is close to highest on 2; it is the lowest of all the groups, however, on variable 4. Group PBS is highest on 4, lowest on 6 and 7, and second lowest on 3. Group SS is highest on 5, close to highest on 4, and lowest on 3. Group L is highest on 8 and close to highest on 2 and 4. Group *H* is highest on 7 and tied for highest on 4.

On the whole, it appears that the groups with the most favorable background are M-B and PSR, that those with the least favorable background are E and PBS, and that groups SS, L, and H hold an intermediate position.

Abilities Evidenced

Three special abilities described by the parent as superior in 1922 yielded highly reliable group differences. Highest on "mechanical ability" was group E with a frequency roughly twice that of SS, L, or H. Nearly tied for high on "nature study and science" were groups PSR and M-B, with frequencies nearly twice that of SS. On "dramatic ability" the situation was reversed, with groups H and L highest and groups PSR and E lowest.

On the number of parents who named some field of science as a suitable occupation for the child, groups PSR, E, and PBS were almost tied for top place with frequencies several times that of SS, L, or H. On teachers who suggested a science as a suitable occupation PSR was highest and M-B next highest, with lowest positions again occupied by SS, L, and H. On the child's preference for a science as an occupation, groups PSR, E, and M-B all had frequencies two or more times as high as SS, L, or H. On the child's specific choice of engineering as the preferred occupation, group E had a frequency over four times that of SS, L, or H.

Two ability tests have yielded significant differences. On a test of information in language and literature given in 1922, high scores were about three times as frequent in groups M-B and L as in group PSR. On the Concept Mastery test given in 1940 four groups (PSR, M-B, L, and H) were almost tied for top place, and groups PBS and SS were tied for low place.

The top groups on scholastic record, both in high school and college, were PSR and M-B; lowest in high school was SS and lowest in college were PBS and SS. On graduation from college before age 22, groups PSR, M-B, and L

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engineering, there is reason to believe that in psychological characteristics the group is more homogeneous than some of the other groups. The same is probably true of the M-B group. In contrast, each of the PBS, SS, and H groups represents a wide range of occupations engaged in.

were tied for high place and groups E and PBS for low place.

Two fields on which subjects rated their interests in 1940 were science and mechanics. On interest in science the highest groups were PSR, E, and M-B, and the lowest SS and L. On interest in mechanics the high groups were E and PSR and the lowest SS, L, and H.

The above lines of evidence are well nigh unanimous in showing that early ability or interest in science is far more common among children who later become physical scientists, engineers, or biologists than among those who enter nonscientific fields. This has long been recognized but has not yet received the attention it deserves in educational and vocational guidance.

Vocational Interest Scores

The Strong blanks were scored for six kinds of scientists: chemist, engineer, psychologist, physician, architect, and math-science teacher.[3-6] On every one of these occupations the PSR group was either highest or second highest in frequency of superior scores, group E was highest or second highest on three, and group M-B was highest or second highest on two. At the opposite extreme group SS ranked lowest or second lowest on all six, and group L was lowest or second lowest on five. The only one of the nonscience groups that rated high on any of these six occupations was group H, which scored second highest for architect.

Consider next the eight occupations of lawyer, author-journalist, artist, musician, minister, YMCA secretary, social-science teacher, and school superintendent. Here the situation was reversed, with the science groups ranking usually among the lowest. Group E was at the bottom in seven of the eight, and PSR near the bottom in two and high in none. Group M-B ranked second highest for two of these occupations (artist and minister) but neither very high nor very low in the others. At the other extreme, the highest or second highest rank was held by group H on six of these occupations, by group L on four, and by SS on two.

Then follow the data for eight business occupations, and on nearly all of these the three science groups ranked at or near the bottom. Exceptions were two high ranks for group E on purchasing agent and production manager. Group SS ranked highest on six of the eight business occupations and second highest on another; group L was highest on one and second highest on three. Group H, which most often ranked highest in the professional occupations discussed in the preceding paragraph, was among the lowest of all the groups on four business occupations and high on none.

Finally, on masculinity of interests the science groups ranked high and the nonscience groups low. On proportion scoring A in their own occupation, groups PSR and E were highest, and PBS lowest. On proportion who scored B+ or A on seven or more occupations, groups PSR and M-B were tied for top place and groups E and PBS were lowest.

In general the results of the vocational interest tests are clearly in line with the evidence offered in the preceding section of this summary.

Social Adjustment

There were some 18 variables that yielded significant group differences on matters related directly or indirectly to social adjustment. On a majority of these the nonscience groups made a better showing than the science groups. Most consistently high were groups L and SS; most consistently low were groups PSR and E. Data obtained in 1922 on four variables in this category put group L at the top on every one; these include sociality as rated by interest in plays and games, average of composite parent-teacher ratings on five social traits, and similar parent-teacher ratings on four moral and four volitional traits. The lowest or second lowest rank on all of these four variables is held by group E, and on two of them by PSR. Only one science group ranked as high as second from the top on any of the four; this was group M-B as rated by parents and teachers on social traits.

Data on social adjustment obtained in 1940 showed a similar picture. Participation in high school activities was greatest in groups L and H, and lowest in groups E and PSR. The self-rating on interest in politics showed group L highest and group E lowest. The self-rating on interest in social life showed groups SS and M-B almost equally high and group E again lowest. Also throwing light on social traits are the vocational interest scores on such "uplift" occupations as those of minister, YMCA secretary, social-science teacher, school superintendent, and life insurance salesman. On all but one of these occupations both the highest and next to highest ranks were held by groups SS, L, and H. The exception was group M-B which ranked second highest on score for minister. Group E was lowest or second lowest on all five of these occupations, and group PSR was lowest or second lowest on two of them.

The biographical blank filled out by subjects in 1951 furnished data for ten variables that yielded significant group differences on matters related to social adjustment. These include eight ratings on the following: interest in social success at ages 12–20, interest in outdoor sports at ages 12–20, social adjustment in childhood and youth, extent to which S felt "different" from other children, admiration for mother in childhood and youth, extent of affection and understanding between son and his father, extent to which S has suffered from inferiority feelings, and degree to which S has tended to conform to authority or convention (as contrasted with tendency to rebel). Two other variables were based on the proportion of Ss who mentioned certain factors as having contributed to life accomplishment, the two factors that yielded significant group differences being "good social adjustment" and "good personality."

Here again the science groups tend to rate much lower than the nonscience groups. Group PSR was lowest of all the groups on six of the ten variables and second lowest on another. Group E was lowest or close to lowest on three and group M-B on four. The only high rank of a science group on any of these ten variables was that of group E, which was second on freedom from inferiority feelings. At the other extreme, group SS was highest for social adjustment on five of the ten variables and second highest on another. Group H was highest or second highest on four and group L on one. In the nonscience groups SS, L, and H, there was only a single variable on which one of these groups rated at the bottom for social adjustment; that was on feeling "different" from other children, most often reported by group H.

Occupational Success and Life Satisfactions

There are eight variables worth noting in this category that yielded significant group differences. These concern the proportion of subjects who—

1. Reported that occupation was definitely chosen, not drifted into
2. Began first consideration of their life work before age 16
3. Chose the occupation that was first seriously considered
4. Reported an earned income of $10,000 or more in 1949
5. Checked their "work itself" as an aspect of life giving greatest satisfaction
6. Checked their "income" as an aspect of life giving greatest satisfaction
7. Reported that life offers satisfactory outlets for their mental capabilities
8. Double-checked "adequate education" as a factor in life success

Highest place or close to highest went to group PSR on variables 2, 3, 5, 7, and 8; to group E on 2 and 3; and to group M-B on all except number 3. In the nonscience group L was highest or second highest on 1, 4, and 7. Group SS was lowest on 1, 2, 3, and 5; group H was lowest on 4 and 6, and second lowest on 2; group PBS was lowest on 4, 7, and 8. . . .

The present study of scientific achievement in our group of 800 men, despite its many inadequacies,[2] has yielded valuable clues to many characteristic differences between scientists and nonscientists, and also to differences between groups of scientists. Especially significant for the purpose of counseling and guidance are the differences observable in childhood behavior, interests, and preoccupations that are found many years later to discriminate between scientists and nonscientists. The results we have obtained by Strong's vocational interest test argue strongly for the value of this test in vocational guidance, especially if later research confirms Strong's data on the relative permanence of an individual's interest patterns after the age of high

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[2] EDITORS' NOTE: This study of gifted subjects undertaken in 1921 was not specifically designed to throw light on variables differentiating between scientists and nonscientists. Its purpose was broader and more general, namely, identifying characteristics of children rating in the top 1% for their respective ages in "general" intelligence.

school graduation. The group differences we have found on this test, given at the average age of 30 years, are all the more significant in view of the fact that very few of our men had been exposed to vocational counseling either in high school or college.

It is disappointing, however, that so many of the variables provided by the blank on Supplementary Biographical Data failed to yield significant group differences. It had been hoped that the ratings on parents and on parent-child relationships, together with other questions in the blank calling for information on factors influencing life achievement, might throw much-needed light on motivational factors. A few of these items proved to be discriminating but not enough of them to document what we believe to be the decisive role motivation has played in shaping the lives of these men. A new approach to this problem should be made when the next follow-up of the group is undertaken.

We wish to call attention to a fact not primarily related to the purpose of this study; namely, the frequency of superior achievement in this group of 800 men, selected in childhood solely on the basis of high IQ, in comparison with what could have been expected of a group of 800 boys of corresponding age picked at random in the school population. The number who became research scientists, engineers, physicians, lawyers, or college teachers, or who were highly successful in business and other fields, is in each case many times the number a random group would have provided. But for the fact that a majority of our group reached college age shortly before or shortly after the beginning of the great depression, which prevented many from getting as much schooling as they would have obtained in normal times, the general level of achievement would have been even higher than it was.

Another fact that is of interest, though not germane to the purpose of the present study, is that the intellectual ability of the group, as measured by the Concept Mastery test, increased in the twelve-year period between 1939–40 and 1951–52. Form A of the test was given when the average age of the subjects was about 29 years, and Form B when the average was about 41. After the two forms of the test were equated for difficulty by administering them both to new populations (half in the AB and half in the BA order), Bayley and Oden (1) analyzed the score changes of 772 gifted subjects who had taken both the earlier and the later test and found that the great majority of changes were in the upward direction. The average gain was statistically significant for both sexes, and for the older as well as for the younger subjects.[3]

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[3] EDITORS' NOTE: The original article also contains an appendix listing many variables that did not yield significant differences. Because so many of these closely resemble variables which were statistically significant, the author suggests caution in the interpretation of numerical findings.

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B. Parental Influences

Introduction

Science is an intellectual profession which demands high achievement, and aspiration toward intellectual goals. Science demands originality, a questioning of authority, an eagerness to examine the "taken for granted." Thus it places a high premium on independence in thinking, autonomy, setting goals for oneself, and a willingness to work in isolation. What characteristics in a child's early environmental milieu, what attitudes and values espoused by the family, would predispose him to such activity? The selections in this section have investigated family interests and practices, attitudes toward work, achievement, and accomplishment to establish the kind of milieu that encourages the attributes and resources valued in scientific performance.

Using a retrospective design with a large number of faculty members from university science departments and research scientists from industrial laboratories, West examines whether the father's occupational class is a good predictor of the son's terminal level of education. When the cut-off point for education is beyond the level of high school graduation, class appears to have little or no effect, for strong selective effects occur below this period of life. The probability of getting the doctorate once science has been chosen as an occupation seems to be the product of two independent factors, the father's class and the son's age at graduation. However, the general notion that social class does affect career preferences is borne out in the Werts study, which examined this question through data from questionnaires filled out by more than 30,000 college freshmen. Werts was able to bring in an additional factor, that of father modeling, because he questioned subjects about the specific nature of the father's occupation. Therefore, for some occupations he was able to separate the social class effects from father-model effects, i.e., the desire of sons to follow their father's occupations. Since the modeling tendency is noted differentially in various socioeconomic groupings, the relationship between social class and career preference seems more complex than appears on the surface.

The study of socioeconomic class and father's occupational level, as well as the mother's and father's educational background (studied in some of the investigations cited in the list of additional references), rests on the often-tested relationship between these variables and the intellectual background of the home, and, in turn, the educational and cultural opportunities offered to the child. The study of the social origins of American sociologists, undertaken by Glenn and Weiner, has one additional goal—to examine how social origins affect the development of the discipline of sociology. From what background does an interest in sociology and orientations toward sociological matters come? The authors find that Jews, Westerners, lower middle class, and families headed by clergymen contribute more members to the sociology profession than their proportional share, whereas a lesser than proportional share of Negroes, Catholics, Southerners, working class, and rural population become sociologists. Their findings also provide some contrasting data with other professionals and with the changes in the backgrounds of sociologists over a period of time, showing that sociologists represent a more culturally mobile population than some of the other professional groups.

Whereas the variables of occupational level of the father and the socioeconomic status of the family relate to the child's educational achievements, the study of the implications of religious background for the child's choice of work derives from quite another focus. It has frequently been reported that achievement in the area of science varies systematically with religious background. The inference is that some values and attitudes associated with particular religious backgrounds are more conducive to scientific interest and achievement than are those associated with other religious backgrounds. Datta's study, therefore, attempted to determine if the reported relationship between religion and "creative" achievement found among adult male scientists was also found among adolescents who differed in potential scientific creativity, as demonstrated by the projects they submitted to the Westinghouse Science Talent Search. Her results showed that in the adolescent samples potential scientific creativity ratings of projects submitted by students from Jewish families were higher than the ratings of projects submitted by students from Catholic, "liberal" Protestant, and "fundamentalist" Protestant backgrounds. However, she cautions that ascribing support of independent and creative thinking to certain religious attitudes can be misleading, since the association is very much reduced for subjects from larger cities. It was statistically reliable only for subjects from smaller home towns and of lower socioeconomic status.

The Nichols article examines childrearing attitudes of parents for subsequent creativity in the child. This is one of the few studies in which the relationship between mother and child, or at least certain aspects of it, has been successfully attacked. Developmental psychologists have considered the emotional aspect of the home and the subtleties of the relationship between parents and children important, even decisive, variables in the socialization of the child. However, these variables have not been readily accessible to study. Nichols has used the Parental Attitudes Research Instrument with more than a thousand National Merit finalists at the end of the senior year in high school, and assessed the originality of the children with a variety of scales: self-ratings, interests, and activity checklists and performance measures. Of the three factors measuring childrearing attitudes (authoritarian-control, hostility-rejection, and democratic attitudes), only the first correlated with creativity and originality beyond chance expectation. Children of authoritarian mothers obtain better grades in school or favorable ratings by their teacher even though authoritarian childrearing practices tended to stifle originality.

While the readings in this section illustrate the strategies used in the search for precursors that are salient for later scientific career choice, they also show methodological difficulties that befall work in this area. The choice of variables in these, and in the studies cited in the Suggested Readings, has not been accidental; at this stage of knowledge, the variables have been those most likely to be implicated on the basis of general understanding of development, and in feedback, on the basis of the demands of research work, and on the characteristics that are valued in the scientific milieu. But it is still impossible to say that the variables which thus far look interesting are, in fact, the salient variables. Unfortunately some of the most provocative developmental variables are of the semiquantitative or qualitative nature, and thus extremely difficult to test.

Nor have sufficient systematic comparative analyses been undertaken to know whether those characteristics that seem unique for the scientist sample, are in fact so. Also, at this stage of our understanding, it is difficult to know whether the variables singled out *as precursors* are truly predisposing factors: whenever such large chronological gaps exist between events studied and later scientific work; positive relationships between early developmental variables and later choice may be more apparent than real.

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Class Origin of Scientists

S. Stewart West

The study reported here examines effects of parental class in determining who will become research scientists, using a sample of persons employed in research. Its intent was two-fold: to discover what characteristics of personnel differentiate research organizations, and to identify mechanisms which govern the selection of individuals for training in science. A retrospective design was used because the phenomena of selection were of interest chiefly as they determine the characteristics of men who are ultimately employed by research or-

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ganizations. One wishes to know primarily what kind of person produces new knowledge, in order to predict the nature and quantity of the product.

SAMPLE

The data were obtained as part of a larger study of research organizations by means of questionnaires administered to the entire staff of each of five industrial laboratories and seven science departments of a large midwestern university, including all persons classified in scientific grades by the industrial organizations and all persons of faculty rank in the university departments. Numbers of questionnaires and characteristics of organizations are summarized in Table 1.

The sample is strictly representative only of the cooperating research organizations. These lie in the northeastern quarter of the United States and have sizes reasonably characteristic of those industrial organizations whose scientific employees are used for research and for fundamental improvement of products rather than for optimization and control of manufacturing processes. Departments surveyed at the university (Organization I) were anatomy, biochemistry, mathematics, physics, physiology, psychology, and sociology —all in basic science.

It is doubtful that a genuine probability sample could be drawn on the basis of present knowledge of relevant variables, and a survey of the total research population, even if feasible, would be hard to justify. The similarity of our six sub-samples suggests that variation within an organization greatly exceeds variation between organizations. Since inferences are chiefly to be drawn from intra-sample comparison, the sample may therefore be regarded as adequate for the purpose in view.

Table 1. Composition of Sample

Research organization	*I*	*II*	*III*	*IV*	*V*	*VI*	*Total*
Type of research*	R	R,A,D	A,D	A,D	D	D	–
Percentage persons with doctorate	97.7	43.4	11.8	3.7	2.5	0	34.7
Total persons in research (total questionnaires)	238	218	60	213	129	70	928
Completed questionnaires with father's occupation	176	201	51	180	110	59	777
No. in mathematics, physics, or chemistry	79	112	51	180	110	59	591
No. in biological science	38	89	0	0	0	0	127
No. in social science	59	0	0	0	0	0	59

* R denotes basic research. A denotes applied research. D denotes development.

Father's occupation (at the time the respondent was in college) was classified according to the scale described by Kinsey, Pomeroy, and Martin (2, pp. 77–79), in part to obtain direct comparability with their data on the social mobility of a sample of 2945 males from the same general region as that from which our sample was drawn (2, p. 418). Distributions of respondents in father's class are shown in Table 2 for the persons with the doctorate and persons without, in each of the six organizations. Class 2 contains unskilled labor, class 3 semi-skilled labor, class 4 skilled labor, class 5 lower white-collar occupations, class 6 upper white-collar occupations, class 7 professionals, class 8 major business executives, and class 9 the independently wealthy. Farmers, who normally fall in classes 4 and 5, are here distinguished as a separate class.

DISTRIBUTIONS IN FATHER'S OCCUPATIONAL CLASS

Differences between Organizations

Comparison of the distributions in Table 2 indicates that research organizations differ much more in the amount of training they require of their employees than in respect to the distribution of these researchers in class of origin. Chi-square tests show no significant differences between any two of Organizations

Table 2. Distribution of Sample in Father's Occupational Class

Category	*Research organization*	*Number of persons with fathers in stated class*								
		2	*3*	*4*	*F*	*5*	*6*	*7*	*8 or 9*	*Total*
With doctorate	I	0	7	16	11	57	41	39	1	172
	II	4	4	8	9	23	23	14	2	87
	III	–	–	–	2	1	2	1	–	6
	IV	–	–	1	1	1	1	1	2	7
	V	–	–	–	–	2	1	–	–	3
	VI	–	–	–	–	–	–	–	–	0
	Total	4	11	25	23	84	68	55	5	275
Without doctorate	I	–	–	–	–	2	2	–	–	4
	II	1	15	23	17	14	25	17	2	114
	III	2	4	10	6	9	9	5	0	45
	IV	5	25	43	5	55	24	15	1	173
	V	2	14	19	7	31	23	10	1	107
	VI	–	6	11	4	23	8	7	–	59
	Total	10	64	106	39	134	91	54	4	502
Some graduate training (82% to master's degree but not beyond)		1	15	25	14	26	17	18	1	117
B.A. or B.S.		9	47	71	17	94	66	34	3	341
No college degree (75% with high school only)		0	2	10	8	14	8	2	0	44
Total persons in sample		14	75	131	62	218	159	109	9	777
Percentage of sample		1.8	9.7	16.9	8.0	28.1	20.5	14.0	1.2	100.0
Percentage of persons with doctorate		–	15.0	19.0	37.0	39.0	43.0	50.0	–	–

II to VI in respect to the distribution of persons without the doctorate, and no significant difference between Organization I (the university) and the total of Organizations II to VI (industrial laboratories) in respect to distribution of persons with the doctorate. Here class 2 was combined with class 3, and classes 8 and 9 with class 7, leaving five degrees of freedom. The chi-square probability lay between 0.3 and 0.7 for all the above comparisons. It is therefore appropriate to combine distributions of persons with the doctorate and combine distributions of persons without the doctorate. The combined distributions differ at the .001 level of chi-square.

This is to say (on the basis of the data of Table 2) that the university is indistinguishable from the industrial laboratories and the industrial laboratories from one another, when one controls for differing proportion of persons with the doctorate. However, probability of possessing a doctorate appears to be related to parental class, as may be shown by computing the fraction of persons from each class who have the doctorate (last line of Table 2). This fraction shows a sharp discontinuity at the boundary between manual and white-collar workers, and increases only gradually from that boundary toward higher classes. It is, however, almost as large for class F (farmers) as for the white-collar classes, so that in this respect the farming fathers resemble persons of class 5 rather than class 4. This is reasonable in that most responses suggest farms which represent rather large capital investments.

Probability of Achieving the Doctorate

The data do not permit finer discrimination than that between manual-labor and white-collar classes; that is, between a stratum A containing classes 2, 3, and 4, and a stratum B containing classes F, 5, 6, 7, 8, and 9. Within each stratum, the distribution in class of persons with the doctorate is indistinguishable by chi-square test from that of persons without, the chi-square probability being 0.43 (at 2 df) for stratum A, and 0.64 at (4 df) for stratum B. Of 220 persons in stratum A, 40 had the doctorate, while of 557 persons in stratum B, 235 had the doctorate, or 18.2 per cent and 42.2 per cent, respectively.

Delay in completion of undergraduate study suggests itself as an explanation of the markedly lower probability of possessing a doctorate among children of manual workers. In our sample, probability of a doctorate decreases exponentially with age at bachelor's degree, by almost tenfold per decade (Table 3). The ratio of number of persons with the doctorate to total persons who received a bachelor's degree at a given age of *t* years can be expressed as $e^{-(t-18)/4.6}$, which fits the observations very well, with chi-square probability larger than 0.9. This ratio is unity at 18 years of age, and decreases to .074 at age 30. Some error is introduced by neglect of persons who will later obtain doctorates by leaving their jobs to return to school or by part-time study, but this error may be expected to be small.

Table 3. Distribution of Sample in Age at Bachelor's Degree

	Number of persons				
		With doctorate			
Age t at B.A. or B.S.	*Total (n)*	*Observed*	*Predicted*	*Diff.*	*Probability of doctorate (from fitted curve)*
18	1	1	1.0	0.0	1.00
19	4	4	3.2	0.8	0.80
20	33	20	21.3	—1.3	0.65
21	93	46	48.6	—2.3	0.52
22	185	80	76.9	3.1	0.42
23	96	35	32.3	2.7	0.34
24	66	18	17.8	0.2	0.27
25	67	15	14.5	0.5	0.22
26	53	4	9.3	—5.3	0.18
27	24	5	3.4	1.6	0.14
28	22	3	2.5	0.5	0.11
29	20	1	1.8	—0.8	0.09
30	12	1	0.9	0.1	0.07
31	10	1	0.6	0.4	0.06
32–42	13	0	1.0	—1.0	0.05 to 0.005
Over 42	0	0	0	0	—
No B.S.	44	0	0	0	—
Total	743	252	—	—	—

Predicted number with doctorate = $n\ e^{-(t-18)/4.6}$ by fitted curve. Totals are here reduced by missing data.

Children of manual workers do graduate from college somewhat later than children of white-collar workers and farmers, as shown by the distributions in Table 4. However, it appears also that the manual/white-collar differential in fraction having the doctorate has little or no dependence upon age at receipt of bachelor's degree, since expected values of number of persons with the doctorate computed according to a hypothesis of independence do not differ significantly from the observed numbers. This is a stronger statement than to say that, when age at bachelor's degree is controlled, the overall class-differentiation does not change, and it is worth demonstrating in detail.

Referring to the lettered columns of Table 4, let *a* be the total number of persons and *b* the number with the doctorate in stratum B in a given interval of age at receipt of bachelor's degree, and let *c* and *d* be the corresponding numbers of persons in stratum A. The hypothesis that the ratio of the probability of possessing a doctorate in stratum A to the corresponding probability in stratum B is independent of age at receipt of the bachelor's degree then requires that $d/c = k(b/a)$, or $d = kbc/a$, where *k* has the same value for any age.

Expected values of *d* computed from this formula with k = ½ are shown in Table 4. Since comparison with observed values yields a chi-square probability of 0.74, it cannot be asserted that delay in graduation from college produces the manual/white-collar differential

Table 4. Effect of Age at Bachelor's Degree on Relation of Probability of Doctorate to Parental Class

	Number of persons in class F or 5 to 9		*Number of persons in class 2, 3, or 4*			
				With doctorate		
Age at B.A. or B.S.	*(a) Total*	*(b) With doctorate*	*(c) Total*	*(d) Observed*	*Expected*	*Diff.*
18 to 20	32	23	5	2	1.8	0.2
21	73	41	20	5	5.6	—0.6
22	138	69	43	9	10.8	—1.8
23	59	25	33	8	7.0	1.0
24	42	14	22	4	3.7	0.3
25	43	11	19	3	2.4	0.6
26 or more	78	10	61	3	3.9	—0.9
Total	465	193	203	34	35.2	—

The predicted number of doctorates among persons from classes 2 to 4 is computed on the assumption that the probability of a doctorate is half the probability among persons from classes F to 9. Totals are reduced by missing data.

in probability of possessing the doctorate. Hence the data indicate that probability of possessing the doctorate, having graduated from college and chosen research as an occupation, is expressible as the product of two independent factors, one which is a function only of father's class and one which is a function only of age at graduation. Their independence seems as important as their functional forms.

Persons without the Doctorate

Division of the non-doctorate category of respondents into persons without college degree, persons with B.A. or B.S. only, and persons with some graduate training (Table 2) indicates that parental class affects chiefly the latter portion of doctoral training. If class F (farmers) is excluded, these three component distributions in parental class do not differ significantly among themselves, although their sum does differ from the distribution of persons with the doctorate, as pointed out previously. However, children of farmers constitute 12 per cent of the high-school graduates and 18 per cent of persons with some graduate training, but only 5 per cent of those who terminated their education at the bachelor's degree. The absolute differences in number are too small to warrant more speculation than that rural children may possibly be less inclined than urban children to persevere in education merely to reach an approved terminal level. The important point here is that, for persons oriented toward research as an occupation, parental class appears to exert little selective effect between the end of high school and the mid-point of graduate training.

The 44 persons who were engaged in research without college degrees (Table 2), three-fourths of them with no formal training beyond high school, were following a quite different career sequence. They were not merely technical assistants, but were employed at the same levels of task and salary as many of the persons with college degrees. It should not be forgotten that any of the formal stages of training can be by-passed under special circumstances. One can enter college without graduating from high school, or enter graduate school without having been an undergraduate, although such events are rare. Nevertheless, it is surprising to find that the self-taught scientist has so large a place in modern research, although Visher's sample contained many such who attained eminence at an earlier period (10, pp. 358–360). . . .

EFFECT OF PARENTAL CLASS ON ACHIEVEMENT OF EMINENCE

Comparison with samples of other investigators (Table 5) indicates only a small overall effect of parental class on achievement of eminence, although a significant one as between Visher's sample and the persons with doctorate in Table 2. However, the markedly smaller contribution of classes 2 and 3 to the

Table 5. Comparison with Samples of Other Investigators

*Composition of sample**	*% Fathers in stated class* 2 or 3	4, F, or 5	6 to 9	*Sample size*
Men with I.Q. over 135 (Terman and Oden)	8	26	66	766
Professional men (Kinsey *et al.*)	8	36	56	671
Eminent physicists and biologists (Roe)	0	42	58	42
Eminent scientists (Visher)	1	48	51	849
Researchers with doctorate, Table 2	5	48	47	275
Physicians, 1940 (Rogoff)	12	46	42	105
Researchers with some graduate training, Table 2	14	55	31	117
Researchers with four years of college, Table 2	16	54	30	341
Engineers and scientists, 1940 (Rogoff)	9	63	28	178
Researchers without college degree, Table 2	5	73	23	44
Contemporaries of persons in Table 2 (adjusted for the effect of siblings)	44	46	10	Total USA

* For sources, see references 1, 3, 4, 5, 7, 9.

samples of eminent scientists raises again the question of whether class-related traits of personality may not restrict the range of opportunity accessible to children of unskilled and semi-skilled fathers. Average parental class trends upward in Table 5 with amount of formal training, but it has been shown previously that by far the most critical point of selection occurs at the level of the master's degree, and that here the critical distinction in parental class is between blue-collar and white-collar fathers. . . .

SUMMARY

Faculty members in seven science departments of a large university and research scientists in five industrial laboratories were found to be distributed in father's occupational class in such a way that (a) there were no significant differences between organizations when the fraction of persons having the doctorate was controlled, and (b) there were no significant differences (when children of farmers were excluded) between terminal levels of education from high-school graduation to master's degree. The fraction of persons having the doctorate averaged 18 per cent for children of manual workers, and 42 per cent for children of white-collar workers and farmers. In either of these categories, there was no significant difference between the distribution of persons with the doctorate in father's class and that of persons without the doctorate.

In this sample of persons engaged in research, the fraction of persons possessing the doctorate decreased exponentially with age at bachelor's degree, from unity at 18 years to 0.07 at 30 years. This relation was independent of the effect of father's class.

Reports of motivation for engaging in research, obtained from a subsample of the respondents, showed a well-defined hierarchy of motives, but no significant differences in report of the four major motives in relation to (a) possession of the doctorate, (b) basic vs. applied research, (c) blue-collar father vs. white-collar father, (d) organizational status, (e) birth order or number of siblings, or (f) productivity among persons at the university.[1] However, non-mobile persons (having professional fathers) did differ significantly in reported motivation from persons who had been mobile in class.

The major observations were therefore (a) that the researchers were quite homogeneous in reported motivation, (b) that father's class produced little selective effect between graduation from high school and the level of the master's degree, (c) that strong selective effects occurred below and above this period of life, and (d) that the latter selection was chiefly characterized by a contrast between blue-collar and white-collar fathers. As an interpretation of these phenomena, it is suggested: that there exist certain motives and modes of thinking characteristic of research;

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[1] EDITORS' NOTE: See original report for studies of motivation toward research.

that these are induced during childhood (with probabilities directly related to parents' occupational class); that persons in whom they have been induced will seek the congenial activities of learning and (ultimately) employment in research; and that selection at the highest level of graduate training is produced largely by other characteristics of personality, related to parental class but not to ability in research.

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Social Class and Initial Career Choice of College Freshmen

Charles E. Werts

In an excellent study of 33,982 recent college graduates, Davis found considerable variability with respect to the social class composition of freshman career preferences.[1] He found engineering and education to be overchosen by low SES students, whereas medicine, law, humanities and the social sciences were overchosen by high SES students. Preference for business, biological science and the physical sciences was not clearly related to SES in Davis' study. This procedure could involve error due to faulty recall of freshman career preferences. Added to this problem is the fact that only about 60% of entering freshmen graduate. Thus, it is not certain that the same SES bias would be observed in the career choices of a more representative group of freshmen. Since information on freshman career choices at the time of entrance into college was available from a previous study,[2] these results were examined to see if the SES relationship to career preferences was similar to that found by Davis.

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[1] James A. Davis, *Great Aspirations, Graduate School Plans of America's College Seniors*, Chicago: Aldine Publishing Co., 1964. Following Davis, the initials SES indicate socioeconomic status, derived herein from father's occupation.

[2] Alexander W. Astin, "Recent Studies of College Environments," paper presented at the annual meeting of the American Personnel and Guidance Association, Minneapolis, 1965.

DATA COLLECTION

In the fall of 1961, a brief questionnaire was administered to 127,212 freshmen entering 248 heterogeneous colleges and universities.[3] With a few exceptions, this sample included the entire entering freshman class at each institution. In the summer of 1962, approximately equal numbers of students from each institution were sent a 12 page follow-up questionnaire. In large institutions, random sampling was used to select those sent a questionnaire. In coeducational institutions, males and females were chosen in approximately equal numbers. Of the approximately 60,000 questionnaires sent out, about 55% were returned. The percentage of respondents per institution varied from 20% to over 90%, high rates being obtained from the more highly selective colleges and universities. After discarding questionnaires with large amounts of missing information, a sample of 16,141 males and 14,417 females remained. The analysis was done on 30,558 students for whom sophomore information was available. This information would be useful in later studies concerning changes from the freshman year career choice.

Information on father's occupation and student's probable future career was obtained from free response items on the initial freshman questionnaire. Father's occupation was coded by hand into the categories shown at the top of Tables 1 and 2. ("Other" includes a variety of small occupations which generally require college graduation.) The percentage of fathers in each category is noted at the top of these tables. Analysis was done separately for men and women because the social class distribution of college women is different from that of men. Probable future occupation was originally coded into 49 categories, but only categories containing more than 1% of the males or of the females were included in the

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[3] The details of the college selection procedure can be found in Alexander W. Astin, _Who Goes Where to College?_, Chicago: Science Research Associates, Inc., 1965.

Table 1. Percentage of Each Career Choice by Occupation of Father[a]
Men

		Father's occupation									
		Group I								_Group II_	
		_6.02%_	_4.08%_	_3.51%_	_1.14%_	_3.82%_	_6.81%_	_1.94%_	_1.34%_	_23.72%_	_7.96%_
Son's career choice	_N = 100%_	_Farmer_	_Manual laborer_	_Semi-skilled_	_Service worker_	_Clerical_	_Skilled worker_	_Fore-man_	_Tech-nical worker_	_Busi-ness-man_	_Sales-man_
Engineer	(1999)	6.15	_5.10*_[b]	_4.55*_	1.45	_4.60*_	_9.25*_	_2.80*_	_1.80*_	22.61	6.45*
Teacher	(1813)	_10.26*_	_7.78*_	_5.24*_	1.49	_5.24*_	_10.87*_	2.04	1.20	17.82*	6.39*
Physician	(1575)	1.33*	2.16*	2.16*	.89	3.17	5.14*	1.27*	.83*	_26.67*_	8.57
Businessman	(928)	3.99*	2.26*	1.51*	.65	2.48*	3.99*	1.83	.86	_44.07*_	_9.81*_
Lawyer	(869)	3.34*	2.88*	1.61*	.92	2.88	3.11*	1.61	.46*	26.35	_11.74*_
Chemist	(484)	4.75	5.58	3.10	1.24	5.99*	8.26	2.27	1.24	20.45	7.44
Accountant	(420)	_10.00*_	_6.43*_	4.29	1.67	_6.67*_	_9.05*_	2.86	1.90	20.95	5.95
Physicist	(391)	3.32*	3.32	3.84	.51	5.63*	6.14	1.79	1.53	18.16*	7.67
Clergyman	(305)	6.23	5.25	3.28	1.64	4.92	8.52	1.31	2.30	18.36*	9.51
Dentist	(297)	3.03*	2.69	4.04	1.35	3.70	5.39	2.02	_3.03*_	24.92	9.09
College prof.	(239)	1.26*	2.93	3.77	.42	2.51	6.28	2.51	.42	18.41*	8.37
Mathematician	(235)	2.55*	5.96	3.40	1.28	4.26	7.66	2.13	2.55	20.85	8.09
Farmer	(230)	_39.13*_	3.91	3.04	.43	2.61	7.39	1.30	1.30	12.61*	5.65
Architect	(193)	2.07*	3.11	4.66	1.04	2.59	5.70	1.55	0	22.80	_12.44*_
Foreign ser.	(179)	5.59	3.91	1.68	1.68	3.91	2.23*	.56	2.23	24.02	7.26

[a] Reading horizontally from the career choice of engineer, one can see that 5.10% of the 1999 males choosing engineer were sons of manual laborers, whereas only 4.08% of the males in the total sample were sons of manual laborers.

[b] Where the percentage of those in a given career is significantly different from the expected value it was noted with an asterisk. Those percentages which are significantly high are italic and asterisked. Those percentages which are significantly low are merely asterisked.

tables. This included 15 career choice categories for men and 11 for women (one of which was housewife). Cross tabulations of career choice with father's occupation were done. Tables 1 and 2 were constructed from these cross tabulations by converting the number of persons in each cell of the cross tabulation to a percentage of all those choosing that particular career. To facilitate examination of SES trends, father's occupations were further categorized into four groups in Table 1.

I. Fathers' occupations in which the modal education of the fathers was a high school diploma or less
II. Father's occupations in which the modal education of the fathers was some college
III. Father's occupations in which the modal education of the fathers was a baccalaureate degree
IV. Father's occupations in which the modal education of the fathers was an advanced degree

The results of this grouping are shown in Tables 1 and 2.

If father's occupation did not influence career choice, the percentage of sons or daughters in each cell would be the same as the percentage of fathers having that particular occupation. In Table 1, for example, 3.82% of the males were sons of clerical workers. If careers were selected randomly with respect to father's occupation, 3.82% of those in each career choice would be sons of clerical workers. Insofar as the percentage of sons of clerical workers in any career choice is statistically different from 3.82%, it may be hypothesized that being the son of a clerical worker does influence the choice of that career. Sons of clerical workers can be said to overchoose accountant since they make up a greater percentage (6.67%) of those choosing accounting than would be expected from the percentage of sons of clerical workers (3.82%) in the sample. Every entry in Tables 1 and 2 was tested to see if it was statistically different (at .05 level of significance) from the percent-

Table 1. (continued)

Father's occupation											
Group III					*Group IV*			*Summary*			
2.19%	*1.65%*	*3.69%*	*1.41%*	*7.70%*	*1.18%*	*3.07%*	*2.43%*	*28.67%*	*31.68%*	*16.64%*	*6.68%*
Accountant	*Teacher*	*Engineer*	*Clergyman*	*Other*	*College professor*	*Physician*	*Lawyer*	*Group I*	*Group II*	*Group III*	*Group IV*
2.35	1.35	_5.90*_	.80*	6.75	.50*	1.15*	.75*	_35.72*_	29.06*	17.16	2.40*
1.60	_2.54*_	1.99*	1.88	4.47*	.66*	1.16*	.66*	_44.13*_	24.27*	12.47*	2.48*
2.35	1.52	4.06	1.21	8.76	.89	_13.71*_	2.79	16.95*	_35.24*_	17.90	_17.40*_
.97*	1.08	2.16*	.43*	4.31*	.54*	2.05*	1.51	17.56*	_53.88*_	8.94*	4.09*
2.42	1.38	2.88	1.04	7.59	1.04	1.96*	_12.54*_	16.80*	_38.09*_	15.30	_15.54*_
2.48	2.48	4.55	1.24	8.06	1.03	1.24*	1.65*	_32.44*_	27.89	18.80	3.93*
_4.25*_	.95	1.19*	1.43	3.57*	.48	.48*	1.19	_42.86*_	26.90*	11.67*	2.14*
_3.84*_	1.79	_6.65*_	1.02	_11.25*_	1.79	3.07	2.56	26.09	25.83*	_24.55*_	7.42
1.31	1.97	1.64*	_7.54*_	6.23	.66	1.64	.33*	_33.44*_	27.87	18.69	2.62*
2.36	1.68	3.37	.67	_14.81*_	.34	2.02	2.02	25.25	34.01	_22.90*_	4.38
2.51	_3.77*_	_6.28*_	2.09	_10.88*_	_8.79*_	3.35	3.35	20.08*	26.78	_25.52*_	_15.48*_
_4.68*_	2.13	5.11	2.13	8.51	1.70	1.28	.85	29.79	28.94	_22.53*_	3.83
2.17	1.30	1.74	.87	3.91*	0	0*	.43*	_59.13*_	18.26*	10.00*	.43*
1.55	2.07	_8.81*_	.52	10.88	.52	2.59	1.55	20.73*	35.23	_23.83*_	4.66
2.79	3.35	4.47	2.23	_11.73*_	.56	1.68	3.35	21.79*	31.28	_24.58*_	5.59

age of fathers in that occupation. For the rest of the discussion, "overchoose" will refer only to statistically larger percentages than expected and "underchoose" to statistically smaller percentages than expected in any cell of Tables 1 and 2.

RESULTS

Looking at the summary portions of Tables 1 and 2, it can be observed that students from the lowest SES grouping, Group I, overchoose the following careers:

Men	*Women*
engineer	teacher
teacher	nurse
chemist	lab technician
accountant	
clergyman	
farmer	

Not only did no other SES group overchoose these careers, but they are all underchosen by the highest SES grouping, Group IV.

Those careers overchosen by students from the highest SES grouping, Group IV, were:

Men	*Women*
physician	social worker
lawyer	physician
college professor	foreign service
	psychologist
	housewife

These careers are underchosen by the lowest SES grouping, Group I, (except for the choice of social worker, which is low but not significantly so), and in no case were they underchosen by SES groups I and III.

The third broad grouping of career choices are those overchosen by SES Group II and/or Group III, but in no case overchosen by either Group I or Group IV. These were:

Men	*Women*
physicist	journalist
dentist	speech therapist
mathematician	
architect	
businessman	
foreign service	

An unexpected finding in Table 1 was that in every case where son's career choice matched father's occupation a significant degree of overchoice was observed. This occurred whether or not that career was overchosen or underchosen

Table 2. Percentage of Each Career Choice by Occupation of Father[a]
Women

		Father's occupation									
		Group I								*Group II*	
		6.39%	*2.94%*	*5.77%*	*1.01%*	*3.46%*	*2.96%*	*1.66%*	*1.32%*	*23.69%*	*7.71%*
Daughter's career choice	*N = 100%*	*Farmer*	*Manual Laborer*	*Semi-skilled*	*Service worker*	*Clerical*	*Skilled worker*	*Fore-man*	*Tech-nical worker*	*Busi-ness-man*	*Sales-man*
Teacher	(5814)	*8.00**[b]	*4.02**	*7.29**	1.22	*4.27**	3.18	*2.24**	1.34	21.84*	7.93
Nurse	(583)	*9.09**	3.77	*8.40**	.86	3.43	3.77	2.57	2.06	18.52*	8.23
Lab. technician	(416)	8.65	2.88	7.69	.96	*5.53**	*5.05**	1.20	.72	21.15	8.41
Social worker	(412)	4.61	2.18	7.04	.97	3.40	3.40	.49*	1.21	23.79	6.55
Physician	(331)	1.81*	1.81	3.02*	1.21	3.32	2.42	1.81	.30	19.03*	9.05
Foreign serv.	(293)	1.71*	1.71	1.37*	1.02	2.05	2.05	1.02	.34	*30.03**	6.48
Clerical	(283)	*9.54**	4.24	6.36	1.06	1.06*	4.59	.35	1.77	25.44	8.83
Psychologist	(232)	.86*	2.16	3.02*	.86	2.16	3.45	2.59	.86	20.69	9.05
Housewife	(217)	7.83	2.30	4.61	.46	1.84	1.38	.92	0	23.50	7.37
Journalist	(212)	1.89*	2.36	3.77	.47	4.72	1.42	1.89	1.42	27.36	9.43
Speech therapy	(172)	2.91	2.33	4.07	1.16	2.33	2.91	1.16	1.16	*30.81**	8.72

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[a] Reading horizontally from the career choice of teacher, one can see that 4.02% of the 5814 females choosing teacher were daughters of manual laborers, whereas only 2.94% of the females in the total sample were daughters of manual laborers.

[b] Where the percentage of those in a given career is significantly different from the expected value it was noted with an asterisk. Those percentages which are significantly high are italic and asterisked. Those percentages which are significantly low are merely asterisked.

by others from the same SES group. Thus, it can be observed that engineers' sons are the only ones in SES groups II, III, or IV who overchoose engineering. To explore the implications of this finding, the percentage of sons who chose their fathers' occupations (henceforth called a "father-model choice") was calculated along with the percentage of other sons who made the same career choice. This comparison was necessary in order to separate the general SES effects, noted previously, from the father-model effects. This information is presented in Table 3, along with appropriate percentages taken from Table 1. Some father's occupation categories (professions only) which were previously discarded were re-examined and it was found in each and every case that sons significantly overchose their fathers' occupations.

DISCUSSION

Although somewhat different career categories than those used by Davis were used in this study, the results show a considerable area of agreement. In both studies, engineer and teacher were overchosen by low SES groups. The present findings indicate that chemist, accountant, clergyman and farmer should be added to the list of careers favored by men of lower SES background. The list should include nurse and lab technician for women, in addition to teacher. The most significant overchoice for this group was teacher. In both studies, lawyer and physician were overchosen by higher SES groups. The present findings indicate that higher SES men also favor college professor. Higher SES women overchoose social worker, psychologist and foreign service, in addition to physician.

The finding of a group of careers which are not overchosen by either the highest or lowest SES groups, but are overchosen by intermediate SES groups can be interpreted in a variety of ways. One explanation is the intermediate prestige and status level of these careers. The careers which fit this category for men (physicist, dentist, mathematician, architect and foreign service) are probably more rewarding in prestige satisfaction or financially than teacher, engineer, chemist, accountant, clergyman or farmer, but usually less rewarding than lawyer, physician or college professor. The same seems true for women's careers. That is, journalist and speech therapist may be more rewarding than teacher, nurse or lab technician, but less so than social worker, physician, psychologist or foreign service.

One may further speculate that, for men,

Table 2. (continued)

Father's occupation											
Group III					Group IV			Summary			
2.14%	1.96%	4.74%	1.69%	8.71%	1.27%	3.66%	2.54%	25.52%	31.40%	19.23%	7.46%
Accountant	Teacher	Engineer	Clergyman	Other	College professor	Physician	Lawyer	Group I	Group II	Group III	Group IV
2.01	2.24	3.75*	1.69	7.38	.79*	2.18*	2.01*	31.56*	29.77*	16.72*	4.99*
2.06	2.23	4.12	2.74*	6.00	.86	4.80	.69*	33.96*	26.76*	17.15	6.35
2.16	2.16	6.49	1.68	6.25	.96	3.37	.96*	32.69*	29.57	18.75	5.29*
2.18	1.94	4.37	2.43	8.25	2.43*	5.58*	4.37*	23.30	30.34	19.17	12.38*
1.81	2.11	8.16*	2.11	11.18	3.63*	9.97*	3.63	15.71*	27.19	25.38*	17.22*
2.73	2.04	4.78	1.02	12.63	.34	8.53*	2.73	11.26*	36.52*	23.21	11.60*
1.06	2.12	4.95	1.41	2.47	.71	1.77	1.41	28.98	34.28	12.01*	3.89*
2.16	2.16	6.03	1.72	12.17	.43	7.33*	3.45	15.95*	29.74	24.14	11.21*
2.30	1.84	4.61	1.84	11.06	2.30	6.91*	4.15	9.35*	30.88	21.66	13.36*
1.89	.94	4.25	.94	16.51	1.89	1.89	3.30	17.92*	36.79	24.53*	7.08
1.16	1.74	4.65	0	6.98	1.16	5.81	1.74	18.02*	39.53*	14.53	8.72

Table 3. Father-Model Career Choice Data on Sons Choosing Their Fathers' Occupations

Son's father-model career choice	*Percentage of all fathers in occupation whose sons make a father-model career choice*[a]	*Percentage expected to make this career choice at the same class level*[b]	*Percentage of all sons choosing this career who make a father-model career choice*
Engineer	19.80%	10.77%	5.90%
Teacher	17.29%	7.44%	2.54%
Physician	43.64%	9.95% estimated	13.71%
Businessman	8.02%	3.50% estimated	44.07%
Lawyer	27.74%	3.80% estimated	12.54%
Chemist	7.95%	3.23%	1.45%
Accountant	5.38%	1.29%	4.52%
Physicist	22.22%	3.45%	1.02%
Clergyman	10.09%	1.38%	7.54%
Dentist	22.92%	1.78%	7.41%
College prof.	11.05%	2.03%	8.79%
Farmer	9.26%	1.26%	39.13%
Architect	6.67%	1.63%	1.55%
Biologist	16.67%	.97%	1.43%
Journalist	11.25%	.78%	6.72%
Pharmacist	11.25%	.60%	6.98%
Military	12.75%	.50%	10.83%

[a] All those listed are significantly higher (at .05 level) than the percentage expected at the same class level.

[b] Actual percentage of appropriate class who make this career choice, calculated with father-model occupation excluded from the class. In cases where there would be distortion, the percentage was estimated.

there is a definite "range" of careers to which children of a given social background aspire. Thus the lower SES levels, Group I, aspire to the more pedestrian careers such as engineers, teacher, accountant, chemist, clergyman or farmer. Some of the most favored from this group might aspire to intermediate status careers such as mathematician, physicist, or dentist, but very few aspire to physician, lawyer, or college professor. Probably in the effort to maintain their status high SES sons overchoose only the highest status careers such as physician, lawyer or college professor, although some may be drawn to such respected careers as physicist, mathematician, architect or foreign service.

This description corresponds with the idea that it is difficult to take more than one step up the social ladder without special advantages (i.e., financial, personality, or intellectual). The present findings suggest that one step up from the lowest SES groups is equivalent to aspiring to teacher or engineer. One step up for sons of intermediate SES groups is equivalent to aspiring to physicist, mathematician, architect, lawyer, physician or college professor. Sons of high SES groups are obviously more limited in their range of choice if they are to maintain themselves at the SES level of their parents.

The most notable finding in Table 1 is the significant degree of overchoice whenever the son's career choice matched the father's occupation, even if that particular career is generally underchosen by persons from that SES group. One may observe that sons of engineers, teachers, and accountants, who made such a father-model choice, are the only sons of professional parents to overchoose those careers. This effect is of particular importance to career choices such as business. The same is true of farming. There, 9% of the sons of farmers choose farming and comprise 39% of all those making the choice. If the father-model effects were corrected for, the underchoice of businessman and farmer by all other SES groups noted in Table 1 might indicate that these careers have even lower prestige than teacher. The methodological point is that the father-model effect must be controlled if general social class effects are not to be obscured. When the desire of higher SES sons to maintain their social level is reinforced by the father-model effect, the sons' overchoice can be highly overdetermined, as seen by the fact that 43.6% of physicians' sons choose medicine and 27.7% of lawyers' sons choose law. Thus, looking at Table 1, it can be observed that sons of physicians overchoose medicine, sons of lawyers overchoose law, and sons of college professors overchoose college professor. In no case did sons of physicians, lawyers, or professors overchoose any other career but their fathers'.

The father-model effect is generally not observable in women's career choices because, as seen in Table 2, women seldom choose a male career such as their fathers might follow. In the one case where a comparison is possible, physicians' daughters overchoose medicine but only to about the degree that others of their SES level do.

SUMMARY

In general, the findings by Davis about the social class distribution in various career choices is supported. Neither the fact that his sample of graduating seniors represented only 60% of those who started as freshman, nor the use of retrospective data on freshman career choice seems to have introduced serious error in the generality of his findings. Engineer and teacher are overchosen by lower SES groups and physician and lawyer by higher SES groups. Chemist, accountant, clergyman and farmer also appear to be overchosen by low SES males and college professor by high SES males.

The present study indicates that it can be useful to obtain the father's specific occupation rather than getting merely a general classification (e.g., Professional) when studying college students. Such information allows the separation of general social class effects from father-model effects (i.e., the desire of sons to follow their fathers' occupations). For some cases, such as farmer and businessman, the father-model effect totally obscured the general SES effect.

The practice of analyzing male and female data separately is advantageous because:

1. Social class effects are not as clear-cut for women as for men.
2. The same professions cannot be assumed to have the same prestige for both men and women. (Thus, it was noted that foreign service, which is overchosen by females of high SES background, is overchosen by males of intermediate SES background.)
3. Female career choices are probably based on a different value orientation than those of men. (This is suggested by the overchoice by high SES females of service-oriented fields such as social worker, psychologist, physician and foreign service.)
4. The major career choices of men and women rarely overlap.
5. The father-model effect is not generally applicable to women.

Some Trends in the Social Origins of American Sociologists

Norval D. Glenn and David Weiner

. . . The question of how sociologists' backgrounds affect their perceptions of and orientations toward their subject matter is an important problem in the sociology of science that we are investigating in some of our current research.[1] So far we have uncovered little evidence that a sociologist's pre-college background has much influence upon the kind of sociologist he becomes (Weiner, 1968). However, our analyses are incomplete, our negative findings may well result from a lack of sufficiently sensitive measures of orientations

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[1] For a list of a large proportion of the social origins studies, see Glenn et al. (1969). Data on the social origins of sociologists or social scientists in the United States are reported in Harmon (1966), Lazarsfeld and Theilens (1958), and Palmore (1962).

to sociology, and background factors on which we have no data may be influential.

The purpose of this paper is more modest than the ultimate goal of our research in this area. We report some trends in the background characteristics of American sociologists without trying to assess here the importance of these trends for the development of the discipline. However, we hope some of our readers will be stimulated to formulate fruitful hypotheses concerning how these trends have affected and are affecting the discipline. The findings on sociologists' backgrounds also have value for other reasons; for instance, they reveal some large and relatively untapped potential sources of sociological talent in the American population, including Negroes, Catholics, and Southerners of all races and religions. Furthermore, the curiosity of sociologists about themselves should in itself be sufficient justification for reporting these data.

METHODS

We collected information on the backgrounds of sociologists by means of a questionnaire mailed September 1, 1967, to 760 active members and fellows of the American Sociological Association, and by means of a shorter instrument mailed about two months later to those who had not responded to the first mailing. We drew the sample randomly from all male active members and fellows listed in the 1967 ASA directory. The two mailings yielded 429 completed and usable questionnaires—a return of about 56 per cent.

Even though the respondents were about an eighth of the universe, considerable sampling error and mail-back bias were possible. To check for bias we compared the respondents with the universe on several variables reported in the ASA directory, and in each case, the respondents were very similar to the universe. The comparison that revealed the greatest difference is shown in Table 1. Although we cannot be certain there is not more bias on other variables, we can think of no reason to suspect that our respondents are highly unrepresentative with respect to any variable treated in this paper.

Table 1. Comparison of Universe, Random Sample, and Respondents on Date of Doctorate (in per cent)

Date of doctorate	*Universe* (*N* = 3,245)	*Random Sample* (*N* = 760)	*Respondents*[a] (*N* = 429)
1917 and earlier	0.3	0.3	0.5
1918–1927	1.9	1.6	0.9
1928–1937	5.6	6.3	7.5
1938–1947	9.2	9.9	10.8
1948–1952	13.2	13.0	11.9
1953–1957	18.8	20.3	20.6
1958–1962	21.8	20.8	20.3
1963–1967	14.7	15.9	19.6
Date of doctorate not given in ASA Directory	14.6	12.0	7.9
Total	100.0	100.0	100.0

[a] This information is from the ASA Directory rather than from the completed questionnaires.

For the tabulations reported here, we divided the respondents into "academic cohorts" based upon year of receipt of the doctorate. This seems a better way to trace changes in the recruitment patterns of sociology than use of cohorts based upon date of birth, since the respondents entered sociology at widely varying ages. Thirty-one of the respondents reported no earned doctorate and we considered placing them in academic cohorts on the basis of the date they became employed full-time as sociologists. However, it was difficult to determine from the job histories just when many of the respondents became practicing sociologists, and therefore we tabulated separate data for persons with no reported doctorate rather than include them in the cohorts.

For the original tabulations, we divided the respondents with doctorates into six cohorts, namely, those who received doctorates in 1937 or earlier, in 1938 through 1947, in 1948 through 1952, in 1953 through 1957, in 1958 through 1962, and in 1963 through 1967. However, to report the data, we collapsed these six cohorts into three, except when such collapsing would obscure important information. The *N*'s for these cohorts are as follows:

Date of doctorate	*N*
1947 and earlier	85
1948–1957	144
1958–1967	169
No doctorate reported	31
Total	429

A comparison of the earlier and later academic cohorts should give a rather accurate picture of trends in the background characteristics of people who have become eligible for active membership in the American Sociological Association during the past few decades. A possible source of distortion, aside from sampling error and mail-back bias, is the fact that we have data only on the surviving members of each cohort who have remained members of the Association, and more of the original members of the earlier cohorts have died. However, there is little reason to suspect there has been much variation in mortality among sociologists with different backgrounds, and it seems unlikely that enough people have left the profession, and thus the Association, to introduce much bias.

The questionnaire asked for background data for each of three age levels, namely, (1) 0 through 12, (2) 13 through 16, and (3) 17 through 20. The distributions of most of the background variables are about the same at all three levels, and therefore for these variables we report the data only for the oldest level, which we term "late adolescence." However, the distributions by size of community of residence vary enough among the age levels to require consideration of the data for all three levels.

When possible, we placed the distributions of characteristics of our respondents and their families in perspective by comparing them with data for the entire U.S. population for the census year when the largest percentage of the respondents were in late adolescence.[2] For the entire sample and the middle cohort, this year is 1940, for the earliest cohort it is 1930, and for the latest cohort it is 1950.

Although we collected data on a wide range of background characteristics, we report here only the few we consider most important. Readers interested in other background variables are encouraged to contact the senior author, who may be able to supply the desired information.

FINDINGS

Fathers' Occupations

Previous studies have shown that sociologists' families of orientation typically have ranked above average in the major dimensions of stratification but below the families from which such professionals as lawyers and medical doctors typically have come.[3] One source describes the modal social origins of sociologists, as well as those of other academicians, as "lower-middle-class" (Mills, 1951:129–130). However, recently there has been speculation that more of the new recruits to sociology are coming from higher social levels. For instance, Gamson (1968:288) hypothesizes "that a larger proportion of sociologists are now being drawn from backgrounds with relatively high socio-economic status." If this were true, a corollary would be that sociology "is less an avenue of upward mobility for those who are attracted to it today than for those of a previous generation" (288). There are several reasons to expect these hypotheses to be correct. Sociology has gained in prestige, respectability, and economic support, and the incomes of sociologists have risen steeply. The field should, therefore, have become more attractive to sons of upper-middle-class families, since most of them can now enter it without being downwardly mobile to any great extent. Furthermore, we are told by some observers that many sons of affluent families are disillusioned with business and the business-serving professions and are seeking careers they perceive to be more socially useful and more relevant to major political and social issues.

However, the data in Table 2 do not support Gamson's hypotheses. The differences among the three "academic cohorts" in the distribution of fathers by Duncan's Socio-

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[2] Many data more or less comparable to some of ours are available for scientists in general and for persons in other specific disciplines. Because of space limitations, we do not attempt a point-by-point comparison of these data with ours, but readers may make their own comparisons by referring to Hirsch (1968), Strauss and Rainwater (1962), Clark (1957), Harmon (1966), and a number of other publications. In general, the similarities between the background characteristics of sociologists and those of other scientists are more striking than the dissimilarities.

[3] To our knowledge, no one study has compared the backgrounds of sociologists with those of independent professionals, but a comparison of more or less comparable data from different studies indicates that most kinds of independent professionals have come from higher social levels on the average than sociologists (Harmon, 1966; Duncan, 1965; and Zelan, 1967).

Table 2. Father's Occupation, by Date of Doctorate (in per cent)

Date of doctorate	*Duncan's Socio-Economic Index*									*No response*	*Total*	*Mean*
	0–19	*20–29*	*30–39*	*40–49*	*50–59*	*60–69*	*70–79*	*80–89*	*90–99*			
1947 and earlier	24.7	2.4	8.2	4.7	25.9	7.0	4.7	10.6	7.0	4.7	100.0	48.4
1948–1957	21.5	2.1	2.8	11.1	29.2	9.7	7.6	6.9	6.2	2.8	100.0	49.9
1958–1967	20.7	4.2	7.1	11.8	19.5	10.6	10.6	6.5	7.1	1.8	100.0	49.7
No doctorate reported	22.6	3.2	9.7	0.0	35.5	3.2	3.2	0.0	16.2	6.4	100.0	49.0
Total	21.9	3.0	6.1	9.3	25.2	9.1	7.9	7.0	7.4	3.0	100.0	49.5
Male experienced civilian labor force, 1950[a]	48.9	10.8	11.4	9.3	5.6	6.7	3.8	2.5	1.0	—	100.0	29.1[b]

[a] Data for 1950 rather than 1940 are reported because the occupational titles used in 1940 do not correspond precisely with the titles for which SEI values are available.

[b] This value differs slightly from that reported by Duncan because it was computed from grouped data.

economic Index (SEI)[4] are so small that they could easily reflect only sampling variability. The mean SEI of the fathers is about the same in each of the three cohorts *in spite of* the considerable upward shift in the occupational distribution of males in the United States during the past several decades. Therefore, if our respondents were perfectly representative of the universe, the data would show a decline in the average relative position of sociologists' fathers in the occupational hierarchy. Because of sampling variability, we cannot confidently conclude that this change has occurred, but, on the other hand, it seems unlikely that sampling error masks any appreciable increase in the average social level from which sociologists have been recruited. Use of the broad cohorts may obscure a small and very recent increase in the average social level of sociologists' families of orientation, because the mean SEI for the fathers of the sociologists who received their doctorates in 1963 and later is 51.3—somewhat higher than any of the means shown in Table 2. However, even if this apparent change is not the result of sampling error, it is only about proportional to the upward shift in the occupational distribution of the entire male labor force.

Nor is there evidence in our data that sociology has become less of an avenue of upward mobility for those attracted to it. Since sociologists are in the range of 80 through 89 on the SEI, at least 77.7 per cent of the respondents in the earliest cohort shown in Table 2 are upwardly mobile, compared with 84.6 per cent in the latest cohort. At least 87.5 per cent of the respondents who received their doctorates in 1963 and later have risen above the occupational levels of their fathers. Furthermore, at least a fourth of the upwardly mobile sociologists in each cohort have traveled a very substantial distance up the occupational hierarchy. More than a fifth of the respondents had fathers in the SEI range of 0 through 19, and the very highest ranking occupations in that level include automobile mechanics, parking attendants, barbers, bartenders, waiters, and the like.

Our data agree with the impression that the modal social origins of sociologists are "lower-middle-class." More than a fourth of the fathers were in the SEI interval of 50 through 59, which includes such occupations as bank tellers, bookkeepers, labor-union officials, mail carriers, photographers, clergymen, and several kinds of foremen. Although the term "lower-middle-class" is ambiguous and has no precise and generally accepted meaning, it might also be applied to the fathers in the SEI intervals of 40 through 49 and 60 through 69. If so, around 45 per cent of the respondents have lower-middle-class origins, about a third have lower-class origins, and almost a fourth have upper-middle or upper-class origins. Each broad social level has considerable, although not proportional, representation among sociologists, and therefore the discipline as a whole should be able to view the pattern of social stratification in the United States from a variety of perspectives. . . .

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[4] Duncan's SEI is an estimate of occupational prestige based upon the income and education of males in the occupations in 1950 (Duncan, 1961).

Regional Background

Early American sociology thrived especially in the Midwest (the North Central Region, in census terminology), perhaps because of the very rapid urbanization and social change in the region in the late nineteenth and early twentieth centuries and the relative lack of academic traditions to hamper introduction of sociology into college curricula (Hinkle and Hinkle, 1954:3, 19). A large percentage of the founding fathers were from the Midwest or spent their careers there, and by the 1920's Midwesterners were clearly dominant in the American Sociological Society (Hinkle and Hinkle, 1954:3, 19).

Vestiges of the traditional Midwestern dominance remain in contemporary data on the regional origins of sociologists (See Table 3). In the earliest cohort, the proportion of our respondents from the Midwest is about 30 per cent greater than it would be if sociologists had been recruited randomly from the population of young adults in the United States in the 1920's and 1930's. However, the proportion of American-reared sociologists from the Midwest in the latest cohort is almost exactly the same as the proportion of the total population in that region in 1950, the census year when many members of that cohort were adolescents. The difference between the earliest and the latest cohort in percentage from the Midwest approaches statistical significance (p = .07) and the monotonic decrease in the percentage from the earliest to the latest cohort also indicates the apparent trend has very likely been real. At least by the criterion of regional origins, the Midwestern domination of American sociology is apparently ending.

Table 3. Region of Origin,[a] by Date of Doctorate (in per cent)

Date of doctorate	*Northeast*	*North Central*	*South*	*West*	*Outside U.S.*	*No response*	*Total*
1947 and earlier	24.7	36.5	14.1	15.3	5.9	3.5	100.0
1948–1957	29.2	29.2	16.7	13.9	10.4	0.7	100.0
1958–1967	33.7	26.6	13.6	17.2	6.5	2.4	100.0
No doctorate reported	35.5	32.3	9.7	3.2	16.1	3.2	100.0
Total	30.5	29.8	14.4	14.7	8.4	2.1	100.0
Regional distribution of U.S. population in 1940	27.2	30.4	31.5	10.9	—	—	100.0

[a] Based upon reported region of residence during late adolescence. The regions are delineated in the same manner as in the census reports.

In the earliest cohort, the ratio of the real to the "expected" or proportional percentage is even greater for Westerners than for Midwesterners. About 17 per cent of the American-reared sociologists in the cohort who reported a region of origin are from the West, but only 10 per cent of the total population of the United States was in that region in 1930. In other words, the proportion of Westerners in the cohort is about 1.7 times what it would be if there had been no regional selectivity in the recruitment of sociologists. In the latest cohort, Westerners are also substantially overrepresented, being about 1.4 times the "expected" or proportional number.

Perhaps the recent disproportionate recruitment of sociologists from the West has resulted from conditions similar to those that made sociology thrive in the Midwest in the 1920's and earlier. The region has experienced rapid urbanization, population growth, and social and cultural change and has lacked strong traditions. Also, the relatively high educational attainments—including especially the high level of college attendance—undoubtedly help make the region a good source of sociological talent.

The proportion of American-reared sociologists from the Northeast in the earliest cohort of respondents is about the same as the proportion of Northeasterners in the total American population in 1930. However, in the latest cohort, the proportion of Northeasterners is about 1.4 times the proportion to be expected on the basis of the regional distribution of the total population in 1950. Although the difference between the earliest and the latest cohort in percentage of Northeasterners is not statistically significant ($p = .09$), it is large enough to indicate a probable increased tendency for sociologists to be recruited from the Northeast. If so, this trend has occurred while the proportion of the population of the United States in that region has declined, from 28 per cent in 1920 to 24.9 per cent in 1960.

The Northeast is highly urbanized and secular and its population is relatively well educated, and therefore it is hardly surprising that it now produces a disproportionate share of sociologists. If Hinkle and Hinkle (1954:3) are correct, its earlier failure to do so resulted at least partly from academic traditions that impeded introduction of sociology into college and university curricula, and these traditions have been rather completely overcome only within the past two or three decades. In addition, the recent increase in recruitment of sociologists from the Northeast is related to an increase in the recruitment of Jews (see Table 5), who are highly concentrated in that region.

The representation of Southerners in American sociology is low and apparently is not increasing. In the earliest cohort, the proportion of Southerners is only 0.51 the proportion expected on the basis of the regional distribution of the total U.S. population in 1930, and in the latest cohort, it is only 0.48 the proportion of the U.S. population in the South in 1950. This small representation of Southerners partly reflects the very small representation of Negroes in American sociology (see Table 6), but the proportion of our respondents from the South is even much smaller than the 26.6 per cent of the white U.S. population that was in the South in 1940. Perhaps a major reason for the underrepresentation of Southerners is that the South is still more traditional than the other regions (Glenn and Simmons: 1967), is less urbanized, has lower average educational attainments, and has relatively poor schools and colleges. The degree of urbanization may be especially important, since the ranking of the regions in urbanization is the same as their ranking in ratio of real to "expected" contribution of sociologists to our latest cohort. That is, the West and Northeast are the most urbanized, and they contributed more than their proportional share of sociologists, whereas the South is least urbanized, and it contributed much less than its proportional share. The relationship is not so neat in the earlier cohorts, but perhaps only because of the complicating factor of Eastern academic traditions.

Size of Communities of Orientation

Whereas many of the early prominent American sociologists had rural backgrounds, only 29.8 per cent of our sample reported living in a rural community at any stage of their childhood or adolescence, and only 12.1 per cent have a strictly rural pre-adult background (see Table 4). Not surprisingly, the percentage of sociologists with rural backgrounds has declined as the United States has become more urbanized. Almost half the respondents in the earliest cohort spent some of their formative years in a rural setting, compared with only about a fifth of the latest cohort. More than a fifth of the earliest cohort lived only in rural communities, compared with only 6.5 per cent of the latest cohort. Therefore, any influences of rural backgrounds upon American sociological thought are clearly waning. For instance, the alleged anti-urban bias of American sociology may soon be replaced with an anti-rural bias, if that has not already occurred.

We have no data for the entire population comparable to the information on our

Table 4. Population Size of Communities of Orientation,[a] by Date of Doctorate (in per cent)

Date of doctorate	*Less than 2,500*	*2,500–49,999*	*50,000 and up*	*More than one size class*	*No response*	*Total*	*Lived in community of less than 2,500 at some pre-adult stage*
1947 and earlier	22.4	12.9	30.6	31.8	2.4	100.0	48.3
1948–1957	12.5	15.3	36.8	34.0	1.4	100.0	29.8
1958–1967	6.5	11.8	47.3	32.6	1.8	100.0	21.3
No doctorate reported	12.9	25.8	32.2	16.0	12.9	100.0	25.7
Total	12.1	14.2	39.4	31.7	2.6	100.0	29.8

[a] Respondents were not asked to report size of communities lived in for less than two years.

respondents in Table 4, but it is clear that the backgrounds of sociologists are somewhat more urban than they would be if recruitment had been random. In 1940, which is near the median time when our respondents became adults, 43.5 per cent of the U.S. population was rural, according to the definition of rural then used by the Bureau of the Census. This is well above the 29.8 per cent of our respondents who lived in rural localities any appreciable time during childhood or adolescence, and it is far above the 12.1 per cent with strictly rural backgrounds. Using the old census definition of rural, 40.4 per cent of the U.S. population was rural in 1950—almost twice the proportion of our latest cohort who reported any pre-adult rural residence.

Furthermore, unless there is substantial sampling error in our data, the origins of sociologists have become more urban at a rate that greatly exceeds the rate of urbanization of the population as a whole. From our middle to latest cohort the percentage who reported some rural residence declined by 8.5 points and the percentage with strictly rural backgrounds declined by 6 points, whereas the percentage of the U.S. population that was rural declined by only 3.4 points from 1940 to 1950. Stated differently, the proportion of our respondents with some rural background declined by 29 per cent from the middle to the latest cohort, and the percentage with strictly rural backgrounds declined by 48 per cent. In contrast, the proportion of the U.S. population that was rural declined by only 8 per cent from 1940 to 1950.

If this apparent trend is real, it and the continued urbanization of the entire population will soon make sociologists with rural backgrounds very rare. American sociology may be rapidly losing a perspective that probably has considerable value to the discipline.

Religious Background

The religious backgrounds of our respondents and their current religious preferences are shown in Table 5. We report the religious preferences of the mothers rather than of the fathers because we assume that mothers typically are more influential in the religious socialization of children. However, the fathers' and mothers' preferences were almost identical, in the aggregate, except that more of the fathers had no preference.

The comparison of the religious distribution of our respondents' mothers with that of the total U.S. population gives a rough indication of the religious selectivity in the recruitment of American sociologists. We use the 1957 data because they are probably the most accurate available. Ideally, we would use earlier data, since the median date when our respondents became adults is around 1940, but other data from national surveys indicate the distribution of the American population by religious preference did not change substantially from 1940 to 1957.

The most striking revelations in Table 5 are the underrepresentation of Catholics and the much greater than proportional representation of Jews. Apparently, there is only about a third of the "expected" or proportional number of sociologists with Catholic backgrounds, whereas sociologists with Jewish backgrounds are about six times the proportional number.

Some of the underrepresentation of Catholics no doubt reflects their relatively low rates of college attendance and completion in the recent past (Glenn and Hyland, 1967). However, this influence for low Catholic representation in sociology should be largely or completely offset by the fact that a relatively large proportion of Catholics are non-Southern, white, and urban—characteristics associated with disproportionate recruitment into sociology. It seems that a Catholic background, or perhaps education in Catholic schools and colleges, tends, or in the recent past has tended, to make people relatively disinclined to go into sociology.[5]

However, the data in Table 5 show an increase in the percentage of Catholics, and of persons with Catholic backgrounds, from the earliest to the latest cohort. The consistency of the data indicate the apparent trend has very likely been real, although none of the

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[5] Needless to add, the evidence for this interpretation is less than conclusive. The important influences could be correlates of religious background, rather than religious background itself. We do not have space here to go into the various controversies and complex issues concerning the possible effects of religious background upon attitudes toward science and the choice or nonchoice of scientific careers. For relevant data and discussion and references to related studies, see Greeley (1963), Warkov and Greeley (1966), and Datta (1967).

Table 5. Mother's, Late Adolescent, and Current Religious Preference, by Date of Doctorate (in per cent)

Date of doctorate	*Protestant*	*Catholic*	*Jewish*	*Other*	*None*	*No response*	*Total*
Mother's religious preference:							
1947 and earlier	72.9	7.1	14.1	1.2	2.4	2.4	100.0
1948–1957	62.5	8.3	16.7	4.2	6.3	2.1	100.0
1958–1967	52.7	10.1	24.3	5.9	4.7	2.4	100.0
No doctorate reported	58.1	3.2	12.9	0.0	12.9	12.9	100.0
Total	60.4	8.4	18.9	4.0	5.4	3.0	100.0
Late adolescent religious preference:							
1947 and earlier	63.5	5.9	11.8	1.2	12.9	4.7	100.0
1948–1957	51.4	6.9	12.5	4.2	22.9	2.1	100.0
1958–1967	41.4	10.0	19.5	5.3	20.7	3.0	100.0
No doctorate reported	45.2	3.2	12.9	0.0	25.8	12.9	100.0
Total	49.4	7.7	15.2	3.7	20.3	3.7	100.0
Current religious preference:							
1947 and earlier	49.4	5.9	8.2	2.4	31.8	2.4	100.0
1948–1957	39.6	5.6	11.8	2.8	36.1	4.2	100.0
1958–1967	29.6	8.3	16.0	5.3	37.9	3.0	100.0
No doctorate reported	38.7	3.2	12.9	0.0	32.3	12.9	100.0
Total	37.5	6.5	12.8	3.5	35.7	4.0	100.0
U.S. population in 1957	66.3	26.2	3.0	1.9	2.1	—	100.0

differences between the earliest and the latest cohort approaches statistical significance. Any increase in Catholics has been only moderate, at most, and at the apparent recent rate of change, persons with Catholic backgrounds will not be proportionally represented in American sociology within the next few decades.

Jewish representation in American sociology has long been high, and it is almost certainly increasing. The difference between the earliest and the latest cohort in percentage of Jews is either statistically significant or approaches significance in all sections of Table 5 (e.g., for mother's religious preference, $p =$ .04). In the latest cohort, the percentage of our respondents with Jewish mothers is about eight times what it would be if recruitment of sociologists had not been selective by religious background.

The high representation in sociology of Jews, and of persons with Jewish backgrounds, may be partly, but not entirely, explained by the high and apparently increasing representation of Jews among college graduates and professional workers in general. Although small sample sizes make national survey data on Jewish occupations and education rather unreliable, it appears that Jewish college graduates and professional workers are about twice the proportional or "expected" number (Glenn and Hyland, 1967:79; Lazerwitz, 1964:428). Even more important in explaining the large number of Jewish sociologists is the fact that the Jewish population, considered as a whole, has experienced rapid and substantial upward mobility during the past few decades (Glazer, 1958), and the strong intellectual emphasis of Jewish culture has led many Jews into the academic avenue of upward mobility. Jewish representation in psychology, economics, political science, philosophy, and the physical and biological sciences may be as high as it is in sociology, although perhaps the concern of Jews with their minority status has given them an unusual interest in sociology.

More than a third of our respondents, contrasted with only about 2 per cent of the respondents to most American national surveys, said they have no religious preference. The finding will surprise few sociologists, but in view of the explanations often given for the widespread apostasy of social scientists, it is of interest that 20.3 per cent of our respondents said they had no religious preference in late adolescence. The explanation that exposure to sociology, and socialization into the discipline, tend to kill religious belief could hardly account for much of the apostasy around ages seventeen or eighteen. Evidently, sociology is rather likely to recruit persons who have already rejected conventional religious beliefs. Since the percentage who reported no religious preference in late adolescence is greater in the latest than in the earliest cohort ($p = .09$), the tendency for sociology to recruit persons with no religion may well have increased.

Race

Only seven, or 1.6 per cent, of our respondents identified themselves as Negro, and of the 136 respondents who reported receiving doctorates before 1953, not one reported he was Negro (see Table 6). However, among those who received their doctorates after 1962, 4.5 per cent are Negro, and the difference between the earliest and the latest cohort shown in Table 6 is statistically significant ($p = .02$). Therefore, our data indicate that Negro representation in American sociology is very low but has recently increased.

Table 6. Percentage Negro, by Date of Doctorate

Date of doctorate	*Per cent*	*(N)*[a]
1952 and earlier	0.0	(136)
1953–1957	1.1	(93)
1958–1962	1.2	(81)
1963–1967	4.5	(88)
No doctorate reported	3.2	(31)
Total	1.6	(429)

[a] Number of respondents on which percentage is based.

There is evidence that the percentage of Negroes among the active members and fellows of the American Sociological Association is somewhat higher than our data indicate. In a study conducted in 1966, Conyers (1968) located 121 living Negroes with doctorates in sociology, and if all these were members of the ASA, they were about 3.3 per cent of the active members and fellows. Of course, some of these people may not have been members of the ASA, and the exclusion of females may have reduced the percentage of Negroes in our sample. On the other hand, the difference between 1.6 and 3.3 per cent could easily have resulted from sampling error.

In any event, the number of Negroes among the active members and fellows of the ASA is only about a third of proportional representation, at most. Negro representation is apparently smaller in sociology than in the professions as a whole, and it is probably only slightly greater in sociology than in such fields as medicine, dentistry, and college teaching as a whole.[6] Of course, the proportion of Negroes may be somewhat higher among all persons working as sociologists than among the active members and fellows of the ASA.

Political Background

We asked our respondents to identify their political views during late adolescence, and their fathers' views during the same period, on a five-point scale varying from "very liberal" to "very conservative" (see Table 7). We also asked for the respondents' current political orientations, but unfortunately we did not structure the alternatives so that the responses would be comparable to those on late adolescent orientations. Rather, we allowed the respondents to identify themselves as liberal, moderate, or conservative or to use some other label (see Table 8).

The responses to all the political questions must be interpreted with caution, because the meanings attached to the terms undoubtedly varied somewhat among the respondents, and any attempt to place political views on a continuum violates the variety and complexity of reality. Nevertheless, the data give a rough picture of the political backgrounds and current political views of the respondents.

One might suspect that such trends as the increased recruitment of Jews and the decreased recruitment of persons with rural backgrounds have led to an increase in sociologists with liberal backgrounds, but our data do not indicate this has happened. There is no apparent trend from the earliest to the latest cohort in the reported political orientations of either the fathers or the respondents. Although a larger percentage of the earliest than of the latest cohort said they were liberal in their current views, an examination of the "other labels" reveals virtually equal proportions "left-of-center" in all the cohorts (Table 8). On the other hand, there could be a systematic difference in the meanings attached to the terms "liberal" and "conservative" by the respond-

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[6] For data on nonwhite representation in the professions in 1968, see Glenn (1969), and for data on the representation of Negro males in specific professions in 1960, see Broom and Glenn (1965:112–113).

ents in the different cohorts, but we doubt that any such difference is very great.

Our data agree with other relevant evidence in indicating a substantial majority of American sociologists to be politically liberal.[7] The percentage "left-of-center" on the political spectrum is greater than the 63.2 per cent who said they were liberal, because many of those who used "other labels" said they were radical or used other "leftist" terms. Only 2.6 per cent admitted to being conservative, and few of the "other labels" indicated an unambiguous "right-of-center" orientation. Although we have no strictly comparable data on the political identifications of the total U.S. population, our respondents are clearly unusually liberal, on the average, and also are unusually liberal for professional workers. In 1961, a Gallup poll asked a national sample which political party they would prefer if there were only two parties, one for liberals and one for conservatives. About 30 per cent chose the liberal party, another 30 per cent chose the conservative party, and 40 per cent had no opinion. Among male professional workers, 38.3 per cent chose the liberal party, 44.3 per cent chose the conservative one, and 17.4 per cent had no opinion.

The unusual prevalence of liberalism among sociologists clearly does not result primarily from an unusual amount of liberalism in their families of orientation. According to the respondents' reports, the fathers were distributed very nearly symmetrically along the

Table 7. Father's and Late Adolescent Political Orientation, by Date of Doctorate (in per cent)

Date of doctorate	*Very liberal*	*Moderately liberal*	*Middle of the road*	*Moderately conservative*	*Very conservative*	*No response*	*Total*
Father's political orientation:							
1947 and earlier	14.1	28.2	16.5	29.4	8.2	3.5	100.0
1948–1957	11.1	24.3	19.4	32.6	5.6	6.9	100.0
1958–1967	13.6	29.0	19.5	25.4	8.3	4.1	100.0
No doctorate reported	19.4	22.6	16.1	22.6	3.2	16.1	100.0
Total	13.3	26.8	18.6	28.4	7.0	5.8	100.0
Late adolescent political orientation:							
1947 and earlier	41.2	37.6	14.1	4.7	1.2	1.2	100.0
1948–1957	45.8	32.6	10.4	4.9	0.7	5.6	100.0
1958–1967	42.6	34.3	12.4	4.7	1.8	4.1	100.0
No doctorate reported	35.5	25.8	16.1	6.4	0.0	16.1	100.0
Total	42.9	33.8	12.4	4.9	1.2	4.9	100.0

Table 8. Current Political Orientation, by Date of Doctorate (in per cent)

Date of doctorate	*Liberal*	*Moderate*	*Conservative*	*Other labels*	*No response*	*Total*
1947 and earlier	72.9	15.3	3.5	7.1	1.2	100.0
1948–1957	63.9	14.6	1.4	16.7	3.5	100.0
1958–1967	60.4	17.2	2.4	15.4	4.7	100.0
No doctorate reported	48.4	19.4	6.5	12.9	12.9	100.0
Total	63.2	16.1	2.6	14.0	4.2	100.0

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[7] Most of the other evidence deals with party preference rather than identification with such labels as "liberal" and "conservative." See Palmore (1962), Eitzen and Maranell (1968), Lazarsfeld and Theilens (1958), and Turner et al. (1963).

The data on the political orientations of sociologists are not consistent with the generalizations that (1) high-status people tend to be politically conservative, and (2) upwardly mobile people in the United States tend to be even more conservative than the stable people in the social levels they enter (see Lopreato, 1967). Furthermore, we found virtually no difference in reported political views between the stable and upwardly mobile sociologists (cf. Palmore, 1962).

political spectrum, except that a few more were listed as "very liberal" than as "very conservative." The distribution is bimodal, with many fathers being "moderately liberal" and "moderately conservative" and relatively few being "middle of the road" or at either extreme. Although we cannot be certain, it seems that the fathers' political sentiments were probably more or less representative of those in the total population at the various times when our respondents were late adolescents. This is true, of course, only if most of the respondents imagined the total distribution to be more or less symmetrical and rather accurately perceived their fathers' relative positions in the distribution.

However, it appears that a large majority of sociologists were liberal before they were recruited into the discipline. Well over two-fifths of the respondents said they were "very liberal" in late adolescence, and more than three-fourths said they were either very or moderately liberal. Only 6.1 per cent said they were conservative, and a meager 1.2 per cent admitted to being "very conservative." To be sure, the recollections of late adolescent political sentiments were probably colored somewhat by current views, but, nevertheless, it seems that the respondents, considered as a whole, experienced no major political shift after late adolescence. According to their reports, virtually the same percentage were "left-of-center" in late adolescence as when they responded to the questionnaires.

Although we have no strictly comparable data on all late adolescents at any of the times when the respondents were in that stage of the life cycle, it seems virtually certain that our respondents were more liberal, on the average, than their contemporaries. The most nearly comparable data are for young adults rather than for late adolescents and were gathered after any of our respondents were adolescents. For instance, in the 1961 Gallup poll referred to above, 36.3 per cent of the persons aged twenty-one through twenty-five chose the liberal party, 18.6 per cent chose the conservative party, and 45.1 per cent had no opinion. Thus the ratio of liberals to conservatives was 1.95. Among those aged twenty-one through twenty-three, 39.6 per cent were liberals, 13.4 per cent were conservatives, and 47.1 per cent had no opinion—a ratio of liberals to conservatives of 2.95. However, among those in this age range who had been to college, 41.7 per cent were liberals, 18.8 per cent were conservatives, and 39.6 per cent had no opinion—a ratio of liberals to conservatives of 2.22. In contrast, this ratio among our respondents in late adolescence was 12.57.

In brief, sociologists seem to differ politically from other adults in the United States, considered as a whole, in that (1) more of the sociologists were liberal in late adolescence, and (2) they have not been so inclined to become more conservative as they have grown older. These two factors may be about equally responsible for the unusual liberalism of sociologists. The discipline tends to recruit people who are already liberal, and then influences within the discipline apparently sustain liberalism within each aging cohort.

We have no data that can provide much insight into why so many adolescents who eventually go into sociology reject their fathers' political views. However, it seems that some of the influences that lead young people to reject conventional religion and the political views of their parents and of most other people in their communities may also be conducive to an interest in sociology. Or it could be that the rejection of traditional and conventional beliefs and values, for whatever reasons, inclines young people toward sociology because it tends to give them the freedom and flexibility of thought necessary to examine their society and culture in a relatively objective and dispassionate manner. Obversely, many young people who have not rejected conventional and conservative values may be deterred from going into sociology because it threatens their values and beliefs and the psychological security they derive from them.

Undergraduate Major

Our findings on undergraduate majors agree with data presented by Sibley (1963: 87) in showing that a large proportion of sociologists did not major in sociology (see Table 9). Sibley reports that well under half the persons who received Ph.D.'s in sociology in 1957–1959 majored in sociology as undergraduates, but he reports substantially larger percentages among sociology graduate students and M.A.'s. One might hope that Sibley's data reflect a tendency for undergraduate departments to give stronger pre-professional training in sociology and a consequent increase in the recruitment of sociologists from undergraduate sociology majors. However, Sibley suggests that his data may reflect instead a high drop-out rate of undergraduate sociology majors from sociology graduate programs. Our data support this interpretation, because they show no recent tendency for a larger percentage of fellows and active members of the ASA to be recruited from undergraduate majors in sociology.

Although less than half our respondents majored in sociology, more than two-thirds majored in either sociology, one of the other social sciences, or psychology; therefore, it appears that a majority did not experience a marked change of interests or career plans after their late undergraduate days. For instance, only 4.2 per cent switched into sociology from the physical and biological sciences. Even many of those listed under "the arts" and "other" majored in such fields as English literature and philosophy—fields rather closely related to sociology.

We suspect that the few sociologists with backgrounds in mathematics and the "hard" sciences have exerted an influence upon sociology disproportionate to their numbers. However, it is clear from our data, and from Sibley's findings,[8] that the dearth of sociologists with more than superficial training in mathematics and the physical and biological sciences must considerably restrict the influence of these disciplines upon sociological thought.

Table 9. Undergraduate Major, by Date of Doctorate (in per cent)

Date of doctorate	*Sociology*	*Other social sciences*	*Psychology*	*Biological sciences*	*Physical sciences*	*Arts*	*Other*	*No response*	*Total*
1947 and earlier	38.8	22.4	5.9	2.4	2.4	8.2	15.3	4.7	100.0
1948–1957	44.4	18.8	6.2	0.7	2.8	13.2	11.8	2.1	100.0
1958–1967	40.2	18.9	10.0	0.6	4.1	14.8	10.2	1.2	100.0
No doctorate reported	29.0	16.1	0.0	3.2	0.0	6.4	9.7	35.5	100.0
Total	40.6	19.3	7.2	1.2	3.0	12.4	11.7	4.7	100.0

SUMMARY

The most important recent trends in the recruitment of sociologists, according to our data, are (1) a decline in the proportion from the Midwest and an increase in the proportion from the Northeast; (2) a steep decline in the proportion with rural backgrounds; (3) an increase in the proportion from Jewish families; and (4) an increase in the proportion who are Negro. Perhaps as important as these trends is the lack of evidence for some trends that might be expected for one reason or another. For instance, there apparently has been no recent tendency for a larger percentage of sociologists to come from the higher social levels, the South, politically liberal families of orientation, or undergraduate sociology majors.

Segments of the population that have not contributed their proportional share of sociologists include Negroes, Catholics, Southerners, the working class, and the rural population. On the other hand, Jews, Westerners, the "lower-middle class," and families headed by clergymen have contributed considerably more than their proportional share.

Our data indicate that a sizable proportion of the people who later become sociologists reject conventional religion and the conservative or moderate political views of their fathers by the same time they reach late adolescence. This proportion is almost certainly larger than among other late adolescents, considered as a whole, although we have no strictly comparable data on the others. Therefore, it seems that the liberalism and apostasy prevalent among sociologists result to a large extent from selective recruitment rather than from socialization within the discipline or exposure to sociological perspectives and information.

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[8] Sibley (1963:87) reports that among sociologists who received Ph.D.'s in 1957–1959, only 3 per cent had undergraduate majors in the physical and biological sciences and less than 1 per cent had majors in mathematics or statistics.

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Family Religious Background and Early Scientific Creativity

Lois-ellin Datta

The data discussed in this paper were collected during the initial phase of a longitudinal study on the development of potentially creative scientists.[1] The primary question during this phase was whether or not variables previously reported either to characterize unusually eminent men or to differentiate more and less eminent scientists would differentiate young men of high potential creativity in science from those who showed less potential creativity as early as the senior year of high school, that is, if hypotheses developed on the basis of adult data would apply to fledgling scientists.

That achievement in the area of science varies with religious background has been reported by some and cited by many. Although not all writers have assumed a direct causal relationship between religion and achievement in science, implicit in much of the literature is the hypothesis that religious background represents an important socialization influence in the development of attitudes and values which may facilitate or inhibit scientific interest and attainment. This hypothesis suggests that values congruent with and supportive of scientific development may not be found equally in all major religions and, therefore, that the religious backgrounds of more eminent scientists may be different from those of less eminent scientists. . . .

The results of a series of studies extend-

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[1] M. B. Parloff and L. Datta, "Personality Characteristics of the Potentially Creative Scientists," in J. Masserman, editor, *Science and Psychoanalysis*, Vol. 8, New York: Grune & Stratton, 1965.

ing from 1926 to 1965 have indicated consistently that eminent American scientists are not a random sample of the general population of church members. In comparison to the proportions in the general population, Catholics, Baptists, Lutherans and Methodists (the latter three to be referred to as Protestant I groups) are considerably underrepresented among outstanding scientists while Congregationalists, Friends, Episcopalians, Presbyterians and Unitarians (to be referred to as Protestant II groups) are overrepresented, in terms of both current affiliation and family religious background.[2] Jews, who were reported underrepresented in earlier studies, have more recently been found to be overrepresented among outstanding young physical scientists and Nobel Prize winners.[3]

Despite the frequency with which these data are interpreted in terms of ascribed group values congruent with the scientific attitude, there is some question as to whether or not a relationship has indeed been demonstrated between scientific attainment and religious background.[4] Among the methodological limitations of these studies are: (a) inadequacies of the general population data, (b) lack of information on the parental or current affiliations of large proportions of the creative samples, and (c) the failure of most studies to demonstrate a relation in the sense of covariation between scientific attainment and religion due to the absence of data comparing non-eminent men of the same educational level and field of interest. . . .

. . . In many reports, factors associated with eminence in science have not been distinguished from factors related to entrance into science as a career or from factors affecting entrance into the professions in general. There is apparently no information on the religious background or affiliation of eminent scholars in fields other than science and of controls matched for age, education and field of interest. Only two studies have been reported which compare eminent with less eminent scientists.

Chambers' subjects (Ss) were 218 eminent and 220 less eminent scientists matched for age, education and opportunity to do research in two fields, psychology and chemistry. Response options both for religious preference and for parents' religion items were "Protestant, Catholic, Jewish, and Other."[5] His results for religious preference are consistent with previous reports comparing the affiliations of Protestant and Catholic scientists with the general population. The only significant difference between eminent and control groups was the tendency for creative scientists to report no religious affiliation. Comparisons of the religious background responses indicated that 'Jewish" or "none" were more characteristic of the creative than of the control scientists while Protestant or Catholic backgrounds were relatively more frequent among the control scientists. Generalizations from Chambers' data are limited, however, by the fact that about 40 percent of each of the originally selected groups failed to return the questionnaire.

Clark, whose Ss were psychologists, reported data from a questionnaire returned by 1,520 scientists (88 percent of the possible sample).[6] Both for significant contributors and control groups matched for education and period during which the Ph.D. was received, it was found that, based on the religious background of their parents, Catholics and Protestant I groups were underrepresented in comparison with the general population and Protestant II and Jewish groups were overrepresented. While parental religious affiliation did not differentiate between the 1930–1934 and 1934–1939 eminent and less eminent psychologists, by 1940–1944 the significant contributor and control samples reporting father's religion as Catholic were 6 percent and 13

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[2] W. S. Ament, "Religion, Education and Distinction," *School and Society*, 26 (1927), pp. 399–406; Chambers, "Comments," *Science*, 147 (1965), p. 67; C. Fry, "Religious Affiliations of American Leaders," *Scientific Monthly*, 36 (1933), pp. 241–249; H. C. Lehman and P. A. Witty, "Scientific Eminence and Church Membership," *Scientific Monthly*, 33 (1931), pp. 544–549; A. Roe, *The Making of a Scientist*, New York: Dodd Mead, 1953.

[3] F. Bello, "The Young Scientists," *Fortune*, 49 (1954), pp. 142 ff.; T. Levitan, *The Laureates: Jewish Winners of the Nobel Prize*, New York: Twayne, 1960.

[4] L. Datta, "Study of Creative Scientists: Comments on Methodology," *Science*, 147 (1965), p. 66.

[5] J. A. Chambers, *Science* 147 (1965), p. 67.

[6] K. E. Clark, *American's Psychologists*, Washington: American Psychological Association, 1957.

percent; as Jewish, 27 percent and 16 percent; as Protestant I, 27 percent and 25 percent; as Protestant II, 37 percent and 42 percent, and as Other or None, 0 percent and 2 percent, respectively.

METHOD

More attention has been paid in the literature to problems in the selection of creative subjects than to problems in the selection of control subjects, although the nature of the control group has varied most and thus is critical in studies comparing more and less creative individuals. Too homogeneous a sample may fail to reveal differences that would appear in a group encompassing a greater range of creativity. Greater heterogeneity, on the other hand, may be associated with variables other than creativity which are themselves related to the factors under study. The degree to which differences appearing in such heterogeneous groups are attributable to creativity rather than to the correlated other variables is often difficult to estimate.

The population selected for study was Westinghouse Science Talent Search (STS) applicants. The primary reason for choosing this group was the availability of a product criterion of creativity. Such a criterion would appear to be more similar to the criteria by which adult samples of creative scientists had been identified than would the largely unvalidated "creativity" tests available or other measures such as teacher and peer ratings which have been used in studies of creative adolescents.

The original experimental design involved variation of creativity and scientific aptitude and required at least four groups of young scientists: high aptitude, high creativity; high aptitude, low creativity; low aptitude, high creativity; low aptitude, low creativity. A pilot study indicated that there were no low aptitude, high creative subjects in the STS group initially selected for study. We then attempted to obtain a group of high aptitude students that was as heterogeneous as possible with regard to early scientific creativity without also having correlated differences in interest in science, scientific knowledge, and general scholastic attainment.

The sample of young scientists was selected from high school seniors who competed in the 1963 Westinghouse Science Talent Search. This competition is aimed at discovering, ". . . boys and girls whose scientific skill, talent and ability indicate potential creative originality. . . ."[7] Of the more than 2,500 male STS applicants, the 573 young men scoring above the 80th percentile on a science aptitude test (SCAT) devised by Science Service, Inc., were selected for further study. Only male applicants were considered since there were too few high aptitude female students for the study contemplated.

Each of the subjects had submitted to the STS a report describing his independently conducted research project. This project was initiated by the student and conducted by him over as long a period as he felt necessary, ranging from a few weeks to three years. The projects were scored for "creativity and potential creativity" by the STS judges.

The standards for this judging have been refined over the period of 22 years since the competition was initiated. The judges were selected on the basis of their acknowledged eminence in one of the following fields: Biology, Chemistry, Mathematics, Medicine, Psychology, Psychiatry, and Physics. Each student's project was rated independently by pairs of judges expert in the area of science concerned. The judges assigned each project a letter grade ranging from A, "Unusually original," through D, "Pedestrian," and E, "A poor essay; rehash of material read with little evidence that it was absorbed; thinking obviously not straight." A 14.0 (A) to 1.0 (E−) scale was used to obtain numerical equivalents of the judges' ratings.

Although some of the projects were judged to be of adult professional calibre, the majority of the ratings indicate potential creativity as demonstrated by the way in which the student went about the work and by the originality and value of the project in terms of information and equipment available to high school seniors. We will thus refer to the "potential scientific creativity" ratings (PSC) of the projects.

On the basis of the project ratings, the

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[7] H. A. Edgerton and S. H. Britt, "Science Talent in American Youth," *Science,* 101 (1945), pp. 247–248.

sample was divided into three groups to represent different levels of potential scientific creativity. Group I, High Potential Creativity, included 112 Ss whose project scores ranged from 14.0 to 9.5 (A to B−); Group II, Moderate Potential Creativity, included 137 Ss whose project scores ranged from 9.0 to 6.5 (C+ to C−); and Group III, Low Potential Creativity, included 287 Ss with project scores of 6.0 and lower (D+ to E−). Since the selection procedures were not consistent with those of the Science Talent Search, the group will be referred to as the Potentially Creative Scientist sample (PCS).

Comparisons (a) between the selected sample and a sample of non-eligible male applicants and (b) among groups I, II, and III indicate that the sample includes students at the high and low extremes of the distribution of creativity ratings for the population of male STS applicants, without correlated variance in science aptitude and verbal and mathematical achievement.

(A) Examination of the project ratings of a sample of young men who scored below the 80th percentile on the SCAT indicated that lowering the criterion of eligibility for the PCS sample would increase the number of subjects in PCS III without a proportional increase in the number of subjects in PCS I, thus reducing the science aptitude scores of the less creative subjects to significantly below the scores of the more creative subjects. None of a sample of students with SCAT scores in the lowest 20th percentile and few students in a sample of applicants with SCAT scores in the intermediate ranges submitted projects judged as high in potential creativity. The mean project rating of the 287 high aptitude, low creativity Ss (3.48) was not significantly different from the mean project rating (3.57) of the sample of 227 non-eligible subjects (CR = 0.9, ns). The PCS sample thus includes virtually all of the "high creative" Ss and a group of 287 Ss whose creativity ratings were as low as those in the non-eligible sample.

(B) In Table 1 are given the mean scores of the project ratings, the SCAT, and of the Scholastic Aptitude Test-Verbal (SAT-V) and the Scholastic Aptitude Test-Mathematical (SAT-M) administered by the Educational Testing Service for groups I, II, and III. Analysis of variance comparisons showed that there were no significant differences among the groups on the measures of scientific and scholastic aptitude and that the project ratings differed significantly (F = 169.86, df 2/500, $p < .001$). Students in the three groups were similar in scientific aptitude, in verbal and mathematical ability and indicated equal interest in becoming scientists. They differed reliably, and, in terms of the judges' descriptions, meaningfully, on demonstrated potential for scientific creativity.

A four-hour battery of personality, attitude, and social history questionnaires was administered by mail at the time of high school graduation to each of the Ss. Complete returns were received from 95 percent of PCS I, 92 percent of PCS II and 94 percent of PCS III subjects. This paper reports data obtained from the personal history questionnaire.

RESULTS AND DISCUSSION

The distributions of family religious backgrounds for each of the three groups are given in Table 2. They indicate that the proportion of students from Catholic, Lutheran, Methodist and Presbyterian backgrounds tends to decrease as potential scientific creativity in-

Table I. Mean Scholastic Aptitude and Scientific Aptitude Test Scores of 536 Male High School Seniors by Potential Scientific Creativity

	Potential scientific creativity			
Test	*I* (*high*)	*II* (*middle*)	*III* (*low*)	*P*
Scientific Aptitude (SCAT)	67.56	67.99	65.54	n.s.
Verbal Aptitude (SAT-V)	686.75	685.62	674.37	n.s.
Mathematical Aptitude (SAT-M)	721.93	722.75	721.22	n.s.
Potential Scientific Creativity Rating	10.87	7.32	3.48	<.001

Table 2. Percentage Distribution by Family Religious Background for Students Classified by Potential Scientific Creativity

Family religious background	*Potential scientific creativity*			*Total*
	I (high)	*II (middle)*	*III (low)*	
Catholic	8.9	8.0	12.2	10.4
Protestant I	10.8	18.3	23.1	19.2
Baptist	5.4	5.2	5.2	5.2
Lutheran	1.8	2.9	7.0	4.9
Methodist	3.6	8.0	10.5	8.4
Mormon	0.0	2.2	0.4	0.7
Protestant II	15.2	14.6	17.7	16.4
Congregational	2.7	2.9	2.4	2.6
Episcopalian	6.2	1.5	5.6	4.7
Friends	0.0	0.7	0.0	0.2
Presbyterian	5.4	8.0	9.4	8.2
Unitarian	0.9	1.5	0.3	0.7
Protestant (unspecified)	2.7	5.1	2.4	3.2
Jewish	45.5	39.4	28.2	34.7
Mixed	9.8	5.8	7.7	7.6
Other religion	2.7	6.6	5.6	5.2
None	2.2	2.2	3.1	2.8
No response	1.8	0.0	0.0	0.4
Number of cases	112	137	287	536

creases. They also show that the percentage of students from Baptist, Congregational, Episcopalian and Unitarian backgrounds tends to be independent of potential creativity while the proportion of students from Jewish families increases from 28 percent in Group III to 45 percent in Group I.

The Ns, means, and standard deviations of the project ratings for Catholic, Protestant I, Protestant II, Other and Jewish students are shown in Table 3. The overall F (unweighted means analysis of variance) was 2.46; with 4,∞ df, an F of 2.37 is needed for significance at the 0.05 level of confidence.

Table 3. Means and Standard Deviations of Potential Scientific Creativity Ratings, by Family Religious Background

Family religious background	*Mean*	*Standard deviation*	*Number of cases*
Catholic	5.4	3.2	56
Protestant I	5.4	2.9	103
Protestant II	5.7	3.1	88
Other	6.1	3.2	103
Jewish	6.6	3.2	186

Both the first comparison between groups, of Catholic vs. Protestant I and Protestant II Ss (cf. hypothesis 1: Weber, Merton, Knapp and Goodrich) and the second comparison, of Catholic and Protestant I Ss. v. Protestant II Ss (cf. hypothesis 2: Lehman and Witty) yielded Scheffé ratios of less than 1.00. (With 4, ∞ df, a Scheffé ratio of 3.08 is required for significance at the 0.05 level of confidence.) There is little evidence in this sample that a fundamentalistic religious background is inimical to early scientific attainment or that a liberal Protestant background is favorable, nor is there any evidence that the potential scientific creativity of these Ss is associated with a Catholic/Protestant dichotomy. The only significant association is between potential scientific creativity and Jewish origin. The Scheffé ratio for this comparison was 3.30. Comparisons between pairs of means indicate that the creativity ratings of Catholic, Protestant I, Protestant II and Other subjects did not differ significantly from each other and that all means except for that of the Other group were significantly lower than the mean project rating of Jewish students.

The attainment of Jewish students is consistent with Feuer's discussion of the congruence between the characteristics of the scientific intellectual and Jewish values.[8] As noted above, there is precedence in the literature for considering these results in terms of similar ascribed values, for example, a "traditional" Jewish stress on achievement, on the possibility of rational mastery of the world, on the value of learning, on a joy in knowledge.[9] It seems premature, however, to conclude that the association is direct until the influence of other factors related to both religious background and scientific attainment has been considered. Davis noted that the association of socioeconomic status and religious background may affect educational opportunities and thus the opportunity for eminence in scholarly fields.[10] The disproportionately high number of eminent scientists originating in certain areas of the country and in larger cities was interpreted by Thorndike as an effect of differential cultural and educational opportunities.[11]

While there was no difference in the proportions of Jewish and non-Jewish subjects of high, relatively high, and middle-to-lower socioeconomic status in the PCS sample, significantly more Jewish than non-Jewish Ss came from New York state (66 percent vs. 12 percent) and from larger cities (72 percent vs. 27 percent). To test the possibility[12] that a combination of environmental circumstances might favor subjects of Jewish origin, a four-way unweighted means analysis of variance was computed for city size (cities of more than 500,000 vs. all smaller cities), home state (New York vs. all others), religious background (Jewish vs. all others), and socioeconomic status as defined by the Hollingshead-Redlich Index. (I is high; II is relatively high; and III–V is middle-to-lower SES.[13] There were no main effects or interactions significant at the .05 level except for that of city size and socioeconomic status.

Since the Ns of the 24 cells ranged from three to 83 Ss, a second unweighted means analysis of variance was computed combining New York with other states. The results of this analysis are shown in Table 4. The main effect of religion and the interactions of (a) religious background by size of home town and (b) size of home town by socioeconomic status were significant at the .05 level. The Ns, means, and standard deviations of the 12 subgroups on which the analysis was computed are shown in Table 5. Exact comparisons indicate:

Table 4. Unweighted Means Analysis of Variance of Potential Scientific Creativity Ratings of 517 Male High School Seniors by Religious Background, Size of Home Town, and Socioeconomic Status

Source	*df*	*Mean square*	*F*
A (religion)	1	61.94	6.30*
B (size of home town)	1	1.49	—
C (socioeconomic status)	2	16.38	1.67
A x B	1	43.18	4.40*
A x C	2	9.23	—
B x C	1	30.87	3.10*
A x B x C	2	26.20	2.67
Within cells	506	9.80	—

* With 1, ∞ d.f., an F of 3.84 is significant at the 0.05 level of confidence. With 2, d.f., an F of 2.99 is significant at the 0.05 level of confidence.

> a. For subjects residing in larger cities, early scientific attainment is unrelated to religious background, to socioeconomic status, or to a combination of these.
>
> b. For students of non-Jewish origin, the combination of residence in smaller towns and lower socioeconomic status appears to reduce early scientific attainment.
>
> c. For Jewish and non-Jewish subjects of high socioeconomic status, early scientific attainment is not related to size of home town.
>
> d. For subjects of Jewish origin, the combination of residence in smaller towns and lower socioeconomic status is not associated with as marked a decrease in early scientific attainment.

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[8] L. S. Feuer, *The Scientific Intellectual*, New York: Basic Books, 1963.

[9] B. C. Rosen, "Race, Ethnicity and the Achievement Syndrome," *American Sociological Review*, 24 (1959), pp. 47–60.

[10] B. Davis, "Eminence and Social Origin," *American Journal of Sociology*, 59 (1953), pp. 11–18.

[11] E. L. Thorndike, "The Origin of Superior Men," *Scientific Monthly*, 56 (1943), pp. 424–432.

[12] I am indebted to Anne Roe for this suggestion.

[13] A. B. Hollingshead and F. C. Redlich, *Social Class and Mental Illness: A Community Study*, New York: Wiley, 1958.

Table 5. Means and Standard Deviations of Potential Scientific Creativity Ratings for 517 Male High School Seniors by Religious Background, Size of Home Town, and Parental Socioeconomic Status

Size of home town and parental socioeconomic status	*Number of cases*		*Mean*		*Standard deviation*	
	Jewish	*Non-Jewish*	*Jewish*	*Non-Jewish*	*Jewish*	*Non-Jewish*
500,000+						
High	34	30	6.13	6.12	2.77	2.76
Relatively high	44	27	6.31	6.65	2.94	3.10
Middle to low	52	32	7.03	6.27	3.26	3.10
Less than 500,000						
High	23	80	6.83	6.18	3.40	3.33
Relatively high	11	80	8.59	5.40	3.35	3.21
Middle to low	16	88	5.72	4.98	4.17	2.82

Note: Socioeconomic data were inadequate for 19 of the 536 Ss in the sample.

Comparison of the Jewish and non-Jewish means for the six conditions indicates that the main effect of family religious background (A) and the interaction of religion and size of home town (A × B) depends on one cell: Jewish Ss of SES II residing in small towns. The N of this cell is small, only 11 Ss, and the mean is extreme. If one is reluctant to attribute much reliability to the project score of this cell, then the relation between religion and attainment shown in Table 2 (which compares the distributions of students of high, middle and low potential scientific creativity by family religious background) depends on variations in size of home town and socioeconomic status within the distributions.

A less conservative interpretation of the analysis of variance results is based on the fact that in five of the six comparisons, the potential scientific creativity ratings were higher for Jewish than for non-Jewish Ss. In this interpretation, the degree to which the ratings of Ss from Jewish backgrounds will exceed that of non-Jewish Ss is contingent on certain circumstances. This interpretation is consistent with the hypothesis that some values present in the Jewish culture sustain intellectual attainment in otherwise adverse circumstances. It is clear, however, that differential cultural experiences possibly associated with the relatively lower ratings of non-Jewish Ss can be modified by factors associated with residence and socioeconomic status. The effect of the combination of residence in small towns with relatively low status directs attention to factors such as the science training offered by city and town high schools, financial support available in the home to develop and maintain science projects, the parental guidance and emotional support available to the child of professional parents, and possibly compensatory facilities such as museums and research institutes.

A second reservation concerns limitations on generalizations from the PCS sample to other groups. Of particular concern is the fact that the PCS distribution of family religious backgrounds differs from that reported for general population samples where Jews are found to comprise 3 percent of the respondents, Protestants 72 percent, and Catholics 22 percent.[14] In comparison, students from Jewish backgrounds were overrepresented in the PCS sample, where they comprised 35 percent of the respondents, while Protestants (39 percent) and Catholics (10 percent) were underrepresented.

There is no adequate information on the religious or social class characteristics of applicants to the STS or similar contests.[15] Other data suggest that the family religious background distribution of the PCS sample (a) is

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[14] B. Lazerwitz, "A Comparison of Major United States Religious Groups," *Journal of the American Statistical Association*, 56 (1961), pp. 568–579.

[15] In a small (N = 39) sample of Florida high school students selected for a six-week summer program in mathematics, Jews (13 percent) and Protestants (74 percent) were overrepresented and Catholics (3 percent) were underrepresented in comparison with the national sample. A. Kennedy *et al.*, "A Multidimensional Study of Mathematically Gifted Adolescents," *Child Development*, 31 (1960), pp. 655–666.

not representative of college students or even of students interested in science, (b) may not be representative of STS applicants, but (c) may provide some basis for generalizations to adolescent samples of high scholastic achievement.

Among a nationally representative sample of seniors graduating from college, 8 percent of the respondents were Jews, 61 percent were Protestants, and 25 percent were Catholics.[16] There was no significant relation between choice of science as a career and religious background. Twenty-two percent of the Jews, 16 percent of the Protestants, and 17 percent of the Catholics listed science, engineering, or medicine as their vocational choices.

Without information on the religions of the STS applicants, speculation at length on why the PCS sample differs from college seniors does not seem justified. One possibility that should be considered, however, is that students who enter science contests come from different backgrounds than do science students in general.

Among the more obvious influences on participation in the STS and on eligibility for the PCS sample would be opportunities for and encouragement of science research in the high school, "self"-selection, and encouragement by science teachers of students likely to win honors.

Opportunities in the School and Community

It has previously been noted that factors associated with residence in larger cities and with higher socioeconomic status have been found to be associated also with scientific attainment in adults. While opportunities in the community, such as museums and colleges, may stimulate scientific attainment in adolescents, an even more direct factor might be the well-documented relation of social class to the quality of education available to the students: for richer families, better schools; in better schools, better science classes; in better science classes, more interest in science contests and higher scores on the SCAT. The overrepresentation of students from Jewish families in the PCS sample could thus reflect the overrepresentation relative to the general population reported for Jewish families among the middle-class and professional families who are likely to live in the "good" school districts.[17]

"Self"-Selection

Students with a high need for recognition and achievement may be more highly motivated to enter contests such as the STS and may try harder to win. Jews have frequently been reported to exceed non-Jews in achievement motivation.[18] The overrepresentation of Jewish students in the PCS sample thus could reflect student motivation and parental encouragement as well as training and research opportunities in the schools and the communities.

Selection by the School

Still another source of differential selection is the fact that an STS "winner" or "honors" award provides prestige to the school as well as to the student. While schools apparently differ in ways of achieving this recognition, the best students from each school are usually encouraged to enter the STS. The contestants thus should be representative of intellectually able students who are interested in science research.

There is some evidence that religious background and scholastic attainment are related. The superior performance of Jewish Ss in these studies was found, moreover, to be independent of socioeconomic status or size of hometown, although both latter variables were associated with scholastic attainment.[19] If Jewish students tend to be better students in science than their classmates, the overrepresentation of Jewish Ss in the PCS sample could be at least partially attributable to selection at the high school level and, as noted previously,

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[16] A. M. Greeley, "Influence of the 'Religious Factor' on Career Plans," *American Journal of Sociology*, 68 (1963), pp. 658–671.

[17] Lazerwitz, *op. cit.*

[18] J. Veroff, S. Feld, and G. Gurin, "Achievement Motivation and Religious Background," *American Sociological Review*, 27 (1962), pp. 205–217.

[19] J. A. Davis, *Great Aspirations; The Graduate School Plans of American College Seniors*. Chicago: Aldine, 1964; A. M. Greeley, *American Journal of Sociology*, 68 (1963), pp. 658–671; the "academic performance index" of Jewish students was higher than that of non-Jewish students and more Jewish students intended to enter graduate school.

to further selection at the level of eligibility for the PCS study.

Generalizations from the PCS study are limited by these and other considerations. Information on family religious background, religious preferences during the period of career choice, and current religious affiliation are clearly needed for "more" and "less" creative scientists and "more" and "less" creative individuals in other fields before the nature of the relations among religion, science and creativity can be discussed meaningfully. . . .

Parental Attitudes of Mothers of Intelligent Adolescents and Creativity of Their Children

Robert C. Nichols

Although knowledge of the relation between the early experience of a child and his adult personality is most important for psychological theory and the practical technology of childrearing, dependable information about this relation has, for practical reasons, been difficult to obtain. Most of what we know is based on experimentation with animals or clinical case studies. Thus, in spite of the shortcomings of retrospective studies in this area, there is still need for studies relating expressed childrearing attitudes of parents to the behavior of their children when both parent and child measures are obtained when the children are adolescents. More rigorous evidence, although potentially obtainable, is currently unavailable.

The present study is designed to test the hypothesis that restrictive, controlling attitudes on the part of the mother are negatively related to originality and creativity of the child, and to explore other relations between the mother's attitudes and the child's personality.

It is assumed that an essential aspect of the process of creativity is the free access to preconscious processes (6). This freedom to use preconscious processes is assumed to be inhibited by external control and coercion and is fostered by permissiveness and psychological freedom. Rogers (7) and many others have stated a similar point of view. Thus, intelligent adolescents whose mothers express favorable attitudes toward authoritarian and coercive childrearing practices should score lower on personality measures of originality than those whose mothers favor less controlling childrearing practices.

METHOD

The present data were collected for another study (5) in which the Parental Attitudes Research Instrument (PARI) (8) was administered to the mothers of a large group (796 males and 450 females) of the National Merit finalists at the end of their senior year in high school. At the same time the children of these mothers completed a group of inventories, rating scales, and check lists which were assumed to be related to creativity and originality. All testing was accomplished by mail, and completed materials were received from 83 per cent of the sample. The mean Scholastic Aptitude Test verbal and mathematical scores of the children were respectively 658 and 698 for the males and 659 and 635 for the females.

Measures

The PARI was scored for the three factors reported by Zuckerman, Ribback, Monashkin, and Norton (11). The factors and the scales entering into each factor are as follows: (a) factor A, Authoritarian-Control, was the sum of the PARI scales, Fostering Dependency, Seclusion of Mother, Martyrdom, Excluding Outside Influences, Suppression of Aggression, Approval of Activity, Avoidance of Communication, Suppression of Sex, Ascendance of the Mother, Intrusiveness, and Acceleration of Development; (b) factor B, Hostility-Rejection, was the sum of the PARI scales, Irritability, and Rejection of the Homemaking Role; (c) factor C, Democratic Attitudes, was the sum of the PARI scales, Comradeship and Sharing, and Equalitarianism.

The measures obtained from the children were the following:

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1. The Gough Differential Reaction Schedule (DRS) (2), an inventory which was designed to predict originality and which contains the following scales: Intellectual Competence, Inquiringness, Cognitive Flexibility, Aesthetic Sensitivity, Sense of Density, Total Score (sum of above five scales), and Potential Success, an index of general drive and ambition derived from a variety of studies of achievement and personal effectiveness conducted at the Institute of Personality Assessment and Research (IPAR).
2. The Complexity-Simplicity, Independence of Judgment, and Originality scales from Barron's Inventory of Personal Philosophy (1). Only 54 of the 150 items in the Originality scale were used. The Complexity-Simplicity scale consists of items from the original 900-item IPAR inventory, selected for their significant correlations with scores on the Barron-Welsh Art Scale. The Originality scale was developed out of an MMPI-CPI pool by correlating each item with a composite score on four measures of originality (Guilford's Unusual Uses, Consequences, and Plot Titles, and the IPAR Word Rearrangement test) in a sample of 343 military officers.
3. The Mastery scale, an 18-item scale derived from three items on Strodtbeck's Value Scale (10).
4. The Deferred Gratification scale, formerly called the Play scale, from the National Merit Student Survey (4).
5. The Vocational Preference Inventory (VPI), a short revision of the Holland Vocational Preference Inventory (3).
6. The Ghiselli Self-Description Inventory. Since the correlations between weighted and unweighted scale scores for the Initiative, Self-Assurance, and Occupational Level scales ranged from .89 to .96, the items of these scales were scored with unit weights rather than being weighted differentially.
7. Self-ratings on 20 traits using a four-point scale, and a Self-Evaluation score based on the number of times a student rated himself above average on the 20 traits.
8. The Creative Activities scale (CAS), a list of hobbies and activities assumed to demand original behavior.
9. Teacher Ratings on ten traits, obtained as part of the National Merit Scholarship competition.
10. A Breadth of Interest score based on the number of activities and interests checked from a list of 47 items.
11. A check list of creative achievements in art and science.
12. High School Rank as reported by the high school principal.

Statistical Analyses

Each of the mother's PARI factor scores was correlated (product moment) with all of the measures on the children. Separate analyses were done for male and female children. Additionally each of the 32 items of the Creative Activities scale, the 20 items of the Achievement check list and the 47 items of the Breadth of Interest scale was checked for its ability to differentiate the children of each sex of mothers scoring in the upper and lower 27 per cent on each of the PARI factors by means of the phi coefficient.

RESULTS

The correlates of the three PARI factor scores are shown in Table 1. Before discussing the individual significant correlates it is perhaps appropriate to consider the possibility that all the correlations are due to chance. Sixty-three correlates are reported for each of the three PARI factors for each sex. Out of this many coefficients, sampling variability alone would account for perhaps four or five being significant at the .05 level and perhaps one being significant at the .01 level in each column. When evaluated against this standard, factor A, Authoritarian-Control, shows clearly nonchance results with 18 correlations significant beyond the .01 level in the male sample and 10 significant beyond this level in the female sample. Factor B, Hostility-Rejection, shows borderline results with one correlation significant beyond the .01 level and six beyond the .05 level in the male sample and two beyond the .01 level and six beyond the .05 level in the female sample. The correlates of factor C, Democratic Attitudes, could well have arisen by chance since none of the correlations reached the .01 level of significance in either sex. Thus, further discussion will be limited to the correlates of factor A.

Table 1. Correlates of Mothers' Childrearing Attitude Factors among Personality Measures of Their Children
N (males) = 796, N (females) = 450

	PARI factor					
	Authorit.-Contr.		*Hostil.-Reject.*		*Democr. Att.*	
Variable	*Males*	*Females*	*Males*	*Females*	*Males*	*Females*
Parent Attitudes (of the mothers)						
Authoritarian-Control			.31†	.30†	.08*	.06
Hostility-Rejection	.31†	.30†			.10†	.09
Democratic Attitudes	.08*	.06	.10†	.09*		
High School Achievement						
High school grades	.11†	.14†	.02	.10*	.05	.03
Science achievement	.05	—.01	—.05	.04	—.04	.00
Artistic achievement	—.03	.02	—.04	—.06	.04	—.08
Creative activities	—.08*	—.06	—.01	—.02	.05	.00
Gough DRS						
Intellectual competence	—.12†	—.07	—.09†	.02	.03	—.03
Inquiringness	—.13†	—.03	—.07*	.08	.07*	—.02
Cognitive flexibility	—.13†	—.13†	—.01	.06	—.01	—.02
Aesthetic sensitivity	—.03	—.15†	.01	—.05	—.03	—.06
Sense of destiny	—.09†	—.05	—.01	.03	.04	—.03
Total (above 5 scales)	—.20†	—.15†	—.05	.05	.03	—.06
Potential success	—.13†	—.24†	—.03	.03	—.02	.07
Teacher Ratings						
Emotional stability	.09†	.03	.00	.06	.02	.05
Maturity	.08*	.03	—.01	.08	.04	.01
Originality	.02	.01	—.03	.10*	.00	.08
Drive to achieve	.04	.05	—.03	.04	.03	.03
Dependability	.07*	.00	.00	.03	.01	.03
Citizenship	.05	.04	.00	—.02	.06	.06
Popularity	.02	.03	—.01	—.03	.00	.01
Social leadership	.01	.01	—.06	.11	—.02	.05
Intellectual leadership	.08*	.11*	—.05	.11*	.02	.05
Physical vigor	.10†	.07	—.05	.01	.01	.01
Sum of the above 10 items	.10†	.13†	—.05	.11*	.01	.09
Self Ratings						
Self evaluation	.04	.00	.03	.00	.02	.00
Social conflict index	.05	.08	—.03	.12†	—.04	—.03
Emotional stability	.04	.01	.00	—.05	.05	.04
Originality	—.11†	—.07	—.01	.10*	.01	—.01
Leadership	.01	—.02	.02	.05	.08*	—.04
Popularity	.03	—.04	.03	.01	.07*	—.02
Athletic ability	.03	—.08	.04	.02	—.06	—.04
Dependability	.03	.03	—.03	—.02	—.05	.06
Drive to achieve	.07*	.05	.02	.05	.02	—.02
Scholarship	.01	.04	—.08*	.07	—.02	.00
Sociability	.07*	.01	.06	—.08	.07*	.03
Aggressiveness	.07*	.04	.06	.03	.01	—.02
Neatness	.07*	—.01	.02	—.07	—.03	.01
Self control	.03	—.02	.00	.07	.07*	.07
Independence	.05	—.05	.05	.05	.01	.02
Conservatism	.08*	.04	.08*	—.02	.01	.00
Practical mindedness	.04	—.03	.01	.05	—.01	.04
Expressiveness	—.11†	—.03	.01	—.05	.03	—.08
Cheerfulness	.03	—.03	.00	—.13†	.07*	.00

(*table continued on next page*)

Table 1 (continued). Correlates of Mothers' Childrearing Attitude Factors among Personality Measures of Their Children
N (males) = 796, N (females) = 450

	PARI factor					
	Authorit.-Contr.		*Hostil.-Reject.*		*Democr. Att.*	
Variable	*Males*	*Females*	*Males*	*Females*	*Males*	*Females*
Self confidence	—.03	.03	.04	.06	.05	.06
Self understanding	—.04	—.03	.01	.03	—.04	—.08
Perseverance	.04	.03	.03	.02	.00	.07
Originality and Personality Scales						
Complexity-simplicity	—.14†	—.19†	.04	.07	—.04	—.11*
Independence of judgment	—.15†	—.13†	—.05	.09*	—.07*	—.10*
Barron originality	—.20†	—.12†	.00	.09*	.04	—.01
Mastery	.05	.03	—.02	—.01	—.04	—.05
Deferred gratification	.09†	.03	—.02	.02	—.03	—.04
Breadth of interest	.00	—.04	—.02	—.04	.05	—.02
Ghiselli Inventory						
Supervisory qualities	—.10†	—.02	—.07*	.00	.06	.03
Initiative	.01	—.04	—.03	.07	—.01	.05
Self assurance	—.03	—.06	—.07*	—.01	.06	.07
Occupational level	—.09†	—.02	—.02	.06	.00	.01
Vocational Preference Inventory						
Response bias	.08*	.04	.05	.07	.03	—.04
Infrequency	.05	.02	.05	—.06	.07*	.09*
Realistic orientation	.05	.01	—.07*	.06	.03	—.02
Intellectual orientation	.02	—.06	—.01	.02	—.01	.02
Social orientation	.04	.04	.03	—.08	—.01	.00
Conventional orientation	.06	.15†	.02	.09*	.05	.01
Enterprising orientation	.05	.07	.01	—.03	.08*	.03
Artistic orientation	.01	—.04	.01	—.03	.04	—.06
Aggression	.04	—.01	.03	.08	—.02	—.06
Control	.07*	—.01	.03	—.11*	.01	.08
M-F	.00	.04	—.05	.03	.03	—.04
Status	.02	.09	.00	—.02	.05	.01

* $p < .05$.
† $p < .01$.

Of major interest are those child personality measures which were significantly related to authoritarian childrearing attitudes of the mother in both sexes. Most of the inventory measures of originality were negatively related to the mother's authoritarian attitudes. The DRS scales of Cognitive Flexibility, Aesthetic Sensitivity, Potential Success and the Total score were negatively related ($p < .01$) in both sexes as were the Complexity-Simplicity scale, the Independence of Judgment scale and the Barron Originality scale. For male subjects authoritarian attitudes of the mother were negatively related to the child's self ratings of originality and expressiveness ($p < .01$). Conversely, the sum of the teacher ratings of the child on ten generally favorable traits was positively related ($p < .01$) to the mother's authoritarian attitudes in both sexes. All correlations of the individual teacher ratings of the child with mother's authoritarian attitudes were positive in both sexes, but most were not significant. Mother's authoritarian attitudes were also positively related to high school grades in both sexes ($p < .01$).

Thus, expressed authoritarian childrearing attitudes on the part of the mother are related to lack of originality, and to conformity of thought and expression on the part of the child. However, the child of the authoritarian mother tends to make better grades in high school than the child of the non-authoritarian mother and he tends to be rated more favorably by his teachers.

Correlations were computed between the three PARI factors and the individual items of the check lists of interests, creative hobbies, and achievements. Ninety-nine items were correlated with each factor score. Factor A had 16 significant ($p < .05$) correlates in the male sample and 14 in the female sample. Factor B had six significant ($p < .05$) correlates in the male and four in the female sample. Factor C had six significant ($p < .05$) correlates in the male sample and three in the female sample. In view of the number of relations computed, the correlates of factors B and C should be attributed to chance. Factor A was related to the interest and achievement items beyond chance expectation. However, the proportion of chance items among the significant correlates is high enough to make interpretation difficult. The items significantly related to factor A are shown in Table 2. In both sexes the students with high authoritarian mothers as opposed to those with low authoritarian mothers tended to check items which require originality less frequently and preference for church work more frequently. The items concerned with preferences for sports and school subjects are ambiguous and are difficult to interpret.

DISCUSSION

With certain qualifications (to be discussed), the hypothesis of a negative correlation between mothers' authoritarian childrearing attitudes and the originality and creativity of their children was supported by the data of this study. The finding that the children of authoritarian mothers obtained better grades in school and more favorable ratings by their teachers is not inconsistent with the hypothesis that authoritarian childrearing practices lead to conformity and "good" behavior, but stifle originality.

There are three possible ways that a correlation between maternal attitudes and child behavior could arise in a cross-sectional study conducted during the child's adolescence: (a) the mother's expressed attitudes reflect her treatment of the child during his early life, and her treatment of the child produced behavior patterns that have persisted or continued to develop into adolescence; (b) the behavior of the child affects the attitudes of the mother

Table 2. Interests and Achievements of the Child Significantly* Related to the Mother's Authoritarian Childrearing Attitudes

796 Male subjects		*450 Female subjects*	
Low factor A	*High factor A*	*Low factor A*	*High factor A*
Like more:			
English	Track	Field hockey	Business
Social studies	Church work	Horseback riding	Church work
Camping		Tennis	Dancing
Reading			Movies
Other			Music
Have done more frequently:			
Won literary award or prize for creative writing	Won prize or award in scientific talent search	Played piano for more than 2 years	Played 1 or more brass or woodwind instruments for more than 2 years
Played piano for more than 2 years	Doing an etching or woodcut	Planning independent experiment	
Writing poetry		Finger painting	
Writing interpretive report of historical event		Writing interpretive report in particular field of study	
Making a mobile or collage		Making a mobile or collage	
Doing experimental photography			

* $p < .05$.

toward childrearing; (c) both the mother's attitude and the child's behavior are affected by some third variable (e.g., socioeconomic status, genetic factors, etc.). On the basis of the evidence at hand it is not possible to decide with certainty between these three possible explanations of the relations observed in this study. However, the possibility that the mother's education might serve as a mediating third variable seemed likely enough to be checked. Zuckerman *et al.* (11) reported correlations around −.40 with education of the mother for the scales entering into the authoritarian control factor. Thus, mothers in lower educational levels would be expected to have more authoritarian attitudes and it might also be expected that they would have the less creative children. In the present samples the relation between education and authoritarian attitudes was considerably lower (−.12 for mothers of boys and −.16 for mothers of girls) than that reported by Zuckerman. This lower relation with mother's education may be the result of the smaller range of education in the present sample. The correlations of mother's education with the creativity and achievement measures of the child were all quite low and most were not significant. In this circumstance, partialing out the mother's education had little effect on the correlation between PARI factor and the child's scores. Most of the correlations with the creativity measures were lowered slightly (by .01 or .02) while the correlations with the teachers' ratings were generally slightly increased. The pattern of significant correlations was unchanged by partialing out mother's education.

Since mother's education does not seem to be a mediating factor, it is difficult to think of some third variable which could be responsible for the observed correlations. From the remaining two possibilities, the hypothesis that the findings result from the mother's effect on the child seems to this writer to be more tenable than the possibility that the more creative child influenced the mother to have less controlling attitudes.

All of the observed relations were quite small and reached significance only by virtue of the large sample. Correlations such as these are much too low to be of any practical significance and are valuable only for suggesting relations between variables which are difficult to study under experimentally controlled conditions and which may be partially obscured by the influence of other variables. If one is interested in relations between parental attitudes and child behavior which are mediated by the mother's effects on the child, correlations of the size of those found in this study are to be expected. This is because of the many possible sources of error in the observed relations: error in the measurement of parental attitudes, the lack of perfect relation between the mother's expressed childrearing attitudes and her earlier childrearing behavior, heredity, error in measurement of the child's personality, and aspects of the child's environment other than his mother which affect his behavioral development. Thus, investigators contemplating studies of this kind should consider N's of 250 to 300 as about the minimum acceptable sample size.

One additional qualification should be mentioned concerning the interpretation of the results of this study. The subjects were all of very high intelligence and the findings may not be generalizable to samples of lower ability. This seems particularly likely for the finding of greater academic achievement among the children of mothers with authoritarian child rearing attitudes, since this relation is in the opposite direction from a similar finding of Shaw and Dutton (9).

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C. Academic Influences

Introduction

In the first two sections of Part II the search for precursors of scientific interests, capacities, and attitudes has proceeded in a highly speculative way. Not only has the identification of salient variables been problematic, but also the postulation of significance has been questionable, relying as it must on the dangerous tendency to establish correlations and to infer a continuous—and often causal—relationship between phenomena that are temporally widespread. Such correlations have a great likelihood to be spurious, since no study has been made of the role and contribution of the many other variables that intervene between t_1 and t_2.

As one moves into academic work some of these methodological pitfalls are lessened. Continuities of events are more visible; and the context in which they are being measured are more alike. As we have learned from the studies of vocational groups to date, the accuracy of prediction improves immeasurably when both predictor and predictive criterion are similar in the life situation, and when the nature of their relationship can be made more explicit. Thus in studying predictors of scientific research work, one can study academic performance, examination grades, discipline-relevant areas, university programs in science, and the motivations and specific precipitants of choice of work area. All these data have the advantage of being relatively "hard" data, whose reliability can be checked by outside validating measures. Further, the hypotheses about their importance for adult work merely capitalize upon conscious decisions and occupational plans for obtaining the relevant scientific training and background. These are decisions often arrived at early in the young scientist's drives to meet the core requirements of his specialty.

It has been known for a long time that children who are motivated to acquire knowledge and to perfect academic skills during their early years retain this motivation during adolescence and early adulthood (Janis, Mahl, Kagan and Holt, 1969). Unless specific activities are considered, findings concerning grades in the elementary years are not good predictors of later career performance. However, grade-point averages in graduate and undergraduate courses, especially in scientific areas, are correlated with career success (Bloom, 1963; Platz, McClintock and Katz, 1959). Further, Harmon's study, which appears as the first reading in this section, showed that grades in mathematics and science at the high school level might be a good predictor for later choice of career. Examining the high school data of all Ph.D.s from American universities in 1958, Harmon found that the performance in math and science is out of line with the general high aptitudes in the case of social science Ph.D.s. Harmon thought the scatter in the grade averages might well be an early indication of an aptitude in social science rather than natural science areas. In his study, Ph.D.s in physical and biological science invariably rank differently in all measures of ability, with the biological scientists even lagging behind social scientists.

One of the pressing questions of the last decade has been the reasons for differences in the number of scientists and scholars produced by different universities. Are there certain characteristics, such as size, orientation, location, of the universities that facilitate achievement in science in general or are the differences in productivity a function of the student body and their motivations and academic goals? Holland, using superior high school seniors in a National Merit Scholarship program found that bright students congregate in institutions which have a high index of scientific productivity. Their choice of college is consistent with their own need for scientific achievement and with the vocational choice of their fathers which was conducive to intellectual achievement. Therefore, Holland found that high productivity appears to be the result of an intellectually well-endowed student body which is oriented toward scholarly and intellectual goals. Astin, investigating the same issue, found that to a limited degree the kind of college a student attends influences his choice of discipline. If a male student attends a technical college or a coeducational liberal arts college, he has a more than chance expectancy of pursuing science; however, this is less so if he goes to a northeastern men's college.

In the same study, Astin also questions the point in a college career when a major is selected. He finds that once in college, a student is more likely to leave the field of science than to enter it. Choice appears to be made at the high school level. Perseverance with a scientific choice is also a function of the field chosen initially, that is, a major in science or engineering is likely to pursue a higher degree after undergraduate work. This graduate training is usually anticipated from the beginning.

Whereas the bulk of the work on the importance of academic events for later career choice has been conducted on the natural scientist, Clark has looked at the motivations for becoming a psychologist, especially a significant contributor in psychology. Interestingly, in this scientific area, a choice of a career usually occurs at the college level, in the first two years for the significant contributors and a little later for the lesser psychologists. Career choice is usually made after a number of other options have been discarded, options that were available because the students were bright in high school and undergraduate courses. Continuity from undergraduate to graduate work, without any disruptive intervention, also tends to encourage persistence toward the Ph.D.

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A Multiple Discriminant Analysis of High School Background Data for the Doctorates of 1958

Lindsey R. Harmon

BACKGROUND

For many years the Office of Scientific Personnel of the National Academy of Sciences–National Research Council has collected data regarding holders of earned doctorate degrees from United States universities. In recent years this work has been supported in large part by the National Science Foundation. Since 1957, the method of data collection has consisted of a questionnaire completed by each doctoral graduate and forwarded by his graduate dean to the Office of Scientific Personnel. Among the questionnaire items is the name and address of the high school from which the Ph.D. graduated. The present study

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is an outgrowth of the opportunity afforded by this knowledge of the high school origins to explore the high school backgrounds of doctorate-holders. In 1960 the National Science Foundation supported a study of the high school backgrounds of the 1958 doctorate population which eventuated in *Scientific Manpower Report No. 3* to the Foundation. An essentially identical paper was published in *Science* magazine on March 10, 1961, with the same title, "High School Backgrounds of Science Doctorates." This report described some of the major findings, but left many avenues of possible research untouched, including more penetrating analyses aimed at determining, as well as could be done far after the fact, what elements in the high school backgrounds seemed to be influential in final choice of doctorate field for that portion of the high school population that eventually attained doctorate degrees.

With a view to exploring these questions more fully, the Office of Scientific Personnel proposed to the Foundation a further study of the data gathered in this background survey. The Foundation agreed, and further statistical analyses were undertaken. One of the outcomes of this further study was a report published in November 1962 in *The Bulletin* of the National Association of Secondary-School Principals entitled "On Decision-Making In High School." The present report is concerned with an additional study bearing on the same general question, but undertaken from a more sophisticated point of view, intended to derive more substantial statistical results. Although it is the final report submitted in connection with the "additional analyses" of the 1958 doctorate population's high school backgrounds, it serves also as a pilot study. It shows what can be done with the more extensive data in the process of being derived from a current study, more intensive in nature, of the high school backgrounds of the 1959–1962 doctorates. This new study is also supported by the National Science Foundation and conducted by the Office of Scientific Personnel.

FIELD DIFFERENCES

In the early analyses of high school backgrounds data, differences between fields of doctorate appeared with respect to every variable analyzed. The significant variables were reported in the published papers noted above. The present report is concerned with the quantification of these profiles of variables in a manner that will permit their use in a statement of the degree of significance of these field differences *as patterns.* The statistical technique used here is one form of multiple discriminant analysis. This technique derives a set of weights for the variables involved which will discriminate among the several groups with a maximal sharpness. These weights are then used to assign each individual in the sample to a group according to the pattern of his scores on the relevant variables. The degree of agreement between this "assigned group" and the actual group to which he belongs on the basis of his actual attainment represents the power of the high school backgrounds data in a pseudo-predictive sense. It does not predict that any given individual will eventually attain a doctorate degree in a particular field, but does say that, given the fact that he has attained the doctorate, the probabilities are such-and-such that it will be in a particular field. This report is an explication of the analyses done to arrive at this kind of statement. It will first consider the reasons for undertaking such an analysis, then describe the results, and finally assess the significance of these results.

CAREER DETERMINERS

Various theories may be advanced as to why a given individual may choose a particular career field. Some of the theories that have been promulgated relate to innate constitutional factors, such as native aptitudes. Others relate to such matters as interest patterns, which may be assumed to be at least partially dependent upon opportunity to explore various aspects of one's world, but which, some contend, are also constitutionally determined at least in part. Other theories would depend wholly upon the influence of environmental factors, such as opportunity to acquire the necessary skills, demand for people in various fields, chance contact with a particularly inspiring individual who is then emulated, etc. The fact is that very little is actually known, of a solid scientific nature, with regard to the determining forces. It should be possible, however, to explore empirically the relationship between a set of variables, some of which are "purely environmental" and some of which represent more nearly native constitution, to see whether any or all of them are related, in fact, to career choice. It cannot be shown, in a post-hoc study such as is feasible with retrospective data, that there is a cause-and-effect relationship between the variables that are explored and the final decision. It may, however, be shown that some are closely related to such choice, while others are related to only a slight degree. It may also be shown that there is a constellation of factors which, acting together, seems to bear a reasonable correlation with career decision, whereas any one factor acting alone may not seem to be important. It would be possible to educe from the data now available a series of "field profiles" showing graphically the various patterns of test scores, grades, and other variables that seem to be related to field choice, as the profiles in fact vary from field to field, and usually in a quite understandable way. However, it might be very difficult to show that these several profiles, and differences in profiles, are highly significant. On the other hand, it might be found that none of the observed differences are truly significant, and that some theories dependent on particular sets of data are therefore invalid, as they will not stand up under an empirical test. It was with thoughts such as these that the present study was undertaken.

FIVE GENERAL FIELDS OF DOCTORATE

It is convenient, in considering the possible field patterns, to use the five general fields into which the doctorate population has been divided for many previous analyses—fields that make general sense and which are rather clearly differentiated. These five fields are the physical sciences, the biological sciences, the social sciences, the arts and professions, and education. The courses of training leading to these several eventual fields are rather clearly differentiated, and there is relatively little crossing-over from one field to another following attainment of the baccalaureate. These five fields were therefore first chosen for analytical groups. However, as this represents a rather gross grouping, it was felt that, if this differentiation could be successfully done, it would be valuable to attempt a somewhat finer differentiation, namely, within the fields of the physical sciences: mathematics, physics, chemistry, geo-sciences, and engineering. These two sets of differentiations, then, were the subject matter fields chosen for the present study.

WHAT THE MULTIPLE DISCRIMINANT DOES

When a set of scores is available for the members of each of several groups—in this case the five fields of doctorate—the multiple discriminant analysis establishes a typical pattern for each field, taking into consideration the mean score of the members of each field on each variable, the variation about this mean for the members of the group, and the intercorrelations of the variables. If the typical patterns of two groups are very similar, little discrimination is possible. The degree of differentiation of the several groups can be stated quantitatively, so that the question of whether the discrimination is reliably established can be examined. With the computer program available for the analyses reported here, it is possible to go one step further and measure the degree of differentiation that could be established on the basis of the scores alone, by assigning each member of the experimental sample to the group whose typical scores most closely resembled his own profile of scores. For each individual, the whole set of his high school scores was thus assessed five times in the present study, comparing his profile in turn with the set of functions that most fully differentiated each eventual field of doctorate from all the others. The outcome of this comparison is a set of five "function scores" stating quantitatively the degree of fit of each individual (at the high school level) to each of the groups determined operationally many years later when doctorate degrees were earned. The computer program then picks out the highest function score and "assigns" the individual to this field. Finally, it compares the assigned fields with the actual fields for the whole sample of cases, and produces a table showing how many actual members of each group are assigned to their own eventual doctorate fields and how many are assigned (on the basis of their high school score patterns) to each of the other doctorate groups. The percentage of correct assignments by this process is a measure of the power of the system, or the degree to which individuals can be properly allotted to their true eventual fields on the basis of information available about them from high school records.

THE VARIABLES EMPLOYED

There were eighteen variables available for the analysis, consisting of tests, grades, courses taken and not taken, and class size, and derived variables stemming from these. In the descriptions that follow, the term "score" will be used when referring to the value of a given individual or a group on any one of these variables, even though it may not be an earned score such as on an IQ test. The complete list of variables, numbered to correspond with their numbers in the tables that will be shown, are as follows:

1. Intel. = intelligence test. The score used is a converted one, in terms of standard score equivalent for a test or average of tests. On the standard scale used, the mean is 50 and the standard deviation 10.
2. Rank = normalized rank in graduating class; the mean is 50 and the standard deviation 10.
3. Math GPA = a grade point average computed for all math courses taken.
4. Science GPA = a grade point average including all science courses taken.
5. Math A-N-T = A count of the numbers of math courses available in that school at that time, but not taken by the particular student.
6. Science A-N-T = a count of science courses available but not taken.
7. Class size = a coded value for size of high school graduating class.
8. El. alg. = whether elementary algebra was taken.
9. Pl. geom. = whether plane geometry was taken.
10. Int. alg. = whether intermediate algebra was taken.
11. Trig. = whether trigonometry was taken.
12. Coll. alg. = whether college algebra was taken.
13. Solid geom. = whether solid geometry was taken.
14. Gen. sci. = whether general science was taken.
15. Biol. = whether biology was taken.
16. Chem. = whether chemistry was taken.
17. Phys. = whether physics was taken.
18. Other sci. = whether any other science course was taken.

It should be noted that for items 8 through 18, the averaged value for a given group of people becomes percentage of people in that group who took the given course. The present analysis used the notation of whether the course was taken, rather than the grade earned, because the grades, which would in any case tend to intercorrelate rather highly, are represented in the math and science grade point averages. These averages would be more reliable, certainly, than grades in individual courses, and should better represent the *level* of performance of which the individual, or the group, was capable. The percentage of the group taking the course would, it was believed, tend to represent better the *direction of motivation* and hence would be more useful in the discriminant analysis.

In order to determine the importance of the various groups of variables, the analyses were replicated with different sets of variables. A set of 16 (omitting math and science GPA) was involved in the first runs to be reported. In the second run, the number was cut to the first seven in the above list to determine the unique contribution of the presence or absence of the various specific courses in the individual records. Finally, the size of high school graduating class was dropped, leaving only six variables involved in the analysis. Whether a particular course was taken or not was, of course, in part a matter of opportunity for some of the students, and a matter of choice for others. The question of high school class size was obviously quite outside the control of the individual. Its inclusion or elimination, therefore, furnishes a test of the unique contribution of at least one purely environmental factor. The effects of these various inclusions and exclusions will be examined in connection with the particular tables.

THE SAMPLES

Three different sets of samples were involved in this study, and each sample was replicated in order to test the reliability of the discrimination produced. Thus there were in fact six different groups of individuals whose records were analyzed in this study. The first two groups 583 and 584 cases each, were drawn proportionately from the five general fields, as mentioned earlier. A random sampling procedure was used to select the cases, the only restriction being that complete records were required for this particular computer program. The restriction tended to sample somewhat more heavily from the younger cases, and from those from urban schools, as the data were somewhat less complete from older records and from rural areas. This restriction did not severely bias the samples, however, as may be seen in the tables.

The second set of samples was also drawn from the five general fields of doctorate, but on an equal-size basis. That is, 150 cases were drawn from each of the five fields, for a total set of 750 individuals in each sample. The assumption underlying this sampling was that one might wish to look at the problem from the standpoint of high school students for whom any one of five general fields of study might be thought of as equally probable. As will be seen in the statistics to be presented, the equal-representation vs. proportional-representation samples were not so different in the final results as to make this variation one of great importance, although the discriminations were slightly less distinct.

The third set of cases, 400 in each of two replicated samples, was drawn proportionately from the physical science fields alone, in an attempt to determine whether it would be possible by this technique to draw the finer discriminations that are involved within one field, rather than between one and another of the five gross fields.

THE STATISTICAL RESULTS

Two types of data are presented in the tables that follow: the means of the several groups on each of the variables involved, and the "classification matrices." The latter give the classifications assigned by the computer program to each individual, summed by field. Thus the number of physical scientists who are assigned to the physical science field on the basis of this analysis of the high school backgrounds data is shown in the upper left-hand corner of the first classification matrix, Table 2. Some of this group are mis-classified, as can be seen, reading down this same column, into biological sciences, social sciences, arts, and education. Likewise for each of the other fields: the number assigned to each field, on the basis of the fit of their patterns of scores to the pattern typical for each field, is given. The figures in the diagonal cells of each table represent correct assignments, or "hits." In each of the tables, the percentage of hits for each field, and for all five fields taken together, is shown. This will permit a comparison of the results for the two replicated samples from each group, a comparison of the discriminant power of various sets of variables, and of the differential power of this set of variables to discriminate among the several fields here analyzed.

Table 1 presents the 18 mean values for each of the five general fields of doctorate. There are two columns for each field, labelled S-1 and S-2 for the two samples used. It may be noted that the pattern of abilities shown for the various fields is similar to that reported in the earlier studies. However, because these are relatively small samples, these means do not have the reliability of the means for the whole 1958 population, and hence some variations from the earlier studies are discernible. The restriction to complete-data cases may also introduce a small bias. It is apparent, also, that the two samples are not entirely alike—again a reflection of sampling variations which are to be expected with the numbers of cases given here.

Some comments on the particular values shown may be of help in examining these tables. The math and science grade point averages are essentially arbitrary values, and are not to be interpreted in terms of the usual 0–4 scale typically used for college grades. On the scale used here, A = 9, B = 7, C = 5, and D = 3. If one reads across the *Math A-N-T* row, it is apparent that the average value for the science fields is less than one course available but not taken. However, the social scientists omitted more than one available course,

Table 1. Means of Two Samples From Five General Fields of Doctorate, on 18 Discriminant Variables

Variable		*Phys. sci.*		*Biol. sci.*		*Soc. sci.*		*Arts and prof.*		*Education*	
		S–1	*S–2*	*S–1*	*S–2*	*S–1*	*S–2*	*S–1*	*S–2*	*S–1*	*S–2*
No.	*Name*	*N 150*	*150*	*125*	*126*	*150*	*150*	*79*	*79*	*79*	*79*
1	Int. test	65.49	65.73	63.06	62.63	66.02	66.77	64.94	66.48	62.14	62.72
2	Rank	61.99	61.79	57.88	57.72	58.85	58.51	59.71	62.22	56.46	56.75
3	Math GPA	7.68	7.51	6.58	6.45	6.43	6.36	6.61	7.07	6.08	6.27
4	Sci. GPA	7.85	7.77	7.20	7.09	6.78	6.68	6.68	7.31	6.50	6.67
5	Math A-N-T	.30	.49	.78	.70	1.03	1.39	1.30	.95	1.03	1.16
6	Sci. A-N-T	.41	.51	.52	.50	.69	.75	.95	.94	.61	.92
7	Class size	5.21	5.55	5.43	5.52	5.80	5.69	4.99	5.53	4.92	5.25
8	El. alg.	.95	.98	.98	.96	.95	.97	.97	.94	.96	.97
9	Pl. geom.	1.00	.99	.98	.95	.98	.95	.96	.94	.95	.92
10	Int. alg.	.95	.92	.81	.79	.83	.79	.65	.77	.56	.68
11	Trig.	.80	.78	.56	.60	.49	.38	.32	.35	.33	.29
12	Coll. alg.	.24	.29	.20	.19	.14	.13	.06	.13	.08	.10
13	Sol. geom.	.73	.71	.50	.44	.39	.30	.29	.32	.33	.35
14	Gen. sci.	.56	.65	.59	.67	.57	.55	.56	.58	.65	.56
15	Biology	.66	.57	.82	.76	.65	.73	.66	.56	.72	.61
16	Chemistry	.95	.93	.87	.87	.79	.75	.58	.68	.57	.62
17	Physics	.92	.93	.76	.76	.69	.73	.49	.48	.62	.65
18	Other sci.	.13	.10	.17	.14	.07	.09	.03	.06	.10	.10

as did also the humanitics doctorates and the educators.

Table 2 gives the classification matrices for the five general fields, proportionately sampled, with three different sets of discriminant variables, in both of the replicated samples. The diagonal cells of these tables, which have been marked for ready reference, can be summed for the computation of the total number and percentage of hits for each table. The actual fields of doctorate are represented by the columns in these tables; the "assigned fields" are given in the rows. Thus at the bottom of each sub-table is a figure, designated "%C" for the proportion correctly assigned, or hits within each field. The percentage of hits for each table as a whole is the sixth figure in this row. All individuals assigned to any given field, regardless of their actual doctorate field, are summed in the sixth data column in each table.

For summations of the percentage of hits in the several sub-tables under Table 2, a table is given at the bottom of the page, representing the *average* percentage of hits in the two replicated samples in each of the three discriminant analyses. Thus the first row of this table shows the power of the 16 variables, omitting only science and math grade point averages. Here the overall hit rate is shown to be 44.5%. When only seven variables are used, the hit rate drops to 36.2%, and when six only are used, omitting the class size variable, the rate drops again to 35.7%. As a preliminary method of judging the significance of these hit rates, one may compare them with the chance expectancy, which is approximately 20%, as there are five fields into which a random assortment could be made.

Some further comment should be made with regard to the relative magnitude of the discriminating power of the several sets of variables. To some extent, the higher percentage of hits with 16 variables constitutes a matching of chance errors. The replication in another sample, even though it maintains the same relative percentage, does not constitute a full-fledged cross-validation of the discriminant function. That is, chance errors in the second sample are also capitalized upon; the computer program available for this analysis did not permit the substitution in one sample of weights derived from the other, to provide a true cross-validation of the discriminant

Table 2. Classification Matrices, Five General Fields—Proportional

		Sample 1 = 583 cases						*Sample 2 = 584 cases*					
Basis of classification	*Classification*	*Actual fields*					*TOT.*	*Actual fields*					*TOT.*
		PS	*BS*	*SS*	*A*	*E*	*N*	*PS*	*BS*	*SS*	*A*	*E*	*N*
By 16 variables	PS	*94*	38	35	14	11	192	*89*	29	27	13	12	170
	BS	29	*40*	19	9	11	108	25	*52*	16	6	9	108
	SS	12	19	*48*	6	9	94	16	18	*58*	8	13	113
	A	11	9	27	*34*	13	94	9	11	25	*37*	13	95
	E	4	19	21	16	*35*	95	11	16	24	15	*32*	98
	%C	62.7	32.0	32.0	43.0	44.3	43.1	59.3	41.3	38.7	46.8	40.5	45.9
By 7 variables	PS	*94*	44	34	18	17	207	*64*	33	26	21	11	155
	BS	22	*32*	24	6	13	97	27	*41*	23	9	19	119
	SS	12	24	*41*	6	11	94	16	26	*56*	16	14	128
	A	14	12	33	*31*	17	107	30	10	23	*26*	18	107
	E	8	13	18	18	*21*	78	13	16	22	7	*17*	75
	%C	62.7	25.6	27.3	39.2	26.6	37.6	42.7	32.5	37.3	32.9	21.5	34.9
By 6 variables	PS	*101*	47	33	19	19	219	*64*	33	27	21	10	155
	BS	23	*31*	24	5	14	97	29	*44*	24	11	20	128
	SS	7	19	*27*	6	11	70	18	27	*58*	15	16	134
	A	10	13	48	*32*	19	122	31	11	26	*26*	15	109
	E	9	15	18	17	*16*	75	8	11	15	6	*18*	58
	%C	67.3	24.8	18.0	40.5	20.3	35.5	42.7	34.9	38.7	32.9	22.7	36.0

PS = Physical sciences, BS = Bio-sciences, SS = Social sciences, A = Arts and professions, E = Education, %C = Classified correctly

Percent Correct Classification, Average of Samples 1 and 2

	PS	*BS*	*SS*	*A*	*E*	*TOT.*
By 16 Variables	61.0	36.7	35.4	44.9	42.4	44.5
By 7 Variables	52.7	29.1	32.3	36.1	24.1	36.2
By 6 Variables	55.0	29.9	28.4	36.7	21.5	35.7

weights. When there are a large number of variables, some of which are not highly reliable, there is opportunity for a considerable capitalization on these chance variations. It should be kept in mind, therefore, that, although the 16-variable hit rate was 43% in one sample and 46% in the other, a true cross-validation might well shrink this rate to about 40% for both samples, or perhaps even farther. The exact percentage of hits is not greatly at issue, and it does not seem profitable to pursue this matter further at this point, particularly in view of the fact that only a portion of the high school record, concerned mainly with math and science courses, was available for analysis in this study. In the later study of the 1959–1962 Ph.D.'s, the whole transcript will be available for analysis, and a more flexible computer program will also be employed. It may be expected to yield more extensive insight into the early influences on career decision.

Turning to the individual fields, and the relative hit rates within them, it is noteworthy that this computer program does best by far with respect to the physical sciences, whichever set of discriminators is used. This is probably in large part because this study, concentrating on the science fields, gathered more information about science and mathematics performance. The more recent study, still under way, with its broader scope, might be expected to produce a somewhat more even rate of hits within the five general fields, because of the inclusion of grades earned in English, foreign languages, and social studies.

Table 3 presents the mean values for all 18 variables for each of the five general fields, for

Table 3. Means on 18 Variables of Two Samples of 750 Cases Each, Drawn Equally From the Five General Fields of Doctorate

Variable		*Phys. sci.*		*Biol. sci.*		*Soc. sci.*		*Arts and prof.*		*Education*	
No.	*Name*	*S–1*	*S–2*	*S–1*	*S–2*	*S–1*	*S–2*	*S–1*	*S–2*	*S–1*	*S–2*
1	Int. test	68.39	67.51	62.84	61.99	65.42	66.69	66.50	65.29	61.15	62.41
2	Rank	63.81	62.80	57.64	57.15	58.94	61.14	61.58	59.97	56.91	56.23
3	Math GPA	7.91	7.73	6.54	6.38	6.54	6.73	6.86	6.69	6.30	6.09
4	Sci. GPA	7.96	7.92	7.21	6.97	6.79	6.97	7.12	6.84	6.63	6.56
5	Math A-N-T	.47	.37	.62	.50	.85	.82	.99	.85	.77	.78
6	Sci. A-N-T	.37	.39	.65	.91	1.21	1.05	1.20	.97	1.03	1.13
7	Class size	5.58	5.83	5.34	5.41	5.47	5.31	5.37	5.27	4.93	5.13
8	El. alg.	.95	.96	.96	.98	.95	.94	.95	.97	.98	.97
9	Pl. geom.	.99	.98	.97	.96	.97	.96	.95	.99	.97	.93
10	Int. alg.	.93	.95	.82	.81	.73	.81	.72	.79	.73	.63
11	Trig.	.80	.80	.59	.52	.45	.47	.34	.48	.31	.32
12	Coll. alg.	.29	.35	.15	.18	.12	.13	.08	.13	.04	.09
13	Sol. geom.	.73	.74	.49	.40	.37	.40	.29	.40	.27	.35
14	Gen. sci.	.58	.67	.63	.65	.53	.55	.57	.58	.71	.60
15	Biology	.63	.73	.81	.80	.65	.61	.61	.65	.65	.67
16	Chemistry	.91	.87	.83	.87	.66	.72	.61	.69	.55	.60
17	Physics	.93	.93	.76	.68	.68	.69	.48	.59	.60	.63
18	Other sci.	.15	.11	.12	.17	.10	.06	.05	.06	.08	.10

a different set of samples. In this case, rather than sample proportionately from the several fields, samples of equal size were drawn. This should result in somewhat greater stability for the patterns in the smaller fields. It may also affect the classification matrix, as a different set of assumptions is involved. The classification matrices for these equal-sample groups are given in Table 4. Here, it will be noted, there was no 7-variable set of discriminators, but only the 6-variable and 16-variable sets. In general, the discriminations are slightly less sharp, perhaps only by random sampling variations from those depicted in Table 2. As before, the physical sciences experience the highest hit rate, and the social sciences the lowest hit rate.

The successful discrimination among the five general fields led directly to the attempt to discriminate among the sub-fields of the physical sciences, which had stood out as distinctly different in the gross-field analyses. Table 5 gives the means on all 18 variables for the sub-fields of the physical sciences, in two replicated samples. Table 6 presents the classification matrices for these five sub-fields on 16, 7, and 6 variables, respectively. Here the percentage of hits is definitely lower than for the five gross fields, yet well above the chance expectancy of approximately 20%.

The probability that the classification achieved by this program operating on these variables is something other than a random sampling deviation from a pure chance distribution can be tested by the χ^2 criterion. This was done for each of the classification tables described above, and in every case the probability was less than two in ten thousand that the classification results would have been achieved through random sampling variations. With this assured, it seemed probable that additional information could be derived from examining the individual columns of the tables to determine whether some fields were more sharply differentiated than others. For the several portions of Table 1, which is concerned with the five general fields, the results were as follows: With 16 variables, all rows and columns showed significant χ^2 values. Here we have assumed four degrees of freedom for each row and each column. The χ^2 value never fell below 18, which is significant at the .001 level. With seven variables, the χ^2 values were not so high, but the minimum was still significant at the .02 level. When the replicated samples are combined, adding χ^2 and

Table 4. Classification Matrices, Five General Fields—Equal Samples

		Sample 1 = 750 Cases						*Sample 2 = 750 Cases*					
		Actual fields					*TOT.*	*Actual fields*					*TOT.*
Basis of classification	*Classification*	*PS*	*BS*	*SS*	*A*	*E*	*N*	*PS*	*BS*	*SS*	*A*	*E*	*N*
	PS	*96*	34	29	30	15	204	*86*	37	35	28	18	204
	BS	28	*65*	25	10	27	155	29	*60*	21	26	21	157
By 16 variables	SS	10	14	*28*	21	21	94	22	12	*44*	27	24	129
	A	11	13	37	*64*	21	146	5	13	24	*38*	21	101
	E	5	24	31	25	*66*	151	8	28	26	31	*66*	159
	%C	64.0	43.3	18.7	42.7	44.0	42.5	57.3	40.0	29.3	25.3	44.0	39.2
	PS	*93*	39	38	43	29	242	*93*	36	40	37	30	236
	BS	22	*61*	28	19	31	161	29	*52*	27	24	38	170
By 6 variables	SS	11	11	*24*	18	27	91	11	18	*43*	36	20	128
	A	13	15	35	*46*	17	126	10	7	22	*25*	20	84
	E	11	24	25	24	*46*	130	7	37	18	28	*42*	132
	%C	62.0	40.7	16.0	30.7	30.7	36.0	62.0	34.7	28.7	16.7	28.0	34.0

PS = Physical sciences, BS = Bio-sciences, SS = Social sciences, A = Arts and professions, E = Education, %C = Classified correctly

Percent Correct Classification, Average of Samples 1 and 2

	PS	*BS*	*SS*	*A*	*E*	*TOT.*
By 16 Variables	60.7	41.7	24.0	34.0	44.0	40.9
By 6 Variables	62.0	37.7	22.4	23.7	29.4	35.0

Table 5. Means on 18 Variables of Two Samples of 400 Cases Each, Drawn Proportionately From the Five Fields of Physical Sciences

Variable		*Mathematics*		*Physics*		*Chemistry*		*Geo-sci.*		*Engineering*	
		S–1	*S–2*	*S–1*	*S–2*	*S–1*	*S–2*	*S–1*	*S–2*	*S–1*	*S–2*
No.	*Name*	*N 37*	*37*	*79*	*79*	*150*	*150*	*31*	*31*	*103*	*103*
1	Int. test	70.27	68.11	69.39	70.19	66.21	66.08	65.68	66.42	67.51	66.05
2	Rank	63.62	65.65	65.16	66.23	62.62	63.63	58.35	58.65	63.94	64.28
3	Math GPA	8.34	8.38	8.19	8.32	7.61	7.70	6.78	6.70	8.06	7.94
4	Sci. GPA	7.68	8.24	8.11	8.16	7.84	8.03	7.48	7.17	8.21	8.01
5	Math A-N-T	.65	.70	.35	.53	.39	.41	.52	.68	.52	.43
6	Sci. A-N-T	.32	.35	.24	.30	.41	.27	.58	.65	.39	.28
7	Class size	6.14	6.05	5.92	5.28	5.39	5.22	5.61	5.32	5.80	5.50
8	El. alg.	1.00	.95	.95	.92	.97	.97	.94	1.00	.96	.97
9	Pl. geom.	1.00	.97	.99	.96	.98	.97	1.00	1.00	.99	.99
10	Int. alg.	.97	1.00	.94	.95	.93	.93	.87	.94	.94	.92
11	Trig.	.76	.95	.86	.81	.74	.81	.71	.55	.85	.86
12	Coll. alg.	.41	.49	.47	.33	.21	.21	.35	.06	.35	.29
13	Sol. geom.	.76	.86	.80	.76	.71	.73	.61	.52	.83	.80
14	Gen. sci.	.62	.62	.62	.67	.61	.57	.65	.45	.64	.57
15	Biology	.68	.41	.75	.58	.72	.68	.65	.52	.51	.54
16	Chemistry	.89	.86	.85	.86	.91	.95	.94	.90	.91	.90
17	Physics	.86	1.00	.97	.87	.92	.85	.94	.90	.92	.98
18	Other sci.	.22	.16	.13	.14	.12	.11	.10	.10	.17	.17

Table 6. Classification Matrices, Five Physical Science Fields

		Sample 1 = 400 Cases						*Sample 2 = 400 Cases*					
		Actual fields					*TOT.*	*Actual fields*					*TOT.*
Basis of classification	*Classification*	*M*	*P*	*C*	*G*	*E*	*N*	*M*	*P*	*C*	*G*	*E*	*N*
	M	*12*	9	23	3	14	61	*18*	19	20	1	28	86
	P	11	*35*	20	4	21	91	6	*28*	27	4	11	76
By 16 variables	C	7	13	*48*	3	18	89	3	13	*50*	5	24	95
	G	3	6	34	*15*	17	75	4	10	27	*17*	16	74
	E	4	16	25	6	*33*	84	6	9	26	4	*24*	69
	%C	32.4	44.3	32.0	48.4	32.0	35.8	48.6	35.4	33.3	54.8	23.3	34.3
	M	*19*	20	14	3	13	69	*19*	24	28	3	25	99
	P	12	*22*	37	2	26	99	8	*29*	27	5	24	93
By 7 variables	C	2	13	*37*	5	12	69	2	5	*33*	4	16	60
	G	2	8	34	*14*	22	80	4	9	33	*15*	18	79
	E	2	16	28	7	*30*	83	4	12	29	4	*20*	69
	%C	51.4	27.8	24.7	45.2	29.1	30.5	51.4	36.7	22.0	48.4	19.4	29.0
	M	*18*	21	13	4	13	69	*11*	16	23	5	22	77
	P	12	*26*	40	1	29	108	13	*33*	26	5	24	101
By 6 variables	C	1	6	*26*	3	8	44	3	6	*22*	3	11	45
	G	2	9	33	*16*	22	82	3	9	32	*14*	19	77
	E	4	17	38	7	*31*	97	7	15	47	4	*27*	100
	%C	48.6	32.9	17.3	51.6	30.1	29.3	29.7	41.8	14.7	45.2	26.2	26.8

M = Mathematics, P = Physics, C = Chemistry, G = Geo-sciences, E = Engineering, %C = Classified correctly

Percent Correct Classification, Average of Samples 1 and 2

	M	*P*	*C*	*G*	*E*	*TOT.*
By 16 Variables	40.5	39.9	32.7	51.6	27.7	35.0
By 7 Variables	51.4	32.3	23.4	46.8	24.3	29.8
By 6 Variables	39.2	37.4	16.0	48.4	28.2	28.0

degrees of freedom, the minimum value of χ^2 is 26.7, with eight degrees of freedom, for a probability of .001. When the number of discriminators is dropped to six, omitting size of graduating class, the χ^2 value drops somewhat, the minimum value being 27.3 for the combination of the two replicated samples, with a probability of .001. It is thus apparent that even with only six discriminating variables, the individual columns, as well as the table as a whole, are significantly different from random distributions.

Turning to the five sub-fields of the physical sciences, whose classification matrices are given in Table 6, it is found that the individual rows are somewhat less well-differentiated, although the whole matrices are clearly not random. The field that is least well-differentiated from the others is engineering. Here, even with 16 variables, the obtained χ^2 values had probabilities as high as the 10% level, and, combining samples, the probability was still 4.4%. This suggests strongly that engineering is not clearly differentiated from the others in terms of the high school grades, courses, intelligence tests, and class size. One might recall, in this connection, that it is probably the rare high school student who in his own mind clearly distinguishes between engineering and physical science, in terms of career possibilities, while fields which have course content names, such as mathematics, physics, chemistry, and geology can probably be more clearly perceived as different, from the standpoint of the high school student. Also, engineering necessarily involves, in college and graduate training and on the job, the study and application of mathematics, physics, and chemistry, while, as vocational fields, mathematics, physics, and chemistry are often more highly specialized than engineering, involving less extensive excursions into their sister disciplines.

"THROUGH THE LOOKING GLASS"

Having established the fact that people who eventually attain doctorates in various fields are, as groups, distinctly different in their patterns of high school experiences and achievements, it is interesting to take a look at the data as if these discriminant functions had been available at the time these people were actually in high school. Assuming that it could be known that these samples of people would eventually attain the doctorate, the question would be to predict the field of doctorate from the data involved in the present study. What could one say about the probability of correct prediction under these hypothesized conditions? To summarize the data required for consideration of this question, Table 7 was prepared. In Table 7, the two samples are combined for greater reliability, and only the analyses based on 16 variables are used. The top portion of the table presents the data for the five general fields of doctorate, with the proportional samples and equal samples presented on alternate lines for ready comparison. In the bottom portion of Table 7, data for the five sub-fields of the physical sciences are given. There were only the proportional samples in the physical science fields.

The first data column in Table 7 gives the number of cases actually drawn in each field, combining samples 1 and 2. The second column gives the number classified into each field by the computer program—again the sum of the two samples. The third data column gives the summed number of hits in each field, and the fourth column gives the percentage of hits. The fifth column provides a ratio of the number classified into each field divided by the actual number in that field. The sixth and final column gives the "quasi-hit" ratio, that is the number of hits in a given field divided by the number classified into that field by the computer program. This final column may be thought of as the ratio of correct predictions, if

Table 7. Summary Data From Classification Matrices: Average of Two Samples Based on 16 Variables, With Derived Ratios

A. The Five General Fields of Doctorate

Field	*Sample design*	*Actual numbers*	*Classif. numbers*	*Hits*	*Hit rate*	*Classif. actual*	*Quasi-hit rate*
Phys. sci.	Prop.	300	362	183	.61	1.21	.51
	Equal	300	408	182	.61	1.36	.45
Biol. sci.	Prop.	251	216	92	.37	.86	.43
	Equal	300	312	125	.42	1.04	.40
Social sci.	Prop.	300	207	106	.35	.69	.51
	Equal	300	223	72	.24	.74	.32
Arts and prof.	Prop.	158	189	71	.45	1.20	.38
	Equal	300	247	102	.34	.82	.41
Education	Prop.	158	193	67	.42	1.22	.35
	Equal	300	310	132	.44	1.03	.43

B. The Five Fields of Physical Science

Field	*Sample design*	*Actual numbers*	*Classif. numbers*	*Hits*	*Hit rate*	*Classif. actual*	*Quasi-hit rate*
Mathematics	Prop.	74	147	30	.41	1.99	.20
Physics	Prop.	158	167	63	.40	1.06	.38
Chemistry	Prop.	300	184	98	.33	.61	.53
Geo. sci.	Prop.	62	149	32	.52	2.40	.21
Engineering	Prop.	206	153	57	.28	.74	.37

one were to base his predictions on the number of people who, at the high school stage, "looked like" the people in a given field. "Looking like" such a group is, operationally, having a pattern of attributes matched most closely with the typical pattern of people who eventually attain doctorates in that field.

Reading across Table 7, it can be seen that the "quasi-hit" rate, in the proportional samples, is lower than the actual hit rate in the fields where the computer program assigned more people to a field than occurred there in actuality, i.e., where the classification/actual ratio is greater than 1.00. Where this ratio is less than 1.00, the quasi-hit rate exceeds the actual hit rate. This is true in both the five general fields and in the physical science subfields. This can most simply be interpreted in terms of "target size." The larger the target, the greater the percentage of hits, when one looks forward in time from the stage of high school graduation to doctorate attainment. It may be observed, in this connection, that there is less variability in the quasi-hit rate than the actual hit rate, for the proportional samples. Suppose, however, that the actual "targets" are artificially constrained to the same size: What happens to hit rate and quasi-hit rate? This can be examined by looking at the data for the equal-size samples. It is found, as was noted earlier, that the hit rate tends to drop off somewhat, presumably because the natural "structure" of these vocational fields and their relations with one another is violated by the artificial restriction to equal size. This effect on the quasi-hit rate is two-fold: there is the general tendency to lower values, but there is also much less *variability* in the quasi-hit rate. The simplest and most straight-forward interpretation here is that the several fields, when thus artificially constrained in size, present more nearly uniform targets. Although more elaborate explanations, either mathematical or psychological, might be involved, it is difficult, with the present data, to sustain any more recondite hypotheses.

SUMMARY AND CONCLUSIONS

The present report is an outgrowth of a larger study of the high school backgrounds of the doctorates of 1958. For the present analyses, samples were drawn from several thousand cases upon whom complete data were available on 18 variables descriptive of the high school grades, test scores, and high school graduating class size for the 1958 doctorate graduates. The samples averaged somewhat over 100 cases per field for the five general fields of doctorate, drawn proportionately from these fields, and slightly smaller samples drawn from the five sub-fields of the physical sciences. Another set of samples, all equal in size, was drawn from the five general doctorate fields for a supplementary analysis.

All samples were subjected to analysis by a multiple discriminant analysis technique which isolates the characteristic pattern of traits for members of a particular field. The computer program then matches all members of all samples with each of the field profiles isolated in the discriminant analysis, and classifies each individual into a quasi-field which matches most closely his individual profile. The result is a "classification matrix" which shows how many members of each actual field are classified into each of the quasi-fields, including their own. The proportion allotted to their own actual field constitutes the "hit rate" for the program. In the present study it was found that by employing 16 variables in the discriminant analysis, the hit rate could reach about 40%, or twice the number who could be assigned to each field on the basis of chance alone. Analysis of these results by the Chi-squared technique showed that all of the classification matrices were reliably different from chance distributions, and that most of the individual fields also were reliably discriminated. Least well-differentiated from the others of its group was engineering, as distinct from the other sub-fields of the physical sciences. This was interpreted as due in part to the fact that engineering, an applied field, draws heavily upon the disciplines of all the basic sciences, which themselves were more highly differentiated from each other. All of the general doctorate fields—physical sciences, bio-sciences, social sciences, arts, and education—were reliably differentiated from each other.

It is concluded that there are characteristics observable at the high school level which clearly distinguish *as groups* people who will eventually attain doctorate degrees. Although these groups can be distinguished with a frequency twice that of a random assignment procedure, it cannot be held that individual cases are reliably assigned to eventual doctorate fields. When the groups who, at the high school stage "looked like" characteristic people in the several doctorate fields were sorted out, and then re-sorted into their actual eventual fields, it was found that the "quasi-hit rate" so derived was somewhat less than 40%, on the average, and that this rate depended in appreciable degree on the particular experimental sampling design, being less satisfactory when equal-sized samples were drawn than when the samples were drawn in proportions approximating the actual field sizes.

The present results are not regarded as being sufficiently definitive to permit the description of typical profiles which would be useful at the high school level in predicting which members of a given class would eventually attain doctorates in a particular discipline, even when that discipline is broadly defined. It is found, however, that differences characteristic of *groups* do exist. The characteristics of these group differences, at the high school level, can be more adequately studied by means of the data presently being assembled in a further study of the high school backgrounds of a large number of doctorate-holders of the period 1959–1962. The new study, which will analyze the entire record of each student, rather than just the science and math portions, should yield a more balanced and more discriminating profile. It is expected that it will improve upon the "hit rate" found in the present study. It is to be noted further that statistical techniques have advanced in the meantime, permitting a more meaningful description of the typical field profiles. The final recommendation is that these improved statistical techniques be employed with a wider range of data on larger samples from the 1959–1962 doctorates, with the confident expectation that results of wider applicability will be forthcoming.

Undergraduate Origins of American Scientists

John L. Holland

The role of undergraduate institutions in the production of scientists and scholars is ambiguous, despite two comprehensive studies by Knapp *et al.* (*1*, *2*). These studies suggest that certain colleges and universities are more productive of scientists and scholars than others —a kind of "institutional productivity" hypothesis. The results of the present study, however, argue for an opposed hypothesis, or a "student quality and motivation" hypothesis; namely, differential institutional productivity appears to be a function of the concentration of bright students at certain institutions and of differences in student motivation for scientific and scholarly achievement. Specifically, bright students congregate in institutions with high indices of scientific productivity; their explanations for their choice of college seem consistent with a need for scientific achievement, and they have fathers whose choice of occupation appears conducive to intellectual achievement. In contrast, bright students attend institutions with low indices of productivity in fewer numbers; they give explanations for their college choice which are suggestive of a more external or practical orientation to education, and they have fathers whose occupations suggest a fostering of persuasive or leadership skills and achievement.

The first study of this problem, by Knapp and Goodrich (*1*), implies that certain colleges and universities are more productive than others of men who are later listed in *American Men of Science*. The 50 colleges in this group, for the period 1924–34, are characterized by their small size, liberal arts orientation, and geographic location—Middle and Far West. In a second investigation, Knapp and Greenbaum studied the undergraduate origins of promising young scholars (*2*). For this study, the criterion of productivity is the number of graduate fellowships and scholarships and of Ph.D. degrees awarded per institution in proportion to the total number of their graduates for the period 1946–51. Taken together, these analyses

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imply that the scientist who has achieved distinction and his younger counterpart, the promising scholar in science as well as in other fields, originate the most frequently in two select sets of 50 colleges and universities and that the special qualities of these institutions facilitate achievement in science or scholarship in general.

The present study reexamines the "institutional productivity" hypothesis in the light of some new evidence obtained in the course of the National Merit Scholarship program. This scholastic talent search yielded several national samples of scholastically superior high-school seniors, with sufficient background data to explore three fundamental questions concerning institutional productivity. (i) Are differential rates of college productivity a function of differential attendance rates by scholastically superior students? (ii) Are differential rates of college productivity a function of student socioeconomic status? (iii) Are differential rates of productivity a function of paternal vocational motivation and the implied attitudes and values concerning science, scholarship, and achievement?

BASIC DATA

The high-school senior samples studied in the following sections include four similar groups obtained in the National Merit Scholarship program for the years 1955 and 1956: (i) The winners in the 1955 program, 556 National Merit Scholars; (ii) a 10-percent sample of the Certificate of Merit students, or 430 near winners for 1955; (iii) a 73-percent sample, or 3000, of the 4226 Certificate of Merit winners who replied to a questionnaire concerning present college attendance and scholarships (this sample overlaps sample ii); and (iv) a 10-percent sample of the 7500 finalists for 1957, or 750 Merit Scholars and Certificate of Merit students. Groups i, ii, and iii represent the survivors of a nation-wide testing program. All students ranked at least in the top 5 percent of their graduating class. Their average verbal and mathematical aptitude scores place them in about the top 2 percent of the graduating high-school population. Group iv represents a sample of the survivors of the 1956 program, in which 162,000 high-school seniors participated. The average scholastic ability score for this sample places them in about the top 5 percent of the high-school graduating population. Within these samples, more than 71 percent of each male sample have chosen a scientific career. The concentration in science for females is about 35 percent. More explicit descriptions of these populations are provided in several other reports (3).

All four samples are biased, since school participation in this program varies from state to state (22 to 80 percent) and since urban and larger schools participate more frequently than rural and smaller schools. No exact estimates of bias exist, but census estimates suggest that about 88 percent of the 1955 senior population did participate (*4*). The sampling for the 1956 program is indeterminate with respect to scholastic ability, since students could be nominated by their principal or could enter the program by nominating themselves without regard to their class rank.

RESULTS

Are differential rates of college productivity a function of differential attendance rates by scholastically superior students? A first test of this hypothesis was made by calculating the "expected" percentages of student attendance for both the Knapp and Goodrich and the Knapp and Greenbaum "high productivity" institutions. These estimates were obtained by totaling the college population for each of the two criterion lists of 50 colleges and determining what percentages they represent of the total undergraduate population for the United States (5). This simple method yielded "expected" percentages for groups of "high" and "low" productive institutions. These percentages were then compared with the "observed or actual" percentages of attendance for the Merit Scholar and Certificate of Merit samples of the 1955 program (groups i and iii). This analysis is contained in Table 1.

The data in Table 1 reveal that these talented groups attend "high" productive schools in frequencies which are 3 to 15 times the "expected" frequencies. For example, 7.3 percent of the Certificate of Merit males would be "expected" to attend the 50 Knapp and Greenbaum "high" productive colleges, and 92.7 percent would be "expected" to attend all other "low" productive colleges. Their actual attendance, however, at "high" and "low" productive colleges is 42.8 and 57.2 percent, respectively. Their actual attendance, then, at "high" productive colleges (42.8 percent) is about 6 times their expected attendance rate (7.3 percent). Similarly, their attendance at "low" productive colleges is less than their expected attendance. These results are consistent and statistically significant for both criteria. It is interesting to note that these trends hold even when the Knapp-Goodrich criterion is applied to female samples, although that criterion was devised to assess the institutional productivity of male scientists only.

It appears significant, too, that the productivity ratios for "high" productive schools approximate the ratios of students which they attract beyond their expected quotas. For the Knapp and Goodrich criterion, the median productivity ratio of "high" to "low" productive schools is about 4 to 1. High-ability students are attracted to these colleges in ratios of about 3 and 4 to 1. For the Knapp and Greenbaum criterion, the median productivity ratio of "high" to "low" productive institutions is about 4 to 1 for males and about 11 to 1 for females. The corresponding ratios of "observed attendance" of high-ability students to "expected attendance" are about 6 and 12 to 1 for males and females, respectively.

The present results are in agreement with the finding of Knapp and Goodrich (*1*), who report a correlation of 0.39 between scholastic ability and institutional indices of productivity for a sample of 50 colleges. Their limited data did not permit, however, a more complete exploration of this relationship or an estimate of the concentration of bright students in institutions with high indices.

It is possible that student knowledge of the colleges' productivity indices may have a marked effect on their choice of college, so that talented students shop for colleges with high indices. Accordingly, a sample of 7500 finalists (group iv) in the 1956 National Merit Scholarship program was studied in order to explore this possible bias.

The data for this analysis were obtained from scholarship application blanks in response to the query, "Why have you selected the college named on page 1 [the student's choice]?" The students who had selected one of the 50 institutions that ranked highest in productivity in the Knapp and Goodrich study were contrasted with an equal, random sample of students who had selected other colleges and universities. The blanks were reviewed for indications that the students had any knowledge of the productivity studies that are reported here or of similar information, the implication being that such information might have influenced their choice. This search was negative.

To judge from their verbal reports, students select colleges largely from considerations of convenience, cost, familial affiliations, academic reputation, and so on. No student mentioned the productivity articles, but one of the 1,628 students sampled appeared to have some secondhand knowledge of this material. It appears, therefore, that the differential attendance of high-ability students at the 50 selected institutions was not influenced by publication

Table 1. Percentage of Expected and Actual Attendance of High-Aptitude Students at "High" and "Low" Productive Institutions. Each 2 by 2 Table is Significant beyond the 0.001 Level of Significance

	Males				*Females*			
	Cert. of Merit college attendance		*Merit Scholars college attendance*		*Cert. of Merit college attendance*		*Merit Scholars college attendance*	
Criterion	*Expected*	*Observed*	*Expected*	*Observed*	*Expected*	*Observed*	*Expected*	*Observed*
Knapp and Greenbaum								
High productivity	7.3	42.8	7.3	43.3	1.6	16.2	1.6	24.7
Low productiivty	92.7	57.2	92.7	56.7	98.4	83.8	98.4	75.3
Total	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
Knapp and Goodrich								
High productivity	3.9	13.4	3.9	16.2	3.9	14.9	3.9	17.5
Low productivity	96.1	86.6	96.1	83.8	96.1	85.1	96.1	82.5
Total	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0

of the Knapp studies; an explanation of the differential rate must be sought elsewhere.

Are differential rates of institutional productivity a function of differential socioeconomic status among student populations? Since earlier work by Knapp and Goodrich (*1*) suggests that scientists may originate more frequently in "lower," rather than in "higher," socioeconomic groups and that conceivably, then, institutions with high indices of productivity may attract larger proportions of students of lower socioeconomic status, a test of this hypothesis appeared desirable.

For this test, the socioeconomic status of groups i, ii, and iv was determined by means of the Minnesota Scale for Paternal Occupations. For males, the distributions of fathers' occupations were found to be not significantly different for "high" and "low" productive institutions. For females, the differences within groups i and ii are not significant. The distributions of status for group iv—a 10-percent sample of 1956 finalists—reveal statistically significant differences. Students selecting "high" productive schools come from higher socioeconomic status than those selecting "low" productive schools. The efficiency of this discrimination is moderate. However, since five out of six tests are clearly negative, there is little evidence that differences in institutional productivity are due to different levels of socioeconomic status among student bodies.

Are differential rates of institutional productivity a function of parental vocational motivation and their implied attitudes and values concerning science, scholarship, and achievement? Restated, students attending "high" and "low" productive colleges may represent different, socially derived familial vocational motivations which might account for the differential productivity of scientists between these groups, and, by implication, "high" and "low" productive colleges may attract different kinds of people, or personalities.

Groups i, ii, and iv were employed to test this hypothesis. The fathers' occupations for these samples were classified in one of the following six vocational interest groups: physical activity (manual and skilled trades), science, welfare or social service, clerical or business detail, persuasive or verbal activity (oral language occupations, including selling, law, and supervisory occupations), and esthetic or artistic occupations (6).

Distributions were then formed for institutions with high and low indices of productivity for each sample. The Knapp-Goodrich criterion was employed in this analysis, since it appears to be the more valid of the two criteria—valid in the sense that it represents an ultimate, rather than an immediate, criterion. The results of this classification for males are shown in Table 2. The results for females are clearly negative for all samples and are not reported.

Table 2 displays two meaningful trends in

Table 2. Percentages of Fathers' Occupations Falling in Various Fields of Work for Students Selecting "High" and "Low" Productive Institutions (Males)

	Group i *Merit Scholars*		*Group ii* *Cert. of Merit*		*Group iv* *finalists*	
Field of work	(*No. = 65*) *H.P.*	(*No.,= 337*) *L.P.*	(*No. = 47*) *H.P.*	(*No. = 261*) *L.P.*	(*No. = 76*) *H.P.*	(*No. = 449*) *L.P.*
Physical activity	24.6	18.7	19.0	15.5	21.1	17.8
Science	16.9	12.2	21.4	15.5	15.8	12.7
Welfare	12.3	10.9	16.7	9.1	14.5	7.1‡
Clerical	12.3	10.7	4.8	8.3	6.6	7.8
Persuasive	21.5	40.1*	28.6	44.2†	27.6	46.1*
Esthetic	3.1	2.9	4.8	0.0	0.0	1.1
Unclassified	9.2	4.5	4.7	7.4	14.4	7.3
Total percentage	99.9	100.0	100.0	100.0	100.0	99.9

* Difference between percentages significant at 0.01 level.

† Difference between percentages significant at 0.06 level.

‡ Difference between fathers' occupations for students selecting institutions with "high" and "low" indices of productivity is significant at 0.05 level of significance.

all three samples. Students attending or selecting "high" productive colleges tend to have fathers who are engaged in occupations characterized by their physical-activity, scientific, or social-service aspects. In contrast, students in "low" productive colleges have fathers who are engaged in occupations which are characterized by their persuasive, sales, supervisory, and leadership orientation. The differences for the "persuasive" category are statistically significant at the 0.01 level for groups i and iv and at the 0.06 level for group ii. The remaining categories are not statistically significant; however, the grouping of the physical-activity, science, and welfare occupations and of the clerical, persuasive, and esthetic categories for an overall test is significant beyond the 0.05 level for all three samples. It should be emphasized, however, that the total difference between "high" and "low" productive samples is largely due to differences found for the "persuasive" category.

The implications of these findings are several. They suggest that the "high" and "low" productive colleges attract somewhat divergent student populations among male, high-aptitude high-school seniors. On the one hand, "high" productive colleges attract students whose fathers work with their hands (machines or tools), with scientific ideas or apparatus, or with people in a social-service sense (especially teaching). Such a background appears conducive to achievement in science or to an emphasis on intellectual attainment. In contrast, "low" productive schools attract students whose fathers' work is characterized more by its oral, persuasive, or leadership activities (in particular, supervisory and ownership positions in business, law, and government). Correspondingly, these backgrounds appear to be less fertile for the development of young scientists.

These parental vocational differences also suggest differential values and attitudes toward achievement in American society. Earlier psychometric analyses indicate that interest in science is associated with theoretical values, with a relative deemphasis on political and economic values; business interests are positively correlated with political and economic values, with a deemphasis on theoretical and scientific values. This evidence has been summarized recently by Roe (7).

The verbal and social quality of the "persuasive" occupations suggests another area of disparity in the backgrounds of students selecting "high" and "low" productive colleges. Studies by Stern *et al.* (8), Roe (7), and others consistently reveal that scientists as a group are less social than many other occupational groups, especially men in sales and leadership occupations. It seems reasonable to assume, then, that the children of scientists and workers would acquire somewhat similar social skills and interests and experience which, in some measure, would impel them toward achievement in relatively nonsocial pursuits. Conversely, the children of fathers who have oral and leadership skills may have their verbal and social skills fostered so that their achievement is directed toward verbal or language fields. Such a formulation implies that good business and government leaders may originate the most frequently in institutions which produce relatively few scientists, and vice versa (9).

This loose theorizing suggested the need for a more elaborate analysis of the college-choice evidence reviewed previously for information about productivity only; that is, student explanations of college choice for groups selecting "high" and "low" productive colleges should contain differences which are consonant with the foregoing formulations. The test of this formulation follows.

Verbal Explanations of College Choice

For this analysis, the verbal explanations of college choice for all 1956 finalists (group iv) selecting "high" productive colleges were compared with those of an equal random sample of students selecting "low" productive institutions. Male student statements were classified by two graduate student judges having no knowledge of either this report or of similar reports. Female statements were classified by a single judge. The discrepancies in the two judges' coding of verbal reports range from 0.4 to 6.6 percentage points.

For males, the statistically significant differences in Table 3 reveal that a college of "academic standing, small size, research reputation, or liberal arts orientation" is desired more frequently by students selecting institutions with high indices of productivity. Students choosing institutions with low indices are less concerned with these qualities and are more concerned with attending a "good college, one which is close to home, or in a desirable location."

The differences for females form a similar pattern. Women selecting "high" productive colleges prefer a college of "small size" and a "liberal arts orientation." Among women selecting a college with a low index, these qualities are less valued, and attending a "good college, or one of prestige" is held more valuable.

In all, the significant differences in Table 3 appear consistent with the formulation outlined earlier. To interpret, the selection of institutions with high indices appears to be a function of needs for a small social group or organization and a broad, intellectually oriented educational experience. In contrast, the selection of institutions with low indices appears to reflect confidence in living in a large, social group and a more external—perhaps more practical—orientation, including considerations of "prestige" and a "good college."

Table 3. Summary of Verbal Reports of College Choice by High-School Seniors Selecting "High" and "Low" Productive Institutions

Reported reasons for choice of institution	*Males (No. = 1088)*		*Significant difference P*	*Females (No. = 540)*		*Significant difference P*
	H.P.	*L.P.*		*H.P.*	*L.P.*	
Good college	47.9	55.1	<0.05	35.6	45.6	<0.05
Close to home	14.3	18.4	<0.07	12.9	10.0	
Low cost	3.9	5.5		2.9	5.9	
Academic standing	20.6	15.6	<0.05	33.3	27.4	
Small size	27.9	8.5	<0.001	38.5	15.2	<0.001
Recommended by friends and others	15.1	15.8		12.6	11.5	
Good faculty	10.3	8.6		4.1	6.7	
Prestige of college	6.3	5.1		0.0	5.2	<0.001
Desirable location	9.9	13.6	<0.06	9.6	10.7	
Religious affiliation	4.2	6.3		8.9	11.1	
Physical facilities	6.8	8.1		0.7	1.9	
Research reputation	2.0	0.2	<0.01	0.4	0.4	
Liberal arts orientation	17.1	11.2	<0.01	22.2	10.0	<0.001
Miscellaneous	25.2	18.6		15.9	29.6	

This evidence also tends to negate the assumed effects of the small, liberal arts college strongly implied by the Knapp studies. The student concentration and motivational hypothesis appears more probable; that is, small, liberal arts colleges may attract a disproportionate number of students who are consciously seeking a small, liberal arts college and are relatively homogeneous and potentially high achievers with respect to scientific and scholarly eminence. And while these criterion colleges may have beneficial and nurturing qualities, the student talent and motivation they receive by virtue of their reputation and orientation may outweigh the college influence itself. In short, differential student populations among colleges appear as a more probable explanation of differences in productivity than the special qualities of individual institutions.

DISCUSSION

The validity of this study rests largely on its sampling of high-school seniors in the United States and on several psychological assumptions concerning the relationship of scientific eminence or scholarship to scholastic aptitude and achievement at the high-school senior level. For 1955, the high-school senior samples appear to be approximately representative of high-aptitude seniors, since an estimated 88 percent of the eligible seniors did participate. Census estimates reveal that the nonparticipating 12 percent is concentrated largely in 13 southern states, a bias which suggests the effect of a more representative sample.

It is likely that the concentration of high-aptitude students in "high" productive colleges revealed by this study would be unaffected by a complete sampling, since these institutions occur very infrequently in the South and with great frequency in the Far West and Middle West. This possibility is reinforced by a previous report which reveals a marked tendency for students to attend colleges close to home or in their own geographic region (*10*).

With respect to psychological assumptions, it is assumed that scientists and scholars are drawn generally from students who are characterized by their high academic achievement in secondary schools and by their high scholastic aptitude and verbal and mathematical aptitudes. This assumption is supported by extensive studies reviewed by Wolfle (*11*).

This study assumes further that early academic achievement is predictive of later scientific eminence (achievement) and scholarship in other areas. For this assumption there is also extensive support. Studies by Cox (*12*), Terman (*13*), and others reveal that later achievement has many forerunners of which academic achievement is only one.

The present study militates strongly against the "institutional hypothesis." In opposition to the latter formulation, the present evidence suggests that differential productivity is probably a function of differential student populations which may be characterized by (i) their divergent proportions of high-aptitude students and (ii) their differences in motivation (scientific and intellectual activities as contrasted with business and oral activities) as well as by a number of implied differences in attitudes and values. The second hypothesis appears to parallel the work of Stern, Stein, and Bloom (*8*), who report marked variability in percentages of personality types or patterns within five divergent institutions. Briefly, high productivity appears to be an expected result of working with an intellectually well endowed student body which tends coincidentally to have a modal orientation toward scientific and scholarly goals. It appears more probable that college productivity indices should be attributed to student characteristics rather than to institutional influences.

This formulation might be modified further by the time dimension. It appears possible that some institutions in their early history may have stimulated in a substantial manner students who later achieved the Ph.D. degree and eminence in science and in other areas and that through such activity these institutions acquired reputations which differentially attracted desired student groups and repelled undesirable ones. As an end-result, investigators are faced with the problem of disentangling the influence of an institution from the quality and character of its student body, both of which are probably changing. In addition, there may be a snowballing effect of college influence, which may be out of proportion to its real effects. This sequence might occur in the following manner. (i) A college influences students to achieve in areas of science and scholarship; (ii) a few talented students respond by attaining fellowships and Ph.D. degrees, and perhaps one becomes a recognized man of science; (iii) prospective students are impressed, possibly by these achievements but more probably by such evaluations as "tough school" and "high standards"; (iv) brighter students with high intellectual goals enroll in great numbers; (v) the odds for the success of college influence are now greater; (vi) there is more achievement; (vii) more bright students enroll, with science or other appropriate scholarship orientation, and students with low academic motivation or motivation for goals which represent the minority of the student body find more suitable institutions; (viii) and so on. This oversimplified analysis suggests that teacher influence may have marked effects on scholar productivity in the early years of the history of a college, but that this influence may decrease to the point where the level and character of the student population become of much greater importance as the institution develops in time.

SUMMARY

An analysis of the college attendance or college choice for four high-aptitude, high-school senior samples suggests that the differential institutional productivity of scientists and scholars is a function of the differential college attendance, paternal vocational motivations, and their implied correlates among high-aptitude students. This formulation appears to be more probable for males than for females. The institutional productivity hypothesis proposed in previous studies is not supported by the present evidence.

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Background and Early Training of Psychologists

K. E. Clark

Every psychologist can provide an anecdote purporting to indicate how he happened to get into the field; many of our respondents did so in their replies to our inquiries about factors leading to their entry into psychology.[1] Attempts to categorize such responses have not been too successful; our classes have been crude and numerous. Yet, while our study of individual responses emphasizes uniqueness, a bit of sense seems to emerge as we examine the composite. This in spite of our having devoted a relatively small part of our schedule to these factors. . . .

To what extent is psychology able to capture the imagination of a person at a fairly young age, to lead him to think about psychology consistently as preparation to enter the field, and to what extent does psychology eventually capitalize upon a long-standing dedication to this area of investigation? As a fairly young profession, psychology has had less opportunity to fire the imaginations of younger persons. Since it is only infrequently taught below the college level, it perhaps suffers from a lack of information about the duties and areas of activity of psychologists. An examination of motivations and of early vocational plans thus seems desirable. The question asked first of our respondents was, "At what period in your life did you first think of psychology as a career?" The analysis of responses to this question are presented in Table 1. Obviously even thinking about psychology does not occur very early. Less than 10 per cent of any of our groups began thinking about psychology in high school; the undergraduate program in colleges does the most to stimulate interest in psychology, especially the last two years, and with particular effect on Significant Contributors. Furthermore, there is evidence of an increase over the passage of time in the number of Significant Contributors who thought about psychology as a career in the first two years of college. No such trend exists for the control group. If the trend in these tables continues, we would expect that persons who first think about psychology as a career during their freshman and sophomore years would be more likely to become Significant Contributors to psychological science than persons who think about this at a later time. Responses in Table 1 also indicate that war or work experiences have a differential influence, in that those persons who first think about psychology in the context of a job or at some time after college graduation tend to be less likely to become significant contributors. This point needs to be related to a set of data which will be presented somewhat later, having to do with the amount of time between receiving a bachelor's degree and receiving a doctoral degree for our Significant Contributors and the Psychologists-in-General. The evidence points clearly to the fact that a person who has a long interval between his BA and PhD is quite unlikely to be a significant contributor to psychological science. The persons who first think about psychology as a career after college graduation or during war experience or work experience are persons whose elapsed time between bachelor and PhD degree is likely to be considerably increased. It may well be also that these are people who are interested in psychology not as a science but as

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[1] EDITORS' NOTE: This article is an abridgement of Chapter 5 of the survey undertaken by the American Psychological Association in 1957 to study the composition of the research force in psychology and the research potential of those in training and those being recruited. To study simultaneously the psychologists who were research scientists and those who devoted themselves to other professional activities, two groups of subjects were surveyed: 150 significant contributors to psychological science and a random sample of other APA members. Of the 150 significant contributors, 50 received their first doctoral degree during 1930–34; another 50 during 1935–39, and the last 50 between 1940 and 1944. The persons embraced by the terms "significant contributors" were not selected primarily on the basis of a large amount of publication, nor on the evaluation of published work. Rather they had to have published a considerable amount or else be known to leaders in the profession, and needed to acquire a substantial number of nominations from their colleagues who were asked to indicate persons they thought to be significant contributors to psychological science.

Table 1. Time of First Thinking About Psychology as a Career

	Year in which doctoral degree was received					
	1930–34		*1935–39*		*1940–44*	
	*A**	*B**	*A*	*B*	*A*	*B*
In high school	2%	6%	2%	5%	8%	6%
In first 2 years of college	15	20	26	21	29	19
In last 2 years of college	59	33	44	29	42	36
Shortly after college graduation	20	21	14	28	12	22
During war or work experience	0	9	2	5	2	9
At another time	4	11	12	11	6	7
No answer	0	1	0	2	0	1

* A—Significant Contributors; B—Psychologists-in-General.

a profession or technology; that, in other words, they desire to use psychology in its applications in business or industry or to the solution of personal problems rather than to work in psychology to develop a science of behavior. Certainly if it is our desire to stimulate the training of larger numbers of significant contributors to psychological science, these data suggest that the recruiting process ought to occur in the undergraduate college and that opportunities should be provided for students to continue directly into graduate work. While it may be quite profitable to attract the attention of able persons who are otherwise vocationally engaged after completion of college work for further training in psychology, the likelihood that they will make substantial break-throughs in knowledge and will thereby merit the esteem of their colleagues is not very great.

It is frequently stated that psychology draws as students a considerable number of unsuccessful medical school applicants. The better understanding of the potential resources for research personnel in psychology requires some examination of the career possibilities rejected or discarded by people who finally decided to enter psychology. It should be noted that this presents only half of the picture, since it does not sample the opinions and experiences of persons who at one time thought about entering psychology as a profession and then decided against it. The results presented in Table 2, which have to do with the careers rejected by persons who did enter psychology, would yield

Table 2. Career Possibilities Rejected or Discarded When Respondents Finally Made Decision to Enter Psychology (First answer only)

	Year in which doctoral degree was received					
	1930–34		*1935–39*		*1940–44*	
Career	*A**	*B**	*A*	*B*	*A*	*B*
Grade school or high school teaching	4%	10%	6%	8%	10%	12%
College teaching	11	8	10	13	17	11
Education, miscellaneous	0	7	4	7	2	7
Business and related activities	13	14	22	8	19	13
Fine arts and music	4	3	4	4	6	3
Physical and biological sciences	6	3	6	4	8	3
Medical sciences	11	8	16	11	10	16
Mathematics and statistics	2	1	2	1	0	1
Social science and social welfare	2	5	0	4	4	6
Law, gov't. and politics	9	9	12	7	10	3
Engineering and architecture	13	5	10	5	4	8
Farming and forestry	4	0	0	0	0	2
Skilled trades	0	1	0	0	0	0
Miscellaneous	11	7	0	9	4	3
None or undetermined	11	19	10	16	2	11

* A—Significant Contributors; B—Psychologists-in-General.

much more understanding of the process of career choice if they also showed which occupations or careers were accepted at the time psychology was rejected.

Those who obtain PhD's in psychology are obviously not, on the whole, persons who failed to get into medical school or who decided against it. Table 2 suggests that there is no single major source of persons who enter psychology. As many as any other single group were those who wanted college teaching (and who perhaps found it unnecessary to discard this possibility when entering psychology). Grade school or high school teaching had previously attracted a fair number. Business and related activities are about the most frequent of the discarded career possibilities. These data do not indicate any consistent trends with time nor any meaningfully consistent differences between the Significant Contributors and Psychologists-in-General. They do, however, emphasize the point that the sources for psychologists are very diverse indeed.

One question related to factors considered by the respondent to be most important in leading him to decide to enter psychology. This question had been asked in a pretest form of our questionnaire with a series of responses to be checked and with an opportunity for the person to add additional categories of response. As a result, at the time the final questionnaire was prepared it was possible to present a fairly long list of motivational factors which might have influenced a person to enter psychology. (Since these data are retrospective they permit only conjectures about the types of statements a person might have made at this earlier date.) This list is presented in Table 3, with the percentage of each group who checked each item on the list.

The proposition being examined in using a question of this sort is that a psychologist who enters the field because of strong motivations in sciences is much more likely to become a significant contributor than is a person who enters with motivations primarily of a service nature. A number of specific items of Table 3 tap these particular points. Thus the item, "Desire to work with individuals or groups" is probably more service than science oriented, whereas the item, "Interest in human behavior as a field for scientific investigation," is a science item with very little service overtone. It is quite interesting to note that the former item differentiates quite well between the Significant Contributors and the Psychologists-in-General, whereas the latter item is one which is subscribed to by substantial, and about equal, numbers of both groups. Furthermore, items having to do with applications of psychology in various areas make sharp differentiations between the Significant Contributors and the Psychologists-in-General. An item doing this particularly well is "interest in the application of psychological techniques in such areas as clinical, educational, or industrial psychology." If we may summarize the contents of Table 3 in terms of the motivations of Significant Contributors, it would be approximately as follows. A person was more likely to become a Significant Contributor if he entered the field of psychology with a desire for doing research in the area, with an interest in quantitative analysis of psychological data, and if he had been influenced by having heard or read about research in this area. He need not have had any particular desire to work with individuals or groups, nor have had an interest in the application of psychological techniques in such areas as clinical, educational, or industrial psychology and perhaps ought not to have had as a major factor of influence having the field recommended to him by a counselor or friend. It also appears that among persons getting degrees in more recent years it is desirable for someone to have offered him a job or an assistantship and for this, and a desire to do research in this field, to have been important factors in leading him to his decision.

These motivational factors need to be reviewed not only in terms of the differential responses of the Significant Contributors and the Random Control group but also in terms of the general level of percentage response to each. One of the most important of the factors leading persons to select psychology as a field is being interested in human behavior as a field for scientific endeavor. Another is having become interested in the field through the content of courses in psychology, and hearing or reading about research in the general area. Another is wanting to know more about human beings and their behavior. Thus psychologists, whether Significant Contributors or not, have as primary motivation an interest in people,

Table 3. Factors Considered as Most Important in Leading to the Decision to Enter Psychology

	Year in which doctoral degree was received					
	1930–34		*1935–39*		*1940–44*	
Factor	*A**	*B**	*A*	*B*	*A*	*B*
Being influenced by a particular teacher of psychology	59%	60%	48%	48%	58%	54%
Courses were easy for me	20	17	24	28	31	29
Becoming interested in the field through reading books in psychology	37	36	38	32	40	44
Hearing or reading about research in this field	26	22	30	25	44	23
Need to understand myself	17	12	10	21	17	19
Influence or example of relative or friend	9	13	16	6	15	9
Desire to work with individuals or groups	15	34	10	29	10	32
Interest in human behavior as a field for scientific investigation	59	55	62	59	52	47
Doing research in this field	37	26	28	22	48	25
Interest in the application of psychological techniques in such areas as clinical, educational, or industrial psychology	15	46	30	44	35	43
Inability to get training in an area that interested me more	4	2	2	3	4	5
Having a field recommended to me by a counselor or friend	4	11	14	6	4	9
The prestige of psychology	2	2	0	4	4	5
Becoming interested in the field through the content of courses in psychology	43	47	50	46	50	46
Having been helped in personal problems by a psychologist	2	4	0	2	6	4
Desire to solve society's problems	22	11	10	11	19	16
Someone offered a job or assistantship	17	20	22	18	35	18
Desire to enter a profession of some type	15	13	26	13	12	14
Interest in the quantitative analysis of psychological data	26	13	28	12	29	12
Difficulty in getting support for work in another department	0	1	6	3	6	2
Desire to enter a field which seemed to offer fairly lucrative rewards	2	2	2	4	2	1
Wanting to know more about human beings and their behavior	52	52	44	54	40	56
Other	13	18	0	22	21	17

* A—Significant Contributors; B—Psychologists-in-General.

and a desire to work with problems of human behavior. They seem to learn about psychology primarily through teachers, through courses and through general reading. Very little of their contact seems to be through having been helped with personal problems by psychologists, having the field recommended by a counselor or friend, or by need to understand themselves. "Desire to enter a field which seems to offer fairly lucrative rewards" is an item which is not selected by very many persons, nor were the items, "Difficulty in getting support for work in another department," and "Inability to get training in an area that interested me more."

The importance of being offered a job or an assistantship as a differential factor in the recruitment of Significant Contributors to psychological science has already been mentioned. Table 4 presents more specific evidence on this point. Each respondent was asked to indicate whether or not at the time he made first application for graduate work in psychology he was offered a job or an award, and the nature of the award. A considerably greater proportion of the Significant Contributors were subsidized on their entry into psychology. Furthermore, the extent of subsidy apparently increased from the period 1930–34 to the period 1940–44 so that by the last period 71 per cent of the Significant Contributors and almost half of the controls received some kind of financial support for their graduate work. The nature of this support is also indicated. Scholarships, fellowships, and teaching assistantships were the most popular means for subsidizing graduate

Table 4. Was a Job or an Award Offered at the Time of First Application for Graduate Work in Psychology?

	Year in which doctoral degree was received					
	1930–34		*1935–39*		*1940–44*	
	*A**	*B**	*A*	*B*	*A*	*B*
Yes	52%	39%	60%	34%	71%	42%
No	48	56	40	65	29	57
No answer	0	5	0	1	0	1
If "yes," kind of award:						
Scholarship or fellowship	23	16	12	10	27	9
Teaching assistantship or fellowship in psychology	17	19	36	18	29	22
Research assistantship or fellowship in psychology	6	2	4	3	10	7
Related outside work	2	1	0	2	0	2
Unrelated outside work	0	1	6	1	2	2
Clinical or counseling appointments (not VA)	4	1	2	0	2	2

* A—Significant Contributors; B—Psychologists-in-General.

students in psychology through all of the fifteen-year period. . . .

Are persons who are attracted into psychology outstanding students as undergraduates? Table 5 provides rather reassuring data on this point, although it is only a retrospective self-evaluation. Three out of four of our Significant Contributors rate themselves as having been in the top 5 per cent in their undergraduate major subject. This proportion is somewhat lower for the controls. Somewhat more than 50 per cent of our Significant Contributors rate themselves as having been in the top 5 per cent of their college undergraduate class in all courses. Only about one out of three of the controls so rate themselves. Thus while we remain in ignorance about the actual undergraduate record, the achievement as recalled now by our research psychologists and their contemporaries was substantially different for the two groups.

Both groups rate themselves well above average for general college undergraduate populations. This may be due, of course, to a somewhat "mellow" recall and interpretation of one's undergraduate record. Even so, it seems unlikely that these groups as a whole were anything like an average college group, since each one of these persons did receive a doctoral-level degree.

A question frequently asked is whether or not a psychologist can prepare himself better

Table 5. Present Estimate of Standing in Undergraduate College Work

	Year in which doctoral degree was received					
	1930–34		*1935–39*		*1940–44*	
Standing	*A**	*B**	*A*	*B*	*A*	*B*
In Major Subject						
Top 5%	76%	55%	70%	66%	85%	62%
Next 20%	20	34	22	24	6	32
Next 30%	4	5	2	4	2	4
Bottom 50%	0	1	4	1	4	1
In all Courses						
Top 5%	57%	34%	52%	32%	65%	37%
Next 20%	35	45	32	54	19	46
Next 30%	4	14	8	9	8	11
Bottom 50%	2	1	4	1	4	2

* A—Significant Contributors; B—Psychologists-in-General.

by taking an undergraduate major in mathematics, or biology, or zoology, or philosophy, or sociology rather than taking an undergraduate major in psychology. Many persons make such recommendations or hold such beliefs. Obviously it is impossible to give any kind of definitive answer to questions of this sort or to make a general statement that would apply to any substantial and identifiable group of undergraduate students. However, we can say that a very substantial proportion of our PhD groups had undergraduate majors in psychology. This amounts to a percentage between 47 and 60 for our Significant Contributor group and between 29 and 37 for their random controls. There is a larger number of persons with undergraduate majors in psychology in our samples than in any other single area. The Significant Contributor group is made up to a much larger extent of persons who did have undergraduate majors in psychology. If these historical data have any predictive value, they would indicate that a person has a greater likelihood of attaining eminence in psychology if his undergraduate major is psychology. On the other side of the coin, a person whose undergraduate major is in the humanities is less likely to become a Significant Contributor than he is if his major is in almost any other field.

These data are shown in Table 6. No support is given for the frequently stated proposition that the ideal undergraduate major is mathematics. Only a small proportion of either the Significant Contributor group or the control group has such an undergraduate major, and there is no differential in favor of the Significant Contributors as far as this variable is concerned. Likewise Table 6 does not lend any support for the feeling that a substantial number of PhD's in psychology started out as premedical students. The number of persons who were either in medical sciences or in premedical programs is infinitesimal. An additional feature worth noting is the especially high (60 per cent) figure for psychology majors in the 1940–44 Significant Contributor group. This increase suggests that as psychology majors become more frequent, and psychology more conspicuous, fewer ultimate potential Significant Contributors will fail to be caught as undergraduates.

Table 6. Undergraduate Major Subject

	Year in which doctoral degree was received					
	1930–34		*1935–39*		*1940–44*	
Major subject	*A**	*B**	*A*	*B*	*A*	*B*
Psychology	47%	29%	48%	37%	60%	37%
Humanities	21	30	14	25	19	26
Social sciences	9	10	4	7	4	9
Natural sciences	9	10	8	3	4	10
Mathematics	4	4	6	8	4	4
Business	4	2	2	5	4	2
Engineering	2	1	2	0	0	2
Education	4	6	8	6	4	9
Misc. or no answer	0	7	8	7	0	3

* A—Significant Contributors; B—Psychologists-in-General.

Undergraduate Institutions and the Production of Scientists

Alexander W. Astin

The number of undergraduate students who abandon plans to pursue a career in science far exceeds the number who decide to enter science from other fields. Hence, the number of qualified individuals who are available to enter fields in which there is already a shortage of trained manpower tends to be reduced. The factors in the student's undergrad-

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uate college experience which affect his motivation to pursue a career in science therefore seem worthy of investigation.

Among the factors which may influence a student's decision to pursue a career in science at graduation are his personal characteristics at the time he enters college and the type of college he attends. But it is difficult to study the influence of the institution, since students are not distributed randomly among different types of colleges. Under these conditions, variations among institutions with respect to the career plans of their graduates may reflect differences in the kinds of student bodies initially recruited, differences in the effects of the institutions themselves, or some combination of these two factors.

The importance of using rigorous research designs in attempting to compare the effects of different types of colleges on student performance is clearly illustrated by the history of the "Ph.D. productivity" problem. In the earliest studies (*1*) it was found that undergraduate institutions differed markedly in the proportions of their graduates who eventually obtained Ph.D. degrees. Such differences were "explained" in terms of the college's characteristics: type of control, level of training of the faculty, geographical region, laboratory facilities, and so on. However, in subsequent studies (*2*) it was found that these differences in output of Ph.D.'s could be attributed at least partially to the characteristics of the entering students, rather than wholly to the effects of the institutions themselves. Two recent studies (*3, 4*) have, in fact, shown that many of the institutions which were classified previously as "highly productive" turn out to be among the most "underproductive" when selected characteristics of their student inputs are controlled.

The study discussed in this article compares the effects of different types of colleges on the motivation of high-aptitude students to pursue careers as scientists. The technique used to control student input, which is described for each section, was an adaptation of the methods used in recent studies of Ph.D. productivity.

SUBJECTS

The sample of 6254 Merit finalists and recipients of the letter of commendation from the National Merit Scholarship competition included 4235 boys and 2019 girls who entered college in the fall of 1957. Shortly after enrollment, each student completed a questionnaire on his intended course of study in college, his educational and vocational plans, his socioeconomic background, and miscellaneous related matters. Data from these questionnaires, together with scores on aptitude tests and class ranks in high school, which had been obtained 1 year earlier in connection with the Merit scholarship competition, were used as input (control) data. Twenty-one control variables were selected:

1) Career choice in 1957.
2) Major field of study in 1957.
3) Sex.
4) Highest degree sought in 1957 (three-point scale: bachelor's, master's, or doctor's).
5) Verbal aptitude (Scholarship Qualifying Test, or SQT).
6) Mathematical aptitude (SQT).
7) SQT selection score: Mathematical aptitude score plus twice the verbal aptitude score.
8) High school grades (percentile rank).
9) Degree of satisfaction with career choice (dichotomy: "very satisfied" versus all other degrees of satisfaction).
10) High school curriculum (dichotomy: academic versus all others).
11) "Drop-out" predictor, a scale developed in a previous study (*5*) to predict dropping out of college.
12) Number of semesters of mathematics taken in high school.
13) Number of scholarships applied for in 1957.
14) Number of scholarship offers received in 1957 (dichotomy: none versus one or more).
15) Distance from home to college.
16) Commuter versus resident in 1957.
17) Percentage of high school peers attending college in 1957.
18) Father's occupation (dichotomy: executive and professional versus all others).
19) Father's educational level.
20) Mother's educational level.
21) Number of books in the home.

During the late spring of 1961—approximately at the time of graduation—the students completed a second questionnaire in which they again reported their career plans. Students who indicated that they intended to do scientific research or to teach science at the college or university level were designated as pursuing a "career in science." This dichotomous criterion served as the dependent variable for the study.

COLLEGE VARIABLES

A subsample of 1548 students was selected for the analyses of the differential effects of college. Only students who attended one of the 82 colleges and universities which enrolled 15 or more students from the total sample of 6254 in the fall of 1957 were included in the subsample. To minimize the contribution of any single institution, not more than 25 students from any single college were chosen. If a college enrolled more than 25, the students for the subsample were selected at random.

Ten measures of institutional characteristics or "traits" were used to assess the college. Four were general characteristics (affluence, size, homogeneity, and masculinity), for which measures were taken from a recent factor analysis of 335 institutions (*6*), and six were "personal orientations" (realistic, intellectual, social, conventional, enterprising, and artistic), measured according to the Environmental Assessment Technique. Scores on these "personal orientations" are obtained by determining the percentage of baccalaureate degrees awarded by the institutions in each of six broad fields. Recent studies (*7*) have shown that these percentages yield valid information about the psychological climate at the institution.

The 82 institutions were also classified according to five a priori "type" characteristics: coeducational liberal arts colleges, men's colleges in the Northeast, women's colleges, public universities, and technological institutions. Each of these "type" characteristics was scored as a dichotomy.

TRENDS IN CAREER CHOICE

Data for the remaining 4706 students were used to study trends in career choices and to obtain weights for two of the input variables. Table 1 shows the distribution of students' career choices at the time they entered college in 1957 and at the time of their graduation in 1961. In absolute terms, the net change in the proportion of students intending to pursue careers as scientists is not large (a loss of about 4 percent), but in relative terms this trend represents a decline of approximately 17 percent. The decrease in potential engineers is much more pronounced, with less than half as many students planning to be engineers as seniors than as freshmen. It should be remembered that these students are among the top 4 percent of the national population in academic ability. To people interested in increasing the supply of qualified manpower in science and engineering, such trends are a source of concern.

Table 1. Trends in the Distribution of Career Choices of High-Aptitude Students (N = 4706) During the Undergraduate Years

Career choice	*Proportion of total sample* 1957	*Proportion of total sample* 1961	*Percentage change*
Scientist	0.220	0.182	—17.3
Engineer	.190	.091	—52.1
All other	.590	.727	+23.2

Since it was expected that the students' initial choices of careers and major fields of study would have a considerable bearing on their final career plans, the 4706 students were separated into several groups on the basis of their choices, as freshman, of careers and major fields. Table 2 shows the proportions of students in each of these groups who were aspiring to a career in science 4 years later.

As might have been expected, students who initially planned careers as scientific researchers or as college teachers of science were the most likely to aspire to careers in science after 4 years. Yet, even among those who initially planned to be research scientists, there were marked differences, as a function of the major field initially chosen, in the proportions who later aspired to be scientists. For example, among the male students who initially planned to be research scientists, those who majored in natural sciences were about twice as likely to pursue a career in science after 4 years as those who majored in engineering.

It is clear from Table 2 that, for students who choose nonscientific careers as freshmen, the probability of aspiring to a career in science after 4 years is extremely low. In fact, among the students of our sample (both boys and girls) who, as freshmen, chose the career of artist, diplomat, journalist, lawyer, or military officer, none aspired to a career in science at the time they graduated.

Of the students whose initial choices of career and major field are in some area other than science, those who major in English or history are particularly unlikely to pursue a career in science at graduation.

Some of the general trends shown in Table 1 can be accounted for in terms of the probabilities given in Table 2. For example, about one-third of the students who abandoned engineering as a career ended up pursuing a career in science. Thus, the net loss in the proportion of students pursuing careers in science was not great, even though about half of the students who initially chose careers in science changed to other careers. The data in Table 2 also make it clear that, while many students leave science to enter other fields, "recruitment" of scientists from other fields is very unlikely to occur at the undergraduate level. For example, 92 percent of the 854 students who ended up pursuing careers in science began their undergraduate careers either with plans to become scientists or with majors in science or engineering, whereas only about 2 percent of the 1631 students who entered college planning to pursue nonscientific careers and to major in fields other than science ended up pursuing careers in science.

Table 2. Probability of Aspiring to a Career in Science at the Time of Graduation from College (1961) as a Function of Career and Major Field Chosen as a Freshman (1957)

		Males		*Females*	
Career choice in 1957	*Major field choice in 1957*	*N*	*Probability of aspiring to science career in 1961*	*N*	*Probability of aspiring to science career in 1961*
Scientific researcher	Chemistry	186	.61	101	.47
Scientific researcher	Physics	315	.58	31	.61
Scientific researcher	Biology	16	.56	20	.35
Scientific researcher	Mathematics	87	.46	49	.37
Scientific researcher	Engineering	52	.29		
Scientific researcher	Other fields	68	.43	37	.17
College professor	All sciences	55	.47	19	.42
College professor	History or philosophy	35	.00		
College professor	Other fields	62	.10	69	.03
Engineer	All sciences	39	.36		
Engineer	Engineering	783	.16		
Engineer	Other fields	42	.14	30	.23
School teacher	All sciences	30	.33	77	.23
School teacher	Other fields	48	.00	289	.02
Business executive	All sciences	26	.12		
Business executive	Other fields	99	.03	18	.00
Physician or dentist	All sciences	246	.07		
Physician or dentist	Other fields	33	.03	53	.15
Clergyman	All fields	90	.03		
Artist, diplomat, journalist, lawyer, or military officer	All fields	335	.00	108	.00
Undecided	All sciences	86	.27	63	.19
Undecided	English or history	51	.02	67	.03
Undecided	Undecided	196	.08	145	.08
Undecided	Other fields	36	.06	20	.10
Other choices	All sciences	34	.33	94	.10
Other choices	English or history	21	.00	31	.00
Other choices	Other fields	73	.10	241	.03

CONTROLLING STUDENT INPUT

For all students in the subsample of 1548 students, the careers and major fields initially chosen were assigned scores equal to the probabilities in the appropriate cells of Table 2. All students whose initially chosen careers and major fields were assigned probabilities of .00 on the basis of the data in Table 2 were excluded from the analyses of differential college effects, since these students' "criterion" scores can be considered to be completely predictable from input data. Exclusion of these students reduced the subsample from 1548 to 1332.

Multiple-regression techniques were used to control the 21 student-input variables prior to the analyses of differential college effects. Multiple point-biserial correlations between the dichotomous criterion (choice of science career versus choice of nonscience career at graduation) and the 21 input variables were computed by means of a stepwise technique for the selection of predictors. The student-input variables were added to the multiple correlation until further addition of variables failed to produce a reduction in the residual sum of squares which exceeded $p = .05$. These multiple-regression analyses were performed separately for the samples of 976 boys and 356 girls.

The results of the analyses of input variables are given in Table 3. Eight input variables entered into the multiple-regression equation for boys, and five variables entered into the equation for girls. As anticipated, the career and the major field initially chosen carried the largest weights in predicting final choices of science careers for students of both sexes. Other input variables which were common to the two analyses (that for boys and that for girls) were the highest degree sought as a freshman, the educational level of one parent (for boys, the father; for girls, the mother), and academic aptitude (for boys, the SQT selection score; for girls, the mathematical aptitude score). All of these input variables carried positive weights in both analyses.

In short, the entering freshman who is most likely to pursue a career in science after college is the one who initially chooses a career or major field in science or engineering; who initially hopes to attain an advanced degree; who has higher-than-average academic aptitude; and whose liked-sexed parent has a higher-than-average level of education.

Next, higher-order partial correlations (that is, correlations with the input variables held constant) were computed between the criterion and each of the 15 college characteristics (the ten continuous "trait" variables and

Table 3. Results of Analyses in Which the Input Variables Related to the Student's Decision to Pursue a Career in Science are Controlled

Variable entering	R_p*	*Partial R_p* of variable entering with criterion*	*Probability associated with reduction in sum of squares due to variable entering*
	Males (N = 976)		
Initial career choice and major field	.576	.576	<.001
Highest degree sought	.596	.192	<.001
Father's educational level	.603	.120	<.01
"Drop-out" predictor	.609	.103	<.05
SQT selection score	.612	.095	<.05
Number of books in the home	.616	—.084	<.05
Scholarship held	.619	—.083	<.05
Commuter (versus resident)	.621	—.078	<.05
	Females (N = 356)		
Initial career choice and major field	.516	.516	<.001
Mathematical aptitude	.552	.233	<.001
Highest degree sought	.566	.156	<.05
Percentage of peers attending college	.577	—.135	<.05
Mother's educational level	.589	.154	<.05

* Multiple point-biserial correlations corrected for coarse grouping.

the five dichotomous "type" variables). Table 4 shows the results of these analyses. Six of the college-characteristic variables appeared to have significant ($p < .05$) effects on the science-career choices of the male students, whereas only two college-characteristic variables appeared to have significant effects on the science-career choices of the female students.

The two college-characteristic variables that appear to have the most pronounced effects on the male student's choice of a science career are "type" characteristics: men's colleges in the Northeast (*8*), which seem to have a negative effect, and technological institutions (*9*), which seem to have a positive effect. A third "type" variable—coeducational liberal arts colleges (*10*)—also appeared to have a positive effect on the male student's choice of a career in science. "Trait" variables which had significant effects included the "enterprising" orientation (defined primarily by the degree of emphasis at the institution on business or law), the "artistic" orientation (defined primarily by the degree of emphasis on music, art, literature, and foreign languages), and the "homogeneity of the environment" (defined by the extent to which the institution is dominated by a single type of curriculum). When the college-characteristic variables were permitted to enter into the multiple-regression equation after the analysis of the control variables, the three "type" variables were selected; after this, no further environmental variable produced a significant reduction in the residual sum of squares.

Table 4. Effects of College Characteristics on the Student's Decision to Pursue a Career in Science

College characteristic	*Partial R_p* with criterion (after control of input variables)*	p
	Male students	
Northeastern men's colleges	−.134	<.001
Technological institutions	.113	<.01
"Enterprising" orientation	−.100	<.05
Homogeneity of the environment	.088	<.05
"Artistic" orientation	−.079	<.05
Coeducational liberal arts colleges	.079	<.05
	Female students	
Affluence	−.181	<.01
Homogeneity of the environment	−.145	<.05

* Point-biserial correlations corrected for coarse grouping.

Of the 15 college-characteristic variables, the "affluence" variable appeared to have the greatest effect on the female student's decision to pursue a career in science. This factorially derived variable, which reflects the institution's prestige and financial resources, the level of training of its faculty, and the quality of the student body, appeared to have a negative effect on the motivation of the female student to pursue a career in science. When the "affluence" variable was permitted to enter into the multiple-regression equation after the analysis of the control variables, no further college variables were selected.

DISCUSSION

Although several significant effects attributable to the characteristics of the college were identified in these analyses, the absolute magnitudes of these effects are small. Thus, the characteristics of the student of high aptitude as he enters college appear to be much more important than the characteristics of the college he attends in determining his final choice of career. It must be kept in mind, however, that our ability to predict the choice of a science career at graduation is still far from perfect; no doubt the individual student has many unique and important college experiences which we have not seen able to assess. Nevertheless, with respect to gross effects, particular colleges and general college characteristics of the type considered in this study appear to contribute little to the talented student's decision to pursue a career in science.

The results of the college-effects analyses for the male students are consistent with the findings of several other studies. In particular, the findings regarding the men's colleges in the Northeast are consistent with results of three previous studies, two of them made with samples unrelated to the samples of this study. In the first of these three studies (*3*) it was found that the men's colleges in the Northeast tend to produce fewer graduates who go on to get the Ph.D. degree than would be expected on the basis of the intelligence levels, major fields of study, and sex of their entering students. Similarly, in another study (*4*), made with some of the students of the study reported here, attendance at these colleges was found to have a negative effect on the students' plans to obtain the Ph.D. The third study (*11*), of finalists from the subsequent (1958) Merit Scholarship Program, showed that students who attended men's colleges in the Northeast got lower scores on the Mathematical Aptitude Test of the Graduate Record Examination than would have been predicted from their freshman aptitude tests scores, their initial career plans, and their fields of study.

The findings of the present study regarding technological institutions and coeducational liberal arts colleges also tend to be consistent with the results of these earlier studies, with some exceptions. For example, in one earlier study (*11*), attendance at a technological institution was found to enhance the student's mathematical aptitude, but attendance at a coeducational liberal arts college failed to have this effect. Conversely, attendance at a coeducational liberal arts college tended to increase the student's motivation to obtain the Ph.D., but this was not true of attendance at a technological institution (*4*). In the study of Ph.D. "productivity" (*3*), both the technological institutions and the coeducational liberal arts colleges tended to produce more Ph.D.'s than had been expected, though these trends were not statistically significant.

At this time, interpretation of these findings is difficult and at best speculative. For example, the men's colleges of the Northeast are characterized by prestige, affluence, and a high "enterprising" orientation; this combination may result in an environment which discourages the student from pursuing the highly specialized and technical training required in most scientific fields. Some of the findings in a recent study (*12*) suggest that attendance at these colleges tends to encourage the pursuit of careers in the "enterprising" category.

The results for the female students suggest that the effects of colleges on the student's motivation to pursue a career in science are not the same for women as for men. It is difficult to say why the affluence of a college should discourage women of high aptitude from pursuing scientific careers. (Ordinarily one would expect to find that attendance at an institution with highly trained faculty, students of high aptitude, and financial resources would tend to encourage the pursuit of scientific or scholarly careers.) In any case, since this finding has no parallel in previous research, it seems desirable to determine whether it can be replicated in independent samples.

SUMMARY

The effects of different college characteristics on the student's motivation to pursue a career in science were examined in a 4-year longitudinal study of high-aptitude students attending 82 undergraduate institutions. The male student's motivation to pursue a career in science appeared to be positively influenced by attendance at a technological institution or a coeducational liberal arts college and to be negatively influenced by attendance at one of the men's colleges in the Northeast. The female student's motivation to pursue a career in science appeared to be negatively affected by the affluence of the institution attended. The student's decision to pursue a career in science at graduation from college appeared to be much more dependent on his characteristics as an entering freshman than on the characteristics of the college he attended.

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D. Cognitive Resources

Introduction

Just as certain developmental events, background, and family milieu have been thought to predispose an individual toward a scientific career, so certain characteristics of thinking, motivation, and personality have been studied for the push that they give in this direction. The general strategy of these investigations has been to study the intellectual correlates of mature scientists who are actively performing in a research role. The assumption here is that since these are individuals who are successfully performing in work demanding originality and creativity, they are useful exemplars of the demands made on thinking processes. This approach has provided access to the complex and intricate character of the thinking of the scientist and has encouraged exploration of originality, abstracting ability, imagery, integrative tendencies, and interest—to mention only a few of the cognitive characteristics implicated in studies to date.

As soon as this body of scientists was under study, it became apparent from differences within the group that many of these variables were represented on a continuum, with persons who were rated highly creative different in some measure from those who performed at a lesser level. Thus the refinement of within-group characteristics was encouraged. What has emerged in the field are certain requisites or minimal correlates, as well as certain ranges within these variables that are related to level of performance.

One of the findings stemming from early work is that intellectual level per se is not a sufficiently explanatory concept to embrace creative performance. Rather, creativity involves many high-order cognitive processes, to say nothing of the motivational properties, investment in intellectual interests, or persistencies toward intellectual goals. The broadening of the understanding of creative work led to the development of new instruments and the application of measuring techniques that had never been applied before to these groups. These techniques were thought useful in capturing the high-order processes involved—imagination, reasoning, judgment, logic. Here some diversity can be noted from the selections and the additional references.

Although the selections cited have studied the performance of the mature scientists to arrive at intellectual correlates, some of the references allude to another strategy: The study of future scientists—i.e., talented adolescents who have opted for a career in science—to test the predictive capabilities of the variables found in the already well-functioning groups. The Parloff work in Section III B is one example. Garwood's study in Section II E also presents data on undergraduates who were in the final stages of becoming formally professional.

The success with which established correlates can be identified in embryonic scientists supports the assumption that some of the adult's cognitive qualities are part of the psychological armamentarium of the individual early in life, and that these qualities in themselves determine the kinds of interests developed and the stimuli in the environment to which the individual responds. It is generally assumed that intellectual endowment is present early and represents one of the characteristics of the organism which then is developed through his many formative experiences and stages. It may as well be true that some of the other correlates, e.g., the capacity for abstract thinking, similarly mature over time and are shaped by environmental influences. Studies which can identify children who perform creatively support such a position. In line with this is Werner, Bierman and French's (1971) current work stressing that environmental stimulation and support nurture good capabilities which can be identified in early life. The intimate interplay between motivational and personality characteristics that reward and further cognitive strengths come out in some of the selections in this section.

The first selection by MacKinnon and Hall is a very recent report on data on IQ as well as scores on the Terman Concept Mastery Test, which has been obtained on a number of groups of professional and talented people studied by the Institute of Personality Assessment and Research (IPAR) over the last decade. This is one of the few reports on the intelligence level of research scientists, since evaluation of IQ has generally been hampered by instruments which are not calibrated well enough to embrace their level of abilities, especially at the high ends of the range. A comparative analysis with other professional groups, and with the general population, provides some base rates which have encouraged MacKinnon and Hall to generalize about the relationship between intelligence level and successful performance. Drevdahl's work has tested the sensitivity of some of Guilford's creativity measures as well as Terman's Primary Mental Abilities Test on different creative and noncreative samples. His population has been drawn from the academic scene. Although some techniques are found to differentiate scientists who have been rated creative by their superiors, in general, Drevdahl finds no correlation between intellectual ability and creativity in his population. While his findings, in the main, support the position which MacKinnon and Hall espouse, it is interesting to note that while Drevdahl has found some of Guilford's measures of creativity useful discriminating instruments, the IPAR group finds them singularly unsuccessful in this regard.

Stein & Meer's contribution is one of the few which deals with a nonacademic industrial research population, industrial chemists. They have used an experimental task which demands that subjects impose organization on unstructured stimuli in differentiating populations rated as highly creative and less creative. Their experience with the Miller Analogies Test as a test of creativity has also been reported (Meer, Stein, and Geertsma, 1955).

The searching question, of course, in regard to intellectual functioning is the process by which a man creates and produces something creative. To be bright, verbally facile, perceptually alert, or conceptually unconventional is academic unless there is an end product and unless one knows how the end product resulted. The literature on creative process is a vast and fascinating storehouse of intuitive and intimate recall and revelation. So much has been speculated about, conjectured, and enlarged through personal anecdote and occasional observation that it is impossible to include it at this point. Though interesting, the stories of even serendipitous experiences of great scientists share these intuitive qualities. There have been very few successful attempts to document more objectively the creative process itself. One study, however, which attempted to approach the process, was an effort by Anne Roe to compare modes of imagery used by scientists in different disciplines. This work is included here because it remains consonant with one of the mainstreams in psychology which has attempted to objectify the relatively unstructured body of introspective information. The data, while fragmented and unsystematic, are interesting, and suggest that this area should be pursued in subsequent investigations.

While the descriptive and speculative data on the scientific creative processes are omitted in this book, there is attention to the objective reference which has been used to evaluate the creative product. Section IV D brings out the diversity in criteria used to date and speaks to the many methodological issues involved in such a judgmental process. Also relevant is the work on the ages and conditions under which scientific achievement proceeds, and the many factors influencing productivity pertinent to the creative process.

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Intelligence and Creativity

Donald W. MacKinnon and Wallace B. Hall

In our studies of several groups of highly creative persons (writers, mathematicians, research scientists, and architects), intelligence was measured by the Terman Concept Mastery Test (CMT) (1956), which is generally considered to be a test of general intelligence (g). It does not, however, yield a factorially pure or unidimensional measure of intelligence nor can scores on the test be precisely converted to IQs, especially in the upper range of scores.

The CMT had previously been administered to several groups of highly effective individuals studied in our Institute (Institute of Personality Assessment and Research, IPAR) since 1950, and the norms on the CMT for these as well as other groups were available for comparison with our creative samples. Table 1 presents descriptive statistics on the Concept Mastery Test for these groups. Those that have been studied at our Institute are identified by the designation IPAR, and those chosen for study because of their outstanding creativeness are marked by an asterisk.

It can be seen that three of the five groups chosen for study because of their unusual creativeness hold ranks of 1, 2, and 3, and score higher than the adult subjects of the Stanford Gifted Study. Mean scores for these groups are: creative writers, 156; creative men mathematicians, 148; and creative women mathematicians, 144, as compared with a mean score of 137 earned by the adult Stanford gifted subjects. The mean values for the two creative samples that score below the Stanford gifted group hold ranks of 10 and 11 among the 20 groups listed according to their Terman scores. These are creative research scientists with a score of 116, and creative architects who score 113.

When the two samples of women mathematicians—the creative and comparison groups—are combined, there turns out to be a low positive correlation, +.32 (< .05) between their rated creativity and their intelligence as measured by the CMT. When, in a similar fashion, the two samples of men mathematicians are combined, the correlation of the two variables is −.07. The only sample of

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Table 1. Mean Scores and Standard Deviations for Various Groups on the Concept Mastery Test, Form T

Group	*N*	*Mean*	*S.D.*
*Creative writers (IPAR)	20	156.4	21.9
*Creative men mathematicians (IPAR)	12	148.1	20.5
*Creative women mathematicians (IPAR)	13	143.6	31.3
Subjects of Stanford Gifted Study	1004	136.7	28.5
Representative women mathematicians (IPAR)	28	125.6	32.6
Representative research scientists (IPAR)	22	120.8	31.2
Graduate students, University of California	125	119.2	33.0
Graduate students, University of California (IPAR)	161	118.2	33.1
Ford Foundation fellowship applicants	83	117.9	35.1
*Creative research scientists (IPAR)	23	115.7	27.2
*Creative architects (IPAR)	40	113.2	37.7
College graduates, University of California	75	112.0	32.0
Undergraduate students, University of California	201	101.7	33.0
Public health education applicants, U.C.	54	97.1	29.0
Spouses of Stanford Gifted Study	690	95.3	42.7
Electronic engineers and scientists	95	94.5	37.0
Engineering college seniors, U.C. (IPAR)	40	80.4	27.9
Undergraduates, lower division, Stanford University	97	77.6	25.7
Military Officers (IPAR)	343	60.3	31.6
Leisure-time inventors (IPAR)	14	50.8	34.7

* Samples selected for study because of their high creativeness.

architects on which Terman scores could be obtained was the creative subgroup of architects. Their mean score on the CMT was 113, though individual scores on the test ranged widely from a low of 39 to a high of 179, yet in this sample also, the correlation of measured intelligence with rated creativity is —.08.

Given the general level of intelligence of these three professional groups and the finding that in two of them intelligence and creativity are not correlated, while in the third there was a low positive relationship between the two variables, our interpretation of the findings, tentatively drawn, was this: It would appear that within any given profession a certain minimum level of intelligence is required for success in the profession, but an increase of intelligence above that minimum does not guarantee a corresponding increase in creativeness.

Subsequently we were able to administer the Wechsler Adult Intelligence Scale (WAIS) to some of our earlier tested subjects: 88 architects, 37 research scientists, 33 male mathematicians, and 27 female mathematicians.

Table 2 presents the data on the full scale IQ scores for the several groups and subgroups as indicated. The level of creativeness of the subgroups is indicated by Roman numerals. In the case of the research scientists and mathematicians, I indicates the creative, and II the comparison subgroups. In the case of the architects, I designates the most creative subgroup, II the subgroup of intermediate creativeness, and III the least creative subgroup.

With the exception of the female mathematicians where the comparison sample earns a higher mean IQ score (133) than the creative subjects (128), the more creative subgroup in each sample scores higher than the less creative subgroups: Architects I (132), Architects II (130), Architects III (128); Research Scientists I (133), Research Scientists II (132); and Male Mathematicians I (135), Male Mathematicians II (134). But in only one of these samples is the intelligence of the more creative group significantly higher ($< .05$ level) than the intelligence of the less creative group, namely, in the case of Architects I (132) *vs.* Architects III (128). If one combines all the creative males from the several samples and compares them with all the comparison males, the difference in mean score between these two groups, 133 *vs.* 130, is also significant at or beyond the .05 level of probability. When the same analysis is made for all subjects, contrasting the creative with the comparison subgroups, both male and female,

Table 2. WAIS Full Scale IQ Scores

N	*Groups*	X	*Range*
26	Architects I	132	120–141
32	Architects II	130	117–140
30	Architects III	128	107–143
88	Architects, 3 groups combined	130	107–143
19	Research scientists I	133	121–142
18	Research scientists II	132	117–143
37	Research scientists, 2 groups	133	117–143
18	Male mathematicians I	135	119–151
15	Male mathematicians II	134	124–142
33	Male mathematicians, 2 groups	135	119–151
7	Female mathematicians I	128	116–140
20	Female mathematicians II	133	114–144
27	Female mathematicians, 2 groups	131	114–144
63	All male creative subjects	133	119–151
95	All male comparison subjects	130	107–143
70	All creative subjects, combined	133	116–151
115	All comparison subjects, combined	131	107–144
185	Total	131	107–151

the difference in mean score of the two groups, 133 *vs.* 131, is not significant.

Though there is little correlation between intelligence and creativity in the professional groups we have studied, and even less among those who are recognized as highly creative, one can hardly conclude that creative performance or even merely satisfactory performance is unrelated to intelligence in such demanding fields as those of architecture, scientific research, and mathematics.

Table 3 presents the percentages with which the subjects of our various groups and subgroups fall into the conventional categories of intelligence: very superior, superior, bright-normal, average, dull-normal, borderline, and defective. It will be seen that none of the creative subjects' IQs classify them as less than bright-normal. All are of above average intelligence. Indeed, only one of the subjects in all the samples, and that a comparison one, is classified as average, namely, the subject with the lowest IQ in the group of Architects III whose score of 107 places him close to the top of the average range of 90 to 109.

That we are dealing, in all of our samples, with highly intelligent persons is to be seen in the following comparisons. Where in the Wechsler standardization sample only 2.2% are classed as very superior (IQs 130 or above), 61.6% of all our subjects fall in this range. Where 6.7% of the standardization group score as superior (IQs 120 to 129), 33.5% of our subjects are so classified. In the bright-normal range (IQs of 110 to 119), the percentage of the standardization population, 16.1%, exceeds the percentage of our subjects, 4.3%. And where 50% of the standardizing group are classified as of average intelligence (IQs 90 to 109) only 0.5% of our subjects can be so designated. In Wechsler's distribution, 25% of subjects are classified as below average in intelligence, but none of our subjects falls within this range.

Of the three creative male professional groups, mathematicians have the highest percentage of subjects who score as very superior, 83.3%. Then in descending order come research scientists, with 73.7% and architects with 65.4%. In each of these samples, comparison subjects constitute a lesser percentage of subjects who score as very superior: Mathematicians, 73.3%, Research Scientists, 61.1%, Architects II, 59.4%, and Architects III, 36.7%. Combining the three male samples, 73% of the creative subjects are of very superior intelligence but only 54.7% of the comparison subjects are so classified.

In each of the creative male professional

Table 3. Intelligence Classification of WAIS IQ's

	Percentage included				
Sample *IQ:*	*Very superior 130 and above*	*Superior 120–129*	*Bright-normal 110–119*	*Average 90–109*	*N*
Wechsler standardization*	*2.2*	*6.7*	*16.1*	*50.00*	
Architects I	65.4	34.6	0.0	0.0	26
Architects II	59.4	37.5	3.1	0.0	32
Architects III	36.7	56.7	3.3	3.3	30
All architects	53.4	43.2	2.3	1.1	88
Research scientists: Creative	73.7	26.3	0.0	0.0	19
Comparison	61.1	33.3	5.6	0.0	18
All research scientists	67.6	29.7	2.7	0.0	37
Male mathematicians: Creative	83.3	11.1	5.6	0.0	18
Comparison	73.3	26.7	0.0	0.0	15
All male mathematicians	78.8	18.2	3.0	0.0	33
Female mathematicians: Creative	42.9	42.9	14.3	0.0	7
Comparison	65.0	20.0	15.0	0.0	20
All female mathematicians	59.3	25.9	14.8	0.0	27
All male creative subjects	73.0	25.4	1.6	0.0	63
All male comparison subjects	54.7	41.1	3.2	1.0	95
All creative subjects	70.0	27.1	2.9	0.0	70
All comparison subjects	56.5	37.4	5.2	0.9	115
All IPAR subjects	*61.6*	*33.5*	*4.3*	*0.5*	185

Classification	*IQ*	*Percentage included (Wechsler standardization)*	*IPAR*
Dull-normal	80–89	*16.1*	*0.0*
Borderline	70–79	*6.7*	*0.0*
Defective	69 and below	*2.2*	*0.0*

* From D. Wechsler, *The measurement and appraisal of adult intelligence.* (4th ed.) Baltimore: Williams & Wilkins, 1958. P. 42.

groups, the largest percentage of subjects is of very superior intelligence, the next largest percentage of superior intelligence, while bright-normal subjects constitute the smallest percentage of subjects: Architects I, 65.4%, 34.6%, and 0.0%; Research Scientists, 73.7%, 26.3%, and 0.0%; and Mathematicians, 83.3%, 11.1%, and 5.6%.

Once again female mathematicians are an anomalous group, this time with 65.0% of the comparison group as compared with 42.9% of the creative group scoring as very superior in intelligence. In the comparison group 20.0% score as superior, 15.0% as bright-normal. In the creative group 42.9% are of superior, and 14.3% of bright-normal intelligence.

The data of Table 3 constitute convincing evidence that not only the creative subjects assessed at IPAR but also the subjects chosen from the same professions as comparison samples are highly intelligent. Merely to be engaged successfully in such professions as architecture, scientific research in industry, and mathematics would seem to require above average intelligence and more often than not intelligence of a superior or very superior sort. And those who are recognized as the creative practitioners of these professions more often than their less creative colleagues will score as very superior rather than merely superior in intelligence. On the other hand, scoring as more intelligent than a colleague does not guarantee that one will surpass him in creativeness, as data which there is not time to present in detail convincingly demonstrate: in every group the multiple regression equation to predict creativity from WAIS scores failed to approach significance in cross-validation. In contrast, multiple regression solutions to predict the creativity of our subjects from the scales of the Strong Vocational Interest Blank, the Study of Values, the California Psychological Inventory, the Myers-Briggs Type Indicator, FIRO-B, and the Gough Adjective Check List all cross-validated at the .01 level of significance or better (Hall and MacKinnon, 1969).

The conclusion to be drawn from our investigation of the relationship of intelligence to creativity is this: Creative persons in the professions tend to be highly intelligent. In such professional groups as we have studied, however, what differentiates the more creative members of the groups from their less creative peers is not a higher level of intelligence than theirs, though they may often possess just that, but rather distinctive patterns of interests, values, personality traits, and perceptual and cognitive preferences. Above a given minimal level of intelligence required for the successful practice of one's profession, which in the groups we have studied is quite high, what is most importantly determinative of creative performance is not a higher level of intelligence *per se* but particular constellations of non-intellective traits. They are the factors that make the difference between a successful practitioner of a profession and one who practices it creatively.

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Factors of Importance for Creativity

John E. Drevdahl

PROBLEM

Despite the fact that creativity has been a matter of interest among psychologists for many years, most studies and discussions of the subject have been primarily speculative in nature. There have been three general approaches to the study of creativity: the historical-anecdotal approach, best illustrated by Kretschmer(6), the introspective personal report approach exemplified in Patrick's studies(7, 8), and the test approach, used by Terman(14) and Roe(10, 11). With the exception of less than a score of relatively recent studies, some reported and some still under way, the experimental or objective (test) approach to the study of creativity has been studiously neglected. The purpose of this study was to explore some of the possible relationships between ratings of creativity in a high level population and certain objectively measured personality and intellectual factors.

PROCEDURE

The subjects used in this study were graduate or advanced undergraduate students from several of the science and arts departments of the University of Nebraska. All of the subjects were under the close individual supervision of the raters in their area of specialization at the time of testing and for some time preceding it. The raters were faculty members of the University of Nebraska experienced in the task of evaluating the creative abilities of their students.

In order to assure, in so far as possible, the relative independence of the intellectual and personality characteristics under investigation, several factor tests were selected and administered. The personality characteristics were

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measured by means of Cattell's Sixteen Personality Factor Questionnaire(1). The intellectual characteristics were measured by means of Thurstone's Primary Mental Abilities Test(15). In view of Guilford's(3) suggestion that the standardized and accepted intelligence tests may not include many of the critical variables that may be involved in creative effort, a group of tests was selected from his Factor Analytic Study of Creative Thinking(4). Guilford found, in addition to the expected intellectual factors, nine other interpretable factors that may be involved in creative thinking. For the purposes of this study, the test with the highest loading on each of the factors was used as a measure of that factor.

The tests were administered to the subjects in a consistent order during two testing sessions. Of 90 subjects, 76 completed the first session (tests of the intellectual factors) and 64 completed both sessions. Several subjects were dropped because the raters felt that they did not know them well enough to be able to make a valid rating, and several (foreign students) were dropped because of language handicaps. As a result of these omissions, the final group used in the analysis of the intellectual factors numbered 64, while the group used in the analysis of the personality factors numbered 52.

Two raters rated each subject independently and made two ratings on each subject, using each time a seven point scale of creativity. The first rating was based upon the rater's own definition of creativity, and the second rating, which took place at least two weeks after the first, in order to minimize the carryover effects from one set of ratings to the other, was based upon a given definition—a distillation of the various definitions that have been proposed in the literature. There were no significant differences between the defined and undefined ratings, and the given definition may be considered to include the various subjective definitions of the raters. The given definition is as follows:

> Creativity is the capacity of persons to produce compositions, products, or ideas of any sort which are essentially new or novel, and previously unknown to the producer. It can be imaginative activity, or thought synthesis, where the product is not a mere summation. It may involve the forming of new patterns and combinations of information derived from past experience, and the transplanting of old relationships to new situations and may involve the generation of new correlates. It must be purposeful or goal directed, not mere idle fantasy—although, it need not have immediate practical application or be a perfect and complete product. It may take the form of an artistic, literary or scientific production or may be of a procedural or methodological nature.

The subjects were divided into creative and non-creative groups and into arts and science groups. Those subjects whose creativity ratings fell below the fiftieth percentile for the whole group were placed in the non-creative group. The correlation (Pearson) between different judges' ratings on all subjects was .71. The significance of the differences obtained between the various groups was determined by means of a 2 × 2 analysis of variance on each of the 30 factors under investigation. For each of the variables on which the groups showed or closely approached a significant difference, a biserial correlation coefficient was calculated in order to determine the degree of relationship between the scores on that variable and the creativity classification.

RESULTS

Intellectual Factors

As can be seen in Table 1, there were no significant differences found between the creative and non-creative groups on a one-test estimate of Guilford's factors called redefinition, closure, ideational fluency, spontaneous flexibility, associational fluency, or sensitivity to problems. There was a tendency, although not statistically significant, for the creative group to score higher on this latter factor. The creative group scored significantly higher than the non-creative group on the factor of originality, and although the difference between the groups on the factors of word fluency and adaptive flexibility was not statistically significant at the .05 level, as can be seen in Table 2, there appears to be a low, although significant, relationship between creativity and higher scores on these two factors. The science group scored significantly higher than did the arts group on the factor of redefinition, but there were no other

Table 1. Analysis of Variance Ratios and Significance Levels for Creative vs. Non-Creative (A) and Arts vs. Science (B) Groups on All Factors

Factor	*Source of variance*	*F*
Word fluency (Guilford)	A	3.52
	B	<1.00
Redefinition	A	1.65
	B	5.33*
Adaptive flexibility	A	3.62
	B	<1.00
Associational fluency	A	2.22
	B	<1.00
Closure	A	1.46
	B	<1.00
Originality	A	4.76*
	B	<1.00
Ideational fluency	A	<1.00
	B	<1.00
Sensitivity to problems	A	1.52
	B	1.05
Spontaneous flexibility	A	<1.00
	B	<1.00
Verbal meaning	A	5.82*
	B	<1.00
Space	A	<1.00
	B	<1.00
Reasoning	A	<1.00
	B	<1.00
Number	A	<1.00
	B	9.40‡†
Word fluency (Thurstone)	A	<1.00
	B	<1.00
General intelligence	A	<1.00
	B	2.25
Cyclothymia *vs.* schizothymia	A	4.53*
	B	<1.00
General intelligence *vs.* mental defect	A	<1.00
	B	<1.00
Ego strength *vs.* general neuroticism	A	1.29
	B	<1.00
Ascendance *vs.* submission	A	<1.00
	B	<1.00
Surgency *vs.* desurgency	A	8.35‡
	B	<1.00
Super ego strength *vs.* lack of internal standards	A	<1.00
	B	<1.00
Adventurous cyclothymia *vs.* withdrawn schizothymia	A	<1.00
	B	3.28
Emotional sensitivity *vs.* tough maturity	A	<1.00
	B	23.27‡
Paranoid schizothymia *vs.* truthful accessibility	A	<1.00†
	B	<1.00
Bohemianism *vs.* practical concernedness	A	<1.00
	B	21.52‡
Sophistication *vs.* rough simplicity	A	2.13
	B	<1.00
Anxious insecurity *vs.* self confidence	A	<1.00
	B	2.52
Radicalism *vs.* conservatism	A	5.09*†
	B	<1.00
Self sufficiency *vs.* lack of resolution	A	4.21*†
	B	<1.00
Will control and stability	A	<1.00
	B	3.23
Nervous tension	A	<1.00
	B	1.68

Note: The probability of obtaining ten statistics significant at the .05 level for 62 calculated statistics is approximately .001.(12)

* Significant beyond the .05 level of confidence.

‡ Significant beyond the .01 level of confidence.

† Significant interaction. See text for explanation.

significant differences between the arts and science groups on Guilford's factors. Inasmuch as redefining activity would seem to be common to both artists and scientists, some explanation might be considered at this point. Redefinition of the type required by this test involves the use of substitute objects for various purposes. This specific activity is quite common in the science laboratory where the construction of temporary and makeshift equipment is a continual necessity. On the other hand, artistic redefinition may involve, for the most part, an interpretative redefinition of an existent work without involving actual structural substitutions of the type required in the laboratory. This type of artistic redefinition is poorly measured, if at all, by the test used in this instance.

Table 1 also shows that the creative group scored significantly higher than the non-creative group on the factor of verbal meaning on Thurstone's Primary Mental Abilities Test. There were no other significant differences between the creative and non-creative groups on

Table 2. Biserial Correlations Between Creativity Classification and Factor Scores for All Subjects

Factor	r_{bis}
Word fluency	.37†
Originality	.33†
Adaptive flexibility	.29*
Verbal meaning	.37†
Cyclothymia *vs.* schizothymia	.36*
Surgency *vs.* desurgency	.47†
Radicalism *vs.* conservatism	.38†
Self sufficiency *vs.* lack of resolution	.33

Significant beyond the .05 level.
Significant beyond the .01 level.

this test. There was a significant difference between the arts and science groups on the number factor, but also a significant interaction. Inspection of the data suggests that this difference is primarily attributable to the exceptionally high scores of the creative science group. Numerical facility (number) may be merely a symbolic counterpart of verbal facility, and inasmuch as the transformation of concepts and relationships, ordinarily dealt with in verbal symbols, into mathematical symbols, is more characteristic of the sciences than the arts, it might be expected that creative persons in science would display greater facility in handling such concepts than would creative persons in fields where verbal facility is sufficient. In view of this, perhaps number facility may be considered simply a special instance of verbal facility (word fluency, verbal meaning).

Some comment concerning the meanings of these factors and their interrelationships might justifiably be considered at this point. Word fluency and verbal meaning denote a wide knowledge of things, ideas, and concepts expressed in words. Adaptive flexibility is a characteristic that involves the ability to change set or approach to meet new requirements imposed by changing conditions or problems. It is a more restricted type of flexibility than most (e.g.: spontaneous flexibility), inasmuch as the direction of change is dictated by the situation or problem demanding solution. Associational fluency involves the ability to make relevant associations quickly and easily. It is somewhat similar to the preceding factor in its demand for relevancy. Originality involves the ability to produce uncommon, unusual, and "clever" responses. We might, therefore, define creative thought, in terms of the present findings, as the goal directed, easily flexible, manipulation of knowledge (concepts, relationships, etc.), in a wide variety of novel or original ways. Such a process fulfills the specifications for creativity as proposed by Spearman(13), Ribot(9), or Hutchinson(5), or the specifications required by the given definition used in this study. The knowledge referred to here (arising from word fluency and verbal meaning) can be fostered by known means of education so what remains of critical importance appears to be the less educable factors of adaptive flexibility, associational fluency, and perhaps of greatest importance, originality. Evaluation of these three intellectual factors might be of considerable importance in the selection of potentially creative persons if these results can be substantiated in further investigations.

Although there does not appear to be a significant relationship between creativity and general intelligence in this population, it would be erroneous to assume that general intelligence or the other nonsignificant intellectual factors studied are unimportant. It seems more likely that these factors may be necessary but insufficient characteristics for creativity. Also of interest is the finding that, for the most part, the creative artist possesses the same intellectual characteristics as the creative scientist.

Personality Factors

Table 1 shows that there were no significant differences between the creative and non-creative groups on the factors of intelligence *vs.* mental defect; ego strength *vs.* general neuroticism; ascendance *vs.* submission; superego strength *vs.* lack of internal standards; adventurous cyclothymia *vs.* withdrawn schizothymia; emotional sensitivity *vs.* maturity; bohemianism *vs.* practical concernedness; sophistication *vs.* simplicity; anxious insecurity *vs.* self confidence, will control and stability, or nervous tension. The creative group scored higher than the non-creative group on the factors of radicalism *vs.* conservatism,[1] and self sufficiency *vs.* lack of resolution; and lower on the factors of cyclothymia *vs.* schizothymia, and

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[1] When factors are listed in bipolar terms, high scores indicate predominance of the first named characteristic.

surgency *vs.* desurgency. On the factors of radicalism *vs.* conservatism, and self sufficiency *vs.* lack of resolution, there was a significant interaction. Inspection of the data suggests that the creative art group was significantly more radical and self sufficient than the creative science group. In terms of these results we might characterize the creative group as somewhat withdrawn and quiescent, more concerned with ideas and things than with people. Creative artists we may further characterize as considerably more radical and self sufficient. These latter characteristics may reflect the creative artists' tendency to break away from the routinized and accepted patterns of work and behavior. A further question of interest concerns the creative individual's preoccupation with his work. Does this occur because the individual is unable to find gratifications in his relationships with people, or do they simply find sufficient gratification in their work and, therefore, have less need for interpersonal relationships?

There were also a number of significant differences between the art and science groups apart from their classification as creative or non-creative (Table 1). The art group scored significantly higher than did the science group on the factors of emotional sensitivity *vs.* maturity, and bohemianism *vs.* practical concernedness; and the science group scored lower, although not significantly so, on the factors of adventurous cyclothymia *vs.* withdrawn schizothymia and nervous tension. There was a significant interaction on the factor of paranoid schizothymia *vs.* truthful accessibility, and the data indicate that the creative art and non-creative science groups scored high on this factor. Thus, it appears that suspiciousness and interest in the internal mental life, or possibly interoceptiveness, may be associated with creativity in the arts, but that trusting attitudes and interest in external things, or extroceptiveness, may be more associated with creativity in the sciences.

DISCUSSION

Inspection of the original data suggests that the differences found in this study may be minimal estimates of true differences. The use of more highly loaded tests, or of subjects whose creativity has been demonstrated, might magnify the differences found here, as might the use of the upper and lower thirds rather than the upper and lower halves on the creativity ratings. It might be reasonable to expect, for example, that further refinement of techniques may demonstrate the existence of the factor "Sensitivity to Problems" that is shown to be of only doubtful significance in this study.

The results of this study provide considerable support for the contention that individuality and non-conformity are desirable for creative effort. Several of the intellectual factors are, to a great extent, measures of the degree to which the individual can avoid the usual, routine, conventional ways of doing and thinking of things. As one might expect, this ability, while common to all creative persons, may vary in its degrees of significance and inclusiveness according to its area of application. As far as the science fields are concerned, it may be necessary for the creative individual to be conventional in accepting knowledge, known methods and, perhaps, goals. Once these are accepted and internalized, the scientist's unconventionality (which is more intellectual than personal) can be expressed within this framework by means of his flexibility and fluency in approach, and in his originality. With respect to the art fields, on the other hand, the creative artist may be less dependent upon what has gone before, and may depend to a greater extent upon his personal unconventionality.

A further investigation of the personality factors associated with creativity in science, in a group whose creativity has been conclusively demonstrated, recently has been completed by Cattell and Drevdahl(2), and provides affirmation of some of the findings of this study. A similar study of creativity in the arts is under way.

SUMMARY

An investigation was made into some of the relationships between certain objectively measured intellectual and personality characteristics and ratings of creativity in a population of advanced undergraduate and graduate students at the University of Nebraska. The results of this study suggest the following conclusions:

1. Creative persons in this group appear to be superior to non-creative persons in

their verbal facility, fluency, flexibility, and in their originality.

2. Creative persons in this group appear to be considerably more withdrawn and quiescent than non-creative persons. Creative artists were somewhat more radical and self sufficient than creative scientists or non-creative persons in either the sciences or the arts.

3. Apart from its classification as creative or non-creative, the art group was more sensitive emotionally, and more bohemian.

4. Individuality or non-conformity apappear to be desirable for creativity.

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Perceptual Organization in a Study of Creativity

Morris I. Stein and Bernard Meer

The perception of good *Gestalten* is a function of the perceiver, his needs, defense mechanisms, and integration, and the characteristics of the stimulus-field. When the stimulus is ambiguous, the perceiver has to draw more heavily on his own resources than when the stimulus is structured. Consequently, it was hypothesized that those who have such resources available to them ("more creative" subjects) will develop more hypotheses and better *Gestalten* under varying conditions of ambiguity than those who may not have such resources available to them ("less creative" subjects).

Eighteen industrial research chemists who were expected to be creative and had the necessary facilities available to them for creative work served as subjects in this experiment. Each of the subjects was rated by his superiors

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and colleagues on a global variable "creativity." The high group was rated significantly higher on this variable than the so-called lesser creative subjects.

The ambiguous stimuli in this experiment were the 10 standard Rorschach Cards. To vary the ambiguity of the stimuli, they were exposed by means of a Harvard tachistoscope at four different exposure levels—.01 of a second, .10 of a second, 3.0 seconds, and unlimited ("full") exposure. The subject was seated before the tachistoscope and instructed in the manner suggested by Klopfer and Kelley for traditional Rorschach administration, with one exception. Since each card was exposed at four different levels the subject was instructed not to make any conscious attempts to recall previous responses but if they did occur to him without intentional recall then he should verbalize his response.

Only subject's first responses to the cards were scored for the purpose of this experiment. These responses were first evaluated in terms of an 8-point rating scale including the following steps:

1. *Autistic:* Those responses that fall into this category are of such a nature that there is little, if any, congruence, in the opinion of the raters, between the verbalized percept and the stimulus material.

2. *Rejection:* A Rorschach card was said to be rejected when the subject did not give a response to a card (e.g., "Nothing at all.") or if he suggested a response and then denied it (e.g., "It may be a brooch. No, it really doesn't remind me of anything.")

3. *Card Description:* Responses assigned to this category are those in which the subject described or named the physical aspects of the card but was unable to integrate them into a response that had definite shapes or form characteristics. Thus the subject might say, "I saw red, blue, and green," or, "It was all black." The response indicates that there is an "out-thereness" quality about the card. There is, however, only a bare minimal interaction between the subject and the stimulus.

4. *Sensory:* This rating was reserved for those responses in which the subject utilized only the sensory aspects (or literal factors in Gibson's terms) of the cards. These factors were color, shading, and those responses that were determined solely by form where the form is of such a general nature that it could refer to most any type of stimulus (e.g., a "map" response that had no specificity).

5. *Sensory-form:* Responses that received this rating were those in which the sensory aspects of the card played a more important rôle than the form qualities of the card. Here is the first stage at which the controlling factors of the individual (if we may interpret the form qualities of a response as such) come into play. The sensory-form responses represent a more active attempt on the part of the perceiver to do something with his environment in a specific and socially communicable fashion than do the responses that were included in Categories 1–4.

6. *Fair-form:* In this category were included those responses in which there was a fair amount of agreement, in the opinion of the raters, between the responses and the stimuli. Form here is used in a general way to refer not only to "form" responses in the traditional Rorschach sense, but also to those responses that involve form plus the sensory aspects of the blots.

7. *Form dependent primarily on the demand characteristics of the stimuli.* This category was reserved for those responses that are scored as "populars" or are regarded as "near populars." This category reflects the fact that the perceiver's perceptions are, in general, congruent with those developed by the general community.

8. *Well-integrated form:* This rating was assigned to those responses in which the perceiver provided sufficient evidence for his response and, in the opinion of the judges, the response was congruent with the stimulus. It usually involved the integration of achromatic or chromatic areas that were separated by white spaces or the integration of adjacent areas. A well-integrated response could also be assigned to a single area in which color or shading was utilized for a precise delineation of parts. Finally, a well-integrated response could be given to a popular area if a subject included something that was not so usual in his response and substantiated it with evidence. This rating suggests that a subject was capable of achieving good *Gestalten* and that, by inference,

one may suggest that this reflected the integration of the individual personality. To be sure, even responses in this category as in the previous ones reflected some of the needs and interests of the subjects, but unlike some of the responses that were assigned to other categories, here the subject was able to present sufficient data to substantiate his percept.

The reliability of this scale had been previously established in another study.

Utilizing a previous population of 1,250 responses, a weighting system was then established which took into account the form-level rating of the response, and the frequency with which that specific rating appeared to a specific card at each of the exposure levels. Thus, for example, well-integrated responses to the most difficult cards (i.e., cards which the normative data indicated would not yield many well-integrated responses) at the shortest exposure level received the highest weights, while autistic responses and rejects at "full-exposure" received the greatest penalties. The range of weighted scores was between −3.5 and +4.0.

Having assigned a score to each response a total weighted score was established for each of the subjects. A bi-serial correlation between the weighted form-level score and the rating on the creativity variable was +.88, significant at the .01 level. Furthermore, analysis of the difference between mean weighted scores at each of the exposure levels indicated that high and low groups were significantly different from each other beyond the .01 level of confidence at all four exposure levels.

Further analysis of the data revealed that the sheer number of hypotheses suggested also differentiated the high from the low subjects. Looking at the data differently, we found that the high subjects rejected only 9 per cent of the cards while the low subjects rejected 31 per cent when exposure levels were collapsed. At the .01 exposure-level, the "highs" rejected 11 per cent and the "lows" 38 per cent, and at full-exposure, the respective figures were 1 per cent and 24 per cent.

The fact that the high subjects offered more hypotheses than the low subjects resulted in more autistic responses by the "highs" than the "lows" at the .01 exposure-level (31 per cent to 20 per cent). On the other hand, 51 per cent of the "highs' " hypotheses were rated fair or better at this exposure level while only 29 per cent of the "lows' " hypotheses achieved this rating.

As exposure-level became longer, the difference in autistic responses between the high and low subjects disappeared (8 per cent as against 4 per cent) while the difference in well-integrated responses between the two groups increased (41 per cent as against 16 per cent). The difference in rejects between the two groups remained relatively constant. At full-exposure, even when all information about the stimuli was available, the low subjects still rejected 24 per cent of the stimuli. Of the remaining 76 per cent, almost one-half (32 per cent) of the hypotheses suggested were of the popular variety. Only 16 per cent of the responses at this exposure level achieved well-integrated forms. In contrast to this, the high subjects gave well-integrated responses to 41 per cent, populars to 29 per cent, and rejects to only one per cent of the stimuli.

The overall analysis showed the "highs" achieved significantly more well-integrated responses than the "lows" (.001 level of confidence). The question still remains whether these differences are due to a difference in intellectual ability and/or differences in personality factors, such as anxiety, defensiveness, etc., which result in over-cautiousness and over-criticalness which would tend to inhibit the development of hypotheses. We found significant correlations between the Wechsler-Bellevue Full Scale Scores and creativity and between the Rorschach scores and the Wechsler-Bellevue Full Scale Scores. Partialling out the effects of intelligence it was found that most of the variance contributing to the relationship between the Rorschach and creativity is due to factors other than intelligence, although the latter still is of some importance here.

Our data provide some additional evidence that personality factors are probably most crucial. This stems from the fact that if we hold the number of rejects constant we find that the two groups of subjects do not differ significantly from each other in terms of the number of well-integrated responses. This strongly suggests that the real difference between our high and low creative individuals may turn out to be a function of defensiveness or over-criticalness which inhibits the generation and communication of hypotheses.

In conclusion it may be said that "more creative" research chemists show a greater freedom in the offering of perceptual hypotheses and in the level of organization of their responses, than do their colleagues who are rated as "less creative." This appears to be a function of personality factors which we hope to delineate more specifically in the future.

A Study of Imagery in Research Scientists

Anne Roe

The data reported here were gathered in the course of a study of personalities of research scientists as related to vocation. The 64 subjects of that study are eminent research scientists in the fields of biology, physics and physical chemistry, psychology, and anthropology. Most of the men are members of the National Academy of Sciences, or the American Philosophical Society, or both. Age range is 31 to 60, with a mean of 47.7. . . . The research plan included intensive interviews on life history, discussion of their work and working habits, and three tests, the Rorschach, the Thematic Apperception Test, and a special test of verbal, spatial, and mathematical functions.

During the first year's work, which dealt with biologists, I slowly became aware that their description of working habits very frequently implied mental processes quite foreign to my own, and it occurred to me that these might have some relevance to the problems under investigation. From then on I made attempts to get definite information on this point, and also checked it with the subjects already visited. The results of this part of the study are embodied in this paper.

It must be explicitly stated that the raw data on thinking processes are highly unsatisfactory. They are introspective reports by subjects of whom few (even among the psychologists) were trained in introspection; in order to get any information at all it was frequently necessary to ask leading questions. There are no standardized tests or techniques by which anything in the way of objective evidence can be obtained. The justification for this paper lies in the fact that, despite the crudeness of the data, psychologically meaningful relations do appear, and in the hope that it will stimulate more research in this area.

There have been some tests suggested in this field, none of which is sufficiently worked out or strictly relevant here, and all of which would require more time than could be allotted to this relatively minor part of the study. In 1909 Betts (1) studied types of imagery, distinguishing spontaneous and voluntary, and the frequency and clarity of occurrence for seven sensory modalities. He found, as have all who have worked with the problem, that most subjects can employ a wider range of imagery than they normally do and that thinking can go on without the intervention of imagery.

Griffitts (4 and 5) offered a long series of tests and evidence obtained from these, on incidence of types of imagery, relative clearness and dominance, and so on. A major difficulty in all such work is equating intensity ratings of one subject with those of another. . . .

This sort of inquiry dates back at least to Galton (3), whose remarks on the subject are worth recalling:

> To my astonishment, I found that the great majority of the men of science to whom I first applied, protested that mental imagery was unknown to them, and they looked on me as fanciful and fantastic in supposing that the words "mental imagery" really expressed what I believed everybody supposed them to mean. . . . On the other hand, when I spoke to persons whom I met in general society I found an entirely different disposition to prevail. . . . The conformity of replies from so many different sources which was clear from the first, the fact of their apparent trustworthiness being on the whole much increased by cross-examination, and the evident effort made to give accurate answers, have convinced me that it is a much easier matter than I had anticipated

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> to obtain trustworthy replies to psychological questions. . . . Here, then, are two rather notable results: the one is the proved facility of obtaining statistical insight into the processes of other person's minds . . . and the other is that scientific men, as a class, have feeble powers of visual representation. There is no doubt whatever on the latter point, however it may be accounted for. My own conclusion is, that an over-ready perception of sharp mental pictures is antagonistic to the acquirement of habits of highly generalized and abstract thought, especially when the steps of reasoning are carried on by words as symbols, and that if the faculty of seeing the pictures was ever possessed by men who think hard, it is very apt to be lost by disuse. The highest minds are probably those in which it is not lost, but subordinated, and is ready for use on suitable occasions.

In what particular field of science his scientific subjects worked is not clear, but my data would indicate that this is important.

I asked my subjects in what form thoughts were handled by them. Usually this had to be expanded and then I asked specifically about their use of visual imagery, and if it appeared that they used it, whether it was concrete, diagrammatic or otherwise symbolic, three-dimensional, freely manipulable, etc. An example of such a report is:

> I believe that I think in visual images of the object under consideration, in 3 dimensions . . . in patterns. I could say that my imagery consists probably of a composition of the plants I've seen, the diagrams I've seen. I see a forest in my mind and the sand and the clay. In thinking on theoretical problems, it's again a succession of visual images. I even conjure up mental pictures of the Cretaceous and Tertiary and watch how they change.

I also inquired specifically about verbal imagery, which was consistently auditory or auditory-motor, and which was usually described in such terms as, "It comes awfully close to talking to myself most of the time." Some of the subjects, particularly theoretical physicists, rely heavily upon symbolic thinking which is related to, but not strictly, verbal thinking in auditory or auditory-motor terms.

I then also inquired about their use of imageless thinking in cases where no reference to this process had been spontaneously included in their report. When specific imagery as accompaniment to thinking was denied, I subsumed under this heading such descriptions as the following (there is, to be sure, a hint of kinesthetic involvement in some of the reports, but it is not at all clear whether this is really sensory rather than imaginal; there may likewise be accompanying visceral sensations, I think): "I just seem to vegetate; something is going on, I don't know what it is"; "I often know intuitively what the answer is, then I have to work it out"; "it's a feeling of relationships." A number of the psychologists spoke of kinesthetic imagery as important; I could not determine whether this was actually distinct from imageless thinking as the term is used here.

When subjects who primarily use other forms are faced with the problem of communicating their findings, they will then of necessity make use of some verbal imagery. I have not credited them with verbal imagery on this account alone, nor have I included as users of verbal imagery those who say they can construct visual images (as most do claim) but who practically never construct them, so far as they are aware.[1] Categorization of the data thus obtained was very difficult, but the scheme followed here seemed to be the best for the material treated.

IMAGERY AND SCIENTIFIC FIELD

In Table I, each of 61 scientists (there were 64 in the total group studied, but adequate data on imagery were not obtained from three of them) is listed under every heading to which some statement about his thinking is relevant. The code letters used in designating each man refer to his special field, according

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[1] It occurred to me that it might be easier to get information on hypnagogic imagery, and that it was worth checking whether this seemed to be the same type of imagery as that used during waking hours. This information is also difficult to get, but such data as I obtained would indicate greater use of visual imagery in this stage than in any other, even among those who do not employ it to any extent while fully conscious. On the other hand there are a number of subjects who insist that at such periods it is a "jumble of words" that goes through their minds; these subjects are mostly verbalists. There are others who describe considerable action. The relation to waking imagery is not clear, and seems to me a point worth further investigation.

Table 1. Mental Processes Utilized by Subjects

	Visual Imagery						
	Concrete, usually 3-dimensional	*Diagrams, geometrical, etc.*	*Symbols, visualized*	*Formulae, etc. verbalized*	*Verbal imagery (auditory-motor)*	*Imageless thought (variously described)*	*Kinesthetic (not otherwise described)*
Biologists							
	A2	PG2	PG1		A2	A1	
	A3	PG4	ZG4		P1	P4	
	P1	ZG4	B3		P5	PG1	
	P2				ZG2	ZG2	
	P3				ZG3	ZG4	
	PG2				B1	B3	
	PG3					B4	
	PPG4						
	ZG1						
	B2						
	B3						
Physicists							
	EP3	EP3	EP4	EP5	TP4	EP1	
	EP4	EP9	EP5	TP4	TP5	EP3	
	EP5	EPC3	EPC3	TP6	TP6	EP6	
	EP6	TP3	TP7	TP9	TP8	EP7	
	EP7	TPC4	TPC4			EP8	
	EP9					EPC3	
	EPC3					TP1	
	TP3					TP6	
	TP9					TP8	
	TPC4					TPC1	
						TPC2	
						TPC4	
Psychologists and Anthropologists							
	EPs5			EPs3	EPs1	EPs1	EPs8
	EPs9				EPs4	EPs2	EPs9
	An6				EPs6	EPs3	CPs10
					EPs9	EPs4	CPs14
					CPs10	EPs6	
					CPs11	EPs7	
					An1	EPs8	
					An2	CPs11	
					An3	CPs12	
					An4	CPs14	
					An8	An1	
						An3	
						An4	
						An5	
						An8	

Legend:

A	anatomy, physiology	EP	experimental physics
An	anthropology	EPC	experimental physical chemistry
P	botany	TP	theoretical physics
PG	botanical genetics	TPC	theoretical physical chemistry
ZG	zoological genetics	EPs	experimental psychology
B	bacteriology, biochemistry	CPS	clinical, social or child psychology

to the scheme given on the table; the numbers are arbitrarily assigned. In the interest of anonymity I have not further subdivided the anthropologists.

A pattern emerges clearly from the table. The biologists are concentrated in the visual imagery group. So are the experimental physicists, while the theoretical physicists more characteristically employ verbal or other symbolizations. The psychologists and anthropologists are heavily concentrated in the verbal group (this includes all of the cultural anthropologists). That fewer biologists are recorded as using imageless thought in addition to imagery may be due to less adequate inquiry on this point among them—they were the first group studied. I feel quite sure that imageless thought, to varying degrees, is almost certainly utilized by most of these men. Use of it is so frequently combined with use of some type of imagery that it seems justifiable to make the categorization shown in Table 2.

Table 2 summarizes the data of Table 1 in larger categories. Subjects A2, P1, PG1, TP1, E5, Ps3 and Ps9 are not included, since they use rather uncommon combinations. The association shown in the table is statistically significant. It cannot be deduced from this whether possession of appropriate imagery is conducive to choice of vocational field or whether work in the field tends to develop a particular type. There are some suggestions in the material that follows which would lend greater weight to the former supposition, but the question is clearly open.

I would like to suggest also, and very tentatively, that the subjects who do not follow the imagery pattern most typical for their own group are also somewhat less like their colleagues in their work and personalities. Since all of these men have made original contributions, this difference is not easy to estimate; so far as I can assess it in their work, it seems to consist largely in manner of approach to problems. The life histories furnish some indication that there were also differences in interests (as shown in spontaneous activity) in high school and early college days. Among biologists and physicists, very strong interest in the classics—music, art, and literature—were mentioned only by A2, P1, P4, P5, ZG3, EP1, EP8, TP1 and TP9. It will be noted that four of these biologists are among the six biologists who use verbal imagery and that three of the physicists are among the four physicists who report only imageless thinking. Among the psychologists, interest in literature, or in writing as a career, is very common, and their concentration in the verbal group has already been noted. Ps5 and An6, who use visual imagery, had no real interest in literature or art, so far as I could determine, at any age. . . .

IMAGERY AND TEST DATA

It seemed worthwhile to examine some of the test data for possible associations with imagery classification. For this purpose only the subjects who could be classified in Table 2 were used. This number is further reduced by some deficiencies in the test data, but 51 subjects were available for most of the comparisons. Table 3 gives the figures for such test data

Table 2. Association between Field of Science and Imagery Types

	A. Visual	*B. Verbal*	*C. Imageless*	*Totals*
Biologists	10	4	3	17
Physicists	10	4	4	18
Psychologists and anthropologists	2	11	6	19
	22	19	13	54
	X^2 11.65	P .05 — .02		

A includes subjects using visual imagery, or this with visual symbolization or imageless thoughts or both.

B includes subjects using verbal imagery, or this with verbal symbolization or imageless thought or both.

C includes only subjects who describe no visual, verbal, or other imagery modality and are classed as using imageless thought only.

Table 3. Comparison of Visual and Verbal Groups on Various Test Data

				*For difference between means**	
		A	*B*	*t*	*P*
	N	22	19		
Age	range	37–58	31–58		
	mean	47.4 ± 1.45†	47.4 ± 1.65		
Rorschach data					
R	range	10–81	11–186	2.960	<.01
	mean	27.6 ± 3.88	61.7 ± 11.09		
T/R	range	22–97	17–65	2.635	.02 > P > .01
	mean	46.4 ± 4.79	32.1 ± 2.84		
ITS	range	2–16	5–15		
	mean	9.8 ± .79	10.5 ± .74		
M	range	0–9	1–18	2.671	.02 > P > .01
	mean	3.1 ± .48	6.1 ± 1.05		
W%	range	11–100	6–67	3.003	<.01
	mean	4.60 ± 4.56	28.7 ± 3.75		
Rorschach Dd%					
	range	0–29	0–37	3.187	<.01
	mean	9.1 ± 1.79	18.0 ± 2.23		
F%	range	10–63	16–59		
	mean	38.5 ± 3.09	39.6 ± 2.73		
F′ + %	range	64–100	67–100		
	mean	88.5 ± 2.34	87.1 ± 2.31		
TAT					
length of stories	range	5–47	7–49	1.510	<.10
	mean	14.2 ± 1.77	18.6 ± 2.41		
VSM test					
V raw score	range	8–72	43–75	1.763	.05–.10
	mean	52.5 ± 3.93	62.2 ± 4.05		
S	range	3–22	3–19		
	mean	11.2 ± 1.12	11.4 ± 1.38		
M	range	8–27	2–27	1.570	<.10
	mean	16.6 ± 1.89	12.5 ± 1.97		

* t and P are entered only for mean differences with P < .10.

† Standard error of the mean.

as seemed to be relevant. I have considered only the visual (A) and verbal (B) groups in this comparison, since both groups contain subjects who also use imageless thinking.

Several of the subjects remarked that they now make less use of specific imagery than they used to, but differences are not noticeable within this age range. Galton suggested the possibility of changes with age.

The tests of verbal, spatial, and mathematical functions (supplied by the Educational Testing Service) are of conventional types, and it may be seen that there are some differences in means. The most marked is in that for the verbal test (antonyms): those whose statement of typical thinking denotes high reliance on verbal imagery do very well on this test, although P for the difference is .05 to .10. The spatial test (a test of recognition of representations of 3-dimensional figures rotated through space) seems not to differentiate between the groups. The verbal-spatial correlation for the total group was +.32. From descriptions of how the test was done, it is evident that more than one approach is possible. Mare (6) found that "spatial imaginative faculty," which I take to be related to that involved in the spatial test used, can be compensated considerably in some tests by intellectual reflection.

The mathematical test (involving reasoning as well as calculation) also shows a difference between means for the groups, but in the reverse direction. The numbers in this test are very small, since it was not difficult enough for the physicists. Correlation between verbal and mathematical tests for the total group is +.14.

Brower (2) found no relation between the intensity value of images of various modalities, experienced in response to verbal stimulation, and the Otis. Since the Otis is an omnibus test, this does not contradict the results reported here.

Difference between the groups in number of responses (R) on the Rorschach is very great, with the verbalists giving many more responses. If this merely indicates considerably greater verbal fluency in this group, one would expect length of TAT stories to show a difference also, but such is not the case. I suggest the difference may be due rather to an inhibiting effect—perhaps subjects accustomed to relying upon visualizations are more constrained when an unstructured stimulus is actually present; the fact that the visualists use significantly fewer Dd's is pertinent here. On the other hand, if this were the case, one would expect some differences in F% and F'+ % (F designates a response determined by form alone, and F'+ % the percentage of these responses which are good form). W% (or percentage of responses using the whole blot) is markedly higher in the visual group. This is probably largely if not entirely a function of the increased R which would not affect F% in the same way. This large difference in R makes evaluation of other differences almost impossible, except the T/R or time per response. This is significantly longer in the case of the visual group. A recent paper by Raju (7), utilizing a word-association technique for visual and auditory stimuli, suggests that fast reactions are seldom accompanied by sense impressions. . . .

My data offer no information on why subjects have come to rely on some modes of thinking rather than others. Whether there is a hereditary factor, as Galton suggested, or whether it is largely training or experience, and if so how early and by what means the mode is set and how changeable it is are unsolved but fascinating problems. As to manner or cause of development, leads might be obtained from cultural anthropology as well as from case histories, as has been done here. . . . The first need, of course, is for some more adequate techniques upon which to base categorization of subjects. Development of such techniques would open all of these problems to direct investigation and would, finally, shed much light on the whole problem of thinking, particularly of "creative thinking."

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E. Personality and Motivational Characteristics

Introduction

Search for personality correlates which identify scientists rests on the assumption that certain personality and emotional characteristics are felicitous to scientific performance, while others are not. Studies of research performance, research milieu, and the institutionalization of scientific practices encourage us to look for persons whose needs and drives are homogeneous with those that optimize performance, and which are syntonic to research work.

In the case of the personality of scientists, the questions of whether these needs and drives have been present early, influencing the socialization experiences which then directly lead to a choice of work in science, and of the extent to which scientific work role has molded the character and its values remain unanswered.

The articles in this section do not speak directly to such theoretical issues. Rather they attempt to find personality correlates of the men who become research scientists. The research in this area has generally fallen into three groups: (*1*) the studies which compare personality of the mature scientist with that of the general adult population or with the college student population; (*2*) the studies which compare personality and emotional characteristics of those who are less eminent or less creative; and (*3*) those studies which compare the personality and emotional characteristics of different scientists in different disciplines. The investigations in this latter group will be found in Section IV A, where considerations which have led the choices among disciplines are included.

These different research strategies have made for different methodological problems and different measures in the articles that are included here. Given these, it is not surprising that the composite portrait of the scientist that has arisen, while not entirely consistent, shows high redundancy in terms of the traits identified. For example, two articles in this section, those by Cattell and Drevdahl and by Eiduson used contrasting methods to study personality and emotional characteristics of mature scientists. Cattell and Drevdahl's sample involved only eminent or successful men while Eiduson makes no claim that her sample was representative and instead studied a cohort which varied so far as success is concerned. Cattell and Drevdahl used a paper-and-pencil personality self-inventory, the 16 P.F., to compare scientists with norms of the general population and with a student population, while Eiduson looked to the projective techniques, the TAT and the Rorschach. Despite procedural differences and limitations, both studies show similar results. The scientists' strong ego-involvement in work and in intellectual activities comes through, as does their curiosity which directs their interests in work and their desires to interpret natural forces or reality. In both, the self-sufficiency of scientists and their independence in thought and action are found. Scientists are preoccupied with things and ideas rather than with people in the Cattell and Drevdahl study, and thus these investigators consider scientists to be more withdrawn than the general population, while Eiduson interprets her data to indicate emotional constriction and control rather than withdrawal.

Garwood, like Parloff and Datta (Section III B), has tried to determine if creative young people interested in science have characteristics similar to those of adult scientists. If both groups are identical in personality characteristics and different from the general population, it could be assumed safely that personality is involved as a precursor to vocational choice. If adult and adolescent samples differ in personality, then it could be assumed that professional socialization later operates to mold the personality of the young scientist to that of the professional group, or that young scientists having dissimilar traits from professionals might drop out of science as a career. Garwood's criterion of creativity is the Guilford Creative Test Battery, as contrasted to the Parloff definition of adolescent creativity by means of ratings by experts of projects submitted in the Westinghouse Talent Search program. Garwood notes that a number of personality characteristics differentiated high from low creatives, such as scores on dominance, sociability, social presence, self-acceptance. Her work also implicates the psychological roots of some of these differences in intra- and interpersonal relationships and interests, in sexual identification, and in the degree to which nonconscious and conscious material are integrated. In a similar vein, McClelland, using adult scientists, attempts to find the genesis of some of the characteristics which regularly turn up in studies of the personality of scientists, such as their pull toward the rational and the logical in thinking, their attempts to reject impulsiveness, their preoccupation with things rather than people, their social aloofness, and their interest in nature. He suggests that scientists are strongly identified in their achievement needs with fathers. This early and complete identification is the mode adopted for resolving the intense Oedipal conflict. This identification also encourages the neutralization of aggressive drives and an interest in things rather than people.

The Stein article turns to a typological model for describing individuals on the basis of the dynamic interrelationships between their needs. Assuming that different needs, shared to some extent by all scientists, can be ranked in a need hierarchy, Stein finds these kinds of typologies more meaningful than merely the types which might emerge from descriptive characteristics. Stein also suggests the ways that the organization is related to the various scientific roles that are also described by Gough and Woodworth (Section IV B), by Hagstrom (Section V A), and Andrews (Section V B).

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A Comparison of the Personality Profile (16 P.F.) of Eminent Researchers with That of Eminent Teachers and Administrators, and of the General Population

R. B. Cattell and J. E. Drevdahl

THE SETTING OF THE PROBLEM

Research on research is a recent conception, but it has come none too soon. As the investigations of Wolfe (1952) indicate, the well-springs of professional talent never flow too freely, and of all professional groups that of creative researchers is probably the most uncertain in its supplies of talent. Nevertheless, because of the importance of this small band to continued social prosperity and scientific advance, its occupational selection and conditions of working creatively need to be studied, despite their complexity, as soon as understanding of personality measurement and the mental processes of creativity (Kretschmer, 1931; Spearman, 1933; Guilford, Wilson and Christensen 1952), make possible an improvement upon present practices. . . .

Three well-known attacks upon the problem, which have recently rallied interest, are those of Guilford *et al.* (1952) upon the nature of creativity, and of Roe (1953) and Van Zelst and Kerr (1954), upon the personality of the researcher. This article does not purpose to survey and integrate, but we may

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point out that our objective is substantially the same as that of Roe, though our results cannot be aligned, because Roe had to depend at the time of planning her research on the Rorschach and the T.A.T., whereas our measurement is directed to personality dimensions more recently factor-analytically investigated (Cattell, 1950*c*) and confirmed (Cattell, Day and Meeland, 1953; Cattell, 1955*a*) in the Sixteen Personality Factor Questionnaire (Cattell, 1955*b*).

While this measurement of primary personality dimensions has the advantage of greater reliability (including lesser subjectivity of scoring) than the Rorschach and T.A.T., and also of higher validity in the sense of accounting for more of the variance of more demonstrably independent directions of personality variation, it has the disadvantage of all questionnaire techniques: possible motivational distortion and possible undue demands on literacy. Distortion has been avoided, as far as possible, in the 16 P.F. by indirection of questioning, but with persons eager for a particular job, some distortion undoubtedly remains for correction, and low educational level can also produce random error. It may be claimed, however, that the present investigation of teachers, administrators and researchers presents the most ideal situation for the use of the questionnaire, in that these subjects are educated people, appreciative of the scientific need for candour, secure in their positions, and assured of anonymity in their results.

Vocational selection and guidance depend in principle upon either or both of two statistical-conceptual systems. In one the personality-ability *profile* is determined for the typical member, by such devices as used by R. L. Thorndike (Thorndike and Norris, 1952), Cronbach and Gleser (1953) or one of the present writers (Cattell, 1949) in the formally identical problem of classifying culture patterns (Cattell, 1950*b*). Any individual propositus then has the similarity of his personal profile to this successful (or at least surviving) type determined by the pattern similarity coefficient, r_p, simple or weighted (Cattell, 1949; Cronbach and Gleser, 1953). In the other, a criterion of success within the occupational group is set up, and regressions, linear or curvilinear, of the various personality factors (including abilities) upon this criterion are worked out. Enlightened use of the values from either requires it to have the signature of the particular social supply-and-demand conditions obtaining at the time.

The present investigation was planned with the first of these objectives—to determine the personality profile of the typical researcher in relation to the average adult and the profile types of other occupations, especially of those for the related callings of scientific teachers and administrators, among which distinction most needs to be made in guiding students to careers. It seems to us that this delineation of the occupation *per se*, relative to others, should precede, logically and statistically, the investigation of the variance *within* the occupation. Besides, the preparation of criteria of success, involving, if it is to be effective, a good deal of time on the part of high-level professional people, was too expensive for the present, unendowed research.

RESEARCH PLAN AND PROCEDURE

The testing instrument, having been frequently described in other investigations (Cattell, 1950*a*, *b*, 1955*a*; Cattell *et al.* 1953; Drevdahl, 1954; Shotwell and Cattell, 1954; Stice and Cattell, 1954) needs little comment, except to say that it has recently been re-factored and independently rotated with confirmation of all but one of the factor positions (Cattell, 1955*a*). Besides general intelligence, it measures fifteen other factors, some of which are well known from clinical or general psychology; e.g. cyclothymia *v.* schizothymia (Factor *A*), emotional integration *v.* neuroticism (Factor *C*), surgency *v.* desurgency (Factor *F*), dominance *v.* submission (Factor *E*), etc., while others are new, e.g. sensitive emotionality *v.* toughness (Factor *I*) and self-sufficiency (Factor Q_2). Each factor score is based on ten or thirteen items in the *A* Form and the same number in the *B* Form, and the reliabilities of the factor scores (not to be confused with that of the items, by re-test) average 0·69. The whole test, which is untimed, takes from three-quarters of an hour to two hours.

Profiles, as means and sigmas for each factor, are already known on the 16 P.F. for the student population, the general adult population (Cattell, Stice and Saunders, 1950), and about twenty rather varied occupations. In addition to comparing the researcher's profile with these, we wished to compare it more sharply with those of teachers and administrators in the same areas, as indicated above. The areas taken were three: Physical Science (represented by Physics); Biological Science (represented by Biology as such); and Psychology (which, as Roe's results (1953) suggest, may be taken as representative to some extent of the social sciences).

Committees were set up to choose from the roster of their professional society members, especially the starred scientists (Visher, 1947; Cattell and Brimhall, 1921), between 200 and 250 individuals in each of the three areas. Since we were more interested in the researcher than in the others, they were instructed to choose about twice as many researchers as teachers and administrators (typically 100 R, 50 T, and 50 A). By definition the subjects were to be eminent or 'successfully engaged' in their profession and specifically noted, in the one group, for research contribution, in the second for teaching (including text-book writing), but with low or negligible research publication (less than one article in 3 years), and, in the third, for administration (heads of departments, national committee organizers, board members, etc.), again with negligible comparable research contribution. For uniformity in an initial study we took American scientists, rather than British and American mixed, until such time as the test standardization should become wider in Britain. The numbers chosen and responding are shown in Table I.

As usual we have no control on the selection by rejection, but it is at least satisfying that the percentage thus lost is surprisingly steady for different specialities and groups. We end, as we began, with about twice as many in the research group as in each of the other groups. The ages are also fairly uniform (the psychologists running somewhat younger) and high enough for the men to have had their work stably assessed by time. The selection of respondents by special committees rather than by simply taking, for example, starred men from *American Men of Science* (Visher, 1947), was required by various considerations, but chiefly by the need for separate ranking of general scientific faculty within the subgroups of teachers, administrators, and researchers. . . .

Table I. Samples of Leading Faculty Members Studied

Later reference no. of group	*Biologists*	*No. designated*	*No. agreeing to co-operate*	*No. satisfactorily completing questionnaire*	*Mean and sigma of age of completers*
1	Teachers	62	29	29	44·7 (8·1)
2	Administrators	62	27	21	50·1 (9·1)
3	Researchers	93	55	46	48·7 (9·4)
	Subtotals	217	111	96	
	Physicists				
4	Teachers	50	28	25	43·0 (10·9)
5	Administrators	55	25	20	53·1 (8·3)
6	Researchers	121	53	46	51·4 (12·2)
	Subtotals	226	106	91	
	Psychologists				
7	Teachers	52	30	27	42·1 (9·0)
8	Administrators	48	33	28	48·0 (5·8)
9	Researchers	105	68	52	42·9 (8·1)
10	Selected researchers[*]	—	68	39[*]	43·2 (8·7)
	Subtotals	205	131	107	
	Grand total	648	348	294	

[*] This group of 39 was selected from the 52 by more exacting standards as to significance of research. It is interesting to see that in subsequent results it behaves "like the research group (no. 9) only more so." Not re-included in grand totals.

DIFFERENCES FROM THE GENERAL ADULT POPULATION

. . . It can be seen that this group of leading scientists departs markedly from the pattern of the general population, and in a very consistent fashion, despite differences of field or mode of activity.[1] They are in general more intelligent, more schizothyme, of greater ego-strength and dominance, less surgent, more adventurous, more sensitive, have less free-floating anxiety, and are slightly more radical and more self-sufficient. On super-ego strength, which is represented in a dual way, not yet entirely clear, by Factors *G* and Q_3, the evidence is conflicting. The Factor *G*, which represents emotional adoption of standard moral goals, is lower; but Factor Q_3, sometimes called 'will control' and which represents strict, internalized and intellectualized standards, and exacting demands on the self, is decidedly higher.

. . . For economy of presentation we shall group the data into teachers (biologists, physicists, and psychologists together), administrators (similarly grouped), and researchers (similarly grouped), thus reducing to forty-eight comparisons. These are shown, as raw score differences with associated *P* values, in Table 2.

Thus, without attention to differences *among* teachers, administrators, and researchers, which are examined in the next section, we can see that the deviations from the general population . . . proved to be uniformly significant for all three groups. In intelligence, ego strength, dominance, desurgency, lack of group super-ego, adventurousness, sensitive emotionality, freedom from paranoid trends, freedom from free-floating anxiety, and higher 'internalized compulsive super-ego' (formerly labelled 'will control') the differences for all three groups are significant away beyond the 1% level. In schizothymia, radicalism, and self-sufficiency the uniformity is broken by the

Table 2. Differences of Means of Teachers (T), Administrators (A), and Researchers (R), from General Population

(*Positive scores indicate* higher *than the general population in the usual direction of measurement of the factor, i.e. that of the name on the left of the bipolar title.*)

	T		*A*		*R*	
	Diff.	*P*	*Diff.*	*A*	*Diff.*	*P*
A. Cyclothymia *v.* schizothymia	—3·32	<0·001	—1·59	0·03	—5·05	<0·001
B. Intelligence *v.* defective general ability	3·60	<0·001	3·89	<0·001	4·24	<0·001
C. Ego strength *v.* general emotionality	7·60	<0·001	9·12	<0·001	6·53	<0·001
E. Dominance *v.* submissiveness	3·03	<0·001	3·23	<0·001	4·52	<0·001
F. Surgency *v.* desurgency	—6·60	<0·001	—5·20	<0·001	—6·38	<0·001
G. Super-ego, identification with group standards *v.* lack of super-ego	—5·80	<0·001	—4·41	<0·001	—5·48	<0·001
H. Adventurous cyclothymia *v.* withdrawn schizothymia	4·36	<0·001	8·42	<0·001	5·14	<0·001
I. Sensitive emotionality *v.* toughness	3·88	<0·001	3·34	<0·001	4·23	<0·001
L. Paranoid trend *v.* lack of paranoid trend	—3·07	<0·001	—4·72	<0·001	—3·45	<0·001
M. Bohemian unconcern	0·21	0·37	—1·76	<0·01	0·79	0·08
N. (Psychological title uncertain)	—0·30	0·25	0·54	0·13	0·02	0·48
O. Free-floating anxiety *v.* lack of anxiety	—6·40	<0·001	—8·35	<0·001	—6·74	<0·001
Q_1. Radicalism *v.* conservatism	2·63	<0·001	0·76	0·10	2·63	<0·001
Q_2. Self-sufficiency *v.* lack of self-sufficiency	2·31	<0·01	1·47	0·02	4·44	<0·001
Q_3. Super-ego, will control *v.* lack of will control	5·58	<0·001	6·86	<0·001	5·07	<0·001
Q_4. Psychosomatic anxiety	—0·08	0·46	—2·84	<0·01	—1·42	0·01

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[1] EDITORS' NOTE: The reader is referred to the original article for raw score means and sigmas for each of the ten groups on all factors, and for their conversion into 10-point standard scores according to Table 5 in tabular supplement A, for the Sixteen Personality Factor Handbook (Cattell, Stice, and Saunders, 1950).

Administrator group's divergence falling to only the 1–10% significance level.

It is interesting to compare these deviations of the 'genus' *successful academic man*, as measured from the 'order' *general population*, also with their deviations from the 'family' *general university student*, as shown by undergraduate norms. Due to the large age difference that would here be involved, and the impossibility of accurately bridging it with present knowledge of personality factor life changes, no table has been set out. However, from table 2 in Supplement A of the 16 P.F. Handbook (university undergraduate students, men and women), and using the allowances for age trends over 25 years, given in table 6 therein, we can at least state indications. With the student profile thus projected forward 25 years the outstanding faculty now show no difference on ego strength (C), dominance (E), super-ego: group standards (G), sensitive emotionality (I), free anxiety (O), and internalized super ego (Q_3). They switch over on adventurousness (H) from high to somewhat low, on paranoid trend (L) from low to somewhat high, and on psychosomatic anxiety (Q_4) from slightly low to slightly high. They still deviate, almost as much as before, in the direction of being more intelligent (B) and more radical (Q_1). Finally, in schizothymia ($A-$) and self-sufficiency (Q_2) they deviate as they did from the general population, only more so!

The only uncorrected influence of unknown magnitude in the above is from the larger proportion of males (see Table 1) in the outstanding faculty than in the standardization groups for students and adult occupational composites. However, surprisingly few significant differences have been found between male and female populations, either in the form or the level of their general personality factors. The only observed differences significant at the 1% level (Cattell *et al.* 1950, 1953) are for women to be more cyclothyme ($A+$), less dominant ($E-$), and more sensitively emotional ($I+$). On this basis, if more balanced for sex, our outstanding academics might deviate less in schizothymia and dominance, and show some slight deviation from the student group in the same direction as from the general population in Factor I, toward sensitive emotionality. If entirely male norms were substituted for mixed norms in our control general population and student groups the deviation of academics on these three sex-modifiable factors would still be in the direction of greater schizothymia, dominance, and sensitive emotionality, regarding the general population, but dominance would cease to have significant deviation from male students.

These differences of pattern against different backgrounds may become confusing to the reader without time to study the various tables and corrections, so in conclusion we will concentrate on the contrast with the student type. Here it is interesting to observe that the deviations of these successful professional men are in agreement with those found by Wright (1954) to distinguish men of equal intelligence but higher scholastic achievement from those of lower achievement. His differences significant at $P = 0{\cdot}01$ were: greater schizothymia ($A-$) and withdrawn schizothymia ($H-$); greater ego strength (C), absence of free-floating anxiety ($O-$) and of somatic anxiety (Q_4-). The three last do not distinguish our men from students, but do distinguish them from the general population. To Wright's picture our study adds that the scientific 'achievers' are higher in intelligence (B), radicalism (Q_1), self-sufficiency (Q_2), and perhaps sensitive emotionality (I) and paranoid trend (L). In personality they are thus not simply 'academic over-achievers,' and indeed, considered in the light also of their profile deviations from the general population, which is the more basic comparison, they are distinguished, in addition to the above 10 factors, by high dominance (E), high internalized super-ego or will control (Q_3) and marked desurgency ($F-$).

It is to be hoped that these findings on well known factors will lead to fuller understanding of the psychological conditions of and selection pathways toward scientific eminence, as well as to additional insight and predictive power in regard to the psychological nature of the personality factors themselves.

THE DIFFERENCES OF RESEARCHERS FROM ADMINISTRATORS AND TEACHERS

Having defined the genus of 'outstanding academic scientists,' we can now discover within it the characters of the particular species—the researcher—in which we are primarily interested.

Table 3 sets out the personality factor means and sigmas for the three groups (from which the values both of Table 2 and Table 4 are derived).

The significance of differences of means among the Table 3 groups is shown in Table 4. The P values are for a two-tailed distribution, i.e. assuming no hypothesis has been made as to the direction of difference, though actually hypotheses in some cases *were* made (Cattell, 1955*b*) and the P values might be halved in those instances.

Suggestive differences are visible in the tendency of researchers to have higher intelligence (B) than teachers ($P = 0{\cdot}07$), to be more dominant (E) than either teachers ($P = 0{\cdot}09$) or adminstrators, and to be more paranoid (L) than administrators ($P = 0{\cdot}07$). Differences immediately acceptable at the $P = 0{\cdot}05$ to $0{\cdot}01$ level are: the tendency to more withdrawn schizothymia ($H-$) and to lower compulsive super ego (Q_3) than administrators. Any difference is a contribution to definition of both parties, and some of the above are as much comments on the more outgoing qualities of administrators as on the more withdrawing qualities of researchers; but finally we shall stress the definition of the researcher in terms of the ways he differs simultaneously from administrators and teachers.

Thus, at or beyond the 1% level of certainty he is more schizothyme ($A-$) and more self-sufficient (Q_2) than teachers or administrators. At the same level he shows more emotional instability ($C-$) and Bohemian unconcern (M) than administrators, but in this case teachers are similar to the administrator block at a less clear P level. Also beyond the 1% level is the researcher's tendency to be more radical (Q_1) than the administrator.

Since some differences may be partly peculiar to certain scientific areas, and thus may be masked by arranging all areas together, about one-half of the total possible area breakdowns—those of sufficient interest to justify space—are presented in Table 5. (All have been examined and the full table is presented elsewhere (Cattell, 1955*b*).) In general we may note a tendency for differences to be more significant among psychologists, reflecting either greater specialization or greater skill of that selection committee (motivated by both scientific and psychological interests) in sorting out the criterion types. There is also a tendency for the more selected psychological research group, R′, to show more significant differences than the larger group, R.

The agreement of the differences for the subgroups in contributing to those 0·01 level differences for the total group cited from Table 4 is so uniform that only one of those five factors needs to be relisted here in order to inspect an inconsistency in the parts. This is M, Bohemian unconcern, where it will be seen that there is an inversion in psychology, teachers being slightly more 'Bohemian' ($M+$) than researchers. Factor E, not reproduced here, also shows an inversion of the usual greater dominance of researchers in the special case of biology, and these biological researchers are also significantly ($P = 0{\cdot}01$) more desurgent ($F-$) than their colleagues, which suggests perhaps aesthetic rather than mastery interests in much biological research.

Turning to the differences that are of rather low significance in the composite picture and examining them in individual areas we note first that the higher 'paranoid trend' score is really peculiar to psychological researchers, in whom it reaches $P<0{\cdot}014$ and $<0{\cdot}001$. Possibly, in the more chaotic state of a new science, some tenacity of conceptualization is a necessary escape from despair! And perhaps those more interested in nature's laws than man's theories choose a more developed, structured science than psychology. The withdrawn schizothymia factor, H, is perceived in Table 5 to be no longer selective of researchers from teachers in the case of physics, but to be highly selective of psychological researchers, especially in the purer research group (R′). The effect is partly due to psychologist *teachers* being more adventurous cyclothymic ($H+$) than most. Presumably teaching in an area in which research has yet provided so little of an exact nature to talk about attracts a more sociable, adventurous type, perhaps more interested in the interactive process of teaching than in the subject-matter.

Factor I is included in Table 5 because it shows in each group a uniform tendency toward significance which is nevertheless missed in the composite picture. Researchers, relative in one case to administrators and another to

Table 3. Means and Sigmas for Teachers, Administrators, and Researchers on All Factors

	A	*B*	*C*	*E*	*F*	*G*	*H*	*I*	*L*	*M*	*N*	*O*	Q_1	Q_2	Q_3	Q_4
Teachers (81)																
Mean	12·98	21·10	34·10	22·93	20·70	22·00	28·56	19·38	17·33	20·91	23·90	13·40	25·73	24·91	26·78	15·72
Sigma	5·20	2·35	5·34	6·58	7·39	5·22	10·35	4·25	4·47	5·64	3·67	7·12	5·26	5·32	4·99	7·82
Administrators (69)																
Mean	14·71	21·39	35·62	23·13	22·28	23·39	32·62	18·84	15·68	18·94	24·74	11·45	23·86	24·07	28·06	12·96
Sigma	6·61	2·54	4·93	5·98	6·40	5·47	8·95	4·33	4·31	4·83	3·67	8·12	4·56	5·78	4·67	7·09
Researchers (144)																
Mean	11·25	21·74	33·03	24·42	20·92	22·32	29·34	19·73	16·95	21·49	24·22	13·06	25·73	27·04	26·27	14·38
Sigma	4·83	2·61	5·17	5·97	7·31	5·64	9·92	4·57	4·71	6·49	4·04	7·56	5·10	5·91	5·68	7·23

Table 4. Significance of Differences among Teachers (T), Administrators (A), and Researchers (R)

(Positive differences indicate higher mean scores in the category mentioned second in each comparison; measurement in the same direction as in Table 2.)

	T v. A		*T v. R*		*A v. R*	
	Diff.	*P*	*Diff.*	*P*	*Diff.*	*P*
A. Cyclothymia-schizothymia	1·73	0·08	—1·73	0·01	—3·46	0·001
B. Intelligence	0·29	0·47	0·64	0·07	0·35	0·36
C. Ego strength	1·52	0·07	—1·07	0·15	—2·59	0·001
E. Dominance	0·20	—*	1·49	0·09	1·29	0·15
F. Surgency	1·58	0·16	0·22	—	—1·36	0·17
G. Super-ego	1·39	0·11	0·32	—	—1·07	0·18
H. Adventurous cyclothymia	4·06	0·01	0·78	—	—3·28	0·02
I. Sensitive emotionality	—0·54	0·44	0·35	—	0·89	0·17
L. Paranoia	—1·65	0·02	—0·38	—	1·27	0·07
M. Bohemian unconcern	—1·97	0·02	0·58	0·46	2·55	0·001
N. (Psychological title uncertain)	0·84	0·23	0·32	—	—0·52	0·36
O. Free anxiety	—1·95	0·12	—0·34	—	1·51	0·19
Q_1. Radicalism	—1·87	0·02	0·00	—	1·87	0·01
Q_2. Self-sufficiency	—0·84	0·37	2·13	0·01	2·97	0·001
Q_3. Super-ego, will control	1·28	0·11	—0·51	0·48	—1·79	0·02
Q_4. Psychosomatic anxiety	—2·76	0·02	—1·34	0·21	1·42	0·17

* Any *P* value beyond 0·50 is represented by a —.

Table 5. Significance of Differences among Teachers, Administrators and Researchers for Separate Scientific Areas

Read as for Table 4; italicized figures refer to "Selected researchers" (R′) (see Table 1).

	T v. A		*T v. R*		*A v. R*		*R v. R′*	
	Diff.	*P*	*Diff.*	*P*	*Diff.*	*P*	*Diff.*	*P*
Biologists								
H. Adventurous cyclothymia	3·67	0·08	—0·22	0·46	—3·89	0·04		
I. Sensitive emotionality	1·79	0·05	2·04	0·03	0·25	0·41		
L. Paranoia	—0·24	0·43	—0·74	0·23	—0·50	0·35		
M. Bohemian unconcern	0·71	0·31	2·00	0·06	1·29	0·16		
Q_3. Super-ego, will control	—0·15	0·46	0·00	0·50	0·15	0·45		
Q_4. Psychosomatic anxiety	2·45	0·23	0·59	0·36	—1·86	0·19		
Physicists								
H. Adventurous cyclothymia	2·80	0·17	2·67	0·15	—0·13	0·48		
I. Sensitive emotionality	—1·46	0·10	0·41	0·33	1·87	0·04		
L. Paranoia	—1·57	0·07	1·62	0·05	—0·05	0·48		
M. Bohemian unconcern	—4·25	<0·01	—0·24	0·44	4·01	<0·01		
Q_3. Super-ego, will control	1·85	0·10	—0·14	0·46	—1·99	0·05		
Q_4. Psychosomatic anxiety	—2·71	0·09	—2·33	0·10	0·38	0·41		
Psychologists								
H. Adventurous cyclothymia	4·30	0·02	—0·29	0·44	—4·59	0·01		
			—1·00	*0·25*	—5·90	*<0·01*	*—1·31*	*0·27*
I. Sensitive emotionality	—1·91	0·07	—1·33	0·44	0·58	0·01		
			—1·83	*0·25*	*0·08*	*<0·01*	*—0·50*	*0·27*
L. Paranoia	—2·80	0·02	1·02	0·21	3·82	<0·001		
			0·61	*0·32*	*3·41*	*<0·01*	*—0·41*	*0·35*
M. Bohemian unconcern	—3·05	0·01	—0·31	0·41	2·74	0·01		
			—0·93	*0·26*	*2·12*	*0·05*	*—0·62*	*0·32*
Q_2. Super-ego, will control	2·31	0·03	—1·11	0·18	—3·42	<0·01		
			—1·18	*0·19*	*—3·49*	*0·01*	*—0·07*	*0·48*
Q_4. Psychosomatic anxiety	—7·23	0·01	—2·49	0·11	4·74	<0·01		
			—2·49	*0·11*	*4·74*	*<0·01*	*0·00*	*0·50*

teachers, tend to be more sensitively emotional ($I+$). Only the psychologists upset this. The trend is most clearly shown in the biologists, again suggesting aesthetic-emotional involvements in the motivation of biological research.

Factor M (Bohemian unconcern) is set out because the breakdown shows that the researchers' deviation is largely due to administrators being distinctly low in it. The latter show marked 'group concernedness' and indeed in physics and psychology the teachers are higher on unconcern than the researchers. Factor Q_3, internalized compulsive super-ego or will control is similar, in that the difference is principally due to the administrators showing the high regard for exactness, high standards of behaviour and rather obsessional trends which form the essence of the factor, though teachers are also higher than researchers. The same form of relation holds for Q_4, the absence of somatic anxiety being characteristic of administrators rather than its presence characteristic of researchers.

SUMMARY AND DISCUSSION

1. The personality profile on the 16 P.F. Test has been compared for larger groups of scientists (96 biologists, 91 physicists, and 107 psychologists) than in any previous investigation of the personality of the researcher, and several differences significant at the $<0{\cdot}01$ or $<0{\cdot}001$ levels have been discovered: (*a*) between scientists and: (i) the general population, (ii) the student population; and (*b*) between researchers, teachers and administrators, all in the field of science.

2. The outstanding professional, academic scientists as a whole differ from:

(*a*) *The general population,* at beyond the 1% level, in the personality factors called general intelligence (B), ego strength or stability (C), dominance (E), desurgency ($F-$), lack of group super-ego standards ($G-$), adventurousness (H), sensitive emotionality (I), lack of paranoid trends ($L-$), lack of free-floating anxiety ($O-$), and compulsive super-ego (or will control) (Q_3). Except for administrators they are at the same significantly higher level in radicalism (Q_1) and self-sufficiency (Q_2). Administrators alone differ at the 1% level in showing lower somatic anxiety (Q_4-).

(*b*) The university *undergraduate population,* approximately age corrected, by being decidedly more schizothyme ($A-$) and self-sufficient (Q_2), more intelligent (B), desurgent ($F-$), withdrawn schizothyme ($H-$), radical (Q_1) and, probably, more paranoid (L) and of higher somatic anxiety (Q_4).

3. Researchers, relative to teachers or administrators, are, at the 1% level of certainty, more schizothyme ($A-$), self-sufficient (Q_2), emotionally unstable ($C-$), bohemianly unconcerned (M), and radical (Q_1). They are also significantly, but less uniformly, more dominant (E), paranoid (L), withdrawn schizothyme ($H-$) and lower on compulsive super-ego (will control) (Q_3). In the last and in radicalism they differ more from administrators than from teachers. There are some interesting differences indicated among the sciences, researchers in biology being, among biologists, less dominant and more desurgent; but these inter-area differences are more fully discussed elsewhere (Cattell and Drevdahl, 1955).

These results may be compared with Drevdahl's study (1954) of 'creative' *v.* 'non-creative' students in science and in the arts, which used the same personality factors and the same measuring instrument. The creative were significantly more schizothyme ($A-$), self-sufficient (Q_2), withdrawn schizothyme ($H-$), sophisticated ($N+$), desurgent ($F-$), and radical (Q_1). There is thus important agreement, while there are also some special characteristics of creative persons at the student level which do not particularly show up in those called to research (and vice versa).

Certain hypotheses had been reached from reading biographies of one hundred eminent British, American and other researchers prior to these experimental determinations (Cattell, 1955*b*). These personal records suggested that researchers would be more schizothyme, intelligent, dominant, paranoid, self-sufficient, and of greater super-ego development than the generality of professions of the same educational level. These characteristics of past researchers seem well matched by our findings on living researchers, except for super-ego strength, where the difference tends to be, if anything, reversed.

Brevity of space compels deferment of discussion of the psychological and social operation of these personality factors to another place (Cattell, 1955*b*). But it is easy to see that the schizothymic preoccupation with things and ideas, rather than people; the self-sufficiency which favours creativity and independence of mind; the dominance which gives satisfaction in mastery of nature for its own sake; and the emotional instability which permits radical re-structuring and creativity, would all be vital to the best kind of basic research performance—though perhaps unpleasant in an administrator and inapt in a business man. The remaining factors of Bohemian unconcern and radicalism may perhaps best be regarded as condition and consequence of continued adjustment to the researcher's comparative earnings!

It is suggested that until further research, especially with the improved personality factor measures now becoming available (Cattell, 1954, 1955*a*), is prosecuted on this problem any vocational selection in progress be based on the standard score profile derived from row 5 of Table 3, and which is set out in standard form in Table 6. Naturally it should be used with due regard to its tentative character and in conjunction with whatever other evidence is available for selecting researchers from examination-passers at the university level.

This personality profile reminds us, after the above comparisons of the genus 'scientist' with the average, and the species 'researcher' with other specialists, of the essential divergence of the species 'researcher' from the average man. For example, though on the unstable side ($C-$) relative to other professionals in science he is yet decidedly above general, average stability. And though no more desurgent than other specialists he is yet emphatically below the general adult level of surgency.

Table 6. Profile of the Existing Typical Researcher (in General Population Standard Scores)

Factor		
A	Cyclothymia *v.* schizothymia	3·34
B	Intelligence	8·65
C	Stability	6·89
E	Dominance	7·24
F	Surgency	3·50
G	Super-ego, group standards	3·40
H	Adventurous cyclothymia	6·44
I	Sensitive emotionality	7·1
L	Paranoia	6·92
M	Bohemianism	5·78
N	—	5·50
O	Free-floating anxiety	3·66
Q_1	Radicalism	6·20
Q_2	Self-sufficiency	6·52
Q_3	Super-ego, will control	6·78
Q_4	Psychosomatic anxiety	5·11

As in other I.P.A.T. tests the 16 P.F. Standard Score is arranged in *Stens* not Stanines, i.e. each point is ½ a sigma and the population mean is 5·5, so that 1 and 10 are equidistant from the mean and 5·0 to 5·99 is essentially an average score.

Now the use of the pattern similarity coefficient, r_p (Cattell, 1949; Cronbach and Gleser, 1953) to determine the congruence of various propositi to the above profiles, when selecting promising men for research openings, involves certain assumptions which should be watched. First it gives equal weight to the various factors and secondly it assumes that the values for the existing researcher are the best values. While the first is the best guess with our present knowledge, and the second must be accepted unless we can claim to be wiser than society's laws of supply and demand, yet we can hope to find out a little more. Just as the first step in vocational selection research is to define the type profile, so the second is to determine the linear or curvilinear relation of each factor to a criterion of success in a population *within* the occupation . . . .

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Personality Factors Related to Creativity in Young Scientists

Dorothy Semenow Garwood

The purpose of this investigation was to test some predicted relationships between creativity in young scientists and personality factors. These factors may be classified in three groups: (*a*) characteristics of intellectual processes, interests, interpersonal relationships, and intrapersonal relationships; (*b*) integration of nonconscious with conscious material; and (*c*) sexual identifications. While there exists a growing body of empirical evidence bearing on

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the validity of the hypotheses in Group *a* (Garwood, 1961; Stein and Heinze, 1960), empirical support for the predictions in Groups *b* and *c* has heretofore been lacking. This latter situation is particularly noteworthy with regard to Group *b* hypotheses in view of the fact that a recent excellent review of the literature on creativity (Stein and Heinze, 1960) cited over 50 theoretical and/or speculative publications in which a crucial role in creativity was attributed to nonconscious factors.

METHOD

Creativity Criterion

The creativity criterion employed was based on psychometric scores and *not* on behavioral ratings or productivity indices. The criterion consisted of a composite score on some measures of factors described by Guilford and co-workers (Guilford, 1959; Guilford and Merrifield, 1960; Guilford, Wilson, and Christensen, 1952; Guilford, Wilson, Christensen, and Lewis, 1951) as within the area of creative performance: namely, flexibility, fluency, originality, and redefinition.

Previous empirical validity studies of these measures which dealt with samples of scientists and/or employed particularly comprehensive creativity criterion ratings are germane to the validity of the creativity criterion of the present research. These validity investigations yielded both positive (Barron, 1955, 1956; Chorness and Nottelman, 1957; Gough, 1961; Hall, 1958; Maltzman, 1960; Zaccaria, Chorness, Gerry, and Borg, 1958) and negative (Gough, 1961) results. A detailed discussion of the validity literature has been presented elsewhere (Garwood, 1961).

The test measures described below were selected as the best available for differentiation of the Higher Creativity (HC) and Lower Creativity (LC) groups for this research.

The five tests were administered in the order given below to groups of 7–33 subjects by a single examiner who used identical instructions for each group. Exact time intervals were allowed for all tests except the final Scientific Word Association Test. Subjects were requested not to discuss the tests with anyone.

Alternate Uses (Wilson, Christensen, Merrifield, & Guilford, 1960). The subject is asked to list uses other than the primary one for common objects. The test was scored for the total number of feasible uses (AU-T) and for the number of original responses (AU-Or) where the latter was defined as a feasible use that occurred as a response to a given stimulus only once in the sample ($N = 105$) studied.

Match Problems III (Marks, Guilford, & Merrifield, 1959). The subject is presented with drawings of squares made up of matches; he is instructed to remove a given number of matches to leave a given number of squares. The test was scored for the total number of correct solutions (MP-T).

Gestalt Transformations (Guilford et al., 1952). The subject is asked which of five listed objects has a part that will serve a specified purpose. The test was scored for a total number of correct solutions (GT-T).

Consequences (Christensen, Merrifield, & Guilford, 1958). This test calls upon the subject to consider what would happen if certain changes were to occur and to list as many such consequences or results as possible. The test was scored for the total number of relevant consequences (C-T) and the number of remote responses (C-R) where the latter was defined as a consequence more distant, temporally or geographically, than an immediate one; responses which were fairly specific ways of adjusting to the changed situation were scored as remote. There was complete agreement between two raters who independently scored 100 randomly selected Consequences responses.

Scientific Word Association (Gough, Hurlbut, & Woodworth, 1957). The short form used was one in which the subject is asked to write down his first association to each of 50 scientific words; the original test contains 100 words. The test was scored for originality (SWA-Or) defined as $A + 2B$, where A was the number of responses occurring with a frequency of 1% or less for a given stimulus word in the sample studied and B was the number of responses of 2–9% frequency in this sample.

Creativity criterion score. The criterion was a composite test battery score computed for each subject by summing the seven Z scores: (Alternate Uses-Total) + (Alternate Uses-Originality) + (Match Problems-Total) + (Gestalt Transformation-Total) + (Consequences-Total) + (Consequences-Remote) + (Scientific Word Association-Originality). This method of calculating the composite scores weighted most heavily those open-ended tests requiring bright ideas or imaginative responses, namely, Alternate Uses and

Consequences. It is noteworthy, however, that a dozen variations in weighting factors of the composite formula yielded essentially the same composition for the top 20% (HC) and bottom 20% (LC) of subjects.

Table 1 is an intercorrelation (Pearson product moment) matrix among the seven criterion component test scores and the composite (sum of the seven components) criterion score; means and sigmas for the components and composite are included. These correlation data are in reasonable accord with one model of originality or creativity as a multi-factorial dimension (Barron, 1956).

Population and Samples

The population of young scientists sampled was one which might reasonably be expected to exhibit a high degree of creativity relative to the general population. Larger personality differences would be expected between groups more different in creativity than the HC and LC groups of this investigation.

All potential participants from three Southern California academic institutions were informed by letter about the general nature of the tests to be taken, that they would be paid, and that anonymity of test protocols would be strictly maintained. Data concerning ages, academic levels, and fields of the 105 young male science majors who completed the creativity criterion test battery are summarized in Table 2.

The 105 subjects were ranked in order of their composite test battery scores. The higher creativity (HC) and lower creativity (LC) groups of potential subjects for the testing of hypotheses were the top 20 and bottom 24 scorers, respectively. In this selection 2 subjects were omitted because their ages were more than four times the standard deviation above the mean age of the entire sample. Of the 44 persons asked to participate in the testing-of-predictors phase of the research, 41 agreed to do so. It was possible to arrange

Table 1. Intercorrelation Matrix among Creativity Criterion Components and Criterion

Test score	*AU-T*	*AU-Or*	*MP-T*	*GT-T*	*C-T*	*C-R*	*SWA-Or*	*Composite*
AU-T		.44†	.13	.38†	.56†	.67†	—.06	.74†
Au-Or			.23*	.10	.25†	.73†	—.09	.56†
MP-T				.19*	.29†	.18	—.25†	.49†
GT-T					.13	.26†	—.17	.45†
C-T						.75†	—.01	.78†
C-R							—.05	.67†
SWA-Or								.10
Composite								
M[a]	26.0	2.3	9.5	13.8	54.5	23.5	54.7	0.0
SD[a]	6.7	2.0	4.3	2.5	15.7	8.0	5.6	3.8

Note.—AU-T = Alternate Uses-Total, AU-Or = Alternate Uses-Originality, MP-T = Match Problems-Total, GT-T = Gestalt Transformations-Total, C-T = Consequences-Total, C-R = Consequences-Remote, SWA-Or = Scientific Word Association-Originality, Composite = Composite sum of battery Z scores.

[a] All means and standard deviations are for raw score data except for Composite which are statistics for Z scores.

* $p \leq .01$.

† $p \leq .01$.

Table 2. Characteristics of Sample that Completed Creativity Criterion Test Battery

			Academic field				
Status	*N*	*M age*	*Biology*	*Chemistry*	*Geology*	*Physics*	*Premedical*
Postdoctoral research fellow	7	35.0	4	3	0	0	0
Graduate school student	24	25.2	4	5	5	10	0
Undergraduate college student	74	20.5	4	24	2	36	8
Overall sample	105	22.5	12	32	7	46	8

testing times and obtain protocols for 36 of these subjects; 18 of these were in the HC and 18 in the LC group. The HC and LC groups were well differentiated on the creativity criterion measure ($t = 17.56$, $p < .001$). The two groups did not differ significantly in composition as regards academic level, academic field, or mean age.

Predictors

The full series of hypotheses has been advanced elsewhere (Garwood, 1961). Included here is a list of test predictors and brief statements of their relevance. Higher scores for the HC than for the LC groups were predicted for all tests unless otherwise specified.

Characteristics of intellectual processes, interests, interpersonal relationships, and intrapersonal relationships. Partly as a check on the criterion differentiation of the HC and LC groups, Test 1 was included. It is a composite measure of personality factors which have been shown to predispose toward originality (Gough, 1957b); the factors are intellectual competence (Test 3b), inquiringness as a habit of mind, cognitive flexibility (Test 2b), esthetic sensitivity, and sense of destiny (Test 9b).

> Test 1: Originality-Total (O-T) scale of the California Psychological Inventory (CPI; Gough, 1956, 1961[1]); this is a shortened form of the corresponding scale of the Differential Reaction Schedule (DRS; Gough, 1955).

Tests 2a and 2b were included to measure the ability to desert old ways and adopt new directions as is required for creativity.

> Test 2a: Flexibility (Fx) scale of the CPI (Gough, 1956, 1557a).
>
> Test 2b: Cognitive Flexibility (Cf) scale of the CPI (Gough, 1961[1]); this is a shortened form of the corresponding scale of the DRS.

The idea that clarity and efficiency of thought is often related to creativity led to the use of Tests 3a and 3b.

> Test 3a: Intellectual Efficiency (Ie) scale of the CPI.
>
> Test 3b: Intellectual Competence (Ic) scale of the CPI (Gough, 1961[1]); this is a shortened form of the corresponding scale of the DRS.

Inclusion of Test 4 was based on the reasonable expectation that persons of high creativity seek an area in which to exercise their gifts at an early age.

> Test 4: The item (Age interest science—Ais), "Age of first marked interest in science as evidenced by hobbies or preference for school science subjects." Predicted: ages for HC group *lower* than ages for LC group.

Since creativity is so regularly associated with the personal mastery of some situation or problem, measures of ascendancy, poise, and self-assurance were included as measured by Tests 5–9.

> Test 5a: Dominance (Do) scale of the CPI.
>
> Test 5b: Standard Dom scale (Dom = dominance minus submission) of the Interpersonal Check List (ICL; Leary, 1956a, 1956b).
>
> Test 6: Capacity for Status (Cs) scale of the CPI.
>
> Test 7: Sociability (Sy) scale of the CPI.
>
> Test 8: Social Presence (Sp) scale of the CPI.
>
> Test 9a: Self-Acceptance (Sa) scale of the CPI.
>
> Test 9b: Sense of Destiny (Sd) scale of the CPI (Gough, 1961[1]); this is a shortened form of the corresponding scale of the DRS.

The frequent association of creativity with departure from the usual led to the inclusion of Tests 10–12, measures of social conformity, and Test 13, a measure which is related to conformity as expressed in affectional relationships. Predicted for Tests 10–13: scores for HC group *lower* than scores for LC group.

> Test 10: Socialization (So) scale of the CPI.
>
> Test 11: Self-Control (Sc) scale of the CPI; *low* scorers tend to be excitable, impatient, self-centered, and uninhibited.
>
> Test 12: Good Impression (Gi) scale of the CPI.
>
> Test 13: Standard Lov scale (Lov = love minus hostility) of the ICL (Leary, 1956a, 1956b).

Integration of nonconscious with conscious material. The idea that conscious effort

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[1] H. Gough, personal communication, February 1961.

alone cannot produce creative achievements and that integrative use of nonconscious material is important (Stein & Heinze, 1960) suggested inclusion of Tests 14a, 14b, and 14c. These tests were designed to measure the integration of nonconscious with conscious concepts for each of: self, 14a; father, 14b; and mother, 14c. Predicted for Tests 14a, 14b, and 14c: "psychological distance" *d* scores for HC group *lower* than (*d*) scores for LC group.

Test 14a: "Psychological distance" (*d*) between ICL (Leary, 1956a) self (conscious) and Thematic Apperception Test (TAT; Morgan and Murray, 1935; Murray, 1943) "hero" (nonconscious self) as determined by an "interpersonal system" analysis (Leary, 1956b, 1957); the smaller the *d*, the greater the integration.

Test 14b: Analogous to Test 14a applied to ICL father (conscious) and TAT "male-other" (nonconscious father).

Test 14c: Analogous to Test 14a applied to ICL mother (conscious) and TAT "female-other" (nonconscious mother).

The validity of the operational definitions employed in Tests 14a, 14b, and 14c rests on the validity of two underlying assumptions: the identification assumption—namely, that (*a*) the TAT "hero" represents self, (*b*) TAT male-other represents father, and (*c*) TAT female-other represents mother; and the subject's statements about the TAT characters represent his *nonconscious* feelings or attitudes about them. Evidence in support of these assumptions is cited elsewhere (Garwood, 1961).

Sexual identifications. Tests 15–18 deal with conscious and nonconscious sexual identifications. *Lower d* scores (greater identification) were predicted for the HC group than for the LC group on Test 15 (conscious father identification) and 16 (nonconscious feminine or mother identification). These predictions were based on the ostensibly male character of scientific creativity in our culture (Test 15) and the suggested importance of "anima" (nonconscious feminine aspects) integration for male creativity (Jacobi, 1951; Jung, 1916) (Test 16).

Test 15: Interpersonal system distance (*d*) (Leary, 1956b, 1957) between ICL self and ICL father.

Test 16: Interpersonal system distance (*d*) between TAT hero and TAT female-other.

Directions of differences for Tests 17 and 18 which measure conscious mother identification and nonconscious masculine or father identification, respectively, were not predicted.

Test 17: Interpersonal system distance (*d*) between ICL self and ICL mother.

Test 18: Interpersonal system distance (*d*) between TAT hero and TAT male-other.

Experimental Details

The tests were administered to groups of one to nine subjects in the following order: brief history, TAT, ICL (for self, father, and mother with the order randomized), and CPI. All subjects were requested not to discuss the tests with any person.

The standard test procedures were used except that subjects followed written instructions in writing their own TAT stories. The TAT cards included were those suggested by Leary (1956b, 1957) for use with his "interpersonal system": 1, 2, 3BM, 4, 6BM, 6GF, 7BM, 12M, 13MF, and 18BM.

Since scoring the TAT by the Leary (1956b, 1957) scheme depends on rater judgment, inter-rater reliability was computed. Two raters independently scored the stories (100) for 10 subjects. Of 167 scores attributed to "hero" figures by either or both raters, agreement was obtained for 165 (97.6%) of these. For 62 male-other scores agreement was 97% as it also was for 60 female-other scores.

Raters were not aware of the group membership (HC or LC) of the protocol authors at the time of scoring.

RESULTS

The results of those tests which significantly differentiated the HC and LC groups are summarized in Table 3. The appropriate one-tailed *t* tests were employed for all cases except Tests 17 and 18 where two-tailed *t* tests were used. In the case of Tests 14a, 14b, and 14c an analysis of variance of all three test measures as summarized in Table 4 confirmed the prediction at $p < .01$. The insignificant interaction between groups and tests indicated that the difference between the groups (HC and LC) did not depend significantly upon which test was used. The difference between the tests was also significant ($p < .01$).

Table 3. Tests of Hypotheses

Hypothesis test	*HC* (*N* = *18*) *M*	*HC* (*N* = *18*) *SD*	*LC* (*N* = *18*) *M*	*LC* (*N* = *18*) *SD*	*t*[a]	*p*[b]
Composite personality originality						
1. Originality (CPI, O-T scale)	40.4	5.2	36.6	5.9	2.07	.020
Characteristics of intellectual processes and interests						
2b. Cognitive flexibility (CPI, Cf scale)	9.3	1.4	7.8	1.9	2.70	.004
4. Age interest science (Ais)	10.1	3.8	12.6	3.5	−1.90	.029
Ascendancy, poise, self-assurance						
5a. Dominance (CPI, Do scale)	31.9	5.8	27.4	6.7	2.17	.015
5b. Dominance (ICL, Dom scale)	29.9	6.7	53.8	8.3	2.44	.008
7. Sociability (CPI, Sy scale)	28.1	3.8	24.4	5.2	2.42	.008
8. Social presence (CPI, Sp scale)	42.7	4.1	37.4	5.6	3.19	.001
9a. Self-acceptance (CPI, Sa scale)	24.3	2.7	21.2	3.3	3.04	.002
9b. Sense of destiny (CPI, Sd scale)	7.1	2.7	5.6	2.8	1.66	.049
Conformity						
10. Socialization (CPI, So scale)	34.6	5.7	37.8	4.5	−1.88	.031
11. Self-control (CPI, Sc scale)	25.3	5.9	29.1	6.8	−1.76	.040
12. Good impression (CPI, Gi scale)	14.8	5.6	19.2	5.8	−2.31	.011
13. Affection (ICL, Lov scale)	42.5	9.5	47.6	6.1	−1.90	.029
Integration of nonconscious with conscious material						
14a. Self (*d*, ICL-TAT)	41.7	27.9	59.7	25.3	−1.77	.039
14b. Father (*d*, ICL-TAT)	34.8	26.9	49.6	17.7	−1.99	.024[c]
Sexual identifications						
17. Self-mother (*d*, ICL)	59.2	31.2	36.8	32.2	2.12	.034[d]

[a] Numerator term = $M_{HC} - M_{LC}$.

[b] All one-tailed (as justified by hypothesis statements) unless otherwise specified.

[c] The corresponding *p* value for Test 14c (Mother, *d*, ICL-TAT) fell short of significance ($p = .092$).

[d] Two-tailed.

Table 4. Analysis of Variance Results of HC and LC Groups Tested with Three Measures (14a, 14b, 14c) of Hypothesis 14

Source	*df*	*MS*	*F*
Between HC and LC groups (A)	1	5,778.7	8.91[a][c]
Between subjects in same group (B)	34	648.4	
Between tests (14a, 14b, 14c) (C)	2	3,906.8	5.49[b][c]
C × A	2	127.1	.18[b]
B × C	68	711.7	

[a] Error term is *MS* between subjects in same group.

[b] Error term is *MS* for Interaction: B × C.

[c] $\leqq .01$.

DISCUSSION

Characteristics of Intellectual Processes, Interests, Interpersonal Relationships, and Intrapersonal Relationships

Comparison of young scientists of higher and lower creativity showed that the HC subjects scored higher on a composite measure of personality factors which predispose toward originality.

Assessment of the characteristics of intellectual processes gave conflicting results as regards flexibility. The Cf scale of the CPI showed the HC group to be more flexible, but the Fx scale failed to differentiate the HC and LC groups. The reason for this disagreement is not clear; it may be related to the broader scope of the Fx as compared with the Cf scale. However, scores on the Fx scale of the CPI were previously found to be positively related to creativity or originality for mathematicians (Helson, 1961), architects (MacKinnon, 1961), and medical school applicants (Gough, 1957b). Although findings of the present research fell short of confirming the prediction that HC subjects are higher on intellectual efficiency (Test 3a, $p = .055$; Test 3b, $p = .224$), the results of previous studies on scientists (Roe, 1951a, 1951b) and medical school applicants (Gough, 1957b) did support this hypothesis. As predicted, the HC group first became interested in science at an earlier age than the LC group.

The HC subjects exceeded their LC counterparts significantly on all measures of ascendancy, poise, and self-assurance except one (Cs scale, Test 6, $t = 1.48$, $p = .070$); they scored higher on dominance, sociability, social presence, and self-acceptance.

The HC group was found to be less conforming than the LC group as expressed by their significantly lower scores on socialization, self-control, desire to make a good impression, and affection.

The fact that many of these positive findings are similar to those of previous studies (where behavioral criteria were employed) on a variety of occupational groups suggests that these personality factors may be rather generally more predominant in males of higher creativity, whatever their vocations. A more detailed comparison of the present and earlier results has been presented elsewhere (Garwood, 1961).

Integration of Nonconscious with Conscious Material

Within this author's knowledge, these data provide the first clearly empirical evidence for the association of higher creativity with a greater integration of nonconscious with conscious concepts. This finding may be interpreted within the theoretical framework originated by Freud (1953, 1959) and ably developed by others such as Barron (1958). The highly creative individual obtains satisfaction from the integration of initially disordered and complex phenomena. He thus tends to admit into consciousness disordered, irrational, nonconscious material, which is ordinarily repressed, in order to achieve the satisfaction of integrating it creatively in a complex personal synthesis.

A possible implication of the interpretation advanced is that procedures, such as "insight-oriented" psychotherapies, which tend both to loosen repressions and to facilitate integration of the previously repressed material often will, if successful, augment creativity.

In addition to the differences in degree of integration of nonconscious material between the HC and LC groups, it was found that the extent of integration for both groups depended on the concept specified. Thus, the agreement between conscious and nonconscious concepts for both HC and LC groups was found to be, in order of decreasing agreement: father, self, and mother. This finding of greater conflict over the mother concept is of particular interest in relation to previous suggestions (e.g., Grotjahn, 1957) that the origins of artistic creativity lie in the artist's internalized conflict over the vicissitudes of the earliest infant-mother relationship.

Sexual Identifications

The HC subjects showed significantly less conscious identification with mother than did the LC subjects, but fell slightly short of exceeding the LC subjects significantly in conscious father identification ($p = .055$).

The HC and LC groups were found to be not significantly different in nonconscious feminine (HC $>$ LC, $p = .121$) or nonconscious masculine identification.

Limitations of Investigation

A general limitation of this investigation is that the creativity criterion was restricted to performance on a test battery; no external or "natural" behavioral criterion was employed. Although some of the results obtained for the HC and LC groups so chosen are similar to those obtained in other studies where behavioral criteria were employed, replication using an external criterion is desirable. Similar considerations apply to the relatively small samples employed (36 selected as extreme representatives of a population of 105) from a single geographic area.

It should be noted, also, that in view of the large variability of the integration (psychological distance, *d*) scores within each of the two groups (HC and LC) on Tests 14a, 14b, and 14c and the rather limited number of scores on which the male-other and female-other TAT totals were based (an average of 6 scores per subject for each of male-other and female-other as compared with an average of 17 scores per subject for TAT hero), further work in this area is particularly desirable.

Overall cross-validation (as yet unavailable) is, of course, indicated.

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On the Psychodynamics of Creative Physical Scientists

David C. McClelland

The persistent curiosity of the creative physical scientist presents a challenge to psychologists interested in human motivation. Why is it that some men spend their entire lives in an unceasing effort to penetrate the secrets of the universe? Certainly they must have brains and a good store of scientific knowledge to draw on, but so do many people in our day and age. Why are a few called to intense devotion to such a task? What turns them first to natural science? Is it a single common factor or several factors operating differently on different individuals? The path of least resistance is to believe that a series of "fortunate accidents," different for each case, turns a man toward science, but the psychologist is after all a scientist too; he is not likely to rest content until he has exhausted all possibilities of finding a common explanation for a common effect —namely, scientific curiosity. While there are, of course, many other fields of creative endeavor, the paper will be restricted largely to the motivational makings of physical scientists.

By now the psychologist has conducted an impressive number of investigations into the characteristics of physical scientists. . . . What are the characteristics of physical scientists that have been repeatedly confirmed in these various studies? Before listing them, it is well to be clear at the beginning just whom we are describing. . . . The studies . . . have used as subjects very different orders of "scientists"— all the way from those who had won the Nobel Prize to undergraduate majors in science. So it will be difficult, if not dangerous, to make generalizations that apply to all groups with equal force. One way of minimizing the difficulty is to try to select only those characteristics which are so striking that they apply (with variations, of course) to all scientifically oriented subjects but in greater degree to those who are more creative or more eminent. Another way of minimizing the danger of too-sweeping generalizations is to focus on experimental physical science—in particular on physics and chemistry. Theoretical physics and mathematics shade off in one direction from such a focus and the biological sciences in

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another, so that any statements made need not apply as fully to scientists in these areas. With these guidelines in mind, the following generalizations would appear to summarize fairly well the characteristics of physical scientists as they have been uncovered by investigations up to the present.

1. *Men are more likely to be creative scientists than women.* There are no women among Anne Roe's (1951, 1953b) eminent scientists, and very few in *American Men of Science.* No fact is more obvious than the differential yield for science of the two sexes, though it is saved from being trivial only by the further fact that women have not flocked to experimental physical science in increasing numbers as opportunities for higher education for women have been more nearly equalized. In other words, it may not be a social factor—lack of opportunity for women in science—but rather a personality factor—lack of interest in physical science among women—which accounts for the small number of female physical scientists.

2. *Experimental physical scientists come from a background of radical Protestantism more often than would be expected by chance, but are not themselves religious.* Historically modern science developed in close association with Puritanism. Merton (1949) has described the "point-to-point correlation" between "the principles of Puritanism and the attributes, goals, and results of science," and has shown that the original membership of the Royal Society of London was disproportionately Puritan in background in 1663. Knapp and Goodrich (1952) found that between World Wars I and II American scientists were graduated disproportionately more often from small Protestant colleges, particularly during the period in history when colleges were breaking away from religious orthodoxy. Roe's eminent scientists (1953b) included more individuals than would be expected from radical Protestant backgrounds (e.g., Quaker, Mormon), although her frequencies are not large enough to be very reliable. She further found that her scientists were not personally religious, a fact supported in Terman's (1954) report that his scientists were less interested in religion than any of his other groups of comparable intellectual ability. In other words, scientists appear to come more often from a radical (thoroughgoing or strict) Protestant background and to reject it for science as a "way of life."

3. *Scientists avoid interpersonal contact.* They are less gregarious, more autonomous, prefer working with things to working with people. Evidence for this generalization comes from many sources. McClelland (1956) reports that of the ninety items which consistently differentiated scientists from nonscientists on the Strong Vocational Interest Blank, forty-seven could be classified as having to do with avoiding interpersonal contact. For example, scientists prefer being a lighthouse keeper to being a headwaiter, gardening to house-to-house convassing, dealing with things to dealing with people. They would dislike significantly more than men in general being a lawyer, a politician, a reporter, a social worker, or a traveling salesman. They also dislike dramatics, public speaking, interviewing prospective customers, or bargaining. Cattell and Drevdahl (1955) report very similar findings also based on objective personality tests. Their research physicists are significantly high on Factor A (schizothymia) and Factor Q2 (self-sufficiency) as contrasted with a college group or with other professional groups. The items which make up these factors are very similar to the ones just mentioned (Cattell, 1957). Furthermore, the unsociability of scientists appears as early as age ten according to Terman's (1954) data. He found his future scientists rated lowest in sociability score based on a play and games test at age ten. Finally, significantly more of Stein's (1956) creative chemists are either high or low in *n*Affiliation (82%), as compared with his uncreative chemists (48%), when *n*Affiliation is measured by coding TAT's according to the technique initially developed by Shipley and Veroff (1952). Clinical data suggest that being either high or low on *n*Affiliation leads a person to avoid interpersonal contact, in the first instance because the person is "super-sensitive" to others, and in the second because he simply is not interested in others. What leads to normal gregariousness is the moderate amount of *n*Affiliation which characterized over half of Stein's uncreative chemists, and less than one-fifth of his creative ones. Outstanding scientists are anything but normally gregarious. They like being self-sufficient and like being alone,

probably because people and human relations seem both difficult and uninteresting to them.

4. *Creative scientists are unusually hard-working to the extent of appearing almost obsessed with their work.* Roe (1951, 1953a, 1953b) in reporting on her eminent scientists remarks that the *one* characteristic all of them seemed to have had without exception is an intense devotion to their work (cf. also Kubie, 1953). There was never a question of putting in so many hours a day, a week, or a year. Instead they worked nights, weekends, holidays, all the time. In fact she wondered how they ever found time to be with their wives and families. Terman (1954) also reports that his physical scientists stated more often than any others that their work itself gave them the greatest satisfaction in life. A motive which is known to produce hard striving under certain conditions is *n*Achievement. Perhaps scientists are as a group especially high in the need for Achievement, as I have predicted elsewhere (McClelland, 1956). It gives me considerable satisfaction to report that the data do not support such a logical inference. My apparently quixotic pleasure derives from the fact that my own extensive work on *n*Achievement may have led some of my critics and friends alike to assume that I believe *n*Achievement is the root cause or motive force behind *all* energetic, successful behavior, or in simpler terms that all achievement is produced by a high *n*Achievement. If this were so—if overt behavior bore a one-to-one relationship to inner motivational determinants—it would obviously be more parsimonious to do without the concept of motivation altogether. So it actually underlines the need for a construct of motivation to discover a case where high achievement must be attributed to some motive other than *n*Achievement, as it is usually measured in fantasy.

The facts are not as extensive as they ought to be, but so far as they go they do not point to *n*Achievement as a crucial factor in the persistent striving of scientists. Figures comparing scientists and controls exist for stories told about two pictures from the Murray TAT: card #1 (boy with violin) and Card #7 ("father and son"). Out of sixty-four stories told about the two pictures by thirty-two normal college students, 52% contained achievement imagery, whereas only 28% of the stories told by Stein's forty-five industrial chemists contained achievement imagery, and 47% of the stories told by thirty-nine of Roe's eminent scientists contained achievement imagery. A further analysis of the full *n*Achievement scores for Stein's chemists (based on TAT cards #1, #2, #6, #7, and two specially designed cards) demonstrates that the more creatives ones average *lower* than the less creative ones, but are significantly more often in the middle of the *n*Achievement score distribution. Apparently only a *moderate* amount of *n*Achievement favors success in industrial chemistry. The reason may be that too high *n*Achievement leads to considerable frustration in research because positive results are not obtained often enough and with sufficient regularity to please the person with high *n*Achievement, whereas very low *n*Achievement may simply lead to laziness. There is also the hint that while scientists in general are lower than the normal college population in *n*Achievement, the especially productive eminent scientists are somewhat higher than their colleagues. Still, even the eminent ones do not exceed the normal college population in *n*Achievement, and the explanation of their high achievement must be sought in some other motivational system.

5. *Scientists avoid and are disturbed by complex human emotions, perhaps particularly interpersonal aggression.* By its very nature science as an occupation glorifies objectivity, dispassionateness, or the impersonal search for truth. While personal biases and feelings have sometimes crept into scientific work, ideally they have no place in it. For most scientists avoidance of human emotion is far more than simply an ideal as far as their profession is concerned; it runs as a theme through much of their thinking in other areas of life. Knapp (1956) in particular found that science majors tell TAT stories significantly low in dramatic salience, in aggression, guilt or vindication, and in the tendency to bring the plot to a clear and decisive conclusion. Teevan (1954), reporting on the same subjects, found them the lowest on all variables on the Blacky Test, technically indicating that science majors are least disturbed of all students in psychosexual development, but more probably suggesting that they are unable to enter into the spirit of the test, which involves identifying with a

little black dog. Instead they simply give bland, unemotional, objective responses which are hard to score for any disturbance. These findings might be discounted, since they deal only with undergraduate science majors, except that Roe (1951) reports very similar results based on the TAT's of her eminent scientists. . . . Veroff and others (1957), in a factor analysis of Thorndike's ratings of eminent men have also found that scientists differ from others chiefly in being significantly lower on *Choler* (Anger, Dominance, and Liking for Conflict). To oversimplify a little by way of summary: *scientists react emotionally to human emotions and try to avoid them.*

6. *Physical scientists like music and dislike art and poetry.* On the Strong Vocational Interest Blank they say more than others do that they would dislike being a poet or decorating a room with flowers, and that they like symphony concerts. Terman (1954) reports that liking for art is least common in his physical science groups and that interest in music among the physical science research group rises throughout life until it is highest of all by age forty. The attitude toward modern art is perhaps best expressed by the response of one physicist to a TAT picture when he said: "It's confusing enough to win a prize."

7. *Physical scientists are intensely masculine.* On all interest and attitude scales that differentiate between men and women, physical scientists score very high on masculinity. For example, Terman (1954) found 64% of his physical science research group score above the standard score of fifty on masculinity for the Strong Vocational Interest Blank. Only the engineers and those whose undergraduate major was science score higher, whereas lawyers, social scientists, and those majoring in the humanities score much lower. In general his scientists show a liking for nature and for outdoor sports when they are young, and for working with things rather than with people—all of which are typically male as contrasted with female interests.

Another aspect of their masculinity appears as a positive image of the father figure in the TAT stories combined with little or no rebellion or guilt over rebellion in the father-son relationship. Though difficulty is often present in the mother-son relationship, Roe's eminent scientists portray the father as a benign and understanding influence. The following comments from stories to TAT card #7 are typical: "Father, though, is an understanding person and definitely makes a reconciliation"; "His father gave him wise counsel and took a less serious view of the situation." It is perhaps significant that while about one-half of Roe's cases picture the father in this way, none, or at the most one, of Stein's uncreative chemists give a similar response. While the relationship to the father seems to be respectful, it is also somewhat distant, as Terman's (1954) figures show. His research scientists report little oversolicitousness from their fathers, and also the *least* amount of affection and understanding between father and son. The science majors who did not continue in a scientific occupation report the *most* affection between father and son, a fact to which we must return later since in practically all other respects these two groups are very similar.

It is perhaps also worth reporting here that Stein's creative chemists show more *n*Achievement (mean = .91) in response to TAT card #7—the father-son picture—than do the uncreative chemists (mean = .61). The less creative chemists on the other hand show markedly more *n*Achivement to TAT card #2—the farm scene—where their mean *n*Achievement score is 1.09, as contrasted to the −.05 mean score for the more creative chemists. To put the same result in a different fashion, 56% of the more creative as contrasted with 22% of the less creative chemists gave more *n*Achievement to card #7 than to card #2 ($\chi^2 = 3.97, p < .05$).

Since the figures in card #7 are male and in card #2 predominantly female, apparently *n*Achievement in more creative chemists is elicited by male figures, and in less creative chemists by female figures.

8. *Physical scientists develop a strong interest in analysis, in the structure of things, early in life.* One of the most striking things about the case histories of scientists is the early age at which their scientific interest appears. It is not just mathematicians—who are often child prodigies—but all kinds of natural scientists who typically develop a strong scientific interest between the ages of five and ten. Terman's (1954) results show that his future scientists had a scientific interest by the age of ten that was easily recognized as dominant

by the boys themselves, by their teachers, and by their parents.

That the nature of this interest is analytic is almost self-evident from the goal of physical science; this goal, in the words of one of Roe's theoretical physicists, is to discover the "connections of things," and to get at the "inner secrets of the world." That is, it is the scientist's job to take apart the real world as we perceive it and to discover what lies behind it, to work out the microstructure of reality. . . .

To . . . a psychologist interested in psychodynamics, . . . certain conditions are necessary but not sufficient for the making of a scientist. Chief among these are, of course, a high level of intelligence and opportunity for contact with scientific knowledge through some system of education. Granted these preconditions, why is it that some boys develop a strong scientific interest and others do not? The fact that the interest develops so strongly early in life suggests that the key to the problem may lie in the family, because it is the main educational influence at this period in life.

In thinking of family relationships as a source of motivation, one is of course immediately reminded of Freud's insistence on the central role of the Oedipus complex. What could have happened to the little boy to turn him so strongly to the male role and away from contacts with people? One answer, in terms of the "passing" of the Oedipus complex, is obvious. The future scientist is simply a boy who resolves his guilt over love of his mother and hatred of his father by early and complete identification with his father, probably in the phallic period.

Such an explanation does a pretty good job of accounting for the facts just enumerated. It explains the strong masculine identification and the fact that scientists are more often men than women—because women are not troubled by the Oedipus complex. It could easily account for the avoidance of people, the dislike of human emotions, and the distaste for art and poetry—all because they are connected with the acute anxiety aroused by the boy's first important interpersonal relationship with his mother. That is, in psychoanalytic terms, one can assume that for the first three or four years of his life the future scientist, like most boys, develops an intense love relationship with his mother which produces acute anxiety arising simultaneously from the fear of the strength of his own impulses, from guilt over hatred of the loved father, and from fear of retaliation by the father. Normally a boy is supposed to defend himself against his anxiety by repression and identification with the father. Perhaps the future scientist differs in that he adopts the defense slightly earlier—in the phallic rather than the genital period—so that all his "symptoms" are more extreme than those of a normal boy. He is particularly marked by a tendency to avoid any cue associated with interpersonal relationships which may rearouse the original anxiety. So the scientist dislikes interpersonal contacts, human emotion, and even art and poetry, which frequently deal with human emotions. Finally he is analytic and hard-working because his sexual drive has been repressed earlier than usual and finds its substitute outlets in intellectual curiosity—or more specifically in "looking" and seeking to "penetrate the secrets of nature" which in classical psychoanalytic terms are pregenital, especially phallic, sexual activities. Freud (1916) traced Leonardo da Vinci's compelling scientific curiosity to the same libidinal source. In other words, the scientist does not mature fully, as far as the sexual instinct is concerned, because of the early passing of the Oedipus complex; instead he continues to find a good deal of his sexual satisfaction in phallic activities. He remains fixated somewhat at the level of "looking and knowing," as Kubie (1953) has indirectly suggested. In short, perhaps scientific drive and curiosity derive their energy from a slight perversion of the sexual instinct. And it is a fact, as Anne Roe reports (1953b), that young scientists are typically not very interested in girls, date for the first time late in college, marry the first girl they date, and thereafter appear to show a rather low level of heterosexual drive. . . .

The general picture is one of distance between mother and son rather than of rebellion and conflict.[1] In the physical science research group, the same distance appears between father and son, so that it may simply reflect the already-noted tendency of scientists to report

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[1] EDITORS' NOTE: For illustrative material presented by the author, the reader is referred to the original article.

themselves as self-sufficient. In Terman's (1954) data the scientists in general report low admiration for the mother, little rebelliousness toward her, and little effort by her to resist their efforts at independence.

However, work by Greenberger undertaken in my laboratory has suggested that the sexual problem is not necessarily the central one in the scientist's relationship to people, since there is no special evidence that nature is conceived in feminine sexualized terms.[2]

If the scientist's difficulty in human relations does not lie in the sexual sphere, where does it lie? Maybe it is different for each scientist. Need we assume more than that each scientist has had some particular kind of trauma in his interpersonal relations early in life which has turned him away from people toward nature? Perhaps not, but such a pluralistic explanation runs squarely into two facts: men are more likely to become scientists than women, and boys brought up in a radical Protestant background are more likely to become scientists than other boys. If becoming a scientist is strictly a matter of miscellaneous traumas, why should these not occur as often in one population group as another?

Putting the question in these terms suggests another possibility. There is another problem peculiar to the male sex, and perhaps particularly so to males in radical Protestant households: the problem of aggression. A key characteristic of radical Protestantism is its emphasis on asceticism, on the necessity for curbing impulses early in life. "Give the devil a little finger and he'll take the whole hand," is the maxim in terms of which many such parents operate. They identify the devil with sex in any of its forms, with aggression, and with general willfulness or disobedience. According to well-known facts (Dollard and others, 1939), severe frustration of all such impulses should produce strong instigation to aggression in children. Yet direct expression of aggression is one of the impulses most severely controlled in such families. So a conflict should often arise in the children of such families between the strong impulse to aggression and an equally strong fear of expressing it. The problem should be even more acute and more prevalent among boys than girls, since for boys controlling aggression is more of a problem because of their innately greater strength and destructive power.

A common solution to such a conflict is to convert the aggressive impulses into a more socially acceptable form, such as arguing or participating in strenuous physical activity. Still another common solution dramatized by the authors of *The Authoritarian Personality* (Adorno and others, 1950) is for the boy to identify with his strict father and to behave aggressively toward persons beneath him who are younger or less powerful. But in the case of the future scientist, another solution appears to be adopted. He simply "goes out of the field" and attempts to avoid interpersonal contacts, since they are the most likely to arouse the impulse to aggression and the anxiety over expressing it. It is a fact that scientists are low on aggressive themes in the TAT, and when faced by a problem in aggression typically solve it by minimizing it or attempting to avoid it altogether. There is very little guilt in their TAT stories because there has been very little expression to be guilty about. In fact, one might almost say that the only time a scientist can manage to express aggression is when he is morally indignant. For then, and perhaps only then, are his moral values, which normally frustrate his expression of aggression, in actual support of his aggressive impulses. It is difficult to be very precise in such matters, but it is clear that scientists can get involved in some extremely heated scientific controversies, especially when they suspect that moral values of the scientific code (e.g., honesty, full publicity) have been transgressed.

As was pointed out above, scientists in their personal lives are upset by human emotion, perhaps particularly by aggression, so that their withdrawal from people can perhaps be viewed as a mode of defense against conflicts over aggression. Their avoidance extends even to their own parents (see above), and to art and poetry, which contain many cues that might arouse their aggression-anxiety. What, then, normally becomes of their impulse to aggression if it does not express itself, as it does for most people, in the area of human relationships? To some extent it may simply be decreased in intensity because situations which

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[2] EDITORS' NOTE: For supporting experimental data, see the original article.

arouse it are avoided, but what is left may be sublimated into an attack on nature. It is certainly part of the folklore of science that it represents an attempt to *conquer* nature, to dominate it, and bring it under man's control. Social theorists have noted that the view that man could be more powerful than nature is by no means a common one in history. In fact, the usual belief among the peoples of the world, even today, is that nature is much more powerful than man (witness floods, droughts, earthquakes, and so forth), and that man must somehow placate the gods who control nature. It may not seem too far-fetched to assume that it took an unusual psychodynamic situation to create in some men the apparently irrational belief that they could conquer nature. The blocked aggressive needs of a few scientists, diverted toward nature, may well have fulfilled such an important historic function.

On a more personal level, we have noted that the scientist is intensely analytic in his approach to experience. His response is to freeze the flux of reality, to ask what it is, to take experience apart and see what makes it tick. It is no mere metaphor to say that analysis represents a form of aggression. To take something apart is to destroy it in a very real sense. Of course the creative scientist attempts to put reality back together again on his own terms, but even he would admit that the model he constructs of reality is a poor substitute for the richness of human experience.

But why are scientists so intensely masculine? How can this fact be fitted under the hypothesis that they have adopted a unique method of handling aggression-anxiety? Earlier we pointed out that one of the common methods of handling the aggression problem is for the boy to identify with the aggressor (usually the father), because he gets some gratification in behaving in a similar powerful fashion. The same mechanism may be operating in the case of the scientist, but there is evidence that it does not follow its normal course. According to Terman's (1954) data, physical science research subjects report least understanding and affection from their fathers, while the subjects who were undergraduate science majors but did not pursue research careers report the most understanding and affection from their fathers. In other words, the research group, like the nonresearch group, takes the initial step in dealing with the aggression problem of strongly identifying with the male role. After all, the male is the image of power and aggressiveness. But the next step, of behaving agressively like a powerful male, is further discouraged by a distant unaffectionate, real father in the research group and promoted by an understanding father in the nonresearch group.[3]

Direct confirmation of the hypothesis appears in the fact that the nonresearch group maintains a high interest in outdoor sports (Terman, 1954), whereas the research group has a low interest in sports. That is, while scientists are definitely masculine, they do not express their masculinity in the aggressiveness of outdoor sports as more "normal" males do, but may satisfy it more indirectly through scientific analysis.

So the new hypothesis is that scientists work so hard and love their work so much to satisfy not sexual but aggressive needs. Such a notion has the further advantage that there is no consistent evidence in the literature for sexual difficulties among scientists. One might reasonably expect that if they have so much anxiety over their sexual love of their mothers, more of them would never have married, like Leonardo da Vinci, or at least would have shown a history of homosexual or other perverse sexual outlets. But such is not the case. Scientists as a group appear to be heterosexually normal, although somewhat delayed in their approach to women, probably because of their general avoidance of interpersonal contacts. . . .

Many questions remain unanswered. It would be particularly interesting to know why withdrawal, as contrasted with other defenses, is the method of handling aggression adopted by people who become future scientists. Two possibilities seem worth further consideration.

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[3] It is particularly interesting that, in fantasy, scientists portray fathers as understanding but report their own fathers to have been distant and unaffectionate. It is as if they have interiorized the positive male image but are somehow blocked at the level of *behaving* like a normally aggressive male because of ambivalent feelings toward their own fathers, of incomplete identification with him because of his distance, or of other sources of aggression-anxiety mentioned above.

First, Roe (1953b) found that many of her eminent scientists had had a serious illness when young, and having been in bed for a long time, out of normal contact with normal human relations and masculine pursuits, should certainly provide the opportunity for development of a rich fantasy life and for the discovery that withdrawal may solve many problems. Secondly, I have pointed out in another place (McClelland, 1956) that one of the paradoxes of history is that radical Protestantism produced not only more than its share of scientists but also more than its share of business entrepreneurs. The paradox arises from the fact that on nearly all tests like the Strong Vocational Interest Blank, scientists and businessmen are diametrically opposed in their interests and attitudes toward life.

How could two such negatively correlated character types have been produced by the same family type? The answer may lie in the typical mode of adjustment adopted by the two groups—the scientists *withdrawing from* and the businessman typically *entering into* the field of human relationships. Rosen and D'Andrade (1959) have demonstrated that the parents of boys with a high need for Achievement, which predisposes individuals to business entrepreneurship (McClelland, 1955) set high standards for their sons, but also show a good deal of warmth in praising them for their efforts at achievement. We also know that scientists report that they were distant from both parents and therefore in all likelihood did not receive the parental warmth and support that future entrepreneurs get. It may not be pushing credulity too far to infer that warmth should encourage boys to enter into interpersonal relationships whereas coldness should discourage them. So while the future scientist and the future entrepreneur may both have been exposed to high standards, the former, being treated indifferently by his parents, tends to retreat from people, while the latter, being encouraged by his parents, moves out more confidently into the world of adult human relationships. Similarly, high standards plus coldness make frustration and aggression the chief problem for the scientist, while high standards plus warmth make achievement the chief concern of the entrepreneur.

Many facts appear to be falling into place, but it is time for the analytic side of the scientists to show itself and to remind us that many of these supposed interrelationships have not been firmly established by any means. Fantasy is fun, but it must also be checked and disciplined. After all, the scientist's fancy differs only a little from the normal and very much more from the neurotic in poor touch with reality.

What has been accomplished? If I have succeeded in displaying both the "play" and the disciplinary "aggressive" interests of the typical scientist, I will have managed to illustrate with my own work the main points that I have been trying to make. Yet caution must have the last word. Half-way through our investigations, we thought we had a good explanation of the psychodynamics of the scientist. It certainly sounded reasonable. But further checking showed that it was hardly tenable. We have now ended with a new hypothesis. Further research will undoubtedly show that it too is defective in some ways. So the truth the scientist seeks always just eludes his grasp, and he must gain his satisfaction from his faith that in unravelling the secrets of the universe his attempt to untie the first knot may make it easier to untie the second.

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The Scientists' Personalities

Bernice T. Eiduson

Certain common notions about the personality of the scientist crop up repeatedly in psychological literature; they are shared by psychologists representing various theoretical backgrounds. These concepts have to do with the way scientists meet their emotional challenges, with the behavior patterns that are found regularly in their personality make-up, with their conflicts, with the motivations to which they respond. The interpretations are couched in hypotheses which may take the form of descriptive terms thought to be correlated with the fact of being a scientist, or they may be elaborate psychodynamic formulations which look for a *primum mobile* to account for the scientist's functioning.

In order to test the most commonly held hypotheses concerning the psychology of the scientist, I selected the variables referring to emotional behavior, personality structure, and motivations which appeared repeatedly in the literature on these creative persons. Then, using the personality data on each scientist that

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the projective test instruments provided, judges did a blind rating on these characteristics in the subjects. The results showed that there were a number of personality features which all the subjects shared. This chapter shall discuss these, turning to the interview data for information about how these commonly held personality features seem to be reflected in behavior, and especially how they contribute to scientific performance.

However, before presenting these results on the group I should like to give examples of the kind of personality pictures from which these common features emerged.* In a comparative study such as this, one can lose sight of the fact that we have been studying forty individual persons, each of whom has developed unique ways of behaving, reacting, remembering, interpreting situations. Instead, in looking at the group as a whole, we tend to focus on variables that can be isolated and that still retain their meaning; and on variables that tend to characterize the group. Thus, the context out of which the variables are drawn and which may be highly unique for each man is discarded as "the chaff," with only the features they share in common as "the wheat." It is necessary for us to do this in order to compare the stereotypes of the scientist with these men's actual personality characteristics. Stereotypes by definition are generalized variables which have been drawn from the multiplicity of individual circumstance and characteristic that describe the personal case and can be applied to members of the group. Therefore in order to evaluate their accuracy, it is necessary for us to compare the same kind of generalized common denominators found in the group by empirical study with the characteristics emerging in the stereotypes. . . .

The results drawn from the quantitative analysis of the experimental data indicate that the individual differences within the group are greater than the common denominators. None of the hypotheses that refer to particular behavior patterns or kinds of conflicts proved to be successful in identifying the group. For example, the results indicate that the scientist does not have one specific kind of personality structure defined by the psychosexual level to which his psychological development has progressed; nor is there a predominance of one major kind of defense structure. No hypotheses suggesting any particular emotional constellation as extremely pertinent were tenable: the scientist is not given particularly to changes of mood; he is not particularly passive, submissive or dependent; he does not shy away from strong interpersonal relationships; the scientist is not bisexual, nor is he ridden by unusually strong ambivalent conflicts. The generalization that scientists are characterized by particular kinds of conflicts over sexuality, authority, or performance does not stand up to experimental test. Rather, the personality pictures are varied and show that the men in this vocation have chosen science to satisfy diverse needs and have found diverse satisfactions. In fact, none of the personality characteristics that emerge as common denominators is significant for one diagnostic category as compared to all the others. They can be considered common to a number of personality pictures, and even find expression within these personality pictures in a number of ways.

I shall first list the emotional and motivational variables that stood up to experimental test, and then I shall discuss how these contribute to the scientist's functioning as a researcher.

The results indicate that (1) the scientist has strong emotional leanings to intellectual activity; (2) he is independent in his thought and actions, and does not mimic others; (3) he is challenged by frustration and anxiety-producing situations; (4) curiosity is likely to be a major determinant in his work; (5) strong ego involvement and conflict are expressed in work; (6) he does not use parental ideals to set up his own goals; (7) he shows a strong capacity for sensual gratification; (8) he is motivated by a desire to master or interpret natural forces or reality; (9) he is sensitive to the moods and feelings of others; (10) he is sensitive to his internal environment, needs, wishes, desires; (11) he values work primarily as permitting expression of inner personality.

These individual variables cluster around a few main trends; I shall focus on these in

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* EDITORS' NOTE: See original article for these data.

discussing the role such personality variables play in scientific performance.

EMOTIONAL INVESTMENT IN INTELLECTUAL ACTIVITIES

The scientists are all excellently endowed with intellectual capacities. They range in intellectual level from high average to very superior.[1] This means that beginning with their earliest days they were probably able to manipulate certain kinds of things, ideas, and people in their environment with facility; they were probably able to explore and cope with their world and manage it with superior skills and modes of response. These abilities, however, even tied as they are to normal maturation, are not developed automatically in childhood; they must be learned. The fact that these skills have become "second nature" today indicates how much satisfaction and pleasure the men must have derived from the uses of their resources.

The findings show that the scientist has immersed himself deeply in intellectual interests and activities. Much of his self-realization as an adult is derived from the fact that he is doing work that not only places a premium on the intellect but is often an exciting, intellectual pursuit. His superior mental capacities and his curiosity propel him to look for work that will make use of his resources. We can speculate that, from the beginning of their intellectual development, these men enjoyed putting their wits to different problems and situations, and found both conscious and unconscious gratifications in their investment in intellectual pursuits.

Emotional investment in something is not likely to be an all-or-none process, or dependent on a one-shot experience. It proceeds with trial and error, with ambivalence and unevenness, until the final anchoring of certain interests and pursuits becomes one's consistent pattern of satisfactions. The regularity, consistency, and stability of the satisfiers that one uses make it possible to describe a person's adult identity in terms of the myriad ways in which he has invested himself. These can be hierarchically ordered to show how expenditures of time, energy, and emotions have contributed to forming certain aspects of identity.

In the interviews, the scientists could only speculate retrospectively about the experiences that had made intellectual activities their outlets for personal resources. Some mentioned how intellectual things had never lost for them the character of play, with its experimental possibilities, its stimulus to fantasy and daydreaming, its imaginative spans with no thought of product.[2] They felt that this jumping off into the unknown was a unique pleasure, and many traced its precursors to their very early days. . . .

Such satisfactions cannot fail to keep emotional investment in intellectual activities fresh and constantly revitalized. And only occasionally does one become aware of some of the unhappy implications of such enthusiasms. . . .

However, because emotional investment in intellectual activities is so constantly refired by the curiosity drive, few scientists are really concerned with such consequences. Curiosity may direct the progress from one aspect of an experiment to the other, may more abstractly stimulate the desire to find ways of integrating internal experiences with those in the external world, and may direct the compelling scientific need to master the natural forces of reality. Psychologists once thought that all drives—organic as well as exploratory or manipulative—produce tension within us until they are somehow met or relieved, and that, once met, the tensions abate and the experience of pleasure follows. Now they realize that at times the greatest pleasures may be derived from the tension itself, at least until it reaches the point where it becomes more disruptive than enjoyable. The curiosity drive itself has been described this way; the satisfactions accruing during the building up actually matching and even surpassing the denouement, which becomes a sort of anticlimax. . . .

With so much of the scientist's feeling of self tied up in his work, it is not surprising to find that this becomes the stage at which the passions get spent and the gamut of emotions —at other times concealed—revealed. Scientific work is no impersonal, cut-and-dried matter, yet the rationality of scientific methods is frequently confused with the internal experiences and feelings of scientists.[3]

A close look at the interests of the few men who show tremendous zest and enthusiasm for a diversity of things showed me that these expansive pursuits can all be related to scientific activity or the promotion of scientific sensibilities. All their interests are approached with the same orientation and worked at with the same precision and discipline. Few interests are pursued for nonscientific reasons, and even these interests that seem to be pursued for other reasons often turn out to enhance or whet the appetite for the activities which soon follow. Noontime handball is acceptable only when it comes after some hours of work and will be followed by more work. It is, or at least it is rationalized as, "the physical pause that mentally refreshes."

EMOTIONAL CONSTRICTION AND CONTROL

This leads us to the second major common denominator in personality: emotional constriction and control. Constriction does not mean the lessened intensity of emotion, but rather the narrowness of the emotional experience, the channelized ways of expression, the restricted and controlled ways of response. It is contrasted with emotional lability, overreactivity, fluidity in response. Emotional expression is not undiversified or completely unspontaneous; it is restricted in the ways it gets expressed, and often comes out more openly in fantasy than in direct relationships.

Emotional withdrawal, isolation, and loneliness are words which appear frequently in the stereotyped concept of a scientist's personality. According to this empirical study, this seems to be an incorrect conception. It apparently has been derived largely from the fact that the scientist is often in an isolated setting, and engaged in solitary work. These findings support Lionel Trilling's contention that the major representation of the creative person as being alienated or isolated stems partly from the nature of his work and partly from the uniqueness and originality that make his product different from others. Trilling feels that this is one reason why creative persons' life histories are often conceived of as being long experiences of rejection and misunderstanding.

The picture of the scientists emerging from the psychological tests is not one of withdrawal, as we know it clinically, or of a differentiated, elaborated response to varied stimuli, but one in which emotional expression finds outlets in limited areas. There is no breadth of emotional involvement, nor are intense relationships with people frequent; those that exist are usually with other persons who share work interests and scientific experiences.

Since the main enthusiasms are bound up in what is, to a greater or lesser extent, solitary work, passions may propel work in a way that otherwise would not be possible. They may serve also to sustain these men during the tedious hours, the routine tasks, and the times of failure. How work sustains emotions and how, on the other hand, emotions sustain work, is not simple to sort out; we know this from the way the scientists describe their feelings during periods of strong work power and during unproductive times. None describes feelings of loneliness when he works day and night alone on something urgent. Loneliness is more likely to be the outcome of detachment and aloofness from people during an unproductive period. The comfort derived from work and the closeness to oneself and one's resources experienced during work seem to provide a cloak that insulates the scientist from the emotional distance existing in nonwork situations.

There seems to be almost no activity for the scientist that offers as much gratification as science; it is no wonder that both other activities and activities with others are pale by contrast. The tests indicate that this is an overdetermined reaction, because the scientist feels that he knows and can trust his own personal resources, but he has doubts about relying on others and trusting them.

The biographical information shows that, in general, adult relationships have been stable. There is a minimal amount of internal churning about the troubles and unhappinesses of others or of themselves. For the most part, the scientists seem fairly happy and satisfactorily adjusted, in the sense of not having too much open conflict. They demand much less support from relationships outside of work, and much less of their sense of personal identity comes from their other roles. I was struck, for example, by the fact that family problems, and even some very severe psychological disorders in children and wives, arouse relatively little conflict in these scientists as husbands and fathers—not comparable to the degree that work problems arouse. The migraine headaches come after committee meetings and not after fights with the wife. The affect seems to be siphoned into work to such a degree that everything else seems to have much less impact, and scarcely any other aspect of life experience can compete successfully for the emotional involvement of these men. In a peculiarly circular way, the emotional overinvestment in work seems to reinforce the psychological conditions which originally might have given rise to it, and this, then, turns back to insulate the scientist from being too disturbed by human conflict in other areas.

If the channeling of the emotion into limited areas were really the same as withdrawal or uninvolvement, as is described classically in the case of Willard Gibbs, for example, I think there would be much less conflict within the area of work.[4] It seems to me significant that after one chemist told of his tremendous need to be best in whatever aspect of the field he might have gone into, a colleague mentioned inadvertently that the same man actively discouraged anybody else from trying to do research in areas that he thought infringed on his province. Obviously this kind of competitiveness, with its authority orientation and concomitant narcissistic demands, is one aspect of such singular preoccupation.

This narrowness and singularity of scientific preoccupation prevents the dispersion of devotion and energy, and so propels scientific activity. Take resistance to interference or disturbance in the laboratory. Here, one is often bombarded by noises from all sides. The worker's resistance to distractions is possible only if he uses very strong isolating mechanisms. Also, scientists describe their compulsively disciplined work habits as their greatest assets, and it seems highly improbable that they could have become what they are without keeping emotional and intellectual interests in other life involvements to a minimum, and thus freeing themselves for work. Some men keep rigid schedules, with specific times allotted for specific kinds of work. One geophysicist, for example, tells how he divides his day into two parts: one in which he does research of his own, and the other in which he works for the university. He is extremely proud of his ability to shut off one thing and get started on another immediately, with almost no effort wasted in changing gears. This demands a sharp closing out of tensions and activities that could easily flow from one area into another. It ties up with what psychologists have found experimentally in the field of perception—that the amount of distraction tolerated by an individual seems to be related to the strength of his reliance on emotional isolation as a defense mechanism.[5]

Persistence, too, is facilitated by this isolating mechanism. One scientist has worked on a problem in plant biology on and off since 1941, picking it up and returning to it again as new methods brought fresh promise for its solution. Another received a Nobel Prize for a problem he had worked on for twenty-five years. Another stows away his research ideas in a drawer so that he will not forget any of them. Each idea then becomes so much a part of him that when he later reads that someone else has picked up one of "his" ideas and "cracked it," he feels scooped, although he and his students may never have gotten around to working on it.

Judging from the results of the tests, this emotional constriction and control seems to be an ingrained feature of the personality structure of our sample of scientists. This seems to corroborate, at least partially, a line of development that was suggested in the biographical material. In the adult, after professional channels have captured the emotional energies, work seems to proceed as it does partly because there is so little effort and emotional involvement directed elsewhere. In turn, the rewarding way in which work uses this emotional constriction and control tends to reinforce it in the behavior pattern. Therefore, a personality trait that developed from conditions and propensities independent of science, seems to have found quite felicitously new and independent support because of its value in the vocational role.

ANXIETIES AND FEARS

The foregoing may, mistakenly, create the impression that everything in the scientist's psychology works out happily for his over-all adjustment, that he is left in a state of removed but complete bliss. It is true that his lack of free-floating anxieties and fears is striking. As a group, and with few exceptions, the scientists in our sample would be classified clinically as character types or problems rather than as neurotics merely because they show so little symptomatic anxiety and tension. This does not mean that the scientists have no anxiety; it means rather that the anxiety is bound up in the personality structure in such a way that it creates little or no consciously felt disturbance because the men have enduring, habitual ways of handling it which serve to keep it from making them uncomfortable. In other words, the scientists make relatively constant, habitual adjustments in the face of problem or conflict situations, and these keep anxiety from getting so great that it interferes with performance. Instead, as the quantitative results show, scientists are challenged by anxiety-producing situations, rather than being thrown by them.

Anxiety can be mobilized by any number of situations, as it can be derived from any number of conflicts. Some anxieties are perfectly appropriate to reality situations; others are neurotic and more related to internal conflicts. In actual manifestation, however, one may get only a hint of what these conflicts are. Often neurotic anxieties are displayed in such devious ways that they may be recognized only through understanding the symptoms within the frame of the basic personality structure. Although some psychologists feel that there are core anxieties within a personality to which others become attached, the secondary anxieties can often suggest the nature of the basic difficulties.

The psychological tests tend to reveal some of the latent fears and anxieties that have produced tension within the subjects; the tests indicate also that many of the men in this group who have conflicts expect to resolve them through scientific work. Both the nature of these conflicts and the expectations in regard to the solutions offered by work are different for each subject.

The field of science offers unlimited fantasy possibilities and scientists therefore see it as providing bountiful opportunities for resolution of disturbance. Instead of being viewed as a highly institutionalized vocation in which roles and duties are preset, science appears as a world that is easily manipulated to one's needs, and sufficiently variegated to be able to be adapted in any of the ways one wants to conceive of it. For example, a few of the group see the scientific setting as a platform on which the main dramas of life are played in scenes that are high-powered and colorful. One chemist thinks of himself as an imaginative and responsive fellow who feels that circumstances constantly threaten to blot out his sensitivity. He is aware of getting himself into situations which are unusually demanding of great creativity, and at the same time he has the hunch that despite his great ingenuity and success in handling these he is close to danger. He sees his destiny as a fighter in the big life drama in which he is the maltreated victim, pitted against overwhelming forces of evil, garbed only in his scientific armor. Some of this heightened fantasy is tied up with the image of the father who died early and whom he now pictures as a "fallen hero."

For another scientist, the childhood battle with the father has been removed to the scientific battleground. While a child, he had little to do with the father, scorning him as a nonintellectual, and he attaches himself in imagination—and to some extent in fact—to one great scientist after another. His history showed that he would leave each professor after a few years, convinced that he had been exploited by him, while in reality, it looks as if he is the exploiter.

Some anxieties are related to aspirations, to doubts about achieving goals—and to some extent, these are realistic. Yet they also are found among scientists whose stature is undisputed and whose achievements have even surpassed their earlier hopes. In others, anxieties express more directly underlying feelings of personal inadequacy. . . . This proving of one's self through scientific work as a denial of dependency makes its appearance frequently in the psychological test pictures. Many visualize science as a way to fight the guilt and weight of loneliness brought on by awareness of dependency needs. . . . Many scientists seek out the "rationality" of science. Some fear their own adjustment is shaky and that, were they in a more ambiguous work situation, their own psychological balance would be threatened. A few were aware of how much their need to stick tightly to preconceived notions about science limits their work. For some, the rationality of science serves primarily to check impulsive behavior. The threats stimulated by the pressures of the irrational or uncontrolled are to some extent based upon reality, for some of these men have, at various periods in their lives, had strong pulls to "let themselves go." Yet, for others, the threats are more related to latent wishes, drives, and desires. One fears the "sensuous and animal instincts" that made his adolescence so wild and confused. Science provides him with an outer coat of refinement, as it does a scientist who describes himself as sexually promiscuous. . . .

While some scientists primarily perceive science as rational, others see it, even with its emphasis on the structured and logical, as "irrational." One man, for example, sees his work as confronting him with the frightening and the unknown. His greatest fears are of getting in beyond his depth and not knowing when to ask for help, and of constantly finding himself out on a scientific limb. But for others work is the sanctuary, the peaceful haven, the isolated retreat where passivity can be enjoyed. More than that, it provides a way by which passivity can be regarded as most acceptable and not condemned as "laziness." . . .

It is evident here that scientific research is thought of as serving apparently counterposed attitudes around aggression and passivity. I say "apparently counterposed" because one of the main contributions of psychology has been in showing that two apparently conflicting attitudes can be essentially different aspects of the same personality dimension. We know that what looks like passivity on the surface may at one and the same time be the denial of aggression and its indirect expression. . . .

For some scientists, the two sides of the ambivalence in aggressive conflicts were more directly visible: one hard-working and very devoted chemist feels that were he to give up even to a small degree the long hours and extreme dedication to his duties, he would flee from science completely and would, therefore, be lost to it and to himself. The strength of his needs for dependency frightens him very much, and he sees work as a way of providing a substantial framework for him. . . .

Some men felt that choosing a field which parents could not understand was *ipso facto* rebellion; others interpreted this as independence; obviously, for most it was a little of both—perhaps stemming from the competitive relationship to the father, but ultimately in the service of personal emancipation and freedom. Only a few men were directly encouraged by a parent to go into scientific work. Many more were openly urged toward medicine which had a prestige science had not yet achieved. One subject's mother had impressed him with how superior he was to other children and had decried every relationship which was not in keeping with her notions about how such a "superior boy" should act. Science fortunately fitted into her actions, but her son unconsciously felt that her degree of overemphasis must be compensatory for some real inferiority in him. Even as an adult he feels worthless and ineffectual as a scientist, although others consider his work excellent.

The findings show that as a group scientists were not unusually bound by parental ideals in setting their goals—in fact, they had revolted against them, and were self-directed and self-disciplined in their thinking to a significant degree. These scientists tended to reject unusual imitation of, or dependency on, authority figures. Some assumed responsibility at an early age and, through it, tested and retested their fears of the outside world; others were slower and more cautious. Were I to attempt even a superficial ranking of my subjects in terms of their success—using fame, prestige, or productivity as a criterion—the most successful would show a definite rejection of fathers or mothers as omnipotent authorities, sometime during adolescence.

It is often thought that freedom goes along with assertive action. Yet, it is instructive to see how, for some of these men, personal freedom means the acceptance and indulgence of their needs for passivity and isolation, the denial of daily turmoil, and a turning away from what would seem on the surface to be obvious ways of achieving self-independence. Similarly, one would expect the research scientist to keep his eyes open and his senses alert. However, some of these men see their work as a way of helping them to keep their eyes closed to troublesome outside affairs, and as a way of denying the reality which has made them feel unhappy. Possibly scientists must keep their eyes closed to outside affairs in order to be good scientists; to label this shutting out of irrelevant externals as merely repression, denial, or isolation—as it might be labeled in other psychological contexts—would be to deny perhaps one of the most positive and liberating aspects of functioning as a scientist.

SENSITIVITY

Some have called uncommon sensitivity to experiences—usually sensory experiences—the first great phase in the evolution of the creative experience that leads to original work. This sensitivity may involve heightened awareness to all kinds of stimuli, in terms either of sensory processes or of some persistent or recurrent relationship between them. The curiosity about these experiences and the desire to seize them and put on them a personal stamp, or to delve into their more complex ordering, leads to the desire to create and the effort to produce a creative product.

The psychological tests showed that sensitivity in scientists finds expression in these ways: in their thinking, they are responsive to sensory experience data; they seek out subtle and delicate impressions; they show a strong capacity for sensuous gratifications; in relationships, sensitivity is evident in their awareness of themselves and their own motivations; and in their discernment of the feelings and moods, wishes, and desires of others—without necessarily being responsive to them. They also have a desire to integrate internal and external experiences in a comprehensive way. Such a listing may give the misleading impression that the scientists' sensitivity extends into the many areas of personal functioning, that it is not necessarily confined to certain classes or objects or to certain kinds of experiences. This is to some extent inherent in the definition of sensitivity as increased perception of the world within and without. In the case of these intellectual men, however, their constriction, discipline, and unusual involvement in work make this the area in which their sensitivity is most readily stimulated and in which it is most generously expressed.

I expected the experimental findings to corroborate notions of the scientist's heightened sensory acuity and his keen responsiveness to order or disorder in external phenomena. Here my expectations were confirmed, but I had not anticipated that their sensitivity would be as readily directed toward themselves and to their own motivations. Overtly, these men do not show a great deal of insight into their own motivations and needs, although a few have become sophisticated in the field of psychology through reading. Yet there is a good fit of personality with scientific vocation, which means that, at least unconsciously, the scientist has been aware of his needs, and of the kinds of experiences and situations and areas in which he might function successfully—and he has acted upon these insights.

There is one aspect of the way sensitivity finds expression to which I would like to draw specific attention, and that is how this sensitivity encourages in these scientists what Kierkegaard has called the "paranoid leaps." Heightened sensitivity is accompanied in thinking by overalertness to relatively unimportant or tangential aspects of problems. It makes them look for and postulate significance in things which customarily would not be singled out. It encourages highly individualized and even autistic ways of thinking.

Were this thinking not in the framework of scientific work, it would be considered paranoid.[6] In scientific work, creative thinking demands seeing things not seen previously, or in ways not previously imagined; and this necessitates jumping off from "normal" positions, and taking risks by departing from reality. The difference between the thinking of the paranoid patient and the scientist comes in the latter's ability and willingness to test out his fantasies or grandiose conceptualizations through the systems of checks and balances science has established—and to give up these schemes that are shown not to be valid on the basis of these scientific checks. It is specifically because science provides such a framework of rules and regulations to control and set bounds to paranoid thinking that a scientist can feel comfortable about taking the paranoid leaps. Without this structuring, the threat of such unrealistic, illogical, and even bizarre thinking to over-all thought and personality organization in general would be too great to permit the scientist the freedom of such fantasying. . . . Thus their own cognitive patterns provide internal boundaries and limits which parallel those of the scientific method itself.

One might say that scientific thinking in a way institutionalizes paranoid thinking; it sanctions it not only as proper, but also as the irrational that ultimately promotes the rationality of science. The manipulative nature of scientific models is a case in point: it is common knowledge that occasionally the same phenomenon can be explained by two different models, and that there is no right explanation which necessarily excludes the other. This equivalence of models puts a premium on the cleverness of the scientist, on his ability to concoct fanciful enough or diverse enough explanations to encompass phenomena—and there is a chance that any number of explanations will aptly fit the same empirical data. This encourages free rein to imagination and to the breeding of "crazy" ideas. As one zoologist has said, "One can maintain some crazy ideas for a very long time in science before enough evidence is accumulated to prove you are wrong."

Scientists say that ideas are very cheap, but that ideas that ultimately stand the rigorous test of reliability and validity are not nearly so abundant. The scientist with a vested interest in an idea sometimes holds to it with great tenacity; it may seem that he wishes to make it impervious to the rules and regulations which particularize scientific thinking. In the end, however, these ideas do not become part of the great body of learning unless they can stand up to the rigorousness of scientific scrutiny.

NARCISSISM

Oliver LaFarge has said that scientific life is shaped by the feeling that the ends must be good not for oneself, but for all mankind; and that the scientist must be able to set aside personal advantage, comfort, and glory in his developing effort to make progress. Were this true, all scientists would have to be extremely masochistic, self-denying, martyr-like individuals. Few of our subjects would clinically fall into this category, and yet there is no question that they are dedicated scientific men.

What one sees in their personality pictures is neither selflessness nor selfishness; in their overinvolvement in work, in their fantasies about their omnipotence, in their anticipated accomplishments, in their minimality of interest in others who cannot further their own ends or goals, they are self-oriented. Yet their gratifications come as much from their contribution to the fund of knowledge and from what they contribute socially as from personal gains; in fact, their personal gains seem in some ways neglected.[7]

In relation to creative endeavor, narcissism essentially implies a need to produce and to value one's own products as an extension of one's self. Psychoanalysts have found that while excessive narcissism is a characteristic of the infantile and neurotically developed person, a total absence of narcissism is impossible for psychological sustenance in the mature adult.

One cannot, unfortunately, measure quantitatively how much narcissism is optimal for the individual. As yet, psychology provides no appropriate measuring sticks. Nor do we know exactly what to measure, or how to measure its role in given behavioral settings. P. Federn has presented some qualitative considerations which suggest that narcissism is "healthy" if narcissistic fantasies are slanted toward realistic tasks, relationships, desires, and activities that have a specified goal, and when, in the process of reaching this, a great deal of intellectual work is accomplished.[8] Goals are to be examined continually and critically so that they change and are adapted to realistic conditions. The classification of whether narcissism is "healthy" or "ill," then, depends not on the fact of finding the narcissism but on the use to which it is put in the personality and the way it is integrated with other aspects of personality functioning. . . .

How narcissistic is such involvement with work? This question cannot be answered on purely psychological grounds. Since evaluation of narcissism rests on direction of endeavor, means used, and goals set, these have to be taken into account also. Whether a goal is appropriate for an individual or not, for example, depends partly upon the values that the goal embodies and partly on whether the practices in meeting the goals are acceptable in terms of the larger societal ethic. Such considerations, if not completely decisive, are certainly not irrelevant. Thus, despite the fact that narcissism is a concept derived from intraindividual psychological functioning, and should be able to stand exclusively on a psychological base, we must use sociological referents to supplement their definition.

This inclusion of outside values, goals, or reference to complete psychological categories seems paradoxical, and yet it is not peculiar to the problem of narcissism. It is implicit in many other value judgments that are made in the psychological context, and a recognition of this fact has been one of psychiatry's significant contributions. In recent years, psychiatry has recognized and appreciated that a value system is inevitably built into a judgment about personality and personal functioning. Value judgments are seen as inevitable: first, because of the relativistic framework that defines functioning, and second, because criteria for adjustment would not otherwise have the necessary flexibility to encompass differences among individuals. In deference to scientific method, however, psychiatry does insist that the value system used in any evaluation of personal adjustment be made as explicit as possible, so that the system itself may be open to study and critical investigation.

I press this because I think it will help indicate what devotion of one's psychological energies to scientific work means in terms of psychological adjustment. When Freud attempted to define maturity, or—in his vocabulary—"genitality" or adjustment, he postulated that none of the mature person's efforts or energies was devoted to inhibiting or holding back of impulses; rather, that all were directed or channelized into creative, productive expression. This he held to be true of sexual as well as of aggressive energies. In an elaboration of Freud's position, Kris later pointed out that these sexual and aggressive drives, instead of being expressed directly, become neutralized in order to allow for intellectual or artistic work. Freud used the term "sublimation" for defense mechanisms that were used in mature behavior. The working for social good, or toward the achievement of social aims, was always part of the definition of sublimation and it was specifically different from "reaction formation," a defense mechanism which also often resulted in a great deal of "social good." In this latter case, however, the "social good" resulted from the individual's attempt to deny and conceal other impulses that were not ego-acceptable.

In practical application, the differences between reaction formation and sublimation are difficult to draw; in fact, some psychoanalysts, such as Frederick J. Hacker, say it is almost impossible. When the establishment of a difference is attempted—as is attempted in every intensive psychological study—the value system of the psychiatrist is inevitably drawn into determining what is really in the service of society, free from inhibition, and what is not. Even with one's own value system as a reference point, this is very hard to judge.

The empirical study of the psychological make-up of research scientists . . . reinforces the position of those who maintain that adjustment is inevitably a socio-psychological question, rather than a purely psychological one. Our reference points to looking at the ways and manners in which human beings apply psychological capacities and characteristics inevitably introduce sociological or cultural considerations; without these, there is no way of defining what is in the interest of society or mankind. For scientists, this means that the very fact of doing socially valuable work gives them a leg up on the ladder of adjustment. There is no question that their work is considered to be of greater significance, and certainly of more social import at this time, than is the work, say, of accountants or bookkeepers, or any job which may also be socially oriented but does not have the same prestige.

In summary, then, the empirical study of this group of scientists shows that in emotional and motivational characteristics, they are more different than alike. Their personality pictures cut across diagnostic classifications and classical personality configurations. As a group, they would be labeled as character types, with many men's adjustment falling within the normal range and others described as personality trait disturbances or character disorders. Furthermore, the stereotyped depictions of the scientist as a person given to mood swings, or depressions, or on the other side, overly controlled and logical in his emotional makeup, have not been borne out. On the contrary, in all these aspects there is a wide range of reaction pattern, much more so than one might have expected in the light of the long-standing and deep-seated impressions that have existed in the public mind.

The areas in which common denominators among the group are found center around a few main areas: the deep-seated investment in intellectual things; the expression of a wide gamut of emotional response within the intellectual (and particularly work) framework; the independence in emotional behavior—a feature which mimics the independence that is noted in the analysis of cognitive patterns; sensitivity both to himself, to the motivations of others, and to sensory and even sensual stimuli.

The tremendous role of self in work is first noted in these data (a finding that will also be confirmed in subsequent material); for science becomes the area in which the strong curiosity drive is directed, the center of conflict, the hub for much of emotional experience—especially the intense experience. While, in this setting, work would and does normally engender anxieties and tensions, the data show too that anxieties are kept fairly well in check, because they are intellectualized, and have become ingrained in the personality structure. This, plus the lack of other neurotic features in the psychological pictures, suggest that the researcher in general shows a characterological picture. When the adjustment of the man is somewhat disordered it does not necessarily bring much psychological discomfort. In fact, if it takes a toll in behavior, it is in the direction of making him function less effectively than he might in the light of his potential; or even making him "too adjusted," and thus not allowing his spontaneity to come through.

The study of the personality makeup of the scientists has shown us too that the resources, abilities, and personality characteristics of an individual can and do serve him in a number of ways simultaneously. The very constriction that enables a scientist to focus with little disturbance from outside emotional pressures, to work with dedication, absorption, and devotion in intricately detailed and often very demanding scientific problems, may severely incapacitate him as a husband or a father. The emotional constriction may also make him so socially inept that he seems habitually unresponsive to the needs of others. Still, diverse personality characteristics may not be as conflicting in the total personality picture as they at first appear. While some courses of conduct demand certain personality features and resources, others involve quite different abilities; and frequently psychological energies and characteristics are inevitably contaminated with each other. This makes them difficult to sort out, but perhaps it is this entanglement that enables the men of science to lead their complex lives.

NOTES

1. No specific intelligence tests were administered to the group because no test has sufficient spread in the top ranges for so superior a group. Therefore, these classifications were approximated from the form-level ratings on the Rorschach Test.

2. The Dutch scholar, Johan Huizinga, takes issue in *Homo Ludens,* London: Routledge & Kegan Paul, Ltd., 1949, with the oft-repeated notion that science is merely a game. He has shown that scientific work cannot be subsumed under the definition of play, which occurs within certain limits of space, time, and meaning according to fixed rules. The rules of science, by contrast, are not unchallenged for all time, but are constantly being reformulated. Science also has outside contacts with reality, has purposefulness in its relation to that reality, and is sustained by more than mere pleasure. However, within the closed precincts of its method there are certain parallels: the scientists' continued penchant for system tends in the direcion of play, as do the capriciousness and manipulation within the system, and the competition. Play, like science, is far from random, and reaches a very high degree of order in certain circumstances.

3. Max Weber has put this idea poetically: ". . . whoever lacks the capacity to put on blinders, so to speak, and to come up to the idea that the fate of his soul depends upon whether or not he makes a correct conjecture at this passage of this manuscript, may as well stay away from science. He will never have what one may call the 'personal experience' of science. Without this strange intoxication, ridiculed by every outsider; without this passion, this 'thousands of years must pass before you enter into life, and thousands more wait in silence'—according to whether or not you succeed in making this conjecture; without this you have *no* calling for science and should do something else. For nothing is worthy of man as man unless he can pursue it with passionate devotion." His whole essay, "Science as Vocation" (in H. H. Gerth and C. W. Mills, *From Max Weber,* New York: Oxford University Press, 1958) is a fascinating presentation of "the inward calling for science."

4. For two interesting biographies of the man who has been called America's only truly great scientist, see Muriel Rukeyser, *Willard Gibbs,* New York: Doubleday and Company, 1947, and L. P. Wheeler, *Josiah Willard Gibbs: The History of a Great Mind,* New Haven, Connecticut: Yale University Press, 1952, Rev. ed.

5. G. Klein and his team have conducted some interesting studies distinguishing individuals according to their various perceptual or cognitive styles or "ways" in which their minds work. They find there are individuals who are "levelers," e.g., those who tend to ignore differentiations, when confronted with certain perceptual stimuli, as compared to "sharpeners," those who make differentiations. Their studies on "focusing," which relate to how easily people can integrate or dissociate themselves from the distracting or intersensory effects of competitive stimuli, hint that there is more here than simple cognitive processes related to making or avoiding distinctions, or success in cutting out distractions. These "styles" seem to be related to highly generalized, deeply ingrained attributes which are closely related to modes of defense.
6. EDITORS' NOTE: Wallace Hall, in a personal communication (1971) has pointed out the cultural acceptance of neurotic behavior as it relates to scientists as well as more generally is an important point. "There are a number of instances in the literature of the culture's rewarding certain neurotic behaviors so that the individuals in a particular culture or subculture predisposed to certain neurotic behavior will not exhibit the usual symptoms or anxiety if that behavior receives cultural sanction. The Hutterites, for example, reward compulsive behavior—it is part of the cultural norm—so that it is never looked upon as neurotic, nor does this particular neurosis ever appear among the Hutterites. What about scientists? That which might elsewhere be labeled obsessive-compulsive behavior is almost universally regarded as a required characteristic in scientific research—along with other more socially desirable traits to be sure. It is not necessarily most salient or even present in highly creative scientists, but it seems to be required in day-to-day routine, in experimental work, and in gathering and analyzing data." See also pp. 171 ff. in the original book from which this article was drawn.
7. In *Science and the Social Order,* London: George Allen and Unwin, 1953, Bernard Barber has pointed out that the morality of the scientist differs from the general morality of a liberal society in two ways: (1) the value of commonality, whereby everyone can share past knowledge for everyone is expected to contribute potentially to the future; (2) the value of disinterestedness or other-orientation, to use Parsons' word. Although the scientist's notion of success is directed toward him personally, success is sought by enjoining him to serve himself by serving others. This is partly true in liberal society where people are expected to make some contribution to the general good.
8. In P. Federn, *Ego psychology and the Psychoses,* New York: Basic Books, 1952. See especially Chapter 16, "On the Distinction between Healthy and Pathological Narcissism," pp. 323–364.

Explorations in Typology

Morris I. Stein

TYPES BASED ON SELF-IMAGES

Because of the potential significance a knowledge of types has for both research and theory but with full awareness that types are out of fashion, this exploration was begun. Its aim was to learn whether a typological system based on self-images could satisfy, at least initially, some minimal criteria and whether it could be useful in illuminating some problems that are encountered in applying psychological knowledge. Among the minimal criteria for useful types were the following: they should be internally meaningful and consistent; they should be relatively independent of each other; they should be differentially retated to other criteria; and they should have different developmental histories. Assessment methodology was selected to explore the usefulness of types.

The types are based on . . . a group of 116 Ph.D. chemists employed in industrial research organizations.[1] . . .

The group of 116 chemists, who were in their mid-thirties and came from three different industrial organizations, participated in a study of creativity. They were divided into three categories. One was a group of men regarded as "more creative" by virtue of the ratings they obtained from their superiors, peers,

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[1] EDITORS' NOTE: The original article also contains data on a second population, a group of eighty Peace Corps volunteers.

and subordinates. The second was a group of "less creative" men selected by the same procedure. Both groups represented approximately the upper and lower 20 per cent of the available chemists in their organizations. There was a "middle group" which remained undefined, and this makes up the third subgroup. . . .

The method for obtaining the information on which the types are based is a self-description questionnaire, consisting of twenty paragraphs, each describing the manifestations of a different need after Murray. It includes the following needs: abasement, achievement, affiliation, aggression, autonomy, blamavoidance, counteraction, defendance, deference, dominance, exhibition, harmavoidance, infavoidance, nurturance, order, play, rejection, sentience, sex, and succorance.[2] The test was initially developed for use with the chemists, and the needs were selected because they were regarded as potential inhibitors or facilitators of creative activity. In responding to the questionnaire, the subject is asked to rank the needs from 1 to 20, from the one which is most descriptive of himself (rank of 1) to the one which is least descriptive (rank of 20). . . .

The potential significance of the questionnaire is based on the assumption that the picture an individual has of himself will have an effect on how he will behave. It also assumes that the twenty different needs are shared by all individuals to a greater or lesser extent and that the needs may be manifest in a variety of ways. It is further assumed that individuals vary in their need hierarchy. Theoretically, there are many ways of ranking the twenty paragraphs and many patterns are possible, although I shall consider only the five types found in the data for the industrial research chemists.

. . . The individual rankings of all subjects were intercorrelated . . . and . . . Q-analyses were computed. The principal components of each of the intercorrelation matrices were extracted and rotated via the Varimax method to yield simple structures. The data for the chemists yielded a five-factor solution. . . . To arrive at a picture of the types, subjects who loaded highest on each of the factors were selected as type definers, and their mean ranking of the needs was used in establishing the need hierarchy for the type. In the chemist population 78 out of the 116 men were type-

Table 1. Need Hierarchies among the Five Types Found in the Chemist Population

Type A	*Type B*	*Type C*	*Type D*	*Type E*
Achievement	Affiliation	Achievement	Achievement	Achievement
Affiliation	Blamavoidance	Counteraction	Affiliation	Counteraction
Play	Counteraction	Autonomy	Counteraction	Affiliation
Counteraction	Order	Aggression	Order	Autonomy
Sentience	Achievement	Dominance	Nurturance	Nurturance
Dominance	Deference	Defendance	Defendance	Order
Order	Infavoidance	Rejection	Dominance	Sentience
Exhibition	Harmavoidance	Order	Exhibition	Sex
Autonomy	Nurturance	Affiliation	Autonomy	Succorance
Sex	Play	Sex	Deference	Deference
Deference	Defendance	Harmavoidance	Aggression	Dominance
Nurturance	Autonomy	Infavoidance	Play	Defendance
Harmavoidance	Abasement	Play	Harmavoidance	Infavoidance
Defendance	Sentience	Exhibition	Succorance	Blamavoidance
[Aggression][a]	Sex	Blamavoidance	Infavoidance	Play
[Infavoidance]	Succorance	Sentience	Blamavoidance	Rejection
Rejection	Exhibition	Succorance	Sentience	Aggression
Blamavoidance	Rejection	Deference	Rejection	Abasement
Succorance	Dominance	Nurturance	Abasement	Harmavoidance
Abasement	Aggression	Abasement	Sex	Exhibition

[a] The bracketed needs represent ties.

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[2] The descriptive paragraph for need abasement, as an example, is: "I passively submit to external forces. I accept injury, blame, criticism, and punishment. I surrender. I am resigned to fate. I admit my inferiorities, errors, wrong-doings, or defeats. I blame myself."

definers (26 for Type A, 16 for Type B, 13 each for Types C and D, and 10 for Type E). . . . Since the order in which the needs appear in Table 1 . . . is based on the absolute values of the needs within types, one must be cautioned that in comparing types a need may appear in the same ordinal position, although its absolute value may be different in each instance.

. . . Eighteen needs[3] were found to differentiate significantly among the types by analysis of variance. To condense the data and to investigate which of the needs contribute most to the uniqueness of the type, the average ranking of a need in one type was compared with its average ranking in all other types, for a total of thirty-six comparisons. Only those needs were then retained for which there were differences of one standard deviation or more in twelve or more comparisons. This analysis yielded twelve critical needs: nurturance, deference, autonomy, aggression, blamavoidance, sentience, play, sex, dominance, achievement, exhibition, and succorance. The intercorrelations of the ranks for each of the types were calculated to investigate the degree of resemblance between types both within and between populations. Within population there is a great deal of independence. . . .

TYPES AMONG INDUSTRIAL CHEMISTS

The type descriptions focus on the dynamic interrelationships between the needs, but they should be regarded with caution, for at the moment they are tentative. Complete and thorough descriptions will be possible only after more intensive study. It should be remembered, furthermore, that the type descriptions are based on self-descriptive data and hence refer to self-images; whether and to what extent these self-images are consistent with other types of personality data is an issue that will not be dealt with here. In writing these descriptions we were, however, aided by some knowledge of several individuals who were type-definers and by experience with the questionnaire administered to persons who had also taken rather complete batteries of clinical tests. To avoid the problem of "freezing" their identities in terms that might become clichés, the types are not named. As has been indicated above, the types found among the chemists are identified by letters, and those among the Peace Corps volunteers by Roman numerals.

Type A

The individuals who compose this type are achievement-oriented. But their ambitions and aspirations are not so intense that they overlook interpersonal relationships. They like to be with others and enjoy cooperating with them. They trust others and in turn are loyal to their friends. Although they may want to please others and win their affection, they are not inclined to be submissive nor are they inclined to avoid situations in which they might lose the love of others. Although their social relationships are obviously not without warmth, they are not likely to become involved with others by showing intense sympathy, nor is it very characteristic of this group to go out of its way to support or comfort others. When the occasion demands, members of this type can be critical of others without feeling that they have to be unduly sensitive to others' feelings. By the same token, members of this type do not look to other people for advice, guidance, and emotional support. Indeed, this type ranks need succorance lower than do any of our five other types.[4] Although it is uncharacteristic of individuals of this type to submit passively to external forces and to surrender or resign to fate, they are not insistent upon expressing their autonomous strivings. They can accept restrictions and probably effect compromises. Among the other characteristics of persons in this group is the fact that they appear capable of accepting their libidinal strivings, and that their aggressive needs appear to be under integrative control. Unlike our other types of creative individuals but like Type IV among the Peace Corps population, the men of Type A can do things for fun and without purpose. They enjoy play and relaxation. They like to

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[3] EDITORS' NOTE: These eighteen were obtained from analyses of data for both the chemist and Peace Corps populations. The latter is not considered here but it should be said that it yielded four types.

[4] In a description of a type, the term "lower" refers to a rank that is closer to the least descriptive end of the continuum (toward a rank of 20) and the term "higher" refers to the most descriptive end of the continuum (toward a rank of 1).

laugh, they are easygoing, and can be lighthearted and merry. These men also seek out and enjoy sensuous impressions and aesthetic feelings. Possibly it is this combination of need play and need sentience which gives the impression that members of this type may be characterized as open to new experiences. They can selectively filtrate the most important factors from these experiences and so maintain mastery and control of their environments in an orderly and organized fashion which is more flexible than constricting. All this is not to say that members of this type are without anxieties. Although the sources of their anxieties are not clear and although they may not experience very intense anxiety, it is conceivable that they may become anxious when their freedom is interfered with or when they find themselves lacking in resources to solve the problems that confront them.

Type B

The men in this group find it most important to please others and win their affection. Such behavior comes at the cost of their own spontaneity. They are so insecure that they will seek out many relationships and not be discriminating in their choice of friends. They will avoid situations in which they might lose others' love or in which they might be blamed for their actions. They are not likely to be assertive, forceful, or severe with others. Indeed, they have difficulty in dealing with their aggressive feelings. This type ranks blamavoidance higher and aggression and dominance lower than any of the other eight types. They strive to be inoffensive by avoiding hurting other people's feelings and by striving to overcome their own weaknesses. In social relationships they are likely to appear apprehensive and inhibited. Although they try to maintain their self-respect at a high level, they seem to be lacking in internal sources of evaluation. They probably function best in well-structured situations where they can tell what is required of them. These requirements they will try to fulfill in a reasonable but not necessarily outstanding fashion, for their drive to succeed is not very strong. Nor are they likely to stray far from what they know, since doing so might expose their inadequacies and possibly make them vulnerable to attack.

Type C

This type, like Type A, is also achievement-oriented. But, unlike a man in Type A who combines achievement with affiliation and play, a man in Type C is more driven, and his ambition has a more hostile quality about it. He ranks aggression higher than do any of the other eight types. Achievement may be so important to him that, when he fails in an undertaking, he returns to master it and so to demonstrate that he has few, if any, weaknesses or that he will not allow fears to stand in his way. In this sense, he may be both counteractive and counterphobic. He will be inclined to be defensive, on his guard against criticism, and argumentative in support of his own position. He is likely to perceive others as obstacles to be removed, ignored, or surpassed. He is critical and discriminating in his choice of friends, and he may well be inclined to be snobbish, disgusted, and bored with other people, rather than comforting and supportive. He ranks need nurturance lower than any of the eight other types and therefore is likely to reject others whom he regards as inferior. Because his autonomous needs are high, he is not one to bow to custom or conform to tradition. He likes to dominate situations, and he prides himself on being free and acting according to impulse. The impulses he expresses are likely to be aggressive ones. He is prone to conflict with his superiors, for he does not necessarily respect them; and he is unlikely to fulfill their requests unless he believes they foster his own ends. In this regard he is more extreme than our other types, for he ranks need deference lower than all of them. As one studies the hierarchy of needs within this type, one cannot help but gain the impression that men in this type are compensating for inferiorities and weaknesses.

Type D

The first three needs of this type are achievement, affiliation, and counteraction. Unlike Type C, however, the achievement of a man in this type is not colored by intense personal pressures; and unlike Type A, his achievement is not characterized by playfulness or enjoyment. Indeed, this type is not inclined to express his libidinous strivings. He ranks need sentience and need sex lower than do the other . . . types. For a man in this group, there seems to be a moral commitment to work. In his work as in his other habits he is likely to be neat and precise, and he enjoys arranging and organizing things. Such emphasis on order protects him from criticism and blame. Members of this type do not see themselves as giving in to external forces easily. Rather, they view themselves as controlling their environments just as they control themselves. Men in this type will work together with others in a cooperative enterprise, taking more satisfaction in the efficient accomplishment of a task, however, than in the sharing of feelings. These men are inclined to help others who need their aid. And, on such occasions, one has the impression that their aid stems more from an ethical code than from compassionate feeling. One also has the impression that they have incorporated the values of others rather than finding their own, and those which they have accepted they seek to perpetuate. An individual in this type accepts "the tried and the true," and when he does occasionally venture forth on his own, he does not deviate markedly from the *status quo*. Finally, should anyone frustrate this man's style of life, it is likely that his equanimity will be disturbed and the aggression that was previously channeled into work will be provoked.

Type E

A man of this type is achievement-oriented as are the men in Types A, C, and D. However, whereas Type A has a playful attitude in his achievement orientation and desires positions in which he can be dominant, Type E has little need to be the center of things so long as he is free to do what he wants. In contrast to Type C, who is primarily concerned with his own goals and regards others as sources of frustration, Type E is both more affiliative and more nurturant. And, whereas Type D pursues his goals by checking out the tried and the true, Type E is more inclined to accept his own hunches. Type E differs from all types (including those just mentioned) by placing more emphasis on resisting coercion and restrictions. The members of this type avoid relationships in which they might be dominated; rather, they prefer relationships which are marked by cooperation and trust. They are sympathetic persons; they will help others and not dominate them. They regard themselves as independent and free to act according to their impulses. In satisfying their impulses, however, they will not be exhibitionistic. Indeed, they rank need exhibition lower than do any of the other types. Furthermore, in being independent there is something of a serious or stable cast to their activities, for they rank need play lower than do all of the other types. Although a man in Type E is fairly well organized, the order he achieves does not stem from any attempt to impose structure on his environment, but rather from his capacity to "sense" and "feel" his environment. He enjoys these experiences and does not hesitate to follow his impressions.

These, then, are the five types we found among our 116 chemists. If they are meaningful and useful, then we should say that they differ on a variety of characteristics other than those covered by the twenty needs. Such an investigation is currently under way, but I shall limit myself here to several points relating the types to creativity.

It will be recalled that the 116 chemists were made up of three subgroups. One was a group of "more creative" men (N = 31), the second was a group of "less creative" men (N = 34), and the third or middle group (N = 51) was undefined. Judgments of "more" and "less" creativity were based on ratings obtained from the men's superiors, colleagues, and subordinates. Since our type-definers in the chemist population do not include all persons studied, we also find a decrease in the number of "more" and "less" creative individuals among our type-definers. The data indicate that among the type definers there are twenty-three more creative and twenty-five less creative individuals.

The first question we may ask is how are these persons distributed among the types. We find that more and less creative men appear in all types, which is consistent with the principle of equipotentiality. Of the twenty-three more creative men, 48 per cent are of Type A, 9 per cent of Type B, 34 per cent equally divided between Types C and D, and 9 per cent of Type E. Of the less creative men, 16 per cent are of Type A, 36 per cent of Type B, 12 per cent of Type C, 24 per cent of Type D and 12 per cent of Type E. Thus, the largest proportion of more creative men appears in Type

A, and the largest proportion of less creative men appears in Type B. Type A also yields a larger ratio of more creative to less creative individuals. In all the other types combined there are almost equal proportions of more and less creative subjects ($x^2 = 7.84$, $p <.02$).

In a further analysis of the data, all 116 chemists were categorized by creativity status ("more," "less," and undefined) as well as by their relative loadings on Factors A and B. Analysis of these data indicated that a significantly larger proportion of "more" creative individuals were among those men whose factor loadings were above the median on Factor A and below the median on Factor B, whereas a larger proportion of "less" creative individuals showed the reverse pattern ($x^2 = 18.45$, $p <.01$). Thus, we find a differential relationship between the types and an important aspect of behavior.

Another criterion of the meaningfulness of types is that they should be related to antecedent data and biographical information. There is much biographical information that we have on the types, which will be analyzed in the future. At present, we should like to present data on only one aspect of parent-child interaction for the two types, A and B, on which we have most data.

In attempting to understand how "more creative" individuals develop, we investigated the possibility that they were exposed to complexity early in life. One source of early complexity is the extent to which the subject perceived the mother as inconsistent in relation to himself. It was assumed that an inconsistent mother might be frustrating to the child and that the child, to structure his own environment or to satisfy his needs, would be thrown more onto his own resources than a child reared in a consistent environment; and that this experience of using his own resources would stand him in good stead in future creative work. Obviously, one kind of experience alone would be insufficient to develop a "more creative" adult. Other conditions must also exist, but first it would be necessary to establish the importance of inconsistency.

To gather the necessary data, a questionnaire entitled "Interpersonal Relations in Childhood" was utilized. In this questionnaire subjects were asked to rate on a 7-point scale the degree of consistency or inconsistency they recall having perceived in their mother. The item read, "As a child I felt my mother was," and then the rating was to be indicated on a continuum that ranged from "very consistent" to "very inconsistent."

When the data for all the "more" ($N = 31$) and all the "less" ($N = 34$) creative men among the 116 chemists were analyzed, it was found that the former did indeed regard their mothers as less consistent ($H = 5.04$, $p <.05$). Consequently, if inconsistency of the mother was related to creativity status, it should also differentiate between the Type A and Type B groups. Here we find a trend in the direction of the hypothesis. The Type A group does tend to rate their mothers as less consistent than does the Type B group ($H = 2.65$, $.10 > p > .05$).

It was previously indicated that Types A and B differ in their relationships to creativity status. Let us now ask what is there about the A typological vector that enables individuals who load heavily on this factor to be regarded as "more creative" and what is there about the B typological vector that would limit or restrict creative activity? For the discussion of these relationships it must be borne in mind that both "more" and "less" creative groups conduct their activities in industrial research environments where they must fulfill scientific, professional, administrative, employee, and social roles. Placed in this context it soon becomes apparent that the groups are differently disposed to fulfilling their various roles, and in what follows are several suggestions that will be investigated in the future.

A Type A person, oriented to achievement and willing to cooperate with others, appears well suited to carry out his activities within the organized social system of an industrial laboratory. The fact that he trusts others probably aids him in communication. He can accept information from others and at the same time discuss his own ideas and findings with them, so that he might profit from constructive criticism. At the same time, he is not an abasive individual; he does not submit to others and probably does not passively accept others' ideas, for he has internalized criteria for reacting to and evaluating problems. It is this same lack of submissiveness which is probably involved in his capacity to question existing data and theories. In pursuing the unknown and seeking novel accomplishments he is further aided by the fact that need play is rather high. He can engage in activities that have no immediate purpose and so can break down existing *Gestalten* into their component parts, which he studies to see how they can, through some reintegrative process, be developed into new and useful ideas. In this activity, he is also aided by his aesthetic sensitivity, which enables him to differentiate between the unnecessary or irrelevant and so arrive at more elegant solutions to problems. His behavior is goal-directed. He is not oriented to avoiding situations or being blamed for his actions; he is confident in what he does and not bothered by the ambiguity generated when an existing structure is disrupted. The fact that he is capable of impulse acceptance suggests that internal boundaries are relatively flexible, and thus they may be capable of "regression in service of the ego."[5] He is probably motivated in his search for novelty to demonstrate his mastery and control of his environment, and what he generates in this regard he presents to others in a forceful and masterful fashion. Finally, he can assume positions of leadership where he is responsible for the activities of others.

Type A, characterized by striving and internal freedom which appear well organized and purposeful, seems to be capable of fulfilling the scientific, professional, administrative, and social roles in the industrial research organization. Members of this type would probably have difficulty, however, in fulfilling the employee role.

By contrast to Type A, consider Type B and his capacity to fulfill the role requirements. Type B appears to lack the freedom and spontaneity to disrupt existing products, processes, and ideas. Being more oriented to avoidant behavior, he is likely to find his security in what exists and will not deviate markedly from what confronts him. Consequently, he will encounter problems in fulfilling both the scientific and professional roles. In these roles he will be further incapacitated by the emphasis men of this type place on people rather than objects, theories, and ideas. Both in working on his research and in the fulfillment of the administrative role, he will encounter problems because he finds it difficult to be assertive. He might produce creative products if he were a member of a team in which another individual offered many ideas from which he could select one tenable and worthy of testing. Even under such circumstances he would require reassurance, probably frequently, that what he had done was appropriate. After several such experiences a man of this type might be able to go off on his own, provided he still had a supportive environment. In view of his emphasis on affiliation, his low need rejection, his blamavoidant attitude, and his desire to make up for his shortcomings, a man of this type seems to be better suited for the fulfillment of social and employee roles.

There are too few men in Types C, D, and E to allow for much discussion. However, some speculation, which may lead to hypotheses for future test, is in order. Further study of the men in Type C may indicate that they may have difficulty fitting into an organizational framework. Their needs autonomy, aggression, and dominance may handicap them in relating to others. The intensity of their behavior also suggests that creative members of this type may "burn themselves out before their time." One might also venture the hypothesis that this group, under periods of stress, may experience psychosomatic ailments. These individuals are likely to be more capable of dealing with ideas and thus are better oriented to fulfilling the scientific role, but they will chafe at the bit when they have to fulfill social and employee roles. They will likely "drive" their subordinates and so have problems with the administrative role. And, finally, they may have difficulty in taking orders from their superiors.

Type D, with his emphasis on achievement, affiliation, order, and nurturance, may find the administrative role most to his liking. In addition, he will probably tolerate and not too grudgingly acquiesce to the employee and social roles. But, because his needs dominance and exhibition are high, he hopes not to stay in the confinement of these two roles for very long but to rise in the organization. He is well organized and probably good at translating scientific data to the company client. He lacks, however, the spontaneous and "creative" spark in the fulfillment of scientific and professional roles that is evident in Type A. It may be hy-

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[5] Ernst Kris, *Psychoanalytic Explorations in Art* (New York: International Universities Press, 1952).

pothesized that those of Type D who are regarded as creative have achieved their novel products and processes through problem solving or a trial-and-error approach.

The men of Type E will be aided in fulfilling the scientific and professional roles by their emphasis on achievement, autonomy, and their aesthetic attitude. They will also be able to fulfill the social role but are likely to get too involved with people because their needs affiliation and nurturance are rather high. This consideration would also affect them if they were in administrative positions. However, they may have difficulty in attaining these positions, since they are not very dominant or exhibitionistic. In all likelihood, it may be that they can achieve such positions in research organizations, where their superiors look for people who have valuable ideas but are not likely to push themselves forward.

These, then, are some speculations as to how and why the types may manifest differential effectiveness in fulfilling their roles, and especially why the men of Type A may be better disposed to creative activity in industrial research organizations than are members of other types.

There are several other aspects of the relationship between creativity status and typology that should not be overlooked. As was pointed out previously, the data indicate that there is no single psychological picture of *the* individual who is regarded as more creative; more creative persons appear in all types. Future research may bring to light the different processes these individuals follow in producing novelty. Moreover, being of a type (at least insofar as self-images are concerned) does not guarantee that an individual will produce creative products. The type indicates only that individuals who make it up may possess some of the necessary conditions for creativity. There is much more to be considered about the individual and about the transactions between the individual and his environment before completely accurate predictions can be made.[6]

The data also indicated that *within* a type one finds both "more" and "less" creative individuals. This, again, may be a function of many factors. At the moment, inspection of the similiarities and differences between individuals within a type suggests that the less creative individuals may be positive or negative exaggerations of the critical features of the type, or they may be conflicted about the type they represent. With regard to the matter of conflict, one must bear in mind that the types relate to self-images. Thus, it is conceivable that an individual may try to be of a type of self-image but have difficulty in carrying it off. Stating this point differently, it is suggested that in a "pure" type there is internal dynamic equilibrium. Those who are not of a pure type may be under strain. To investigate this point further, it would be critical to have additional clinical data on the types and especially to investigate the behavior of individuals who are congruent or incongruent with regard to typologies based on both clinical data and self-images.

For a typological system to have significance, as we pointed out earlier, it has to satisfy certain criteria. Thus far, utilizing the data of "more" and "less" creative individuals it has been possible to show that the types are independent and meaningfully related to significant aspects of behavior. For at least two of them it was also pointed out that they tend to differ on a critical developmental variable. Finally, several hypotheses were suggested that require further investigation.

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[6] Morris I. Stein, "A Transactional Approach to Creativity," in C. W. Taylor, ed., *The 1955 University of Utah Research Conference on the Identification of Creative Scientific Talent* (Salt Lake City: University of Utah Press, 1956), pp. 171–181.

PART THREE / MECHANISM OF CAREER CHOICE

A. Theoretical Postulations

Introduction

A loose developmental conceptual framework exists in the field of vocational choice. Within this framework movement into a work role has been regarded generally as a formal expression of a number of precursory early experiences, talents, interests, and values (Borow, 1966). Other characteristics, e.g., identification with parents, leading to choice have been studied either as isolated variables or as clusters of variables, and possible mechanisms by which such variables shape selection of an occupation have been postulated (Taylor and Ellison, in Section II A; Roe, in this section).

Perhaps one of the most well-known theoretical conceptions of occupational choice is that of Roe. Roe's speculation that certain affective relationships between children and parents in formative years were instrumental in directing them toward a person-oriented as compared to a nonperson-oriented vocation, led her to classify vocations into such dimensions, as well as into levels of competence involved. Brunkan's study is one of the many (e.g., Crites, 1962; Green and Parker, 1965) generally unsuccessful attempts to validate this hypothesis. It shows the difficulties of working with a multidimensional criterion.

Some investigators have adopted more traditional psychoanalytic formulations; they see choice of career as an expression of certain defense preferences and strength of impulses for efficient control of basic anxieties and conflict (Bordin, Nachmann and Siegel, in this section; Nachmann, 1960). Bordin's group proposes a framework which attempts to set up a series of dimensions, traceable to infant physiological functions, which will account for all the types of gratifications that work can offer. Any occupation can be described in terms of the relative strengths of these component dimensions and their relation to a series of modifying characteristics. More ego-oriented psychoanalytic theorists add the variable of cognitive interest or investment in primary, as compared to secondary, thinking processes. Bush, in this vein, attempts to examine and clarify some of the psychoanalytic constructs frequently employed in explaining the creative functioning of the scientist. Particular attention is given to the use of regression as an explanatory concept. Bush believes that regression, be it in the service of the ego or otherwise, is not a necessary or sufficient condition for scientific creativity, although it is often overemphasized while the creative use of secondary-process functioning is neglected. It should be noted that nonanalytic personality theorists (e.g., Holland, in Section III B) have also stressed the relationship of personality type to vocational choice.

The Super selection is a classic article in the theory of vocational development. It begins with a critique of the theory of vocational development presented by Ginzberg, Ginsburg, Axelrad and Herma (1951). Ginzberg hypothesized three periods of occupational choice: fantasy choice, tentative choice, and realistic choice. He believes that occupational choice is a developmental process, at least 10 years in duration, and that it is largely irreversible, ending with a compromise between interests, capacities, values, and opportunities. According to Super, Ginzberg did not adequately build on previous research, define his terms, or describe the compromise process by which choice is made. Super assumes that most persons have qualifications to fill a number of niches and that most occupational expectancies are broadly enough defined to accommodate persons with a certain range of characteristics. Occupational choice is a continual process involving a number of life stages with the final establishment of the career being attributed to the interaction of the individual with his environment. Super stresses identification with parents, as does Roe, and the resulting self-concept as a prominent mechanism in directing choice. Vocational development is seen as a compromise between the self-concept and reality, worked out by role-playing in both fantasy and real-life situations.

A few developmental theorists (Rodgers, and Tiedeman and O'Hara, 1963) have discussed changes in family and occupational roles over time, emphasizing the internalized process by which cumulative sets of norms, built up through the family life cycle, ultimately define choice of occupation. Rodgers' model for analysis, based on the developmental framework, views the developing occupational role in terms of a "flow" during which three basic sets of inputs (limiting factors, family inputs and societal inputs) are injected at various periods through the life cycle. These inputs are significant to the degree that they have influence upon the family roles played in the socialization process in reciprocity to the roles which compose the role cluster of the occupant of the child position in the family of orientation.

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Early Determinants of Vocational Choice

Anne Roe

This paper suggests some hypotheses about the relationships between early experience and attitudes, abilities, interests, and other personality factors which affect the ultimate vocational selection of the individual. Although the writer has drawn heavily upon the general literature, as well as some of the psychoanalytical studies, upon studies of early interest patterns, of parent-child relations, and of personality differences related to parent attitudes and to birth order, data from individual studies are not quoted. This is a speculative paper, and there is little *direct* evidence for the hypotheses which are suggested. However, the writer does not know of any contradictory evidence and believes most of these hypotheses would be relatively easy to check. In a paper of this length only an outline can be given.

These hypotheses have been developed with reference to the present United States culture, including the major variations due to gross socioeconomic subdivisions, but the author has not tried to consider alterations which might be introduced by minority positions of one sort or another. Differences between gross cultural subdivisions are primarily differences in percentages of incidence of types of behavior, rather than absolute differences in kind of behavior, and are analogous to the differences in incidence of different bloodgroups in different races. It is to be understood that these hypotheses are intended to indicate major trends, and that other variables not mentioned here can be expected to introduce modifications in specific instances.

Let us first consider some general hypotheses with regard to personality variables as these are expressed in behavior, and particularly in behavior of the sorts that psychologists concern themselves with, for example, intelligence, interests, and special abilities. Some of the individual variation in all of these is undoubtedly due to inheritance, to differences in genetic endowment, but of the extent and precise nature of these genetic differences we know almost nothing certainly. We not only know nothing about probable genetic differences in the strengths of basic needs or drives, but we have not even begun to consider this problem. Gross hereditary differences in such things as specific sensory capacities and the plasticity and complexity of the central nervous system must greatly affect behavior, but beyond these it is uncertain how far specifically genetic elements are primary factors.

In this connection the author offers five hypotheses.

HYPOTHESES ON RELATION OF EARLY EXPERIENCE TO VOCATIONAL CHOICE

1. *The hereditary bases for intelligence, special abilities, interests, attitudes, and other personality variables seem usually to be nonspecific.* There may be a genetic basis for some "factors" of intelligence or aptitudes, but on this there is no clear evidence. Sex, as genetically determined, also involves some differentiation of abilities. It is, nevertheless, probable that in most instances genetic elements limit the degree of development rather than directly determine the type of expression.

2. *The pattern of development of special abilities is primarily determined by the directions in which psychic energy comes to be expended involuntarily.* The statement applies also to interests, attitudes, and other personality variables. Please note the word *involuntarily.* It is intended to emphasize the fact that the things to which the individual gives automatic attention are keys to his total behavior. The point will not be expanded here, but the relevance of these hypotheses to the relations between personality and perception is clear.

3. *These directions are determined in the first place by the patterning of early satisfactions and frustrations.* This is the developing pattern of need primacies or relative strengths. In the earliest years these are essentially unconscious, and they probably always retain a large unconscious element. As noted before,

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we know nothing at all about genetic variability in basic needs, but it can be fairly assumed that it exists.

Maslow's hierarchical classification of needs is the most useful for focussing the present discussion (Table 1).

Table 1. Basic Needs (Maslow)

1. Physiological needs
2. Safety needs
3. Need for belongingness and love
4. Need for importance, respect, self-esteem, independence
5. Need for information
6. Need for understanding
7. Need for beauty
8. Need for self-actualization*

* The author would place this lower in the hierarchy or handle it as a more generalized need.

The hierarchical arrangement is important. Maslow's theory states that higher-order needs cannot appear until lower order needs are at least relatively well satisfied. It seems reasonable to assume that higher order needs are of later evolutionary development in man and some of them may not be well established in terms of species evolution. If this is so, it would follow that they would show greater variability within the species. Lower order needs, on the other hand, are essential for the maintenance of life, and this permits much less variability in their strength. Differences in the degree of variability of these needs are of significance for us, and it is particularly the higher needs with which we are concerned. It would also appear that there is some difference in the age at which these needs or drives may begin to function. By the time the healthy child is a few months old the first five are probably affecting his behavior, although in widely varying degree.

4. *The eventual pattern of psychic energies, in terms of attention directedness, is the major determinant of the field or fields to which the person will apply himself.* This is relevant not only to vocation, of course, but to the total life pattern of the individual. It determines what sort of special abilities and interests will be predominant.

5. *The intensity of these (primarily) unconscious needs, as well as their organization, is the major determinant of the degree of motivation as expressed in accomplishment.* This implies that all accomplishment is based on unconscious as well as on conscious needs, but it does not imply that these needs are necessarily neurotic. There is accomplishment which is a free expression of capacity, although this may be relatively rare. Accomplishment on this basis can generally be distinguished from accomplishment on other bases. The relevance of this hypothesis to eventual vocational performance is evident.

It may not be so evident how the patterns and intensities of these basic needs are affected in the first place by the early experiences of the child. The following three hypotheses are concerned with this problem.

6. *Needs satisfied routinely as they appear do not develop into unconscious motivators.* Intensity of the need is not a variable, since it is stated that the need is "satisfied." The fact that the satisfaction is gained routinely is important, and it implies the need to distinguish sharply between simple, direct, matter-of-fact need gratification and gratification with fuss and fanfare.

7. *Needs for which even minimum satisfaction is rarely achieved will, if higher order, become in effect expunged, or will, if lower order, prevent the appearance of higher order needs, and will become dominant and restricting motivators.* Lower order needs, of course, require some degree of satisfaction for the maintenance of life. The hypothesis would mean, e.g. that a child whose expressions of natural curiosity were thoroughly blocked, would cease to be curious. On the other hand, with less effective blocking, hypothesis 8 would apply.

8. *Needs, the satisfaction of which is delayed but eventually accomplished, will become unconscious motivators, depending largely upon the degree of satisfaction felt. This will depend, among other things, upon the strength of the basic need in the given individual, the length of time elapsing between arousal and satisfaction, and the values ascribed to the satisfaction of this need in the immediate environment.*

The last hypothesis is the most significant for this study. It must be understood that the forms in which need satisfaction will be ultimately sought, in adult life, may not be obviously related to the basic needs referred to in the hypothesis. All of the well-known mechanisms of displacement, projection, etc. may function here. The problem of tolerance of deferred gratification is linked to such experiences as are implied in this hypothesis.

PATTERNS OF EARLY EXPERIENCE WITH PARENTS

Let us turn now to variations in the early experiences of children, and in particular to differences in parental handling of children. We can consider only major variations here, and it must be understood that the classification used is an arbitrary one, intended to delimit, as usefully as possible, nodal areas in a series of essentially continuous distributions. Several levels of classification are suggested, overlapping variously. Major behavioral variations are presented in outline form below, and a figure shows these, together with their relation to basically warm and cold attitudes of parents, and their relation to the outcome in the child in terms of his orientation with regard to persons.

The specific behaviors of the parents are of less importance than their attitudes towards the child. It is impossible here to discuss the relative effect of maternal and paternal attitudes, of similarities or differences in them. The classification used here refers to the dominant pattern in the home, whether shown by one or both parents. The major subdivisions refer to the child's position in the family emotional structure: as the center of attention, as avoided, or as accepted.

A. Emotional Concentration on the Child

This ranges between the extreme of overprotection to that of overdemandingness. Perhaps a sort of mean between these two is the quite typical anxiety of parents over a first child, anxiety which, in the same parents, may be much alleviated for the second child, with resulting considerable differences in the personality pictures of the two children.

1. *Overprotection.* The parent babies the child, encourages its dependence and restricts exploratory behavior. There is often concentration upon physical characteristics and real or fancied "talents" of the child. The parents maintain primary emotional ties with the child.

2. *Overdemanding.* The parents make heavy demands upon the child in terms of perfection of performance and usually institute quite severe training. In later years they may push the child to high achievement in school and work. In somewhat milder forms we may have the sort of family status "noblesse oblige" pattern, in which development of skills is encouraged but the pattern of skills is a prescribed one. This is very typical of upper class families, with emphasis upon development of conceptual as opposed to motor skills. Severer forms may blend into rejection or may be cover for this.

B. Avoidance of the Child

Here, too, two extremes are suggested—rejection and neglect. Care below the minimum adequate amount has well-documented effects, as studies of orphans have shown. Most other studies have few, if any, children in this group. Parents providing this sort of home do not cooperate in psychological studies. (The author would not suggest that non-cooperation is evidence of this type of care!) Minimal need gratification is provided.

1. *Emotional rejection of the child,* not necessarily accompanied by overt physical neglect. Lack of gratifications is intentional.

2. *Neglect of the child.* This may, in fact, be less harmful psychologically than emotional rejection accompanied by physical care. It shades into the next classification. Gratification lacks are generally not intentional.

C. Acceptance of the Child

Children in this group are full-fledged members of the family circle, neither concentrated upon, nor overlooked. Parents are noncoercive, nonrestrictive, and, actively or by default, encourage independence. The minimum amount of social interaction is supplied at one extreme (this may be very low) and at the other extreme the group approaches the overprotecting one. The major breakdown in this group is on the basis of the warmth or coldness of the family climate.

1. *Casual acceptance of the child.* Noninterference here is largely by default.

2. *Loving acceptance.* Noninterference

and encouragement of the child's own resources and his independence may be intentional, even planned, or a natural reflection of parental attitudes towards others generally.

RELATION OF PARENTAL ATTITUDES AND NEED SATISFACTION

Homes in which children are the center of attention provide pretty full satisfaction of physiological and safety needs, and attention to needs for love and esteem, but gratification is usually not entirely routine. The overprotecting home places great emphasis upon gratification, and generally upon immediacy of gratification, which keeps lower level need satisfaction in the foreground. Belongingness, love, and esteem are often made conditional upon dependency and conformity, and genuine self-actualization may be discouraged. There is likely to be encouragement of any sort of any special or supposedly special capacities, however. The overdemanding parent may make satisfaction of needs for love and esteem conditional upon conformity and achievement, which is frequently oriented to status. Needs for information and understanding may be encouraged, but within prescribed areas, and the same is true for self-actualization needs.

By definition, the next group has major lacks in need gratification. Rejecting parents may provide adequate gratification of physiological and safety needs, but refrain from love and esteem gratification, and frequently seem deliberately to withhold the latter or even to denigrate the child. Neglect of physiological and safety needs, but not beyond necessary minimal gratification is much more tolerable than personal depreciation and deliberate withholding of love. If there is no contrast with attitudes towards others in the immediate group there will be stultification of the child's development in some respects but not distortion of it.

Accepting parents offer reasonable gratification of all needs. This is unlikely to be emphasized in the way in which the first group do it, although the extremes of the loving subgroup may tend in this direction. Gratifications will not be deliberately delayed, but neither will delay be made disturbing. The major difference in the subgroups is probably in the way in which gratifications are supplied, and in the degree of deliberate encouragement and gratification of needs.

PARENTAL HANDLING AND ADULT BEHAVIOR PATTERNS

It has been suggested before that perhaps the earliest subdivision of direction of attention, and one which has significance for the whole life pattern of the individual, is that referring to persons, and that this may be towards persons or towards nonpersons. The author does not say towards persons or away from persons, since away from persons may imply defensiveness; the term *object* is avoided since attention may go to animate or inanimate nonpersons, and because object, in psychoanalytic terminology usually comes out to mean other person. Perhaps primary attention to self should be a separate division, or a subdivision of attention to persons. (The exclusiveness with which one of these attitudes dominates the attention of any individual is, of course, another variable, not taken account here.)

Possible relationships between these orientations and parent-child interaction are suggested in Figure 1. The next to the outer segment of the circle indicates the probable orientation of the child in terms of persons or nonpersons. The division is suggested by jagged lines, since it is uncertain. The other subdivions were set arbitrarily.

This basic orientation with respect to persons later ramifies into patterns of special interests and abilities. The degree of social interests is clearly related, and it is likely that verbal abilities are associated with this, since personal interactions are so largely mediated through words. Scientific and mechanical interests reach their fullest development in those who are concerned with nonpersons.

REFLECTION OF EARLY EXPERIENCE IN VOCATIONAL CHOICE

Depending upon which of the above situations are experienced, there will be developed basic attitudes, interests and capacities which will be given expression in the general pattern of the adult's life, in his personal relations, in his emotional reactions, in his activities, and in his vocational choice. More than any other aspect of life, the occupation usually reflects

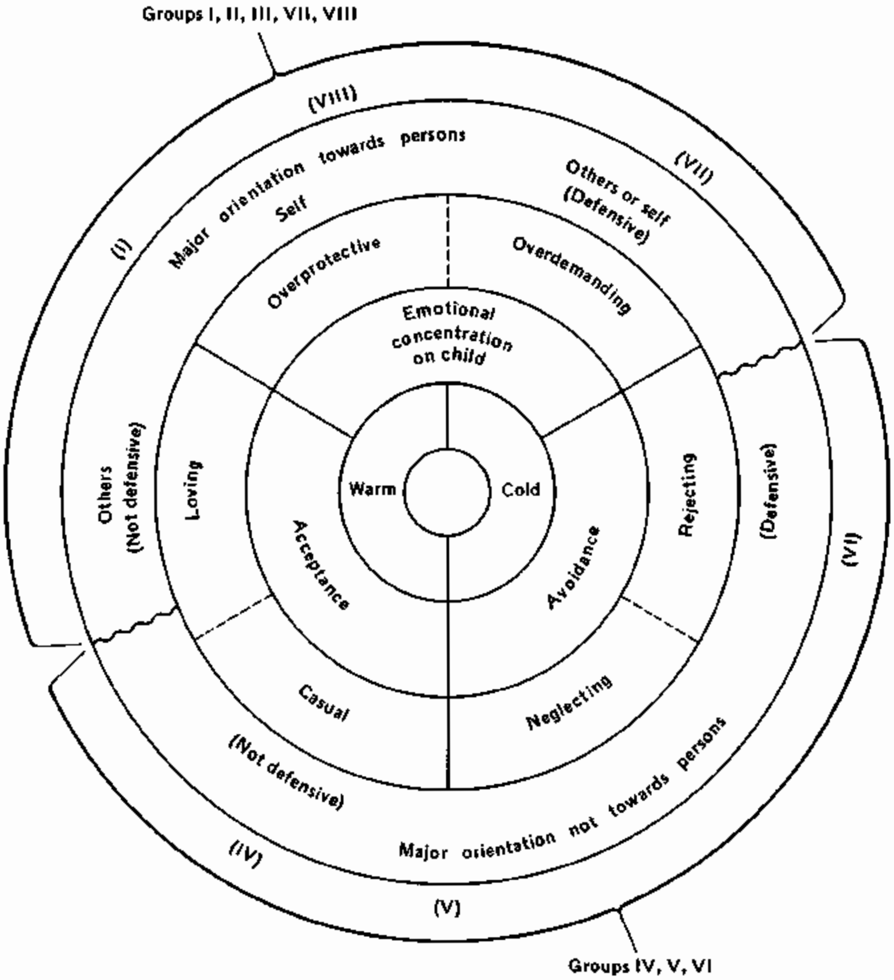

Figure 1

most clearly the coalescence of the genetic and experiential variables discussed above.

Persons from child-centered families who do not develop primary self-concentration will still be quite constantly aware of the opinions and attitudes of other persons towards themselves and of the need to maintain self-position in relation to others.

Persons brought up in rejecting homes may develop intense defensive awareness of others; if so they will probably have aggressive tendencies which may most readily find socially acceptable expression in occupational terms. On the other hand, they may strongly reject persons and turn defensively to nonpersons, or they may be unaware of other persons as different from objects in the environment, so far as their own relation to them goes.

Those from accepting homes may have primary interests in persons or in nonpersons; it will not be defensive in either case, and it will not carry the sort of uncertainty that many in the first group show.

It is possible to relate these attitudes quite directly to occupational choice (Roe, 1). The major occupational groups discussed below can, however, be thought of as indicating general patterns rather than specific occupational groups. This strengthens the general theory. . . .

This classification has two dimensions: focus of activity and level at which the activity is pursued. The categories are shown in Table 2.

Relationships between group categorization and early experience are suggested in the outer section of Figure 1.

Although most of this discussion refers to

Table 2. Categories in Roe Classification of Occupations

Groups		*Levels*
I.	Service	1. Professional and managerial 1
II.	Business contract	2. Professional and managerial 2
III.	Organizations	3. Semiprofessional, small business
IV.	Technology	4. Skilled
V.	Outdoor	5. Semiskilled
VI.	Science	6. Unskilled
VII.	General cultural	
VIII.	Arts and entertainment	

Group rather than Level, Hypothesis 5 has relevance to the latter category. This concerns the degree of motivation. Need intensity may affect, within limits due chiefly to socioeconomic background and intelligence, the level at which the mature occupational life is set; it must definitely affect the relative position within that level which the individual attains, and even more specifically the position attained within his own occupational group. Most of those selecting occupations in Groups I, II, VII, and VIII have major orientation towards persons as do many, if not most, in Group III. Groups IV, V, and VI, are chiefly comprised of persons whose major orientation is towards nonpersons. More exact relations cannot be generally indicated. There are, however, some relations which are strongly suggested; these are shown in parentheses in the figure. Indeed, there is some indication that an ordered, counterclockwise arrangement of these groups is not untenable.

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Perceived Parental Attitudes and Parental Identification in Relation to Field of Vocational Choice

Richard J. Brunkan

Several theories of vocational psychology have proposed that parents have an important influence upon their child's choice of a vocational field (Ginzberg, Ginsburg, Axelrad, and Herma, 1951; Roe, 1957; Super, 1957). According to these theories, parents influence their children not only by their *attitudes*, but also through the *identifications* of their children with them. For example, Roe (1957) has hypothesized that the three predominant parental *attitudes* of (1) emotional concentration upon the child, (2) acceptance of the child and (3) avoidance of the child, are related to vocational choices in specific fields. Attempts to test her theory, however, have produced conflicting results. Little support has been found for her hypothesis in investigations using objective measures of parental attitudes (Grigg, 1959; Hagen, 1960; Utton, 1962; Switzer, Grigg, Miller, and Young, 1962). In contrast, closely related propositions about field of choice deduced from psychoanalytic theory have generally been supported by retrospective interview data (Nachmann, 1960; Galinsky, 1962).

With respect to *identification*, both Super (1957) and Ginzberg et al. (1951) offer general theories of vocational choice which cite identification with the parents as a primary determinant of choice. Although considerable research has been done on the relationship between parental identification and occupational variables, most of it has not dealt directly with the *choice* of an occupation. Only Segal (1961) has provided any direct evidence of a relationship between identification and vocational choice. He found signs of a more rigid fearful identification in accounting students, as compared to indications of multiple identifications in creative writing students. Indirect evidence of the role of identification in choice is provided by studies which have found a relationship between parental identification and vocational interests (Crites, 1962; White,

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1959; Stewart, 1958; Steimel, 1960). However, since the correlation between interests and choice is not large enough to permit using the terms interchangeably, further work needs to be done with choice rather than interests as the dependent variable.

Since none of the theories of vocational choice specifies how choice should be measured, several different criteria have been used. One of the more common criteria has been an individual's present occupation, another has been the occupation a person plans to enter (as indicated by his choice of a college major). These could be termed "actual" and "probable" choices, both of which are based upon factors conscious to the individual, such as abilities, interests, finances, educational opportunities, training time, wages and labor market conditions, as well as upon factors which are partially or totally unconscious, such as personal needs, identifications and parental attitudes. Certain conscious or reality factors may determine the majority of actual and probable choices, and in so doing, may mask the influence of factors such as parental attitudes and identifications. If so, the relationship between the parental variables and vocational choice should become evident if vocational choices are used in which the individual is asked to disregard certain reality factors. These might be called "possible" choices if a limited number of reality factors are disregarded, or "fantasy" choices if all reality factors are disregarded. One way of obtaining such choices is to use a questionnaire like Trow's (1941) which asks the individual to state his probable, possible and fantasy vocational choices. Previous investigators have used choices similar to Trow's "probable" choice, but no attempts have been made to investigate the relationships of "possible" or "fantasy" choices to either parental variable. If significant relationships were found between these choices and the parental variables, they could not be interpreted as indicating relationships between these variables and the *occupation* the individual will eventually enter, but they would indicate that the parental variables are factors in choice, and thus, would demonstrate the need for additional research.

Similarly, investigators dealing with identification have considered only identification with the real parent. It is possible in cases were the conditions are disruptive of normal parent-child relationships that a child would substitute for this "real" identification, identification with an ideal parent, i.e., an imaginary individual whom the child develops by combining desirable characteristics of parent figures in his environment. Therefore, this type of identification should also be considered in relation to occupational choice.

PROBLEM

Since research dealing with the relationship of parental attitudes and parental identification to probable vocational choice is inconclusive, and inasmuch as possible and fantasy choices and identification with a parental ideal have not been considered, a number of hypotheses dealing with these relationships were investigated. The hypotheses were specific or general depending upon the availability of theory and research dealing with a particular relationship. It should be noted that the hypotheses pertain only to males.

Hypothesis I: For (1) *probable,* (2) *possible and* (3) *fantasy choices, degree of parental identification differs for the various vocational choice fields, these differences depending upon which parent is being rated and/or upon whether the real or ideal parent is being rated.* Since only degree of identification in relation to field of probable vocational choice has been studied, and this only by Segal (1961), this general hypothesis was investigated.

Hypothesis II: For probable vocational choices, (1) *choices in the Service, General Cultural, and Arts and Entertainment fields are accompanied by a high degree of parental Concentration,* (2) *choices in Technological occupations are accompanied by a high degree of parental Acceptance and* (3) *choices of Scientific careers are accompanied by a high degree of parental Avoidance.* Roe (1957) proposed this tripartite hypothesis in her discussion of early determinants of vocational choice. Since her hypothesis has not been tested (1) with an adequate measure of parental attitudes (2) with vocational choice as the criterion of subgroup membership or (3) with patterns of differential attitudes between parents as the experimental variable, it was investigated in this study. In discussing the relationship between attitudes and choice, Roe does not differentiate between the attitude of fathers and mothers. However, the attitudes of each parent were considered separately here because the overall differences in attitudes of fathers and mothers found by Switzer et al. (1962) suggest that the interaction of these attitudes with choice might also differ from one parent to the other.

Hypothesis III: For (1) *probable,* (2) *possible and* (3) *fantasy choices, perceived parental attitude scores differ for the various vocational choice fields, these differences depending upon which parent is being rated and/or upon which attitude is being considered.* Since choices in the Service, General Culture, and Arts and Entertainment fields were combined into one group in testing hypothesis II, and since that hypothesis dealt only with probable occupational choice, this additional hypothesis was tested as a more comprehensive investigation of a relationship between parental attitudes and the various types of vocational choice. As in hypothesis II, it was expected that the interaction of attitudes and choice would differ from one parent to the other.

PROCEDURE

Measurement of Variables

Parental Identification. Crites (1962) argues that identification encompasses both projection and introjection, and therefore should be assessed with a measure of similarity of perceptions. On the basis of his argument, it was felt that such a measure should be used in this study. Since the Semantic Differential (SD) is the most reliable and thoroughly studied measure of similarity of perceptions (Bieri, Lobeck, and Galinsky, 1959; Lazowick, 1955; Osgood, Suci, and Tannenbaum, 1957), it was chosen to assess identification. Degree of identification, as reflected by the SD, is based on the difference between the S's rating of "self," and his rating of some other significant figure in his environment. In this study, ratings of "father," "mother," "ideal father" and "ideal mother" were used in addition to the S's concept of "self."

Perceived Parental Attitudes. These attitudes were measured by the Family Relations Inventory (FRI), and instrument developed by Brunkan and Crites (1964) to quantify the familial interpersonal orientations which Roe (1957) has proposed are related to vocational choice. The FRI consists of 202 items distributed among six scales: Father Concentration, Father Acceptance, Father Avoidance, Mother Concentration, Mother Acceptance and Mother Avoidance. Thus, it provides a score on each predominant parental attitude for each parent as perceived by the child.

Vocational Choice. Vocational choice was obtained from Trow's (1941) Vocational Choice Inventory (VCI). In response to this inventory, the Ss gave their probable, possible and fantasy choices, or stated that they were undecided. These choices were classified into one of the eight "field" categories of Roe's (1956) two-dimensional occupational classification scheme, or into an "undecided" category.

SUBJECTS

The Ss for the study were 298 sophomore, junior and senior undergraduate males from psychology courses at the University of Iowa. Participation was limited to those who had lived with their father or stepfather until the age of 14. The mean age of the Ss was 20.39 years, and their mean ACT Composite percentile was 63.46.

Design and Analysis

The research instruments were administered to groups of Ss during the latter part of the first semester and the early part of the second semester of the 1962–63 academic year. Before any analyses were carried out, all parental attitude scores were transformed to *T* scores ($M = 50$, $SD = 10$), the raw scores not being comparable since they were obtained on different scales. Since primary interest was in the interactions between the parental and the choice variables, analysis of variance (Lindquist, 1953) was used in processing the data, the .05 probability level considered significant. Initially, Type VI analyses were made of the data relevant to each hypothesis, the between-groups variable being vocational choice fields, and the within-groups variables being the father/mother dichotomy, and type of attitude or type of identification. Separate analyses were made for probable, possible and fantasy choices.

In the second phase of the analysis, the significant effects found in the Type VI analyses were further investigated. The significant interaction of father/mother with real/ideal parent found in the analyses relevant to hypothesis I led to consideration of the differences in degree of identification with the real as compared to the ideal parent of the same sex, as well as a consideration of differences in the degree of identification with the father as compared to the mother for both real and ideal parents.

RESULTS

Hypothesis I

Table 1 presents the means of the groups used in testing this hypothesis, and Table 2 contains the analysis of variance results for

Table 1. Mean Degree of Identification with Real and Ideal Parents for Field of Vocational Choice Subgroups

Field and type of choice	*N*	*Real parent*		*Ideal parent*	
		Father	*Mother*	*Father*	*Mother*
Service					
Probable	19	4.32	3.79	3.27	4.46
Possible	28	4.66	3.82	3.64	4.30
Fantasy	11	4.48	4.62	3.63	4.51
Business contact					
Probable	15	3.33	4.35	3.21	4.14
Possible	6	3.05	3.80	2.84	3.71
Fantasy	(insufficient N)				
Organization					
Probable	53	4.21	4.44	3.64	4.24
Possible	46	3.87	4.22	3.27	3.97
Fantasy	50	4.13	4.43	3.43	4.01
Technology					
Probable	30	3.79	3.99	3.78	4.04
Possible	27	3.96	4.21	3.85	4.49
Fantasy	32	3.94	4.62	3.45	4.31
Outdoor					
Probable	(insufficient N)				
Possible	(insufficient N)				
Fantasy	10	4.53	4.27	3.04	4.34
Science					
Probable	66	3.77	4.58	3.28	4.21
Possible	90	3.82	4.50	3.38	4.15
Fantasy	56	3.83	4.61	3.54	4.40
General cultural					
Probable	72	3.96	4.61	3.54	4.27
Possible	61	3.97	4.74	3.54	4.43
Fantasy	30	4.24	4.64	3.56	4.32
Arts and entertainment					
Probable	11	4.08	4.66	3.66	4.56
Possible	19	4.08	4.91	3.83	4.91
Fantasy	67	4.14	4.54	3.84	4.66
Undecided					
Probable	32	4.17	4.63	3.74	4.59
Possible	20	3.97	4.71	3.87	4.33
Fantasy	23	3.38	3.88	2.90	3.56
Not classifiable					
Probable	(no N)				
Possible	(no N)				
Fantasy	16	2.85	3.70	3.60	3.88
Weighted mean		3.96	4.45	3.52	4.28

Table 2. Analysis of Variance for Field of Vocational Choice, Real/Ideal Parent, and Father/Mother

	Probable choice			*Possible choice*			*Fantasy choice*		
Source	*df*	*MS*	*F*	*df*	*MS*	*F*	*df*	*MS*	*F*
Field of vocational choice (A)	7	3.02	0.48	7	5.87	0.94	8	9.59	1.56
error (between)	290	6.30		289	6.24		286	6.15	
Real/Ideal parent (B)	1	116.25	47.45†	1	115.07	47.28†	1	115.27	46.06†
Father/Mother (C)	1	28.36	21.47†	1	27.56	20.92	1	27.35	21.37†
A × B	7	2.12	0.87	7	3.06	1.26	8	0.61	0.24
A × C	7	0.79	0.60	7	0.93	0.70	8	2.50	1.95
B × C	1	5.77	4.96*	1	5.91	5.09*	1	5.59	4.71*
A × B × C	7	1.73	1.49	7	1.93	1.67	8	1.07	0.90
error 1 (within)	290	2.45		289	2.43		286	2.50	
error 2 (within)	290	1.32		289	1.32		286	1.28	
error 3 (within)	290	1.16		289	1.16		286	1.19	
Total	1191			1187	2.91		1179	2.92	

* Significant at the .05 level.
† Significant at the .001 level.

probable, possible and fantasy choices, respectively. For each of these types of choice, hypothesis I predicted (a) that degree of parental identification would differ for the various fields of occupational choice, and (b) that these differences would depend upon which parent was being rated and/or upon whether the real or ideal parent was being rated. The latter predictions were investigated in each analysis by testing the two- and three-way interactions involving field of occupational choice (A). As indicated in Table 2, none of these interactions proved significant at the .05 level. Thus, the results of each analysis, i.e., for each type of choice, indicate that the degree of identification is the same for the occupational choice subgroups represented, and that this is true for any combination of father/mother and real/ideal parent.

A significant interaction ($p < .05$) was found between real/ideal parent (B) and father/mother (C). This interaction was the same in all three analyses, since the same Ss were represented in each. Furthermore, it was represented by the weighted means in Table 1, i.e., the means for all occupational groups combined, since the triple interaction was nonsignificant. Tests of the differences between these means indicated that degree of identification with the real father differed significantly from that with the ideal father ($t = 3.49$, $p < .01$), but this was not true of identification with the real and ideal mother ($t = 1.33$). They also showed that degree of identification with the father differed significantly from that with the mother for both the real parents ($t = 5.16$, $p < .01$), and the ideal parents ($t = 8.10$, $p < .01$).

Hypothesis II

The means of the groups used in testing this hypothesis are given in Table 3. This hypothesis was based on Roe's prediction that

Table 3. Mean Parental Attitude T Scores for Fields of Choice Subgroups (Test of Roe's Hypothesis)

		Father			*Mother*		
Field of choice	*N*	*Acceptance*	*Concentration*	*Avoidance*	*Acceptance*	*Concentration*	*Avoidance*
Service, general cultural, and arts and entertainment	102	49.93	50.58	50.68	49.95	50.22	51.65
Technology	30	50.23	47.58	49.18	51.21	47.90	48.09
Science	66	50.32	51.23	49.74	51.41	49.97	48.67

Service, General Cultural, and Arts and Entertainment career choices would be accompanied by a high degree of a parental Concentration, Technical occupational choices by a high degree of parental Acceptance, and Scientific choices by a high degree of parental Avoidance. The analysis was made using probable occupational choice and with the choice fields combined into the groups suggested by Roe. In this analysis, the predicted relationships should result in an interaction between field of choice and type of parental attitude. Further, it was predicted that this interaction would differ from one parent to the other, thus producing a triple interaction among the three experimental variables. These predications were investigated by testing the two- and three-way interactions involving field of choice. Analysis of variance revealed that none of these interactions was significant.[1] These results, in conjunction with the nonsignificant main effect of field of choice, indicate that the various occupational choice subgroups do not differ in degree of perceived parental attitudes on any of the attitude scales for either parent. Therefore, neither Roe's hypothesis nor the additional prediction of a parental difference in the interaction of attitudes and choice was supported.

Hypothesis III

Table 4 contains the means for the groups used in testing this hypothesis. This hypothesis, a more comprehensive test of a relationship between perceived parental attitudes and the child's choice of a vocation, predicted that for each type of choice (a) perceived parental attitude scores would differ for the various vocational choice fields, and (b) these differences would depend upon which parent was being rated and/or upon which attitude was being considered. The latter predictions were investigated in each analysis by testing the two- and three-way interactions involving field of choice. Analysis of variance indicated that one of these interactions was significant at the .05 level.[2] The conclusion, for all three types of choice, is that parental attitude scores do not differ for the occupational choice subgroups represented, and that this is true regardless of the parent being considered and the attitude being rated. Thus, a more general hypothesis of a relationship between parental attitudes and vocational choice also was not supported.

DISCUSSION

Hypothesis I

The lack of support for this hypothesis leads to the conclusion that these male Ss, who differ in their vocational choices, do not differ correspondingly in degree of identification with their father or their mother, regardless of whether the real or ideal parent is being considered. Moreover, this conclusion applies to all three types of vocational choice: probable, possible and fantasy.

These results disagree with Segal's (1961) findings that accounting and creative writing students differ in identification. This disagreement could be due to a number of differences in the studies. However, the most significant difference seems to be in measuring instruments. Segal used a projective test, the Rorschach, whereas the present research was based upon an objective paper and pencil rating form. Since there are marked differences in these instruments and their application, it is likely that different aspects of personality and, consequently, of identification are being considered. Thus, it may be that vocational choice is related to identification as assessed by a projective test, but not as reflected in a rating form.

The present findings are also in contrast to the findings of Crites (1962) that kind and degree of identification are related to vocational interests. However, this difference may simply be due to a median correlation between choice and interest in the .40's or .50's, a cor-

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[1] Table A, presenting analysis of variance results for field of probable vocational choice, type of parental attitude, and father/mother, has been deposited with the American Documentation Institute. Order Document No. 8076, remitting $1.25 for 35 mm. microfilm or $1.25 for 6 by 8 in. photocopies.

[2] Table B, presenting analysis of variance results for field of vocational choice, type of parental attitude, and father/mother, has been deposited with the American Documentation Institute. See footnote 1.

Table 4. Mean Parental Attitude T Scores for Fields of Choice Subgroups

		Father			*Mother*		
Field and type of choice	*N*	*Acceptance*	*Concentration*	*Avoidance*	*Acceptance*	*Concentration*	*Avoidance*
Service							
Probable	19	46.92	51.81	55.88	47.98	50.01	54.64
Possible	28	49.21	49.23	52.71	48.56	50.59	51.95
Fantasy	11	46.28	48.52	56.28	41.80	51.80	52.87
Business contact							
Probable	15	51.13	53.45	49.71	51.14	50.39	49.76
Possible	6	52.56	45.68	45.82	51.90	40.98	51.53
Fantasy	(insufficient N)						
Organization							
Probable	53	49.40	48.79	49.36	47.20	50.39	48.97
Possible	46	51.45	47.91	48.94	48.84	49.76	47.81
Fantasy	50	51.86	48.35	47.31	51.65	48.71	48.99
Technology							
Probable	30	50.23	47.58	49.18	51.21	47.90	48.09
Possible	27	50.23	49.78	50.08	49.16	50.74	50.44
Fantasy	32	51.81	49.94	48.70	52.93	50.02	47.11
Outdoor							
Probable	(insufficient N)						
Possible	(insufficient N)						
Fantasy	10	44.78	49.09	54.32	48.81	48.18	48.29
Science							
Probable	66	50.32	51.23	49.74	51.41	49.97	48.67
Possible	90	49.24	50.76	50.34	50.60	49.77	48.97
Fantasy	56	49.89	50.40	50.15	49.34	51.07	50.69
General cultural							
Probable	72	51.17	49.90	49.09	51.43	49.77	51.04
Possible	61	51.06	50.57	49.39	51.58	49.47	51.10
Fantasy	30	47.90	53.54	51.48	48.56	50.94	51.73
Arts and entertainment							
Probable	11	47.00	52.91	52.10	43.70	53.49	50.46
Possible	19	47.09	54.20	51.50	48.38	53.72	53.24
Fantasy	67	49.91	52.08	49.65	49.77	51.90	50.03
Undecided							
Probable	32	49.83	48.17	50.30	50.20	50.54	51.07
Possible	20	49.86	48.38	48.39	50.51	50.66	49.17
Fantasy	23	51.76	48.34	49.25	50.44	46.77	50.24
Not classifiable							
Probable	(no N)						
Possible	(no N)						
Fantasy	16	49.07	44.82	51.28	50.36	46.06	49.73

relation sufficiently small to permit finding a significant relationship between interests and degree of identification, but not between choice and degree of identification.

Interaction of Real/Ideal Parent and Father/Mother. Although not relevant to the hypothesis as such, the significant interaction of real/ideal parent and father/mother does have heuristic value for those interested in identification. The t tests and the weighted means in Table 1 indicate that these male Ss identify more closely (smaller score = closer identification) with their father than their mother on both the real and ideal levels, and also that they identify more closely with their ideal father than their real father, but that this is not true of identification with the real and ideal mother. The differences found between real and ideal identification add a new dimension to investigations in this area and seem especially relevant to investigations of like-sex and cross-sex identification. For example, it would be interesting to see if the results found here would be reversed for female Ss.

Hypothesis II

Roe's hypothesis of a relationship between the parents' attitudes of Concentration, Acceptance, and Avoidance and their child's vocational choice was not supported by the statistical analysis. These results not only agree with the findings of Grigg (1959), Utton (1962), Hagen (1960), and Switzer, et al. (1962), all of whom found little or no support for Roe's theory, but also reinforce their findings since the entire range of occupations was included in this study. However, the results do not agree with the findings of both Nachmann (1960) and Galinsky (1962) that parental attitudes are related to vocational choice. Inasmuch as both of these investigators used a psychoanalytic approach in assessing childhood experiences, the situation is very similar to the one discussed with respect to identification: methods which employ a more subjective approach produce evidence of a relationship, whereas more objective methods do not. Thus, there is considerable evidence of a basic difference between these two assessment approaches. Is the difference due to a lack of validity and reliability in the assessment techniques or to a bias which is affecting one approach but not the other, or are different aspects of personality actually being appraised? A study comparing the results of these two methods applied to the same Ss would provide a partial answer to these questions.

Hypothesis III

This hypothesis was designed as a more comprehensive test of relationships suggested by Roe. Each field of choice was used as a separate group, and the relationship between mean parental attitude scores and vocational choice was considered for each of the three types of choice. The failure to find significant relationships in any of the three analyses relevant to this hypothesis not only reaffirms the results and conclusions of hypothesis II regarding probable choice, but also indicates that these same conclusions apply when possible or fantasy choices are used. Thus no support was found for Roe's theory regardless of the type of choice used.

Switzer, et al. (1962) found overall differences between the attitudes of fathers and mothers, and suggested that these differences might interact with vocational choice. This interaction was not found. But these results are not very meaningful since their relevance depends upon a significant interaction between attitudes and choice, an interaction which also was not found. However, some information supporting Switzer's findings of overall differences in parental attitudes was obtained from the weighted raw score means. Since these means were from different scales their differences could not be tested statistically. However, these differences do have some relevance because an attempt was made to use parallel items in the father and mother scales for the same attitude. For all choice fields combined, these means indicate that (1) there probably is no difference in the degree of perceived *Acceptance* (father = 24.89, mother = 25.33), (2) the mother may be perceived as more *Concentrating* (father = 7.52, mother = 8.68) and (3) the father probably is perceived as more *Avoiding* (father = 7.36, mother = 4.48).

Implications for Future Research

Attitudes and Choice. The major implications of the findings regarding hypothesis II are for Roe's theory, because the results reaffirm the findings of others who tested her theory. Since the use of different methods of assessing parental attitudes has produced few differences in results, it is now time to focus on the choice variable in Roe's theory. Several aspects of this variable may be influencing its relationship to parental attitudes:

(1) Parental attitudes are only one of the possible factors which affect an individual's vocational choice. A number of others have been cited and an attempt was made to rule out either some or all of these reality factors using "possible" and "fantasy" choices. However, Trow's (1941) definitions are not specific regarding the reality factors being ruled out. Consequently, before specific factors can be eliminated from further consideration it seems necessary to repeat the research using choice questions which explicitly state the factors to be disregarded.

(2) Parental attitudes may influence the individual's choice of an occupational *function* rather than his choice of a field. If so, a rela-

tionship to parental attitudes might be found if subgroups were formed on the basis of occupational function (Darley & Hagenah, 1955; Fine, 1955; Bordin, Nachmann, & Segal, 1963). This could be done either within occupational fields or across fields. In either case, research people might form one group, teachers, another group, administrators, still another group, etc.

(3) Finally, an occupation may have a greater personal significance to one person than another. If so, Ss within each of Roe's levels might be considered separately, or only certain S groups might be studied, such as those who are following their father's occupation or those who report having felt free in making their choice.

Identification and Choice. Since the same choice variable was used in investigating the relationship of identification and choice as was used in testing Roe's hypothesis, many of the same comments apply. In a manner similar to that used in testing Roe's hypothesis, the relationship between real and ideal identification and choice should be investigated using the various modifications of the choice measure.

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An Articulated Framework for Vocational Development

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Over the past decade visible progress has been made toward a theory of vocational development which turns on early formative influences and links the adoption of occupational roles to personality organization. This article aims to further that progress by: (a) presenting a scheme which identifies the gratifications that varieties of work can offer, (b) tracing these gratifications to the physiological functions necessary to their achievement, and emphasizing the importance of early experiences that lead to investments in particular modes of obtaining gratification. We shall start by surveying existing formulations to establish wherein we extend or depart from these conceptions and proceed to a detailed statement of our theory, closing with suggestions for further research in the hope that others will join us in the verification of our ideas.

CHOICE OR DEVELOPMENT?

Beginning with Super's presidential address to Division 17, APA (1953), there has been a seeming controversy over whether to concentrate on vocational choice or development. Super and Tiedeman and their co-workers emphasize vocational development and orient themselves to the prediction of successive choices, or patterns of choices. Roe and Holland emphasize vocational choice—that is, the prediction of the occupational role that the individual is fulfilling at a particular point in time. Our studies have been more in the tradition of Roe and Holland. It seems likely that the seeming differences in orientation are more apparent than real. It is clear that all contributors are unanimous in seeing vocational choice as a specific subgoal in a continuous process, and all agree that this particular choice is *not* synonymous with the end of the process. That point is probably only reached with the death of the individual.

The point at which differences appear seems, then, to be more a question of research strategy than of differences over whether development is continuous. Perhaps the central theoretical issue is that of the tenability of assuming that after certain maturation points have been reached the individual makes a vocational commitment which tends to be persevering. After such commitments, the individual's range of development is considerably restricted and will not change appreciably except in response to radical external forces or, perhaps, psychotherapy. Having gone through the energy, expenditures and trials of medical training and persisted after tastes of the gratifications to be obtained through the work of the physician, the young medical doctor is unlikely to leave the practice of medicine, but may further refine and modify the nature of his development by choosing a medical specialty. Thus, we conclude that even though the full test of a theory of vocational development resides in its capacity to account for all of the major turnings in the individual's journey through the occupational world, expedient and meaningful studies can be conducted by concentrating on the theory's capacity to predict the individual's direction at the time that serious and encompassing commitments are made.

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CURRENT THEORIES

All theories of occupational life take either one or both of two views of the individual, the structural and the developmental. The structural view analyzes occupations within some framework for conceiving personality organization, choosing its terms from that framework. The developmental view attempts to portray the kinds of shaping experiences that can account for personality organization and concomitant vocational pattern. Thus, Tiedeman and Super use the term "self" and speak of the process of acquiring self knowledge and of implementing it. From what is visible in his published work, Tiedeman appears to draw a version of self perception almost devoid of emotional and motivational influences. This is illustrated by the study in which he and O'Hara (O'Hara and Tiedeman, 1959) studied the accuracy with which boys in four different grades can estimate their own aptitudes, interests, social class and values. Similarly, on the assumption that curricular choices are implementations of self concepts, Cass and Tiedeman (1960) analyzed a wide variety of personal characteristics differentiating beginning freshmen in secondary schools the assumption being that their self concepts will reflect their real characteristics. Though emphasizing unconscious elements in the formation of self perceptions, Super (1953, 1956) gives us no explicitly formulated developmental theory. The self is a resultant of the individual's interpersonal experiences, experiences of reflected evaluations and awarenesses of reference groups in family and community. There is little treatment of the self as an active force with complex inner organization, although there is some suggestion in that direction when Super argues for the view of vocational development as a process of synthesis rather than compromise.

In the work of Ginzberg et al. (1951) we have a process formulation with virtually no structural assumptions. Although their perspective is developmental, their scheme, in which the individual is seen as passing through successive stages of fantasy, tentative and then crystalized choice in which fantasy has been tempered by reality, seems to be guided by an emphasis on a single decision point.

Both Holland and Roe strive for a more differentiated structural approach in which occupations are grouped according to personal characteristics or activities. Of the two, Holland (1959) offers a much more thoroughgoing attempt to map occupations in terms of personality characteristics. He defines six different personal orientations which seem to be roughly comprised of various combinations of masculinity-femininity, acting vs. thinking, degrees and forms of expression of aggression, degree of need for achievement, and degree of orientation toward persons. It will be evident, when we present our own views that there are some similarities to Holland. One difference is that where Holland seems content to force occupations into particular classifications, we find it more meaningful to establish a series of pivotal personal dimensions, forming a matrix into which occupations can be mapped. This, we believe, permits more complete and more accurate description of the personality characteristics that are relevant to a given occupation. Although Holland makes explicit the goal of accounting for the development of these personal orientations, he does not move very far in this direction.

Roe's (1956) classification of occupations clings more nearly to those that have arisen out of factor analysis of interest inventories, stressing the activities of primary focus in the occupation. Her use of this classification in connection with her theory of the early determinants of vocational choice suggests that she has only one personal dimension in mind, namely the degree of orientation toward persons or nonpersons (Roe, 1957). Correspondingly, her developmental hypotheses are couched in terms of those formative experiences that might be thought to shape one's orientation toward persons, the warmth or coldness of parental attitudes. Although she acknowledges a debt to Maslow's hierarchical classification of needs, Roe makes little visible use of his stated needs in her classification system. She makes little use also of his principles about the appearance of needs in her basic developmental principle that the needs that will have relevance for vocational development are those for which there has been a delay in gratification, but for which subsequent gratification is achieved.

Thus far all attempts to verify Roe's theories have been directed toward her assumptions about the influences of parental attitudes on vocational choice, with negative results.[1] Though we remain convinced that Roe's basic direction is fruitful, we feel that it suffers from incompleteness in analyzing the basic personal dimensions relevant to occupations and requires much more specificity in stipulating formative experiences. Evidence that differences between the perceived attitudes of fathers and mothers with regard to warmth do exist and that the magnitudes of these differences are predictive of occupational choice (Switzer et al., 1962), point up the need for much greater specificity in examining parental attitudes or other formative experiences. Since we lean on a psychoanalytic orientation toward personality dynamics—we believe Roe does too—, the method of collecting verifying data is important. Evidence based on responses to direct questions will not always be satisfactory because of the presumed effects of repression. In such instances, indirect signs, e.g. spontaneous utterances, inflections, and fantasy materials, will be needed to provide satisfactory evidence.

FRAMEWORK FOR VOCATIONAL COMMITMENT

The theoretical formulation of work and vocational choice which we shall present had its original stimulus in three studies dealing with seven occupations. Each study applied psychoanalytic assumptions regarding personality development to the explanation of the behavior observed in detailed examination of the activities in an occupation. The pattern of the research in each instance was to make predictions on this basis regarding differences in the characteristics of members of differing occupations. The first (Segal, 1961) predicted differences between accountants and creative writers in their reactions to a variety of projective tests, important among them the Rorschach. The second (Nachmann, 1960) tested predictions regarding differences in childhood backgrounds of men in law, dentistry, and social work via biographical interviews. The third (Galinsky, 1962) used the same technique to compare the backgrounds of clinical psychologists and physicists. All of these studies were marked by a painstakingly thorough analysis of the occupation to identify what needs might be gratified and through what modes of expression through participation in it. With such analyses as a base, relevant personality measures reflecting these needs or childhood experiences which fix these needs were selected for study. Contrary to the failures to test Roe's developmental hypotheses, the latter two studies found strong evidences in childhood experiences.

Although each of these studies produced enough positive results to encourage the continuation of this general approach to occupational research, our understanding of the nature of the work and its relation to personality and early development shifted as we progressed. Though many of our predictions proved accurate they often seemed in retrospect to be not as important as others we could have made. There began to develop the need for a consistent framework which would apply to all occupations. We attempted then to set up a series of dimensions (needs, motivations, impulses, activities) which could account for all of the major gratifications which work can offer —which would make it possible to describe any occupation in terms of the relative strengths and the particular modifications of these component dimensions. The original studies only very partially fit into this theoretical framework to which they gave rise.

PRELIMINARY ASSUMPTIONS AND CONSIDERATIONS

We assume:

1. A continuity in development which links the earliest work of the organism in food getting and mastery of the body and coping with the stimulations of the environment to the most highly abstract and complex of intellectual and physical activities.

2. That the complex adult activities retain the same instinctual sources of gratification as the simple infantile ones.

3. That although the relative strengths and configurations of needs are subject to continual modification throughout the life span, their essential pattern is determined in the first six years of life. The seeking out of occupational outlets of increasingly precise appropriateness

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[1] See Grigg (1959), Utton (1962), Switzer et al. (1962), Hagen (1960).

is the work of the school years, but the needs which will be the driving forces are largely set before that time.

We are concerned with the entire age span, from infancy, when the impulses common to all humanity begin to be molded toward the individualized parcels of varying strengths and varying outlets, through all the vicissitudes of the occupational history. We are concerned with all levels of skill and status and all areas of occupations, though our research has thus far been confined to a few professions.

There is, however, one sharp limitation to our interests. Our theory does not deal with, and our research can relate only peripherally, to people who are motivated or constrained mainly by external forces. Certainly, economic, cultural, geographical and other external factors can exercise a severe limitation on freedom of choice and are outside the main structure of our theory. Perhaps however, even where there is little or no freedom of choice at the outset the relation between personal characteristics and job characteristics in terms of the dimensions we are considering may account for whether the job is intolerably unpleasant or not, and hence to productivity, success and failure, etc.

We are concerned with work as sublimation—but in the broad sense of all activity other than direct gratification, rather than in the narrower sense of pregenital impulses turned into artistic activities.

It is a theory of vocational commitment not only in the sense that a point of genuine commitment is a criterion against which our assumptions must be tested, but in the sense that it is only those activities to which the person has strongly committed his energies and his affections that we are discussing.

Consequently, we exclude from consideration those persons who have little capacity to get gratification from work. It may be that these tend to be found more in the simpler trades and the lower economic levels. There are some forces that would push in that direction—the greater pressure of external necessity and the fact that occupations requiring extensive training are not likely to be succeeded at by those lacking the capacity for sublimation, for example.

Knowledge of occupations is an external factor which curtails freedom of choice. The neurotic blocking off of knowledge of opportunities is an internal force and a part of the mechanisms with which we are concerned, but genuine nonmotivated ignorance we assume can occur and limit a person's choice, or lead him into a field which does not in fact offer the satisfactions he anticipated.

THE DIMENSIONS

As has been mentioned, the elaboration and refinement of the structure of dimensions of work has to be carried out via a repeated weaving back and forth between job analysis personality traits and the assumptions regarding the childhood experiences which generate these traits. The list of dimensions presented here is not to be understood as final but rather an early, though by no means first, approximation. The nature of the dimensions is dictated by our theoretical assumptions. The level of breadth or specificity at which they are described is in part arbitrary. One may range from two: sex and aggression, to as many as there are activities listed in the *Dictionary of Occupational Titles.* Although our categories of work activity or of impulse expression may not at this stage be dimensions in a statistical sense our aim is that they approach dimensionality. It is intended that they be independent at both ends of the developmental span—i.e., that each one stems in the beginning from a different physiological function, and that at the level of occupational expression one cannot be substituted for another but satisfies a different need.

In Table I, Column I lists the dimensions. The remaining columns represent modifying characteristics which account for the varying forms of expression which the dimension may take in adult work activity.

1. By the first pair of dimensions, the nurturant ones, we mean those activities that involve the care of living things—feeding, protecting and promoting the growth of people, animals, plants, both literally with food and shelter and symbolically with words. We would see the interest in feeding activities as stemming from the infantile experiences of being fed. Special delight, pain, or anxiety about the taking in of nourishment develop into concerns that food be plentiful, that others not want and, more remotely, to interest in words.

Table 1. The Matrix Consisting of Basic Need Gratifying Activities and Their Further Defining Aspects into Which Three Occupations, Accounting (A), Social Work (S), and Plumbing (P), Have Been Mapped for Illustrative Purposes

I	*II*	*III*	*IV*	*V*	*VI*	*VII*
Dimension	*Occupation*	*Degree of involvement*	*Instrumental mode*	*Objects*	*Sexual mode*	*Affect*
Nurturant	A[a]	0				
Feeding	S	3	Material and psychological supplies	Needs of clients	F	R
	P	0				
	A	1	Financial advice and safeguards	Client's financial affairs	M	A
Fostering	S	3	Encouragement and protection	Client's growth and health	F	R-A
	P	0				
Oral Aggressive	A	0				
Cutting	S	1	Words	Client's resistance	F	I
	P	1	Lathes, gouges, clippers	Pipes	M	I
	A	0				
Biting	S	0				
Devouring	P	1	Wrenches, pliers	Pipes	M	I
Manipulative	A	0				
Physical	S	0				
	P	2	Pipes, valves	Steam, water pressure	M	A
	A	3	Advice, recommendation	Business and govt. policy	M	A
Interpersonal	S	2	Provocation, influence, seduction	Feelings and attitudes of client	F	I
	P	0				
Sensual	A	0				
Sight	S	0				
	P	0				
	A	0				
Sound	S	0				
	P	0				
	A	0				
Touch	S	0				
	P	1	Hands-smoothing-sculpturing	Joints	M	A
Anal	A	2	Recommendations re investment	Fortunes of clients	O	A
Acquiring	S	1	Efforts to equalize distribution	Wealth of society	F	R
	P	0				
Timing-Ordering	A	3	Systems, audits	Financial policy	O	A
	S	1	Records, budgets	Own work, lives of clients	O	A

	P	0–1	Calculating costs, estimating	Materials	M	A
	A	2	Prevent waste, encourage saving	Money of client	O	I
Hoarding	S	0				
	P	2	Prevention of blockage-expulsion	Waste-actual anal products	O	R
	A	2	Systems to combat disorder	Financial affairs of business	O	R
Smearing	S	0				
	P	2	Hands, trowels	Pastes, greases	O	A
Genital	A	0				
Erection	S	0				
	P	0–1	Hands-tools	Faucets-fixtures	M	I
Penetration	A	0				
	S	0				
	P	1	Reaming, coupling	Pipes, joints	M	I
	A	0				
Impregnation	S	1	Prevention or encouragement	Family planning, marital coun.	F	I-R
	P	0				
	A	0				
Producing	S	1	Giving or withholding	Babies for adoption	F	I
	P	0				
Exploratory	A	2	Audits to detect fraud	Financial behavior of others	O	A
Sight	S	2–1	Visual investigation	Homes of clients	O	A
	P	2	Detecting leaks and blockage	In pipes and water systems	O	A
	A	0				
Touch	S	0				
	P	1	Hands-to determine shapes	Where can't see	O	A
Sound	A	1	Questioning	Financial statement of clients	O	A
	S	2	Questioning	Private life of clients	O	I
	P	1	Detecting leaks and disturbances	Sound of running water	O	I
Flowing-Quenching	A	0				
	S	0				
	P	3	Arranging of pipes, valves	Flow of fluids, waste products	M	I
Exhibiting	A	0				
	S	0				
	P	0				
Rhythmic Movement	A	0				
	S	0				
	P	1	Hands, tools, physical movement	Pipes	O	I

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[*] Degree of involvement: 0 = no significant involvement, 1 = peripheral importance, 2 = secondary importance, 3 = primary importance. Sexual mode: M = masculine, F = feminine, O = not sex linked. Affect: A = affect experienced, R = reaction formation, I = isolation.

By fostering we mean those activities that involve either literally or figuratively shielding, comforting, protecting the young or the helpless—giving warmth and shelter as first one was warmed and sheltered. Their physical prototype is seen in the need to burrow into the warmth of mother, bed, home, and in the tactile and temperature sensitivity of the skin.

2. The second cluster of dimensions have to do with the teeth and the satisfactions to be had from biting, chewing and devouring. We would assume that these impulses are translated in the adult into the use of cutting and grinding and drilling tools—that these activities are delegated first from teeth to fingers then to knives and saws and drills, and finally to biting and cutting words and ideas.

3. The next two dimensions involve manipulation—power over people or physical objects. First with the bare hands over whatever comes within the infantile grasp—later on with all the machinery of technology that so enormously multiplies the power of the muscles. Secondly there is the power and manipulation which one exercises over people not physically but psychologically—influencing, persuading, threatening, seducing—first parents and siblings then a widening circle of other people.

4. Next are the dimensions that have to do with the use of the sense organs, not for gaining information, but for sensual pleasure—as the infant looks, touches and tastes because things are pretty and feel good, and are sensually gratifying apart from any other purposes they may serve. In the adult it finds expression perhaps most clearly in the artist whose senses are exquisitely sharpened and whose work is the giving of such pleasures to other people.

5. The group of occupational dimensions whose physical and genetic sources are probably already most thoroughly explored and accepted as commonplace are those stemming from the anal impulses, to hold on to or let go of objects, to hoard, to order in time and space, and the impulses toward smearing or cleanliness. Their counterparts in all of the business detail occupations that require precision and order and economy in the handling of time and money and material are easily recognized.

6. Next are some activities relating to phallic and genital impulses, if indeed the latter can in some measure be sublimated. One may see some similarity between the child's interest in the phenomenon of physiological erection and the erection of sky scrapers and bridges and all kinds of structures, and even theoretical structures.

The dimension of penetration may seem similar to some of the orally attacking ones, but here the emphasis is upon intrusiveness rather than destruction, such as probing into the earth, the sea, and most currently, rocketing into space, as well as penetrations of a more abstract nature.

With the dimensions of impregnation and producing we are obviously dealing with impulses that are not present in children in a completed form but only in fantasy or in partial instinctual strivings. The most direct occupational translation of these impulses would seem to be in the agricultural occupations; a more displaced and abstracted one in the artist and writer who speaks so persistently of being impregnated with ideas and giving birth to artistic products.

7. The dimensions of curiosity have to do with the use of the sense organs for finding out about the world—investigating, exploring, knowing the facts. These dimensions are primary to every field of scientific investigation, the particular scientific area differing only in the object toward which the curiosity is directed and the degree to which it is displaced from the original objects of one's own body and one's parents.

8. The dimensions of flowing and quenching originate in urethral concerns and have their adult expression in such things as plumbing, fire fighting, hydraulic engineering, etc.

9. Next is the dimension relating to the impulse to exhibit the phallus or the body as a whole or later one's intellectual or artistic accomplishments. It is certainly easily seen in the child's "showing off" and in a host of adult occupations—acting, law, advertising, the ministry, etc.

10. Finally we have the dimension which has to do with rhythm—having its origin in the physiological rhythms of heartbeat and respiration or masturbatory rhythmic movement. Its occupational expression is seen in the musical occupations or in the industrial, craftsmanlike, and artistic occupations that involve bodily rhythm or its abstract equivalents.

The final list may differ somewhat, though not greatly from this one. For example, it is unclear whether the "sensuality" cluster should be separated as it is here from the exploratory uses of the sense organs on the one hand or from genital sensuality on the other. At present it seems necessary to do so. Similarly the relation of exhibitionism-voyeurism to exploratory "looking" as well as to genital concerns is questionable. Though some of the anal dimensions can be regarded as reaction formations against others their prominence as occupational motivations seemed to demand their treatment as separate dimensions.

Other Characteristics of the Dimensions

In Column III is indicated the amount or importance, of the dimension in the particular occupation. That is, gradations in the degree of investment which a person has in finding expression for the impulse in question, or the degree to which a job requires this activity. Although we assume a continuous gradation we have as a matter of convenience rated the degree of involvement on a four-point scale.

In Column IV, Instrumental Mode indicates the tools and the activities through which the impulse is expressed. The range of possibilities is from physical actions of the body, the "bare hands," to tools, words, abstract concepts and symbols—a range from the physical and concrete to the abstract and symbolic.

In Column V, Object indicates the person(s) or thing(s) toward which the activity is directed. The range of possibilities is from human beings or body parts to animals, plants, inanimate objects, and abstractions. The differences here involve not only a range from concrete to abstract as in IV but degrees of displacement from the original objects of self and parents—displacement in space and time or restriction by defining a specific area to which the activity is limited.

Entries in Column VI indicate whether the activity appears to have been patterned after a masculine or a feminine model.

Column VII, headed Affect, indicates whether the affective component of the activity is accepted or repressed. In the actual investigation of an occupation it would be necessary to determine the particular mechanism of defense or mastery employed. For purposes of these illustrative examples we can only offer a rough estimation of whether it seems to be more a matter of reaction formation, of activity isolated from affect, or of at least partial acceptance of the affect.

Three Occupations

Entered in the cells of the chart for illustrative purposes are the occupations of *accounting, social work,* and *plumbing,* as one might view them in a highly abbreviated and schematic fashion. Accordingly social work would be seen as having feeding and fostering as its areas of primary investment, manipulation of people and a curiosity regarding their lives as secondary dimensions, and with several other peripheral concerns.

Accounting by contrast would have only a very peripheral involvement in the nurturant dimensions and its primary ones in the manipulation of people (albeit in an indirect form) and the timing and ordering dimension, with all of the other anal concerns as strong secondary investments. The accountant too enjoys a privileged curiosity into the lives of his clients but a much more circumscribed one than the social worker's.

The plumber contrasts with the accountant in another way. He too is unconcerned with nurturance; he manipulates physical objects and forces, not people, and though his investment in the anal dimensions is strong it is the converse of the accountant's. He has to be able to accept and even enjoy direct contact with dirt and disorder and to deal with anal products quite literally and directly whereas the accountant abhors messiness. The plumber's area of primary investment is that of flowing and quenching, in which the other two occupations have no interest at all, but he shares with them some of their peripheral dimensions—though consistently in a more direct and physical mode than they.

Every occupation can be described in terms of these dimensions. No occupation can be explained by a single dimension. On the contrary we would assume that most complex occupations (and most people) would include every dimension, at least in vestigial form; with one or two dimensions of primary importance in its spectrum and several others of secondary importance. Such a theoretical structure makes it possible to describe both jobs and people with the same set of terms. Therefore such issues as success, satisfaction, and productivity may be predicted on the basis of the degree of congruence between the patterning of dimensions in the two. With a relatively small number of basic activity dimensions a high level of specificity may be attained in describing a job by considering modifications of each component dimension in its displacement from its original infantile form and object. It thus becomes possible to deal not only with broad classes of occupations—social, biological, physical scientists, salesmen, administrators, etc.—but with particular jobs within these categories.

RESEARCH APPLICATIONS

We have referred to specific completed studies which have provided some measure of verification of the tenability of our approach to vocational choice and encourage our faith in its usefulness. Now we propose to offer a rough map of the areas of investigation that will be stimulated or nourished by our frame of reference and which are required to realize more fully the direct application of our concepts to the practical task of understanding and aiding vocational development.

Naturally, the core research issue is the verification of our ideas of how occupations can be charted in the space defined by our proposed dimensions. In so doing, we can expect to be jointly validating our theory of vocational development and theories of personality development. The occupations to be studied should be selected as centrally located on at least one of the dimensions until all of the dimensions have been sampled. Ideally, the designs of the Segal study of personality measures and the Nachmann study of childhood experiences should be incorporated within single investigations.

The analysis of occupations for the purpose of mapping them into our chart calls for a new conception of job analysts. For the most part, job analysis has in the past been focused on the identification of the general and special aptitudes and the levels of education or training that performance in specific jobs or groupings of jobs requires. Our view of the role of personality in vocational development points to the need for an entirely different purpose a and style of job analysis. Direct observation of the worker on his job still remains an important procedure. In this instance, however, the observer aims to identify the modes of expressing and controlling one's impulses that the activities required or permitted affords the worker. In seeking insights into the gratifications that work affords, we look to the worker's experience of his work, his descriptions of his experiences, the psychological significance of the products of his work, and his fantasies. The diaries and autobiographies of literate and introspective men, usually in the professions, can be important sources of data.

One of the most difficult and yet most critical tasks in the confirmation of our scheme is the development of measures for the dimensions in terms of personality organization. Segal has demonstrated that this is possible, using our earlier version of the dimensions, but this was accomplished only after an extremely thorough sifting of Rorschach test theory and the available relevant data. We feel sure that a simple process of matching test names to dimension labels will not, in most instances, yield positive results. As research proceeds and the relevance of personality measures to the various dimensions is clarified by the accumulating data, this part of the research task will become progressively easier and, concomitantly, the diagnostic resources of the vocational counselor will have been enriched.

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Psychoanalysis and Scientific Creativity with Special Reference to Regression in the Service of the Ego

Marshall Bush

The intent of this paper is to examine what contributions psychoanalysis may have to offer to an understanding of the creative process in the natural sciences as distinct from creativity in other fields. Of necessity some injustice will be done to the complexity of the problem. Which activities should be considered creative? Is creativity sufficiently uniform a process that it can be considered to be essentially the same in different branches of science, in different research approaches, and in different personality types? A broad spectrum of activity is all too often glibly subsumed under "scientific research," overlooking the fact that innovations in science take manifold forms. Bronowski (7), for example, discusses the differences between discovery, invention, and creation. Cough and Woodworth (22) have statistically isolated eight types of research scientists based on their approach to problems and the kind of contributions they are best suited to make. Stein (51) has worked out a different fivefold typology on the basis of his studies of industrial research chemists. These are but a few of the findings emerging from a recent body of literature which challenge the notion of some single entity called "the creative scientist."

Until fairly recently one could reasonably speak of the "pure research scientist" and the "scientific life style." Today, however, the sociological structure of science is changing in the directions of diversification and specialization, producing many byways from the traditional academic scientific career. This increases the number of roles in which research scientists function, the diversity of personalities drawn into scientific work, and indirectly affects the creative enterprise itself.

The weight of current evidence does not support the notion of some one personality type which alone is characteristic of the creative scientist (even in a single field of research), nor the idea of a single type of personality which alone is creative (12, 22, 23, 40, 51, 54).

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Taylor (54), who has done extensive research in this area, concludes that "there may be not only several different types of creativity, but also several other types of gifted, relatively separate from the I.Q. type and the creative types" (p. 170). "It is therefore quite possible that multiple types of creative talent may exist" (p. 181). The important issue then becomes what psychological processes are operative in creative scientific work and how uniform are they across different areas of science and types of researchers? Since recent findings have indicated that creative performance in science, as in other fields, has little correlation with traditional intelligence measures (assuming a reasonably high degree of intelligence and academic achievement [23, 37, 45, 54]), psychoanalytic considerations may be especially relevant in the attempt to elucidate the creative process and those personality factors which facilitate creativity or serve as impediments.

THE INTERNAL CONTEXT OF DISCOVERY

Recent Findings on the Personalities of Research Scientists

A considerable amount of research is accumulating on the personality characteristics of research scientists. The studies differ considerably in the kind of subjects selected for study, the area of science from which samples are drawn, the type of comparison groups employed (if any), and the techniques of investigation.

Roe (42) and Eiduson (12), who have conducted the most intensive large-scale diagnostic studies of research scientists, conclude that there is no single characteristic personality type in terms of clinical classifications. Eiduson, did, however, find that her scientists would, in general, be considered "character types," showing a lack of overt neurotic symptomatology and demonstrating characterological means of handling anxiety and conflict.

Nevertheless, despite the great variability between the studies, there seem to be several points of general agreement on certain personality and cognitive features of scientific researchers. Summarized below are some of the findings which may have relevance for understanding the creative process in the natural sciences (there have been some quite different findings on the personality of social scientists [43]). They are drawn primarily from the work of Barron (2), Cattell (8), Eiduson (12), Knapp (28), McClelland (35), Roe (40–45), Stein (50, 51), Super and Bachrach (52), and Taylor (54, 55).

Personality Characteristics of Research Scientists. 1. Scientists generally demonstrate adequate adjustment, appearing well adapted to their life situation and emotionally stable with little overt neurotic symptomatology (2, 8, 12, 28, 35, 42, 51, 52).

2. They are highly curious about and emotionally invested in the world of ideas and things, rather than in people (12, 35, 45, 54).

3. Scientists are introverted and avoid close interpersonal contact. They retreat from complex emotional involvements with people (especially disliking open interpersonal aggression) while maintaining superficially smooth interpersonal relations (8, 28, 35, 43, 52, 55).

4. They are unusually autonomous, self-sufficient, and self directed, demonstrating remarkably little overt need for closeness, warmth or support from others (8, 12, 41, 52, 55).

5. Fisher and Cleveland (15) found natural scientists to have less well-defined body-image boundaries than his artists and social scientists.

6. All writers stress the research scientist's extraordinary emotional involvement in his work to the exclusion of practically everything else.

Some Cognitive Characteristics of Research Scientists. 1. They exhibit marked independence of judgment, rejecting group pressures toward conformity in thinking.

2. They display novelty in ideational activity, seeking to depart radically in their thinking from the obvious, the usual, or the hackneyed.

3. They seek out delicate and subtle impressions, displaying a heightened sensory and intellectual sensitivity to certain types of external stimuli.

4. Their interests center on the theoretical and abstract rather than on the practical and concrete. Complex ideas and problems are preferred to simple ones.

5. A need for order, method, and exact-

ness is combined with an excited interest in apparent contradictions, exceptions, and disorder.

6. A liking for manipulating and toying with ideas appears together with a capacity for recombining and reorganizing familiar conceptions.

7. A strong need for ultimate cognitive closure, mastery, and integration of experience is combined with a tolerance for ambiguity and resistance to premature closure.

8. They demonstrate superior intelligence.

9. They can loosen or relax controls in thinking without showing personality disorganization.

(This summary of cognitive characteristics is based on the work of Barron [2], Eiduson [12], Roe [45], and Taylor and Barron [55].)

Developmental and Background Characteristics. The diversity of background and childhood factors is sufficiently great so that most researchers in this area feel that no particular developmental experiences are critical to the formation of a scientist. The eventual scientific commitment seems to be multiply determined by complex interactions among many factors which may differ from scientist to scientist. A few prominent findings have turned up, however, which are clearly not irrelevant to the choice of a scientific career.

1. There seems to be an unusually high incidence of absence of a parent from the home in childhood, either due to death, divorce, or other causes. The lack of fathering seems more prevalent than the lack of early adequate mothering (in later childhood remoteness is reported toward both parents) (4, 12, 42).

2. Family relations were notably lacking in warmth, closeness, and intimacy, and remain tenuous throughout life. Mothers are often reported to have been insecure, inconsistent, frustrating, preoccupied, and not too warm, giving, or protective. While there was usually even more distance between father and son, the father seems to be less disparaged than the mother and viewed with more respect (12, 40, 41, 56).

3. The break from home toward independence occurs relatively early and without much apparent conflict or guilt (12, 52).

4. Periods of extended childhood isolation are commonly reported, due variously to physical factors (illness or physical deficiencies) and/or voluntary or imposed social isolation (12, 42, 52).

5. Both psychosocial and psychosexual development are retarded. Peer relations are slow to develop and dating relationships usually begin quite late (12, 40, 41, 52, 56).

6. Intellectual interests and achievements appear very early and their significance is highly overdetermined. They provide the child with important sources of self-amusement and solace, and in many instances are one of the few gratifying ties to his parents, teachers, and other people (12, 35, 52).

Comparative Data. There are only a few studies which have attempted to isolate the differentiating qualities of highly creative natural scientists from the less creative. While there is no one personality type which is most creative, a few variables seem to stand out.

Stein (50) found that his more creative industrial research chemists, as contrasted with less creative chemists, were more distant from both parents and from adults in general, reported their mothers as being more inconsistent in their attitudes toward them, and engaged to a greater degree in solitary activities as children rather than participating in group activities. They see themselves as more autonomous than their colleagues, possess more distant goals, are more integrative in their attitudes, and have a more differentiated value hierarchy. Taylor and Barron (55) similarly found that the more highly regarded doctoral candidates in the sciences were distinguished by their independence of judgment and resistance to group pressures, and their profound commitment to the search for aesthetic and philosophic meaning in all experiences. In addition to being unusually appreciative of the intuitive and nonrational elements in their own nature, they were marked by a strong need for order and for perceptual closure combined with a resistance to premature closure and an apparent interest in disorder, contradiction, and imbalance from which a complex integration could be extracted. It would seem then that the more creative researchers possessed the general qualities and background characteristics of scientists, as mentioned above, only to a greater degree.

Bearing more immediately on the psychology of the creative process (the implications of which will be discussed later) is Roe's (40) finding that biologists in general differed from her selected sample of eminent biologists by showing *too little or too much* rational control on the Rorschach, and Saunders's (48) finding that the more creative industrial research engineers were distinguished by having more feminine and artistic interests than less creative engineers (although all natural scientists receive high scores on measures of masculinity of interests [35]). . . .

The Motivational Base of Scientific Interests

Scientific interests as well as scientific creativity have received surprisingly little detailed consideration in the psychoanalytic and related literature, one notable exception being the recent work of Giovacchini (19, 20, 21). Unconscious motivations, such as infantile sexual curiosity, that are likely to lead to conflict usually receive the most attention. Pregenital fixations are held to be prominent, while full genital primacy and mature object relations are thought to be lacking (49). Some writers, such as Maslow (36), relegate scientific creativity itself to a secondary and lesser form of creativity.

The kind of generalizations that have been offered do not do justice to the motivational complexity and emotional intensity of scientific pursuits. In this section I shall try to treat programmatically several important origins and developments in the acquisition of scientific interests. Since a commitment to science evolves from a multiplicity of determinants, the following factors will be applicable only to varying degrees in the case of any given scientist. . . .

Autonomous Ego Developments. With the rise of ego psychology in psychoanalytic theory, there has been an increasing emphasis on autonomous factors in ego development. The notion that the ego has independent energies at its disposal (rather than acquiring all of its energy and impetus from instinctual drives) which it employs to test and master reality has been furthest developed by White (57). To find that even in the second year children's play takes on a character "that deserves to be called scientific investigation" surely does not account for the later development of scientific interests. Yet there is something of distinct importance in these observations that should not be overlooked.

Independent ego interests, such as scientific pursuits, are most often accounted for by such concepts as "sublimation," "neutralization," and various other transformations of instinctual drive energies. Following the hierarchical conception of the psychic apparatus developed by Gill (18), Rapaport (39), and others, one must emphasize that it is the *prior existence and autonomy* (from the drives) of reality-oriented ego functions and structures, such as are exhibited in the infant's exploratory investigations, that allow for the "neutralization" and "sublimation" of drive energy. Sublimations are not created *in vacuo;* they must be built upon existing structures.

The important consideration with respect to future scientists is that their intellectual interests and capabilities must have a securely autonomous anchor point in terms of ego structures and "functional pleasures" in the exercise of such structures if they are to become a broadening discharge channel for many other needs and drives in the personality without losing their functional autonomy. Sandler and Joffe (47) have recently suggested that probably the most important factor in the ego's capacity for sublimation is "the degree to which function pleasures can be substituted for instinctual ones" (p. 345).

As previously noted, intellectual interests appear very early in future scientists and are one of the child's most valuable resources in sustaining himself through periods of childhood isolation. Whatever obstructions and disappointments the child may encounter in other types of need-related pursuits, he is often *left free* to the enjoyment and development of his intellectual resources. It is the "functional pleasure" in cognitive play that is likely to consolidate the autonomous aspects of intellectual ego interests and make them available as substitute drive discharge outlets in the form of scientific interests. One of White's contributions has been to underscore the fact that directions in development proceed not only under the aegis of drive pressure and external reinforcement (both positive and negative); but, in addition, the *lack of obstruction* (which includes freedom from the overwhelming frustrations of other needs as well as from severe anxieties) to innate urges toward exploring, manipulating, and mastering one's environment will promote the development of knowledge and skills.

In the case of scientists, it is not just the important extrinsic reward that their intellectual achievements brought them as children which shapes their early intellectual interests. Of equally great importance is the fact that they seem to encounter fewer hindrances in the intrinsically pleasurable exploration and mastery of the nonhuman environment than in their early human relationships, and they subsequently use their intellectual interests and competencies to foster their independence from emotional reliance upon others. Furthermore, the childhood capacity for intellectual play is retained and elaborated in adult life by the creative scientist and appears to be a valuable resource in his work (12, 16, 34, 35).

Drives, Needs, and Wishes. To emphasize a strong primary base in terms of autonomous intellectual structures and intrinsic functional pleasures upon which scientific interests are built is to suggest a necessary precondition for successful scientific sublimations which in no way diminishes the role of drives, needs, and wishes in scientific motivations. Psychoanalytic writers have made their greatest contributions in pointing out just such factors (13, 16, 20, 26, 27, 30, 31, 33, 34, 38, 46, 49). There has been far from unanimous agreement, however, upon which unconscious factors are most important in scientific pursuits. In part this reflects differences among the writers' theoretical orientations, developments in psychoanalytic theory itself, and differences among the individual scientists studied. What does seem clear is that a wide range of unconscious determinants are operative to different degrees among different scientists and in different stages of their work and development.

Early intellectual, scientifically oriented interests involve certain *ego functions* (perception, cognition, fantasy, locomotion, etc.), *ego activities* (investigation, learning, play, etc.), *aims or products of such activities* (knowledge, mastery, tension discharge, object relatedness, etc.), and *objects* employed in carrying out these aims (artifacts, body parts, other persons, etc.). When we speak of drives, wishes, narcissistic libido, infantile omnipotence, object ties, etc., being sublimated into interest patterns, a closer analysis reveals that such factors enter into ego interests in a variety of ways (47). In some instances, ego functions themselves may become highly sexualized or aggressivized. At other times it is the activity, the aims of the activity, or objects used in implementing such aims that may correspond to unconscious fantasies, representations of the self (becoming invested with self cathexis), or representations of other people (becoming invested with object cathexis)....

Trauma and Mastery. Childhood trauma is a universal experience and its mastery an unending life task. Blos (6) has described how the residuals of childhood traumata remain a powerful motivating force throughout one's life, giving it direction, continuity and meaningfulness. "The extent to which trauma hinders progressive development constitutes the negative factor of the trauma; the extent to which trauma promotes and encourages the mastery of reality is its positive factor" (p. 133).

There can be little doubt that scientific pursuits involve such efforts at mastery which contribute to the enormous satisfaction the scientist experiences in his work. "Every attempt at egosyntonic mastery of a residual trauma, often experienced as conflict, enhances self-esteem" (6, p. 134) and the major source of the scientist's self-esteem comes from his work (12, 40). In case studies of scientists similar potential sources of childhood trauma appear as are suggested by research findings, such as childhood isolation and the agony of being left alone (34), either the absence of the father or bitter disappointment and disillusionment in him (13, 16, 20), and an unpredictable and inconsistent mother (19).

Barron (3) has developed the hypothesis that creative individuals have learned to prefer irregularities and apparent disorder as a form of mastery over having been presented as a child with unpredictability and unintelligibility in the child-parent relationship. Roe (44) found that for certain of the eminent biologists she studied, there was a reassurance in the study of living processes stemming directly from their having had to cope with the death of a significant figure early in life.

Giovacchini (20), in a case analysis of a creative natural scientist, clearly illustrates how the patient's "catastrophic disappointment" in his real father led him to seek an idealized and impersonalized father image in the canons of science. This childhood trauma influenced the patient's relationships with teachers and colleagues throughout his entire life and seriously interfered with his creative functioning. Of the eight experimental research scientists Giovacchini (19) describes, all felt secure as long as they could stay in close touch with nature and became extremely anxious upon feeling they were losing this contact. From the case material, it is easy to infer rather severe childhood separation anxiety with respect to their mothers. One of these scientists had married several times, always picking a severely disturbed woman in an attempt to master certain childhood anxieties. "His need was to master the fear associated with unpredictable women whom he found dangerous like his mother. This need to master also contributed to his persistence with research problems" (19, p. 416).

Other Personal Meanings of Science. It is quite apparent, both from analytic case studies and the group studies of scientists, that science becomes the arena in which all variety of complexes may be lived out and hoped-for resolution of personal problems sought. Sandler and Joffe (47) have tried to clarify the manner in which ego activities (although the commitment to a life in science is more than just an ego interest or activity), the products and aims of such activities, and the objects used to carry them out may reproduce every variety and gradation of object relationships, from extremely narcissistic extensions of the self to stable object love. . . .

The psychological vulnerabilities which jeopardize creative scientific work in large part stem from the fact that so much of the scientist's most basic dynamics becomes enmeshed in his research and his relation to science. A few common developmental factors stand out as potentially salient sources of conflict which may affect the scientist's functioning.

(a) A mother who encourages her son's intellectual accomplishment for her own narcissistic gratification. This tends to make intellectual achievements the major source of self-esteem and narcissistic supplies. If the child's intellectual prowess is over-valued from an early age, particularly in conjunction with maternal disparagement of a discredited father, it tends to leave permanent narcissistic vulnerabilities in the child and reinforces dangerous omnipotent fantasies which become associated with scientific achievement. In certain instances it may also generate anxieties about having one's accomplishments exploited or stolen by others.

(b) Early experiences with object loss (conceived broadly so as to include the psychological remoteness of or disappointments in the parent, as well as actual separations due to death or illness). Early object losses or disappointments, when parental images are still highly idealized and imbued with a good deal of projected omnipotence, lead to the accentuation of such qualities in the child's self and object representations. The kind of ideals the child establishes for himself and others, and seeks in his relationships with his activities and people, tends to reduplicate the lost relationship with the omnipotent and idealized parent. Scientists have been known to seek idealized parent substitutes, often experiencing repeated disillusionment, and turning finally to the exalted canons of science and scientific heroes as abstractly perfect parental figures (especially father figures). . . .

Perhaps the most valuable asset the scientific enterprise affords to many individual scientists is the opportunity to consolidate a culturally meaningful and socially valued identity. The childhood social withdrawal and isolation, the lack of closeness to family members, the late or sparse development of peer relationships and heterosexual involvements—frequently noted factors in the backgrounds of scientists—are not conducive to the establishment of a firm personal identity. The institution of science, which has traditionally held out a strong core of values, community, and historical continuity, can in many instances provide the cornerstone for a workable and rewarding personal identity for men who might otherwise experience considerable alienation and identity confusion.

The Impetus to Creativity

For many scientists being a discoverer is the essence of being a scientist, and this is particularly true of researchers in "pure" science. Functioning creatively is often the nucleus of the scientist's identity and ego ideal. Nonetheless, it is most essential to recognize that the creative act may have shades of meaning and specific determinants which are quantitatively or qualitatively different from those underlying the interest in and commitment to a branch of science. While a person's interests and routine functioning as a scientist may be relatively autonomous and conflict-free, his capacity and desire to function creatively may be subject to great conflict and distortion by unconscious factors.

The need to create may have many special defensive functions designed to offset unconscious sources of guilt, shame, anxiety, or depression. Narcissistic and omnipotent strivings may spur creative ambitions, as may rivalrous and destructive wishes, as well as unconscious sexual fantasies. The conflict engendered by such wishes may severely obstruct the scientist's successful creative functioning. Numerous examples are available in the psychoanalytic literature (5, 14, 21, 30, 32).

One such case is reported by Kris: "While any kind of routine work is easily achieved, any form of creative activity is associated with extreme difficulties and accompanied by a number of severe physical and mental symptoms. These difficulties are partly determined by an unconscious identification with his father, which follows the line of aggressiveness and guilt" (30, p. 298).

Kubie discusses the numerous kinds of problems which may be cloaked behind a strong drive for originality such as a "profound block over mastering facts, a fear of competition with others, limitless and insatiable megalomaniac ambitions, or an unconscious rejection of all existing authority" (32, p. 78). Another type of case he cites seems to be an actual concern of many scientists, the fear of not being able to repeat a previous scientific accomplishment. "But the most ubiquitous tragedy of all is the anxiety-driven scientist who lives on a treadmill—the man who has tasted what it means to gain temporary easement from his anxieties by doing a fine piece of scientific work, but who thereafter is driven not by a quest for further truth but by an insatiable need to repeat the same achievement in an effort to assuage anxieties whose origins were unconscious" (31, p. 606).

Giovacchini (19, 20, 21) presents clinical material on creative scientists who would have to interrupt their work because of the anxiety generated by the fear of loss of control attached to the special meanings embodied in the creative act. . . .

Just as the desire to be creative may involve some unique motivational determinants not included (at least not to the same degree) in a person's general interest in science, so too does the creative process itself entail some special features (aside from the wish to be creative) which may produce anxieties leading to an inhibition in functioning. This will be discussed further in the following section. The point to be made is that an *interest in science*, the *drive toward discovery*, and the *creative process itself* may each serve as a separate focus for possible sources of conflict and entail somewhat different psychological processes. . . .

Unconscious Factors in Scientific Creativity[1]

Repressed drives, wishes and fantasies, unresolved unconscious conflicts, unmastered trauma, and primitive modes of ego functioning play an important role in most complex human behavior, interacting continuously with advanced levels of psychic functioning and relatively autonomous ego interests. Only some change . . . in the normally integral operation of the psychic hierarchy should be singled out as a regressive alteration from the usual state of affairs. It is to be expected that unconscious factors will participate in the selection of scientific problems and will provide a continual potential source of distortion and conflict in all phases of scientific work, as Kubie (31) and others have pointed out. While these primitive concomitants of ego pursuits play a motivating role, they do not necessarily make any specific contribution to achieving a creative solution, in fact, their effect may be of just the opposite nature.

The role of unconscious motives and primitive levels of ego functioning seems to vary considerably in its capacity to contribute to creative work, both across individuals and different fields of art and science. It is well known that unconscious fantasies and primitive levels of ego functioning may contribute directly to the form or content of a work of art or literature. In the sciences, however, the situation

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[1] A detailed analysis of the concept of regression in the service of the ego and its applicability to the creative process in science is presented on pp. 158–169 of the original article. An understanding of this concept and of how it has been applied to scientific creativity is essential to the main points developed in the remainder of this article.

is more complex because of the difficulty in reliably discerning such contributions.

In the case of inventors, unconscious fantasies can sometimes be seen to have not only participated in selecting the problem, but to have directly inspired the invention itself. Unconscious wishes or conflicts may present themselves in consciousness in the form of fantasied inventions. Lorand's (34) case analysis of an inventor provides numerous examples. His patient's inventions clearly involved an acting out of fantasies, much as an artist's creations might. This is an extreme case which will not fit the pattern of many, if not most, valid scientific creations and discoveries. While unconscious factors may frequently underlie scientific biases or predispositions, their direct participation in scientific creation in a causally related manner is likely to lead to invalid and distorted scientific ideas. (See, for example, Eissler's analysis [13] of the influence of Goethe's psychosis on his scientific theories.)

Technical innovations may sometimes allow repressed wishes and fantasies a more direct role in influencing a creative solution than generally occurs in other forms of scientific innovation. A particularly colorful historical example is the development of the microscope by Leeuwenhoek (11) and his subsequent discovery of microbiology. He was an extraordinarily suspicious and surly individual (in all likelihood a paranoid character) who developed an obsession with lens grinding, eventually perfecting the microscope, but allowing his zealously guarded precious instruments to be submitted to the Royal Society of England only at the time of his death some fifty years after they had first been requested by that body. In the interim, he was a "maniac observer," who "made better and better lenses with the fantastic persistence of a lunatic." The range of substances he submitted to microscopic inspection could constitute a voyeur's paradise and their selection by unconscious fantasies could hardly have been lacking, but his led him to discover microbes, human sperm, and many other hitherto unseen phenomena.

There are some cases, especially from the early history of science, where mystical notions with obvious unconscious significance may have actually played a guiding role in important discoveries. A case in point is Kepler (29) who introduced physics into astronomy, claiming to have always been guided by the analogy of the Father, the Son, and the Holy Ghost. Kanzer (26, 27) and Koestler (29) both present interesting observations on the mysticism in the work of the early scientists. To claim, however, that unconscious or magical ideas, however well disguised, always play a contributory role in creative scientific work is unjustified.

There are many examples in the history of science where once the problem context had been selected, well planned and disciplined systematic investigation, rather than unconsciously inspired intuitions, led to important discoveries. For example, Becquerel's discovery that uranium salts spontaneously emit rays of an unknown nature fascinated Marie Curie. Whatever complex admixture of psychological determinants led to her fascination with the idea of a substance spontaneously emitting rays, she thereupon undertook a painstaking and systematic examination of this phenomenon in all chemical bodies. Eventually she inadvertently discovered a new element whose radiation was far stronger than that of uranium. At first supposing an experimental error, she became convinced after performing repeated measurements that she had discovered a powerful new radioactive element, radium. The history of science is replete with such examples. Without available analytic material on the scientist, it would seem unwarranted to suppose that regressive phenomena, either in terms of the form or content of psychic functioning, played a causal role in the actual discovery. Nor did Curie's work, for example, lack inspiration or emotional involvement (her own writings [9, 10] and the hardships she endured in carrying out her research attest to a profound dedication and deep aesthetic appreciation of science). Yet it does not seem necessary to attribute her great devotion to her work to primarily unconscious sources.

Many writers have noted that science affords an opportunity for the externalization of unconscious conflicts and that scientific thinking (like all thinking) is never sharply separated from primitive ideation, i.e., it has its unconscious collaterals. It is important to make some distinctions here. Unconscious ideas and conflicts may find expression in scientific work without entering into the creative result as a causal determinant. The work leading to scientific creation or discovery may be entirely conducted by rational, reality-oriented, autonomous cognitive and perceptual ego functions without precluding unconscious wishes from finding displaced sources of discharge. . . .

The Organizing Functions of the Ego

Hartmann (25) suggests that the organizing function of the ego includes its synthetic and differentiating functions. These ego functions can be considered in an analysis of scientific creation from the viewpoints of motivation, integrative processing, recognition, and retention.

To speak of a synthetic, differentiating, and organizing function of the ego is a shorthand way of referring to both principles of psychic organization as well as certain ego processes by which such principles are made operative. . . . In speaking of the research scientist's intellectual style as highly discriminatory, unusually integrative, requiring closure yet capable of tolerating ambiguity, seeking fundamental unities and relationships while rejecting oversimplification, markedly independent of pressures toward conformity, cognitively playful, and capable of violating and challenging conventional reality schemata while ultimately demanding exactness and rigor in reality confirmation, we are not just describing a manner of problem solving. Such a pattern of cognitive organization and reality testing carries with it deep intellectual motivations. We are dealing here with a cognitive matrix which defines intellectual problems as it resonates abrasively against gaps in understanding, logical contradictions, or reality obstructions. Solving problems not only satisfies the requirements of the synthetic function (Kris), but a highly developed synthetic function reacts to contradictions as problems demanding solution. Unlike the paranoid (and in certain respects the artist), the scientist activates his comprehensive need for cognitive synthesis in the mastery of reality rather than in the construction of fantasy. . . .

One feature of creative scientific thinking that has been much emphasized is the establishment of new unities in previous diversities, the perception of unexpected likenesses or new relationships between formerly unrelated phenomena (7, 29, 53). The establishment of the problem context in the intellectual sphere, the uniting of diverse cognitive and perceptual elements, the integration of the accidental with the expected and the new with the old, and the combining of diverse frames of reference are achievements of the ego's synthetic functioning. Discovery depends as well, of course, on penetrating discriminations and varying degrees of capacity to challenge accepted reality schemata.

For a detailed analysis of the manifold intellectual factors which are employed in productive thinking, and especially in creative problem solving, the reader is referred to Guilford's (23) comprehensive factor analytic studies of the structure of intellectual abilities. It is worthwhile noting here, however, that he found "no unitary ability to analyze and none to synthesis: . . . What the [factor] analytical results indicate is that each person is not at all uniformly able to analyze in all situations and with all kinds of material nor is he uniformly able to synthesize. His success in either respect is dependent upon the circumstances. Abilities to perform in tests involving analyzing are general aptitudes of various kinds, depending on the task. The same may be said regarding tests involving synthesizing" (p. 159).

Of particular importance from the psychoanalytic perspective is the role of the ego's synthetic function in establishing a preconscious context which keeps a scientific problem dynamically active and present, i.e., subject to preconscious efforts at solution even when consciousness is directed elsewhere, and safeguards relevant information from being forgotten. . . .

Alexander (1), among others, has stressed the value of prelogical over logical forms of thinking in establishing new connections that may lead to important discoveries, even in dreams. Koestler (29) has developed this hypothesis with great detail in the area of scientific creation, stressing the role of primary-process mechanisms in revealing hidden analogies. This is based on the premise that regressive modes of cognitive functioning can liberate the intellect from the restrictive structure of logical verbal thinking. . . .

Preconscious Thought Processes

The process of discovery is most commonly described in terms of four stages: preparation, incubation, illumination, and verification. During the first stage the problem-solving context is prepared under the direction of conscious critical thinking and the motivational base for pursuing the problem is consolidated. Exhaustive work is followed by a period in which attention is directed to other matters while conscious attempts at problem solving are relaxed (incubation). Illumination then occurs, after some varying interval of time, where the solution, or some glimpse of it, presents itself to conscious awareness with differing degrees of clarity and conviction. The final stage of verification is again one of intensive, consciously directed, critical thinking. This is clearly a gross schematic oversimplification, but a convenient one for purposes of discussion.

Scientists and psychologists alike have recognized the great importance of "nonconscious" processing of scientific problems during the incubation phase when conscious problem-solving attempts are temporarily suspended. Theories differ, however, as to the nature of the mental processes occurring outside of awareness which eventuate in sudden illuminations. Two prominent viewpoints hold that (a) such descriptively unconscious thinking involves an automatic random mixing of cognitive and perceptual elements of which the relevant combinations trigger off some detector mechanism; or that (b) unconscious problem-solving activity is characterized by meaningful thinking which nevertheless may cover a continuum in terms of both the level of thinking and the motivations of thought (psychoanalytic theory).

In psychoanalytic theory, preconscious mental processes involve a multilevel processing of motivations stemming from all layers of the psychic hierarchy and stimuli arising from all sensory modalities. They actively participate in all conscious thought, perception, and behavior, maintaining appropriate frames of reference, preparing automatized behavioral reactions, providing appropriate memories, organizing complex ideas as well as wish-fulfilling fantasies, etc. Hence, it is not the lack of participation of the preconscious processes that distinguishes one stage of creative thinking from another, but rather their utilization in a special way. Here some clarification is needed with regard to how self-regulated ego regression enters into the creative process and its relationship to consciousness.

During periods when conscious attention is not directed toward solving a scientific problem, but while there is yet evidence to infer problem solving has progressed, the gain is attributed to preconscious activity. The following are some of the potential creative resources which may be brought to bear when a scientific problem is subjected to a preconscious working over during "incubatory" phases (i.e., when it drops out of consciousness but continues to be pursued preconsciously). They do not necessarily involve any regressive alteration in the ongoing state of consciousness (although the outcome may), but are typically considered to involve more regressive modes of ego functioning due to the *absence* of conscious organizing forces. (a) The mere absence of consciousness provides freedom from some of the constraints of the cognitive and perceptual schemata which normally characterize that state. Consequently, transschematic and prelogical modes of thinking have freer reign in dealing with the unsolved problem. (b) Metaphorically speaking, as the problem passes out of awareness, it passes beneath countercathectic barriers at the frontiers of consciousness and becomes subject to all variety of unconsciously inspired fantasy elaboration, which, however distorted, unrealistic or primitive, may prove relevant for a creative solution. (c) Energy may be deflected, as Kris notes, from more reality-oriented ego functions to intensify preconscious thinking. (d) Inner boundaries among various realms of experience and between different ego structures (self boundaries, object boundaries, etc.) may be traversed preconsciously in arriving at unusual contiguities, novel connections, or new relationships. . . .

Beyond Regression

I believe that there has been an overemphasis on the role of regression in psychoanalytic discourse on creative thinking. This has occurred for several reasons: an overschematization of the polarities between primary-process and secondary-process functioning; a failure to appreciate the range of individual differences in characteristic modes of cognition; and the lack of an adequate conceptualization of the distinction between a reversion to primitive cognition as it once existed and the elaboration of primitive forms of mentation into highly advanced, adaptive resources.

Secondary-process thinking is usually identified with verbal, logical reality-directed thought, while primary-process thinking is identified with perceptual, symbolic, illogical, wish-fulfilling fantasy. Hence, thinking which involves symbols or perceptual elements is sometimes taken as *prima facie* evidence for regression from a more advanced level of psychic functioning to a more primitive one. This is in fact the most frequently cited evidence for regression in productive scientific thinking (19, 24, 29). It fails, however, to take into account significant individual differences in characteristic thought imagery and forms of cognition. The appearance of visual imagery or certain kinds of symbolization are in themselves insufficient evidence for primitive modes of thinking.

Roe (42) compared the thought imagery of eminent biologists, experimental physicists, theoretical physicists, and social scientists (sixty-one in all) using the categories of visual (including symbolic), auditory-verbal, kinesthetic, and imageless thought. There were significant individual and group differences in characteristic thought imagery. Social scientists showed the highest amount of verbal thinking (which may account for their overemphasis on verbal thinking as being characteristic of normal thought) and the experimental physicists the least. . . .

While verbal thought may be essential for *communication,* it is not necessarily essential or even most efficacious for certain types of reality-oriented thinking and problem solving. We are faced here with conflicting criteria of regression. While perceptual and symbolic thought developmentally precede verbal thought (and hence constitute evidence of a temporally regressive sensory mode of thinking), they may nonetheless be coordinated with highly advanced conceptual structures in a stabilized cognitive facility so that they become the most *progressive* and *adaptive* form of reality thinking. . . .

A developmentally primitive aspect of thinking which becomes elaborated into a highly adaptive and stable feature of reality testing and reality thinking enters into the creative act not as a regression in the service of the ego, but as a special secondary-process cognitive facility. This would seem to apply not only to the use of nonverbal imagery in thinking, but also to certain elaborations of the ego's fantasying function and its capacity to imaginatively project fantasy constructions into reality perception while still maintaining adequate reality testing. Faraday's vivid visualizations of lines of electrical and magnetic force in space was a form of productive thinking in a physicist completely lacking in advanced mathematical training. It is also interesting, in this connection, that Roe's (40, 41) Rorschach comparison of physicists and biologists indicated an unusual emphasis by physicists on three-dimensional visualization and inanimate movement responses, suggesting the prominence of a particular imaginative facility in investigating stimuli. Even Lorand's (34) inventor, whose fantasy capabilities were often used in the service of drive discharge, nonetheless displayed a high ego involvement in *fantasy as a form of thinking,* which must have been a great asset in the reality tasks of planning and constructing his numerous inventions.

When fantasy is used as a substitute for reality in the service of wish fulfillment, it constitutes a regression from the adaptive and dynamic viewpoints, and depending on its affect, form, content, and volitionality, it may also be regressive from an economic, structural and/or topographic viewpoint. But fantasy as a visual form of thinking may also be deliberately employed in the service of understanding, planning, designing, and a host of other intellectual operations which require the presence of objects in thought which are not immediately present in reality and which cannot be adequately represented through verbal thinking. The old saw that a picture is worth a thousand words is relevant not just for wish fulfillment, but for purposes of reality mastery as well.

Another way in which regression may

too hastily be invoked pertains to the notion that freedom from too rigid a reality orientation is dependent upon the mobility of instinctual energy and the disregard of the primary processes for reality. There is no question but that primary-process mentation may in fact provide such freedom, but there are also important individual differences in cognitive styles (i.e., the organizing principles of secondary-process functioning) which determine the "freedom" with which the secondary processes can be employed. Gardner et al. (17) have done some pioneering work in this field within a psychoanalytic context:

> Beyond its sketchily conceived notion of "character defense," however, the traditional form of psychoanalytic theory is relatively poor in structural concepts that can account for individual differences in mental processes. For example, the possibility that "conflict-free" cognitive functions are *idiosyncratically* organized in individuals has not yet been explored by psychoanalysis. It is precisely in this respect that the concept of cognitive control, with its provision for adaptively adequate yet various modes of encountering reality, may prove a useful addition to the theory [17, p. 9].

While cognitive styles may derive from conflict and relate to particular character and defensive structures, they become themselves autonomous structures which may facilitate or hinder creative thinking. The cognitive control dimensions explored by Gardner et al. (such as leveling-sharpening, tolerance for unrealistic experience, equivalence range, scanning, construction-flexibility, and field dependence-independence) have important implications for individual differences in creative functioning, and many of the intellectual qualities which have been found to be characteristic of creative scientists can be translated into cognitive control dimensions.

Very often what is more critical for scientific discovery are the cognitive control principles which regulate the secondary processes. What is loosely called intellectual courage or timidity or overcautiousness often refers to such factors as one's capacity to sustain ambiguity, to resist premature closure, to tolerate percepts and thoughts which do not conform to familiar schematizations, etc.

Taton's remarks about Poincaré's failure to formulate for himself the theory of relativity are pertinent here: "Poincaré, who had so much wider a mathematical background than Einstein, then a young assistant in the Federal Patents Office of Berne, knew all the elements required for such a synthesis, of which he had felt the urgent need and for which he had laid the first foundations. Nevertheless, he did not dare to explain his thoughts, and to derive all the consequences, thus missing the decisive step separating him from the real discovery of the principle of relativity" (53, pp. 134–135).

Of course, more than intellectual factors are involved here. It takes a certain type of personal situation or character structure to be able to dissent from established opinion and cherished ideas when logical conclusions and experimental findings call for it. Many fundamental discoveries (including those by Freud) have involved a sharp break from the most solidly established opinions and preconceptions of the day, often bringing a scientist who would prefer to avoid personally toned controversy into the most bitter disputations with the leading scientists of his time.

The type of psychological freedom which would seem most essential to scientific discovery is not the freedom to regress in a controlled way, but freedom from the complex array of fears and anxieties which inhibit the fullest and freest use of reality-oriented cognitive functions, although the two are not unrelated. What most importantly characterized the creative phases of the scientists Giovacchini studied was an expansion and enhancement of the secondary processes rather than an increased accessibility of drive-related ideational content or more primitive modes of ego functioning. Yet an excessive fear of the primitive in oneself and of the unconscious symbolic meanings of the creative act is a primary source of construction and inhibition in intellectual functioning. While primitive levels of the personality in terms of fantasy content or the formal organization of thought and perception may have no direct causal contribution to make to a creative product in many instances of scientific innovation, the defensive measures directed against the primary process may result in a rigidly schematic and conventional reality orientation which lacks creative freedom and spontaneity. In a similar fashion, an inability to abandon, at least temporarily, the security of established dogma, accepted authority, and the self-affirmation that stems from shared beliefs and the confirmation of one's views by others can curtail the freedom to see and think about things in a new way.

It has been argued here that a creative openness to scientific problems may entail predominantly the advanced reality-testing functions of the ego and need not be based on a reversion to childhood modes of thinking. In part, the overemphasis on regression in creativity would seem to stem from the early psychoanalytic approach to art as a disguised presentation of forbidden wishes and a reversion to childhood modes of perception. More importantly, it may reveal a biased approach to secondary-process functioning. Very often the secondary processes are implicitly equated with thinking, perceiving, and acting in a rigidly conventional manner according to socially shared reality schemata, i.e., functioning as a well-programmed computer. From such a viewpoint, the source of novel, original, or unconventional ideas can only be the primary processes. However, just as people differ in their openness to the primitive within themselves, so they also differ in their openness to what is outside themselves, both in terms of their responsiveness to reality (or certain aspects of reality) and in terms of their capacity to suspend, recombine, and transcend familiar reality constructs—essential ingredients in many scientific advances.

Clearly, primitive wishes, fears, and modes of ego functioning do have a place in scientific creativity. The essential point is that their role must be spelled out in greater detail while the creative use of the secondary process should not be neglected. Such concepts as "overdetermination" and "multiple function," which have been valuable for understanding the manifold significance of human behavior, may also create a predisposition toward enumerating the many sides to the creative act or personality without seriously considering *causal* relationships, the necessity or sufficiency of particular factors for a creative result.

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A Theory of Vocational Development

Donald E. Super

Two and one-half years ago a colleague of mine at Columbia, Dr. Eli Ginzberg, an economist, shocked and even unintentionally annoyed many members of the National Vocational Guidance Association by stating, at the annual convention, that vocational counselors attempt to counsel concerning vocational choice without any theory as to how vocational choices are made. A year later Dr. Ginzberg published his monograph on *Occupational Choice*, in which he stated:

> Vocational counselors are busy practitioners anxious to improve their counseling techniques . . . the research-minded among them devote what time they can to devising better techniques. They are not theoreticians working on the problem of how individuals make their occupational choices, for, though they have no bias against theory, they have little time to invest in developing one (10, p. 7).

Ginzberg continues, apropos of the fields of psychology and economics:

> . . . there are good reasons why the problem [of how occupational choices are made] has not been a focus of investigation for psychology or economics. . . . The process has roots in the interplay of the individual and reality, and this field is only now beginning to be included in the boundaries of psychological inquiry. The obverse formulation applies to

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> economics, which as a discipline concentrates on a detailed analysis of reality forces and satisfies itself with a few simplified assumptions about individual behavior (10, p. 7).

These conclusions were based partly on a review of the research literature which I did at his request, and partly on a number of discussions in which he, his research team, and I participated. Consequently, I have a feeling of responsibility, not for the conclusions which he drew, but for drawing my own conclusions and for sharing them with my colleagues in psychology and guidance.

Basis of Ginzberg's Criticisms. It may help to point out that Ginzberg's conclusions were based on a review of the research literature which was designed to provide answers to specific questions asked by his research team in order to help them plan their own research project. What synthesizing of results I did was undertaken to answer these questions. I did not attempt to answer the question "What theories underlie the principles of vocational guidance now generally accepted by practitioners?"

But I do agree with his analysis of the situation with regard to theory construction: we have done relatively little of it, and for the reasons he has suggested. However, this does not mean that we have operated without theory. It is the principal purpose of this paper to set forth a theory of vocational development, a theory inherent in and emergent from the research and philosophy of psychologists and counselors during the past two decades. But first I should like, as a help in formulating a more adequate theory, briefly to present the theory of occupational choice put forth by Ginzberg and his associates, to show how each of its elements had already been set forth by psychologists doing research in this field, and to point out some of its limitations.

THE GINZBERG THEORY

As Ginzberg, Ginsburg, Axelrad, and Herma summarize their theory of occupational choice, it contains four elements:

1. *Occupational choice is a developmental process which typically takes place over a period of some ten years.* This theory of Ginzberg's, it should be noted, is one of the points made by the official statement of the *Principles and Practices of Vocational Guidance* (33), first formulated by the National Vocational Guidance Association 25 years ago; it is a point stressed by Kitson in his *Psychology of Vocational Adjustment* (14), published in 1925; and, in 1942, in my own *Dynamics of Vocational Adjustment* (28) several pages are devoted to a discussion of the fact that "choosing an occupation . . . is a process which . . . may go on over a long period."

2. *The process is largely irreversible:* experience cannot be undone, for it results in investments of time, of money, and of ego; it produces changes in the individual. This second theory of Ginzberg's is clearly implied in Charlotte Buhler's 20-year-old theory of life stages (5), in Lehman and Witty's equally old studies of play interests (15), in Pressey, Janney, and Kuhlen's 13-year-old discussion of adolescent and adult development (20), and in my own 10-year-old text on vocational adjustment (28).

3. *The process of occupational choice ends in a compromise between interests, capacities, values, and opportunities.* This third theory of Ginzberg's is well illustrated in the practices of individual diagnosis developed by the Minnesota Employment Stabilization Research Institute 20 years ago and described by Paterson and Darley (19); it was further demonstrated and described by the Adjustment Service experiment 17 years ago (2); and it is basic to presentations of the use of diagnostic techniques in texts such as Bingham's (3) and mine (29), both of which appeared before the completion of Ginzberg's study. In fact, Frank Parsons (18), in 1909, discussed vocational counseling as a process of helping the individual to study both himself and possible occupational opportunities, and to work out a compromise between his abilities, interests, and opportunities. He called this last process "true reasoning."

4. Ginzberg's final theoretical formulation is that *there are three periods of occupational choice:* the period of *fantasy* choice, governed largely by the wish to be an adult; the period of *tentative* choices beginning at about age 11 and determined largely by interests, then by capacities, and then by values; and the period of *realistic* choices, beginning at about age 17, in which exploratory, crystallization, and specification phases succeed each other.

Those who are acquainted with Lehman and Witty's early research in the change of interest with age (15), with Strong's more searching work (25) in the same area, with Sisson's research in the increasing realism of choice with increasing age (23), with Charlotte Buhler's research in life stages (5), and with the use made of these data by Pressey (20) or by me (28), will find these three choice periods familiar. The special contribution of Ginzberg and his associates is the postulation of the successive dominance of interests, capacities, and values as determinants of choice before reality begins to play a major role.

It is easy, and perhaps even rather petty, thus to take a theoretical contribution and demonstrate its ancestry, showing that there is nothing particularly original about it. This is, undoubtedly, the normal reaction to claims of originality. But originality is more generally the result of a rearrangement of the old than the actual creation of something new: the rearrangement is original because it brings out details or relationships which have been missed or points up new applications. Ginzberg's theory is indeed an important contribution: this seems clear to me, at least, as I recollect the struggle I had in writing parts of my *Dynamics of Vocational Adjustment* (a struggle which resulted from the lack of a theoretical structure and from inadequate research), and as I work on its revision in the light, among other things, of Ginzberg's theoretical formulation and the thinking which it has stimulated. I have used this critical approach to Ginzberg's work in order to demonstrate that we have not entirely lacked a theoretical basis for our work in vocational guidance, and to show that the elements of theory on which we have based our practice have been sound, at least in that they have foreshadowed the elements which one group of theorists used when they went about constructing a theory of occupational choice.

Limitations of Ginzberg's Theory

But this is not the whole story. Ginzberg's theory is likely to be harmful because of its limitations, limitations other than those of research design and numbers in his basic study.

First, it does not build adequately on previous work: for example, the extensive literature on the nature, development, and predictive value of inventoried interests is rather lightly dismissed.

Second, "choice" is defined as preference rather than as entry or some other implementation of choice, and hence means different things at different age levels. To the 14-year-old it means nothing more than preference, because at that age the need for realism is minimized by the fact that the preference does not need to be acted upon until the remote future. To the 21-year-old student of engineering, on the other hand, "choice" means a preference which has already been acted upon in entering engineering school, although the final action will come only with graduation and entry into a job. No wonder that reality plays a larger part in choice at age 21, when, unlike choice at age 14, it is by definition a reality-tested choice!

A third defect in Ginzberg's theory emerges from these different meanings of the term "choice" at different ages: it is the falseness of the distinction between "choice" and "adjustment" which he and his research team make. The very fact that choice is a continuous process going on over a period of time, a process rather far removed from reality in early youth but involving reality in increasing degrees with increasing age, should make it clear that there is no sharp distinction between choice and adjustment. Instead, they blend in adolescence, with now the need to make a choice and now the need to make an adjustment predominating in the occupational or life situation.

Finally, a fourth limitation in the work of the Ginzberg team lies in the fact that, although they set out to study the process of occupational choice, and although they properly concluded that it is one of compromise between interests, capacities, values, and opportunities, they did not study or describe the compromise process. Surely this is the crux of the problem of occupational choice and adjustment: the nature of the compromise between self and reality, the degree to which and the conditions under which one yields to the other, and the way in which this compromise is effected. For the counseling psychologist's function is to help the individual to effect this compromise. He must not only know the factors which must be compromised and how these have been compromised in the experience of others, but also the dynamics of the compromising process, so that he may facilitate this process in his counselee with constructive results.

ELEMENTS OF AN ADEQUATE THEORY OF VOCATIONAL DEVELOPMENT

An adequate theory of vocational choice and adjustment would synthesize the results of previous research insofar as they lend themselves to synthesis; it would take into account the continuity of the development of preferences and of the differences in the stages, choices, entry, and adjustment; it would explain the process through which interest, capacities, values, and opportunities are compromised. The second part of this paper will be devoted to a sketch of the main elements of such a theory of vocational development as they appear in the literature, and the third and final part will consist of an attempt to synthesize these elements in an adequate theory. The term "development" is used rather than "choice," because it comprehends the concepts of preference, choice, entry, and adjustment. There seem to be a dozen elements to a theory of vocational development: they are taken up in sequence.

Individual differences. One of the basic elements of a theory of vocational development has been the theory of individual differences, a cornerstone of modern educational and vocational psychology. Kitson based much of his early *Psychology of Vocational Adjustment* (14) on this theory and on the findings on which it was based. It was essential to the work of the Minnesota Employment Stabilization Research Institute (19). It is surely unnecessary to document the fact of individual differences in aptitudes, interests, and values, or the significance of these differences for vocational development.

Multipotentiality. A second basic element of theory has been the concept of the occupational multipotentiality of the individual. It was first documented for intelligence by Army psychologists in World War I, and was stressed by Kitson in his early textbook. It was documented for interests by Strong's work on the classification of occupational interests (26). It is a well-established fact and a basic assumption of vocational counseling that each person has the potential for success and satisfaction in a number of occupations.

Occupational Ability Patterns. The existence of occupational ability patterns, that is, the fact that abilities and interests fall into patterns which distinguish one occupation from another, was established by the Minnesota Employment Stabilization Research Institute (19) and has been confirmed in other studies, particularly those of the United States Employment Service (8). People have been found to prefer, enter, remain in, like, and succeed most consistently in occupations for which they have appropriate patterns of traits. The theory of the patterning of aptitudes and interests within individuals and within occupational families and the significance of this patterning for choice, entry, and adjustment are widely accepted and applied by counselors and psychologists today.

Identification and the Role of Models. Much has been made of the importance of identification with parents and other adults in individual development by psychoanalytically oriented writers, and this concept is widely used by counseling psychologists regardless of orientation. It has been little documented, however, in psychological research in the vocational choice and adjustment process. The work of Friend and Haggard (9) and a study by Stewart (1) do, however, provide some objective basis for the theory that the childhood and adolescent identifications play a part in shaping vocational interests, and also provide role models which facilitate the development and implementation of a self-concept, provided that the required abilities and opportunities are present.

Continuity of Adjustment. The continuity of the adjustment process was stressed by Kitson in his 1925 textbook as a result of his analysis of the careers of men whose success was attested to by being listed in *Who's Who in America.* The fact that adolescents and adults face a succession of emerging problems as they go through life, and that some of these problems are peculiar to the various life stages, was brought out by the studies of life stages made by Charlotte Buhler (5) and by those of occupational mobility conducted by Davidson and Anderson (7), Strong (26), and Miller and Form (16). And theories of the development of interests have been formulated by

Carter (6) and by Bordin (4), theories which I modified slightly in my book on testing and upon which I drew in describing the process of vocational choice and adjustment in a speech first made at Ft. Collins, Colorado, in 1949, revised several times, and later published in the journal *Occupations,* under the title of "Vocational Adjustment: Implementing a Self-Concept" (30). These formulations are drawn on again as the cement for the various elements which need to be brought together in a theory of vocational development and as an explanation of the process of compromise between self and reality.

Life Stages. The work of psychologists and sociologists in describing the stages through which growth and development proceed, and in showing how these stages bear on the process of vocational choice and adjustment, has already been referred to. It was drawn on heavily in the text by Pressey, Janney, and Kuhlen (20), in my own first text (28), in Ginzberg's research (10), and in a recent text on *Industrial Sociology* by Miller and Form (16) which is as important for its original contribution and synthesis as it is annoying for its bias against anything that does not conform to sociology as they conceive of it. Buhler's theory of development through the exploratory, establishment, maintenance, and decline stages is translated into occupational terminology by Miller and Form, who also documented the theory for American careers, while Ginzberg, Ginsburg, Axelrad, and Herma have developed in more detail the phases of the exploratory stage. This latter theory needs confirmation with a larger sample and more objective procedures, in view of Small's (24) recent failure to confirm it with a somewhat different adolescent sample, but the general theory of life stages is basic to vocational guidance and will be drawn on heavily in my attempt at synthesis.

Career Patterns. The formulation of a theory of career patterns resulted from the occupational manifestations of life stages first documented by Davidson and Anderson (7), added to for a select group by Terman's genetic studies of gifted persons (31), and then pointed up by Ginzberg and his associates (10) and by Miller and Form (16). Career pattern theory appears to be a key element in the theoretical basis of vocational guidance, for it gives the counselor basic assumptions concerning the social, educational, and occupational mobility of his counselees, and it enables him to foresee types of problems which a given client is likely to encounter in establishing a career.

Development Can Be Guided. Another basic element in a theory of vocational development is the theory that development through the life stages can be guided. Although there is ample evidence that ability is to some extent inherited, and that personality too has its roots in inherited neural and endocrine make-up, there is also good evidence that manifested aptitudes and functioning personality are the result of the interaction of the organism and the environment. It is a basic theory of guidance as we know it today that the development of the individual can be aided and guided by the provision of adequate opportunities for the utilization of aptitudes and for the development of interests and personality traits.

Development the Result of Interaction. That the nature of the interaction between the individual and his environment is by no means simple has been brought out by a variety of investigations ranging from studies of the effects of foster homes and of education on intelligence (17) to evaluations of the effects of occupational information and of test interpretation on vocational plans and on self-understanding (13). The realization of this fact and the acceptance of this principle have led to a greater humility in our claims for counseling and to a greater degree of sophistication in our use of guidance techniques.

The Dynamics of Career Patterns. The interaction of the individual and his environment during the growth and early exploratory stages, little understood though the process actually is, has been much more adequately investigated than has this same process during the late exploratory, establishment, and maintenance stages. We still know relatively little about the dynamics of career patterns. Terman's work (31) tells us something about the role of intelligence, Strong's (26) about interests, and Hollingshead's (11) about social status, but no adequate studies have been made of the interaction of these and other factors in determining whether the individual in question will have a career pattern which is typical or atypical of his parental socioeconomic group. It was partly with this objective that an investigation known as the Career Pattern Study was launched in Middletown, New York, last year.

Job Satisfaction: Individual Differences, Status, and Role. Early theories of job satisfaction stressed the role of intelligence and interest in adjustment to the occupation or to the job, building on studies of the relationships between these traits and occupational stability such as those made by Scott (22, ch. 26) and by Strong (26). More recently other investigations such as the Hawthorne (21) and Yankee City studies (32), anticipated in this respect by Hoppock's work (12) and by a minor study of mine (27) in job satisfaction, have played up the importance of the status given to the worker by his job, status both in the sense of group membership or belongingness and of prestige.

While researchers interested in the role of one kind of factor or another have tended to emphasize the signal importance of that type of factor, there is nothing inherently contradictory or mutually exclusive in these findings. They can all be included in a comprehensive theory of job satisfaction or work adjustment. This is the theory that satisfaction in one's work and on one's job depends on the extent to which the work, the job, and the way of life that goes with them, enable one to play the kind of role that one wants to play. It is, again, the theory that vocational development is the development of a self concept, that the process of vocational adjustment is the process of implementing a self concept, and that the degree of satisfaction attained is proportionate to the degree to which the self concept has been implemented.

Work is a Way of Life. This leads to a final theory, one that has been more widely accepted and stressed by sociologists than by psychologists, but familiar to most counselors and considered basic by some writers in the field. This is the theory that work is a way of life, and that adequate vocational and personal adjustment are most likely to result when both the nature of the work itself and the way of life that goes with it (this is, the kind of community, home, leisure-time activities, friends, etc.) are congenial to the aptitudes, interests, and values of the person in question. In the estimation of many, this is a basic element in a theory of vocational development.

A THEORY OF VOCATIONAL DEVELOPMENT

Now that we have surveyed the diverse elements of a theory of vocational development, there remains the final task of organizing them into a summary statement of a comprehensive theory. The theory can be stated in a series of ten propositions:

1. People differ in their abilities, interests, and personalities.

2. They are qualified, by virtue of these characteristics, each for a number of occupations.

3. Each of these occupations requires a characteristic pattern of abilities, interests, and personality traits, with tolerances wide enough, however, to allow both some variety of occupations for each individual and some variety of individuals in each occupation.

4. Vocational preferences and competencies, the situations in which people live and work, and hence their self concepts, change with time and experience (although self concepts are generally fairly stable from late adolescence until late maturity), making choice and adjustment a continuous process.

5. This process may be summed up in a series of life stages characterized as those of growth, exploration, establishment, maintenance, and decline, and these stages may in turn be subdivided into (*a*) the fantasy, tentative, and realistic phases of the exploratory stage, and (*b*) the trial and stable phases of the establishment stage.

6. The nature of the career pattern (that is, the occupational level attained and the sequence, frequency, and duration of trial and stable jobs) is determined by the individual's parental socioeconomic level, mental ability, and personality characteristics, and by the opportunities to which he is exposed.

7. Development through the life stages can be guided, partly by facilitating the process of maturation of abilities and interests and partly by aiding in reality testing and in the development of the self concept.

8. The process of vocational development is essentially that of developing and imple-

menting a self concept: it is a compromise process in which the self concept is a product of the interaction of inherited aptitudes, neural and endocrine make-up, opportunity to play various roles, and evaluations of the extent to which the results of role playing meet with the approval of superiors and fellows.

9. The process of compromise between individual and social factors, between self concept and reality, is one of role playing, whether the role is played in fantasy, in the counseling interview, or in real life activities such as school classes, clubs, part-time work, and entry jobs.

10. Work satisfactions and life satisfactions depend upon the extent to which the individual finds adequate outlets for his abilities, interests, personality traits, and values; they depend upon his establishment in a type of work, a work situation, and a way of life in which he can play the kind of role which his growth and exploratory experiences have led him to consider congenial and appropriate.

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The Occupational Role of the Child: A Research Frontier in the Developmental Conceptual Framework

Roy H. Rodgers

INTRODUCTION

The developmental conceptual framework has as its chief focus the analysis of change in family roles over time. In carrying out such an analysis attention is paid, primarily, to three areas: (1) the structure of the society in which the family exists, (2) the structure of the family per se, and (3) the role dynamics of the family viewed primarily as a consequence of specific age, sex, and plurality compositions. The details of the framework have been set forth elsewhere.[1] The concern of this paper is with the application of the framework to a specific research problem in an effort to demonstrate its utility in extending our knowledge of family behavior.

Because the developmental framework makes possible both macroscopic analysis in terms of the attempt to account for the entire life span of the family group, and microscopic analysis in terms of examination of the role complexes existent in the family at any point in time, there is a practical necessity for deciding the limitations which will be placed on any given research problem. Ideally, one would study all of the roles, role complexes, role sequences, positional careers, and so on, for the entire career of the nuclear family. More realistically, feasible research may be done, for example, on a given role sequence over its entire career (or portion thereof), on a specific role complex set over a limited period, or, perhaps, on the total role complex content of the family system at one or two points in time. Such choices are governed, of course, by the usual considerations of content areas of interest, financial and time limitations, and available facilities. This paper describes a model being utilized for research in progress on a given role sequence, namely, the development of the occupational role of the child position of a family over a specified period, namely, from early adolescence through young adulthood.

RESEARCH PROBLEM

As the major early socialization agency and as the one in which there is continuity of experience from birth to young adulthood, the family has received considerable attention in the literature on occupational choice, career

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[1] Reuben Hill and Roy H. Rodgers, "The Developmental Approach," in *Handbook of Marriage and the Family* (ed.) Harold T. Christensen (Chicago: Rand McNally & Co., 1964), chap. 5. See also Roy H. Rodgers, "Toward a Theory of Family Development," *Journal of Marriage and the Family*, 26 (August 1964), pp. 262–270, and Evelyn Duvall, *Family Development* (rev. ed.; Philadelphia: J. B. Lippincott Co., 1962).

orientation, achievement motivation, and like issues.[2] There is in this literature, however, a peculiar and serious weakness. While recognition is given to the importance of the socialization experience in the family for occupational and career outcomes, a review of the literature reveals no intensive analysis of the *process* of family socialization as it refers to occupation and career. Rather, there is a tendency to identify conditions of family setting, e.g., authority patterns, emotional relationships, family size, ordinal position, or child rearing techniques, and to relate these to occupational choice, aspiration level, or some other similar dependent variable. While these characteristics are certainly of interest, the developmental conceptual approach would provide a somewhat different focus in attempting to determine the manner in which families go about presenting the world of occupations and work to children and the related process of the development of the occupational role in the child.

This interest in the socialization process over time arises out of the fact that, first, it is this process which instills in the child the concepts of acceptable roles and aspirations with regard to himself and those with whom he interacts.[3] Secondly, as both Super and Ginzberg[4] have argued in their theories of occupational choice, there is a developmental aspect of the process of socialization into occupational roles. Slocum[5] has criticized both of these theorists for their failure to develop fully the sociological aspects of occupational choice, however. This criticism is probably more justified with respect to Ginzberg than it is to Super, particularly in Super's more recent work. It is true, nevertheless, that the sociological aspects of occupational choice have not been the primary concern of either analyst. The developmental conceptual framework, therefore, would demand going further to indicate that a thorough understanding of occupational and career outcomes cannot be obtained until a great deal more is known about the family dynamics of occupational role development. In doing this, an attempt would be made to fill some of the gaps in knowledge with respect to the socialization process which William Sewell so excellently identified in his review of the work in that area.[6]

While, in the view of some, it might be highly desirable to state the research problem in the form of testable hypotheses of relationships between clearly specified variables, the state of development in the areas under consideration is simply too primitive to allow for such sophistication. In the first place, the theories are not well enough advanced to allow for systematic derivation of testable hypotheses. Secondly, substantive knowledge has not been developed fully enough to permit the formulation of alternative hypotheses for test. Therefore, the problem under consideration lies at the "research frontier" on at least three counts: (1) not enough is known about the sociology of occupational choice, (2) not enough is known about the process of socialization in the family, and (3) the developmental conceptual framework has not been adequately specified to make such hypotheses possible.

To be considered here is a research question, rather than research hypotheses. This question is essentially a descriptive one, as opposed to an explanatory one, though it should be pointed out that adequate description generally precedes adequate explanation. Simply stated, the question would read, "What are the characteristics of the family process whereby the occupational (or career) role is developed in the child position of the family system?" The remainder of this paper will be concerned with the description of an analytical model based on the developmental conceptual framework which is designed to explore this research question.

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[2] A selected bibliography of studies of occupational development in which some attention is paid to family factors is available directly from the author.

[3] Francis E. Merrill, *Society and Culture* (3d ed.; Englewood Cliffs, New Jersey: Prentice-Hall, 1965), pp. 153–156.

[4] Eli Ginzberg *et al.*, *Occupational Choice* (New York: Columbia University Press, 1951); Donald E. Super, *The Psychology of Careers* (New York: Harper & Bros., 1957); Donald E. Super *et al.*, *Career Development: Self-Concept Theory* (Princeton, New Jersey: College Entrance Examination Board, 1963).

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THE MODEL FOR ANALYSIS

Assumptions

The family developmental conceptual approach begins with two key assumptions. First, the family system is composed of a relatively clearly defined and limited set of positions each of which is further composed of a set of relatively clearly defined and limited set of roles. The definition of these positions and roles is derived primarily from the social structure of the society in which the family system is located and secondarily from the plurality and age-sex structure of the given family group. Secondly, any new member of the family system has a set of latent roles assigned to him chiefly as a result of his particular age and sex. In this respect, the neonate is seen as being asocial and the development of the particular latent roles into manifest roles is in the main a result of his interactions, i.e., as actor and reactor, with members of the family and other societal members throughout his life span. It is clear that these two assumptions are derived, respectively, from structure-function[7] and symbolic interactionist[8] theoretical formulations. The norms, therefore, of any given role in a position have three sources of definition and modification: (1) the society, (2) the family in which they occur, and (3) the individual actor to which they refer.[9] What is of particular concern, however, to the developmental analyst of the family is the manner in which these normative role definitions change over time as the age, sex, and plurality structure of the family system changes throughout its career. In singling out the occupational role of the child for attention, therefore, the research concern would be with identifying these changes in normative definition as the role develops from latency in early life to a manifest part of the position in young adulthood.

The Basic Model

The analysis of occupational role development might best be seen in terms of a "flow model." Any specific occupational or career outcome has certain prerequisites which, if incorporated into the role of the actor, provide him with the capabilities for carrying on that occupation. These prerequisites might include talent, skills, training, sensitivities, and orientations of particular kinds. If these prerequisites are not attained, the actor will be unable to respond with the appropriate role behavior as designated by the norms for that role. Therefore, certain "inputs" must be injected at points throughout the role sequence of the developing occupational role. An appropriate analogy might be that there are certain "valves" existing along the "pipeline" which either are opened, thus allowing certain inputs, or closed, thus failing to allow certain inputs into the flow. The basic analysis, then, involves the identification of these inputs and the timing of them along the flow line. Figure 1 provides a pictorial representation of the flow model.

Limiting Factors

The first set of inputs may be viewed as the initial composition of the flow. These may be seen as primarily limiting factors in that they predispose certain valves along the flow line to be automatically closed. They would include the basic characteristics of the neonate actor such as physical capacities, mental capacities, and sex. Thus, an actor who was born with some physical handicap, for example, might never be able to attain the appropriate prerequisites for becoming a professional athlete. Or, again, a female actor in a given society would have closed to her certain input valves which would prepare her for occupations defined as "male occupations."

The remaining sets of inputs are seen as having the potentiality of occurring at various periods, and in many cases at several periods, along the flow line. A key part of the analysis would be to identify these periods and then determine the effect of the failure to have these inputs injected at their appropriate places in the flow. It may be hypothesized, for example, that the failure to inject a key input at the appropriate time would automatically close subsequent valves, thus narrowing the possible

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[7] Cf., for example, Talcott Parsons, "The Social Structure of the Family," in *The Family: Its Function and Destiny* (ed.) Ruth Nanda Anshen (rev. ed.; New York: Harper & Bros., 1959), pp. 241–274.

[8] Sheldon Stryker, "The Interactional and Situational Approaches," in Christensen, *op. cit.*, chap. 4.

[9] Rodgers, *op. cit.*, p. 268.

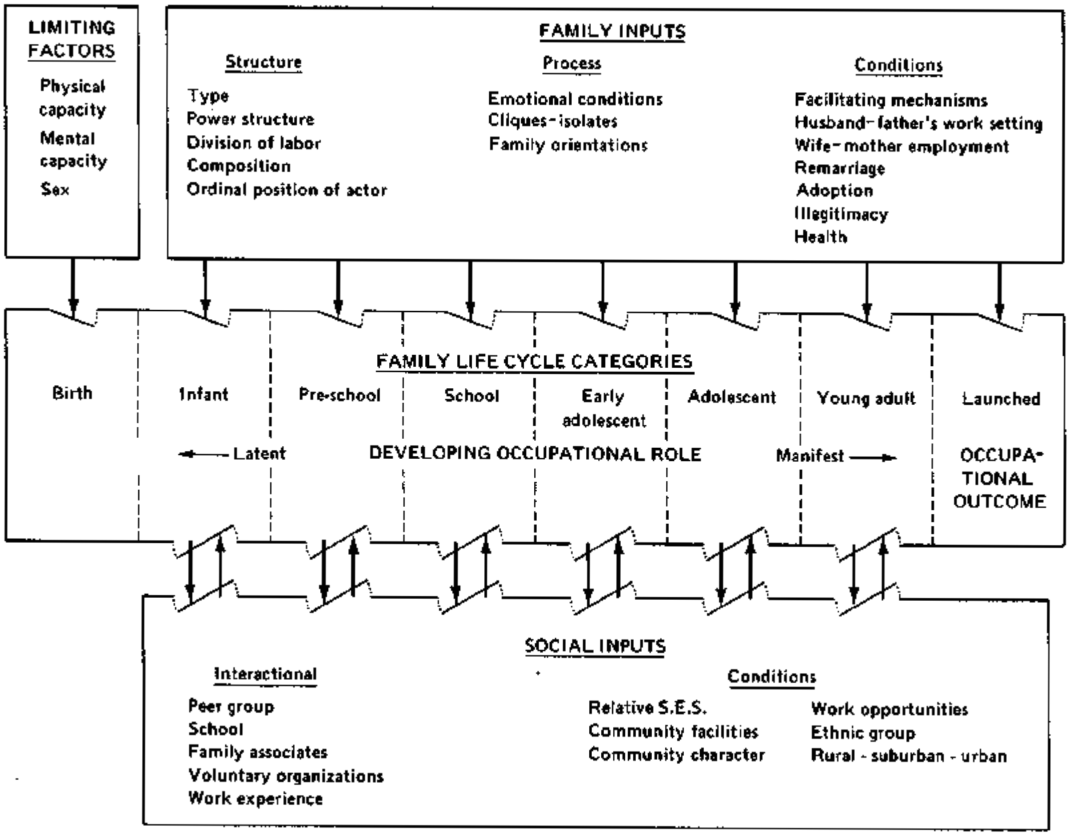

Fig. 1. The Developing Occupational Role in the Child Position

occupational outcomes at the end of the flow. It may be appropriate to emphasize at this time that these inputs are viewed as providing, either directly or indirectly, the basis of a cumulative set of norms which ultimately will define the occupational role of the actor in the society. The focus here will be on those inputs which are directly a part of the family of orientation experience or, if they have their source in the society, have relevance within the family of orientation. As identified earlier, the three primary sources of such normative definitions are the actor, the family, and the society. In the discussion to follow, the actor will be viewed as a continuous dynamic factor, acting and reacting to the various inputs from the other two major sources, the family and the society. Thus, this model is committed to neither a deterministic view of occupational outcome nor to a completely rational view. That is, the model recognizes that the actor will encounter choice points in his individual experience where the selection of one alternative course of action rules out the achievement of other goals. However, at the same time, the model takes into account the fact that the alternatives available and the manner in which the actor perceives them and evaluates them is to some extent determined by his cultural and social experience.

Furthermore, it is important to keep in mind that the developmental conceptual approach consistently analyzes the effect of such inputs on an actor in terms of the actor's general physical, psychological, and social maturational level. An additional important aspect of the analysis, then, would be the determination of the appropriateness of the timing of the inputs with respect to this general maturational level. Or, to put it in terms of the flow analogy, the degree to which a given input "mixes" well with the already existing substance in the flow must be considered. In effect, identified here is the concept of "developmental task" which has been defined elsewhere as

> a set of norms (role expectations) arising at a particular point in the career of a position in a social system, which, if incorporated by the occupant of the position as a role or part of a role cluster, brings about integration and temporary equilibrium in the system with regard to a role complex or set of role complexes; failure to incorporate the norms leads to lack of integration, application of additional normative pressures in the form of sanctions, and difficulty in incorporating later norms into the role cluster of the position.[10]

Family Inputs

The family contributions to the continuing flow may be categorized into those having to do with the *structure* of the family, those having to do with family *process*, and those having to do with certain family *conditions*. While the model is not completely developed with respect to specifying all of the variables within each of these categories, it is possible to indicate the types of inputs to be considered in each category.

Family Structure. In considering the type of family structure, one very appropriate typology might be Litwak's "nuclear, modified extended, and extended" family structure.[11] The significance of such a typology, of course, lies in the fact that it specifies the positional content of the family system and, thus, the kinds of role complexes within which the actor may be placed. Furthermore, in a recent conversation, Litwak indicated that it was his intention to consider modifying his own formulation concerning these typologies to include the possibility that a given family may very well change its type of structure over the life cycle. That is, it is possible that newlyweds in American society may emphasize the isolated nuclear type of family structure, but the post-parental couple may emphasize the modified extended type. It will be necessary, then, to be alert to the possibility of the structural type input changing during the development of the occupational role and to determine the effect which such change has on this role development.

In analyzing family power one available typology is that developed by Murray Straus using the "husband dominant, wife dominant, autonomic, and conflict" breakdown.[12] Closely allied with family power, but distinct from it, is the division of labor which could be placed in such categories as sex stereotyped, sex reversed, cooperative, parallel, and laissez-faire. The family composition input will be concerned with such factors as age, sex, and plurality patterns of families and with the possibility of excess or deficiency in structure.[13] Finally, the ordinal position of the subject actor is a close concomitant of family composition.

These structural variables have importance in the analysis in that they specify the organizational framework within which the occupational role is developed. It may be possible as a result of this analysis to hypothesize certain probable occupational role socialization processes given certain types of family structures. Thus, an autonomic power structure accompanied by a parallel division of labor in a family of a specified composition probably leads to certain types of socialization processes for actors in specified ordinal positions and sexes. The emphasis, once again, is placed on the analysis of the process of socialization which occurs in a given structural setting.

Family Process. Under the general heading of family process inputs, there is a considerable literature on the emotional climate of socialization.[14] Not only will attention be given

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[10] Roy H. Rodgers, *Improvements in the Construction and Analysis of Family Life Cycle Categories* (Kalamazoo, Michigan: Western Michigan University, 1962), pp. 54–55. Cf., also, Hill and Rodgers, *op. cit.*, pp. 182–184.

[11] Eugene Litwak, "Occupational Mobility and Extended Family Cohesion," *American Sociological Review*, 25 (February 1960), pp. 9–21.

[12] Murray A. Straus, "Conjugal Power Structure and Adolescent Personality," *Marriage and Family Living*, 24 (February 1962), pp. 17–25.

[13] Hill and Rodgers, *op. cit.*, p. 185.

[14] Allison Davis, "Socialization and Adolescent Personality," in *Adolescence, Forty-Third Year Book, Part I* (Chicago: National Society for the Study of Education, 1944), chap. 11; Russel R. Dynes *et al.*, "Levels of Occupational Aspiration: Some Aspects of Family Experience as a Variable," *American Sociological Review*, 21 (April 1956), pp. 212–215; William R. Morrow and Robert C. Wilson, "Family Relations of Bright High-Achieving and Under-Achieving High School Boys," *Child Development*, 32 (September 1961), pp. 501–510; Anne Roe,

to parent-child emotional relations, but also to parent-parent and sibling-sibling. Furthermore, attention will be paid to the kinds of interactional cliques and the isolation of certain actors as this affects the socialization process. Finally, the unique concern of the developmental conceptual framework with the manner in which the emotional climate may change in various life cycle categories must be analyzed. Here the research question would be whether or not there are typical emotional climate settings in families in the same family life cycle category.

The area of family orientations is a second major processual category: Included in this area are the following: traditions; values; beliefs; goals; intellectual environment; aesthetic environment; work-recreation orientation; orientations with respect to responsibility, excellence, ambition, and the like, fatalistic-manipulative attitudes; and orientations toward significant extra-family actors and organizations such as teachers, employers, the school, and the work setting. These factors define the "posture" of the family, as it were, and provide both subtle implicit influences on the occupational role development and those which are more explicit. A family, for example, which typically evaluates a potential occupation for the husband-father in terms of the recreational opportunities provided by the occupation or the geographical location of the occupation, communicates something in the socialization process to the child actors who are developing their occupational roles. The manner in which nonwork time is utilized, such as participating in activities which enhance one's occupational skills as opposed to activities which have no direct work significance, is a similar example of this type of orientation variable.

Some analysts might view the kinds of factors included under family orientations as structural inputs. The rationale for placing them in the process category lies in the fact that these factors are directly related to the dynamics of family interaction in constantly influencing the manner in which the socialization process is carried on. While the structural inputs discussed above essentially provide the normative definition of *who* carries out certain kinds of socialization activities, the family orientations inputs provide the basic definitions of *how* socialization takes place and *why*. They are, thus, not part of the structure within which the process takes place, but an integral part of the process itself.

Family Conditions. This area is, in one sense, a situational one. The entrepreneurial-bureaucratic setting of the husband-father's work used in the Detroit Area Studies[15] would be one example of the kind of factor to be considered here. Variations in work setting are of probable significance in the kind of view of work which will be presented to the child actor. The "facilitating mechanisms" listed in Figure 1 refer to the presence of books or other facilities, income, space facilities, travel, and other such factors which would be significant in terms of providing the basis for certain occupational prerequisites. It should be made clear that in the case of income, for example, the orientations of the family would have to be analyzed to determine the manner in which the available income was used. However, a given value orientation is only a necessary, not a sufficient, condition for the achievement of certain goals. By the same token, a given level of income without a given value orientation is a necessary but not a sufficient condition. It becomes clear, therefore, that the input of a given value orientation into the flow does not necessarily predict the input of a given set of facilitating mechanisms. Yet, both would be seen as essential for certain occupational outcomes.

Societal Inputs

It would be easy to lose the central focus of the research contemplated in the discussion of societal inputs. Many of the factors to be considered under this heading might well be the subject of extensive research in their own

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"Early Determinants of Vocational Choice," *Journal of Counseling Psychology*, 4 (Fall 1957), pp. 212–217; Bernard C. Rosen, "Family Structure and Achievement Motivation," *American Sociological Review*, 26 (August 1961), pp. 574–585; Bernard C. Rosen, "Family Structure and Value Transmission," *Merrill-Palmer Quarterly*, 10 (January 1964), pp. 59–76; Super, *op. cit.*, chap. 17.

[15] Robert O. Blood and Donald M. Wolfe, *Husbands and Wives* (Glencoe, Illinois: The Free Press, 1960); Daniel R. Miller and Guy E. Swanson, *The Changing American Parent* (New York: John Wiley & Sons, 1958).

right with respect to their relationship to occupational outcomes. Their importance in this context is limited to determination of the manner in which the actor's experience with them significantly affects the development of the occupational role as it is defined *within* the family. An attempt to show this has been made in Figure 1 by depicting arrows moving out from the flow line of occupational development to the societal inputs and then returning to the flow. The interest, therefore, is with the mediating or "filtering" effect which the family has in utilizing these extra-familial influences on the actor in the definition of the developing occupational role, rather than with seeing societal inputs as injected in their "pure" forms into the flow. These inputs have been classified as being of an *interactional* type or as having to do with certain *conditions*.

Societal Interaction. In considering family associates as one example of an interactional societal input, interest would be centered on the way in which the family utilizes these contacts in order to instill certain attitudes toward desirable or undesirable work roles, as an avenue to openings in certain job opportunities, or as a way of providing a broader scope of knowledge about potential occupations. By the same token, work experience might be used by the family in either a negative or positive way—"Now you have some idea of what it's like to work on an assembly line." This treatment of these inputs does not in any way deny the fact that they may also socialize the actor in terms of gaining actual skills in a given occupation. It is simply a recognition that these kinds of societal experiences are frequently used internally in the family system for other socialization purposes.

Societal Conditions. This area is again a kind of situational factor. In considering the relative socioeconomic status of the family in the community, primary interest is not centered in determining the extent to which middle-class parents influence their children to take on middle-class value orientations and move toward professional or other white-collar jobs. The interest lies in seeing how the fact of their relative status in that community is utilized in opening and closing certain valves which contribute to the accumulating set of norms which define the occupational role. Thus, no tables would be generated of social class categories with frequencies or percentages of particular occupations viewed as desirable. Rather, the process by which these middle-class families interpret their relative standing in the community to the child actor in terms of his occupational role would be analyzed. Perhaps the best way of stating this is to say that these conditions are viewed as *intervening variables*, rather than as independent variables.

SUMMARY

The model developed in this paper is an application of the developmental conceptual framework to a particular role which grows out of both familial and non-familial experiences. It is not presented as a general model to explain occupational choice. Rather, it is designed to handle the manner in which the individual actor is socialized within the family system with respect to his developing occupational role. The model does not attempt to analyze the occupational role relevance of extra-family experiences in peer group, school, or other systems, except as these influence the socialization carried on by the family.

Neither does it attempt to deal with the issue of explaining or predicting specific occupational outcomes by analyzing implications of the stochastic process of making successive choices at various decision points in life. Rather, it is concerned with exploring the manner in which the setting for the various choices is developed in the experience of the actor. While the family life cycle has been demonstrated to have explanatory power,[16] this model is designed to be little more than descriptive of the process in its present state. The development of explanatory power, it is believed, rests upon the ability to adequately describe the process. Furthermore, to explain occupational outcome involves considerably more than family socialization.

Thus, the primary focus of the model is found in the effect of the family on the development of the occupational role in the child position. In attempting to describe this effect, those factors which influence the roles played in reciprocity to the roles which compose the role cluster of the occupant of the

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[16] Hill and Rodgers, *op. cit.*, pp. 190–193.

child position are to be examined over a portion of the career of the child position in the family of orientation. Attention will remain on those interactions which contribute to the normative content of the occupational role as it develops from latency to manifestness during the career. The continual concern is with *process*, thus all of the structure and condition variables, both intra- and extra-familial, will be examined in terms of their processual implications. Such an approach, it is anticipated, should bring significant gains in knowledge of the family dynamics of socialization and of the developmental change in role sequences within the family system.

B. Sequential Characteristics of Mechanisms Involved in a Scientific Career: Continuities and Discontinuities

Introduction

The articles in this section empirically explore certain aspects of the sequential processes involved in the development process of vocational choice, the theoretical formulations of which were discussed in Section III A. They deal with continuity and discontinuity in personality and interests and with the continuities and discontinuities in the actual process of choice of a vocation.

The Parloff, Datta, Kleman, and Handlon selection, like Garwood's paper in Section II E, considers the problem of the causal relationship between personality and vocational choice. Researchers have tried to determine if creative young people interested in science have characteristics similar to those of adult scientists. If both groups are identical in personality characteristics and highly different from the general population, it could safely be assumed that personality is somehow related as a precursor to vocational choice. If adolescent and adult scientist samples differ in personality it could be assumed that professional socialization later operates to mold the personality of the young scientist to that of the professional group, or that young scientists having dissimilar traits from professional scientists are more likely to drop out of science as a career. The Parloff et al. study is especially pertinent because data are directly tied to other studies of adult scientists (e.g., MacKinnon, 1962). Even the same instrument, Gough's California Personality Inventory (CPI), was used in all of these studies. Parloff and his colleagues compared creative and average groups of research scientists, architects, writers, and mathematicians with creative and average adolescents from the Westinghouse Talent Search and found that while, in general, the adult and adolescent creative groups were similar on four CPI factors, creative adolescents scored higher than noncreative adolescents on disciplined effectiveness, while the less creative adults were higher on this factor. These findings suggest that professional socialization or the dropping-out of adolescents with inappropriate personality traits must operate to some extent to create differences between adolescent and adult scientists.

The Tyler selection shows a continuity of interest patterns among boys interested in scientific careers. This 12-year study showed that scientific interests had crystallized by the eighth grade and remained constant for about two thirds of the high school boys until graduation. This early choice of career in the physical and biological sciences, as compared to later career decision in the social sciences, is well documented (Pressey, in Section IV C).

Hollender found that the realism of level of vocational choice (as defined by using individual aptitude scores as a criterion) increases with increasing age and grade from grades 6 through 12, thus providing inferential support for Ginzberg's hypothesis that vocational choices change from a basis in fantasy to a basis in reality considerations. The results also support Beilin's hypothesis (1955) that vocational choices become more realistic with advancing age.

Many theorists have assumed that there is a fit between personality and occupational role. Because the individual has certain personality characteristics, he is more likely to engage in certain occupations than he is to engage in others. The Holland selection is one of the several studies he performed to examine the usefulness of a theory of vocational choice perhaps best characterized as a heuristic theory of personality types and environmental situations. The theory postulates six model types (realistic, intellectual, social, conventional, enterprising, and artistic) and relates each type to certain hypothesized vocational preferences. Vocational preferences were found generally to be associated with self-descriptions, coping behavior, daydreams about vocations, developmental histories of successive vocational choices, and student perceptions of vocations in accordance with the theory.

Although Holland attempts to explain his results in terms of his theory of personality types and vocational choice, the results are also consistent with the theoretical conceptions of Super (1957) and Tiedeman and O'Hare (1963).

Several studies (Holland, Forrest, Werts, and Roe) would seem to contradict Ginzberg, et al.'s (1951) assertion about the irreversibility of choice. Each found that science interests crystallize before high school graduation. While considerable movement out of scientific career choices occurs, movement into scientific careers is minimal. Although 50 percent of Forrest's sample of Merit scholars changed their vocational choice between their senior year in high school and junior year in college, 43 percent of all changes were from science to nonscience fields, while only 7 percent were from nonscience to science fields.

While the reason for such movement out of science as an occupation is not immediately clear, the types of students who leave this particular occupational field can be specified. Students who, in terms of academic ability (Werts, Hollender), social background (Werts), and personality (Holland), were unlike the majority of students with the same initial career choice were more likely to change their career plans to a field in which they were more similar to the other students in these characteristics.

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Personality Characteristics which Differentiate Creative Male Adolescents and Adults

Morris B. Parloff, Lois-ellin Datta, Marianne Kleman, and Joseph H. Handlon

. . . The present investigation is addressed to two issues: (1) to test further the claim that creative adults, independent of field, share a common set of differentiating personality characteristics,[1] and (2) to test whether such personality characteristics may be interpreted as conducive to or consequent to creative performance.

The first question was tested by a reanalysis of personality data obtained by investigators at the Institute for Personality Assessment and Research (IPAR) at the University of California, Berkeley. In their studies of creative adults—architects, mathematicians, research scientists, and professional writers—the IPAR researchers had given careful attention to the selection of the criterion and control groups, and had administered appropriate and consistent batteries of personality measures.

Although researchers at IPAR had individually studied groups of more and less creative adults, they had not done an overall analysis of levels of creativity, vocational affiliation, and the interaction of these two variables.

The second question could not be tested directly. No data were available concerning the personalities of these adults prior to their public recognition. An indirect test was undertaken by administering the same personality instrument employed in the study of the adult sample to a highly selected adolescent sample. The adolescents included two groups of male high school seniors who differed on judged creativity but were comparable on such variables as intelligence, science aptitude, level of school achievement (grades), and socioeconomic status. Both high school groups had received a comparably high degree of recognition for their academic achievements.

Support for the hypothesis that a unique set of personality characteristics may be predisposing to creative performance would be clearest if both the more creative adolescent and adult groups were differentiated from their respective less creative groups in the same direction on the same set of personality dimensions. Support would also be inferred if the more creative groups in both age samples were mutually differentiated from their respective controls on an overlapping subset of personality dimensions. Such findings would suggest that personality characteristics may be conducive to creativity but that the facilitating personality features may vary from age period to age period. Similarly, if the adolescent groups were found to be differentiated from each other but the more creative and less creative groups of adults were not differentiated on any personality characteristics, support for the hypothesis would be equivocal but not precluded. Creative adults may have shown predisposing personality characteristics as adolescents comparable to those found among creative adolescents, but such personality differentiations were modified with age.

Lack of support for the hypothesis would be clearest if neither the adult nor the adoles-

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[1] EDITORS' NOTE: Despite the apparent consensus among previous investigators (Stein, 1962; Barron, 1965) that the more creative individual is more assertive, forceful, highly motivated, radical, and Bohemian and less inhibited and conventional than his less creative peer, these findings must be interpreted cautiously. The original studies are frequently not comparable due to marked differences in the definitions of criterion groups, in the selection of control groups, and in the choice of personality measures. Interpretation is further complicated in that the creative adult was studied only after he achieved professional eminence. It is not clear, therefore, whether some or all of the personality characteristics reported as differentiating the more from the less creative adult are in fact predisposing to, or are the consequence of, earlier creative performance. Also, studies that cut across two or more vocational fields (Roe, 1953; Cattell and Drevdahl, 1955; and Chambers, 1964) have thus far failed to present compelling evidence to support the generalization that "creative men" whatever their field, share a unique set of personality characteristics.

cent more and less creative groups were differentiated on any personality characteristics. Lack of support would also be inferred if only the adult and not the adolescent more creative groups were differentiated from their controls on personality dimensions.

METHOD

Adolescent Sample

The adolescent sample selected was drawn from more than 5,000 successful male entrants to the 22nd and 24th Annual Science Talent Search (STS). The stated aim of the STS competition is "to select from the nation's high school seniors those giving promise of being the creative scientists of tomorrow [Davis, 1963, p. 3]." From these applicants only those who scored above the 80th percentile on a nationally administered science aptitude examination were invited to participate in our study. Of the 1,030 eligible, a total of 938 subjects (91 per cent) accepted our invitation and completed the personality measures. By a criterion described below, 266 were classified as "more creative" and the remaining 672 as "less creative." No reliable differences were found in the rate of participation of these two groups.

. . . Eight vocational groupings were identified in the adolescent sample. The less creative sample was found to be underrepresented in the area of mathematics and somewhat overrepresented in chemistry and engineering. In view of this, data analyses were performed to correct for effects of vocational imbalance in both the more and less creative groups.

In comparing the more and less creative groups no reliable differences were found between group means on the following variables: age (17.7), Science Aptitude Test (50.03), Scholastic Aptitude Test Verbal (681.5), high school grade-point average (3.7), socioeconomic status (2.19),[2] and birth order (62.5 per cent were first born or only children). Group differences were found on geographical and religious background variables. Forty-four per cent of the more creative group came from the Northeastern states, against only 38 per cent of the less creative group; 51 per cent of the more creative group lived in metropolitan areas having populations of more than 500,000 in contrast to 41 per cent of the less creative group; and 46 per cent of the more creative group reported Jewish parentage, as opposed to 32 per cent of the less creative group.

It was assumed that the geographical background differences are unlikely to account for systematic personality differences between the two groups. Since religious differences may be associated with personality variables, we analyzed relationships between personality and creativity for students of Jewish and non-Jewish backgrounds. The observed relationships between creativity and personality were consistent for both religious background groups. Findings are therefore reported here without reference to religion.[3] . . .

Judging Procedure: Adolescent Sample

Each entrant had submitted to a panel of judges a report of a research project which he had selected and independently conducted. Each project was independently rated by pairs of judges trained by Science Service, Inc. Judges represented different areas of specialization, such as mathematics, physics, chemistry, biology, etc. Projects were rated on a creativity scale ranging from A (high) to E (low). If the ratings of the pair of judges differed by more than one letter grade, the judges met to reconcile the discrepancies. On the basis of these ratings we classed subjects receiving grades of A and B in the "more creative" sample, and all those receiving lower grades were placed in the "less creative" groups. . . .

Although many of the projects were judged to be of high creative caliber, the majority indicated potential rather than actual creativity. The ratings also reflected a subtle weighting of the facilities, equipment, and

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[2] The Hollingshead-Redlich Two Factor Index, which is based on paternal education and occupation, was used. Level 1 indicates the highest SES level.

[3] EDITORS' NOTE: A detailed analysis of the association between religious background and creativity of 536 subjects of this sample has been discussed by Datta (1967).

caliber of the teachers available to high school seniors. It is thus appropriate to identify the sample as Potentially Creative Scientists (PCS). The more creative adolescent group used in this investigation overlaps with but is more stringently selected than the group identified as "winners" in the Science Talent Search.

Adult Sample

The total adult sample consists of 200 subjects, of whom 101 were classified as "more creative" and 99 as "less creative." The sample included mathematicians, research scientists, writers, and architects. A detailed description of this sample is presented in Table 1. . . .

The average age of the 200 adult subjects is 45.1 years, ranging from 35.5 for the research scientists to 52.0 for the writers. The mean age of the 101 adults in the more creative sample, 44.7 years, did not differ significantly from that of the 99 less creative adults, 45.5.

Personality Measurement

Of the broad array of tests which had been administered to these samples only the California Psychological Inventory (CPI) (Gough, 1957) had been used in both the adult and adolescent studies. The CPI contains 18 scales measuring significant traits of personality that are involved in social interaction and in constructive achievement. This personality test has been used widely with school, college, and industrial populations, and has been used successfully in differentiating personality characteristics of more and less creative adults (MacKinnon, 1961).

Procedure and Design

Since the 18 scales of the CPI tend to show appreciable intercorrelation it was necessary to determine the independent dimensions represented. Although Gough has grouped these scales into four classes: (1) Poise, Ascendancy, and Self-assurance; (2) Socialization, Maturity, and Responsibility; (3) Achievement Potential and Intellectual Efficiency; and (4) Intellectual and Interest Modes, his groupings are not readily replicated (Crites, Bechtoldt, Goodstein, & Heilbrun, 1961; Leton & Walter, 1961; Mitchell & Pierce-Jones, 1960; Shure & Rogers, 1963). We therefore performed separate factor analyses for each of the present samples.

Separate correlation matrices were computed for the 18 CPI scale scores for the adolescent sample and for the adult sample. The two correlation matrices were subjected to a principal-components analysis (Hotelling, 1933). All components with Eigenvalues greater than 1.00 were orthogonally rotated by the Varimax method (Kaiser, 1958). Each subject's score on the identified factors was determined.

Two-way analyses of variance, unweighted means solution (Winer, 1962) were performed separately for the adult and adolescent samples. Two by eight analyses of variance were performed for the adolescents, representing two levels of creativity and eight vocational preference groups. The adult personality data were analyzed in two by four analyses of variance representing two levels of creativity and four vocational groupings. . . .

RESULTS

Analyses of the CPI intercorrelation ma-

Table 1. Vocational Distribution and Mean Age of More and Less Creative Adult Groups

	More creative			*Less creative*			*Totals*		
Vocation	*N*	*Pct.*	*Mean age*	*N*	*Pct.*	*Mean age*	*N*	*Pct.*	*Mean age*
Mathematicians	27	26.7	38.7	29	29.3	37.9	56	28.0	38.3
Architects	40	39.6	50.9	41	41.4	50.2	81	40.5	50.5
Research scientists	15	14.9	34.1	15	15.2	36.8	30	15.0	35.5
Writers	19	18.8	48.5	14	14.1	56.7	33	16.5	52.0
Totals	101	100.0	44.7	99	100.0	45.5	200	100.0	45.1

trices for adolescent and adult samples yielded, for each sample, four factors with Eigenvalues greater than 1.00. Rotated solutions were computed by the Varimax procedure. . . .

Coefficients of Congruence (Harman, 1960) were computed to determine whether similar factors had been found for the two matrices. The results indicate a high degree of similarity between the structure of the CPI for both the adolescent and adult groups since a good match is found for all factors. The Coefficients of Congruence between matching factors ranged from .96 to .86 with a mean of .91, while the coefficients for unmatched factors ranged from −.33 to .46 with a mean of .17. Since the factor analyses and rotations were completely independent, and since the samples were large and diverse, the results give strong evidence for a consistent organization of the CPI for both samples.

The four factors identified in the adolescent and adult samples in each instance accounted for a total of 69 per cent of the variance. . . . Analysis of the factor loadings on the CPI scales suggest the following descriptions and interpretations of the factors.

Factor I accounts for 28 per cent and 23 per cent of the total variance for the adolescent and adult samples respectively. The highest correlates of this component in each sample appear to be on the following scales: Self-control, Good Impression, Well-being, Achievement via Conformance, Tolerance, and Socialization. The PCS sample also shows sizable loadings on scales measuring Responsibility and Intellectual Efficiency. The generality of this factor appears to be somewhat greater for the PCS sample than for the adult sample. Persons with high scores on these scales tend to be described as disciplined, painstaking, reliable, to be attentive to their impact on others; to have a comfortable sense of self-effectiveness and physical well-being; to be persistent, industrious, and capable of effective endeavor in structured settings; to be accepting of differing social beliefs and opinions; and to be efficient, persistent, productive, and reflective in the pursuit of intellectual goals.

This factor resembles the "adjustment by social conformity" factor identified by Mitchell and Pierce-Jones (1960) and the "Personal Integrity and Mental Health" factor obtained by Shure and Rogers (1963). The patterning of loadings in the present sample suggests that Factor I may best be described as *Disciplined Effectiveness.*

Factor II accounts for 21 per cent of the total variance in both the adult and PCS samples. The scales which show the highest loadings on this component for both samples are Sociability, Self-acceptance, Dominance, Social Presence, and Capacity for Status. These are five of the six scales that Gough included as "Measures of Poise, Ascendancy, and Self-Assurance." These scales describe an assertive, outgoing, self-confident, expressive, competitive style of relating to others. Such individuals are usually perceived as vigorous, ambitious, aggressive, and persuasive. This factor may be titled, *Assertive Self-assurance.*

Factor III accounts for 12 per cent of the total variance in the PCS sample and 17 per cent of the total variance in the adult sample correlation matrix. The highest correlates of this component for both samples are Flexibility, Achievement via Independence, and Psychological Mindedness. High scorers on these scales tend to be characterized by individuality, spontaneity, self-reliance, intraception, lability, interpersonal perspicacity, and high motivation to work independently. Factor III may be entitled *Adaptive Autonomy.*

Factor IV accounts for 8 per cent of the total variance in both the PCS and adult correlation matrices. For the PCS sample the only high loading occurs on the Femininity scale (.84) and the next highest loading is on Responsibility (.50); however, the latter scale has an even higher loading on Factor I (.64). The adult sample shows its highest factor loadings on Communality (.62), Responsibility (.60), and Femininity (.49). The Femininity scale loads almost as strongly on Factor II (−.42). It is to be noted that this factor tends also to be associated with the socialization scale for both samples (.39).

For the adolescents, Factor IV is perhaps best characterized by breadth of interest, sensitivity, and conscientiousness. For the adults, high scores on this factor appear to be associated with a tendency to hold and express conventional ideas, to be dependable, resourceful, sensitive, and efficient. Although this factor appears to be readily described by the general term Femininity for the PCS sample, for the adults this factor is more differentiated and suggests concern with others. Both the PCS and adult loadings have been tentatively subsumed under the term *Humanitarian Conscience.* A somewhat similar factor was described by Mitchell and Pierce-Jones (1960) with the term "Super-Ego Strength."

A summary of the mean factor scores of the more and less creative groups according to vocational preference classification is presented for adults and adolescents in Tables 2 and 3. Analyses of variance based on the CPI factor scores obtained with adult and adolescent groups are summarized in Table 4.

On Factor I (Disciplined Effectiveness) the main effect attributable to creativity was significant for both adults and adolescents ($p < .01$ and $p < .05$, respectively). An inspection of the means (see Tables 2 and 3) indicates that the more creative adolescents are higher on this factor than the less creative adolescents, whereas precisely the reverse discrimination is found between the more and less creative adult groups. While the adolescent groups show no main effect for vocation, the adult groups show significant effects for both vocation ($p < .01$) and the interaction ($p < .05$). The interaction is due primarily to the fact that the mean factor score differences between the creative and control groups were larger for the architects, writers, and mathematicians than for the research scientists. Of major interest is the finding that the more creative adults are lower on Disciplined Effectiveness than their colleagues, while the more creative adolescents are higher on this factor than their less creative peers.

On Factor II (Assertive Self-assurance) the findings for the adolescent and adult samples appear to be quite comparable: the main effect for creativity was significant at the .10 level for adults and the .05 level for adolescents. The main effect of vocation was significant for both samples ($p < .01$) but no significant interaction was found. In both samples the more creative groups tended to describe themselves as more self-assured than their less creative peers, independent of the fact that the absolute levels of self-assurance differ among vocations.

On Factor III (Adaptive Autonomy) the main effects of creativity and vocation were significant for both the adults and the adolescents ($p < .01$) and no interaction was found.

Table 2. Factor Score Means and Standard Deviations[a] for More and Less Creative Adult Groups Within Vocations

	Factor I (Disciplined effectiveness)				*Factor II (Assertive self-assurance)*			
	More creative		*Less creative*		*More creative*		*Less creative*	
Vocations	*Mean*	*SD*	*Mean*	*SD*	*Mean*	*SD*	*Mean*	*SD*
Architect	—3.12	6.00	4.30	4.76	.91	6.41	—.35	6.46
Writer	—4.99	7.64	—2.82	9.67	1.16	7.55	—1.09	8.90
Research scientist	2.35	4.24	1.99	6.10	4.52	6.05	3.03	5.90
Mathematician	—.99	6.72	1.52	6.45	—.95	5.39	—4.02	8.40
Totals	—1.69		1.25		1.41		—.61	

	Factor III (Adaptive autonomy)				*Factor IV (Humanitarian conscience)*			
	More creative		*Less creative*		*More creative*		*Less creative*	
Vocations	*Mean*	*SD*	*Mean*	*SD*	*Mean*	*SD*	*Mean*	*SD*
Architect	—1.27	5.13	—5.31	7.29	—1.23	6.16	—.09	6.98
Writer	1.43	5.40	—3.10	6.28	—.13	6.11	—2.21	7.94
Research scientist	4.00	5.88	2.43	6.21	—.25	4.46	—.78	5.92
Mathematician	4.55	5.81	2.24	6.65	2.33	9.05	1.34	7.24
Totals	2.18		—.94		.18		—.44	

[a] Factor scores multiplied by 100.

The more creative groups showed significantly more autonomy than the less creative, independent of the fact that the levels of autonomy vary significantly among the different vocational groups represented.

Factor IV (Humanitarian Conscience) failed to differentiate the more and less creative groups within the adult or the adolescent sample. The adults showed no main effects attributable to vocation and no interaction. The adolescent sample revealed a vocational effect but no significant interaction.

In brief, the more creative adults and adolescents are similarly differentiated from their less creative colleagues by indicating greater tendencies toward Assertive Self-assurance and Adaptive Autonomy than do their controls; however, the adult and adolescent creative groups are distinguished from their respective less creative groups on the dimension of Disciplined Effectiveness in a diametrically contrasting fashion. Lower scores on this factor are associated with more creative performance among the adult groups, while higher scores are characteristic of the more creative adolescent group. That personality characteristics are differentially associated with vocational choice is evident, but specification of these differences is not germane to the purposes of this paper. . . .

DISCUSSION

Although a nonchance association between personality and creativity has been demonstrated, the strength of association reflected in predictive efficiency is relatively modest. Given the fact that (1) CPI scale reliabilities, although high, are not perfect, (2) that the validity of judging of creativity must be assumed to be limited, and (3) that the fashioning of a creative product depends not only on

Table 3. Factor Score Means and Standard Deviations[a] for More and Less Creative Adolescent Groups Within Vocations

	Factor I (Disciplined effectiveness)				*Factor II (Assertive self-assurance)*			
	More creative		*Less creative*		*More creative*		*Less creative*	
Vocations	*Mean*	*SD*	*Mean*	*SD*	*Mean*	*SD*	*Mean*	*SD*
Mathematics	—.04	3.42	.36	2.98	—.21	3.32	—1.13	3.11
Physics	.61	3.07	.46	3.04	.21	3.00	0.43	3.12
Biology	—.31	3.25	—.61	2.88	1.50	3.67	1.47	3.23
Chemistry	.95	2.49	.12	3.25	.20	2.75	—.77	3.12
English	.59	3.31	—.41	3.57	.32	3.05	.17	3.17
Biochemistry	.34	3.02	—1.26	3.36	1.38	3.43	.62	2.84
Other science	.29	3.37	—.53	3.80	—1.37	3.17	—.38	3.72
Nonscience	—.06	3.14	—.91	3.43	2.61	3.29	.48	2.93
Totals	.30		—.35		.58		.00	

	Factor III (Adaptive Autonomy)				*Factor IV (Humanitarian conscience)*			
	More creative		*Less creative*		*More creative*		*Less creative*	
Vocations	*Mean*	*SD*	*Mean*	*SD*	*Mean*	*SD*	*Mean*	*SD*
Mathematics	.92	3.25	.11	3.29	.31	3.60	.25	3.00
Physics	.27	2.70	.40	3.29	—.39	3.37	—.62	3.43
Biology	.25	3.21	—.59	2.92	—.34	2.98	.86	2.67
Chemistry	1.20	3.82	—.89	3.10	.42	2.54	.65	3.17
English	—1.18	3.20	—1.38	3.03	—.99	2.85	—.71	3.05
Biochemistry	—.32	2.97	—.15	3.63	1.12	3.25	.26	3.69
Other science	.52	3.42	—.15	2.83	—.22	3.13	—.10	3.40
Nonscience	2.44	1.78	.56	3.82	1.13	3.28	.06	2.79
Totals	.51		—.26		.13		.08	

[a] Multiplied by 100.

Table 4. Analysis of Variance of CPI Factor Scores Obtained with Adult and Adolescent Groups

	Adults				*Adolescents*			
Factor	*df*	*Mean square*	*F*	*P*	*df*	*Mean square*	*F*	*P*
I. Disciplined effectiveness								
Creativity	1	.036	8.69	<.01	1	.007	6.14	<.05
Vocation	3	.022	5.30	<.01	7	.002	1.61	n.s.
Interaction	3	.015	3.48	<.05	7	.001	.87	n.s.
Error (within cells)	192				922			
II. Assertive self-assurance								
Creativity	1	.017	3.68	<.10	1	.005	5.11	<.05
Vocation	3	.026	5.55	<.01	7	.007	7.19	<.001
Interaction	3	.001	.21	n.s.	7	.001	1.34	n.s.
Error (within cells)	192				922			
III. Adaptive autonomy								
Creativity	1	.041	10.39	<.01	1	.009	9.04	<.01
Vocation	3	.062	15.68	<.001	7	.004	3.60	<.01
Interaction	3	.002	.50	n.s.	7	.001	1.39	n.s.
Error (within cells)	192				922			
IV. Humanitarian conscience								
Creativity	1	.002	.32	n.s.	1	.000	.03	n.s.
Vocation	3	.009	1.82	n.s.	7	.003	2.46	<.05
Interaction	3	.002	.49	n.s.	7	.001	.87	n.s.
Error (within cells)	192				922			

personality predispositions but also on a variety of cognitive skills and conducive environmental circumstances, it would be overly optimistic indeed to expect that strong predictions of creativity could be made simply on the basis of a single specified set of personality characteristics. On this basis it may be gratifying to find that attention to personality characteristics alone enhances the identification of members of the more and less creative groups (adult or adolescent) to the degree demonstrated. On the other hand, the fact remains that the attempt to differentiate levels of creativity by use of a single set of personality characteristics would even under optimal conditions permit the misidentification of approximately one-third of the more and less creative individuals. It is likely that prediction and identification could be enhanced by further efforts to identify not simply the best central tendency representation but the variety and richness of various personality types associated with creative performance. Efforts to characterize the personality of "the creative man" must inevitably fail to describe a relatively sizable proportion of such individuals. It is appropriate here to consider the findings that were applicable to the majority of subjects. We note that despite the fact that personality characteristics are differentially associated with vocational choice, the more creative adults are distinguished from their less creative peers on a shared set of personality dimensions. These findings give support to the hypothesis that creative adults share a common set of differentiating personality characteristics. The hypothesis that these personality characteristics are predisposing to rather than the consequence of creative performance is also supported; however, the evidence suggests that facilitating personality characteristics may vary from one age period to another.

With the limitations that this investigation is based on but a single personality instrument (CPI) and seeks to identify and apply only group mean personality differences, the findings for the adult sample are remarkably consistent with the general picture of creative personality: relatively uninhibited and unconventional; appearing to have a greater need for independence, self-direction, and autonomy; and being more assertive, forceful, and seemingly self-assured.

With equal weight given to each cell mean, the absence of any true interaction between creativity and the various vocations represented gives the findings the weight of multiple replication. It is clear, however, that the mean differences between the more and less creative research scientist groups on the dimension of Disciplined Effectiveness are not consistent with the overall discrimination found between more and less creative adult groups (see Table 2). This inconsistency may be due to the fact that the more creative research scientists sample is the only group of adults that was not selected on the basis of demonstrated high levels of creativity. According to Gough, "the search was not for men of renown or outstanding creativity. To use an analogy from athletics, the sample was one of varsity players rather than of All-Americans [1961, p. III-6]." The degree to which creativity was represented in this sample may be relatively slight. There may also be a systematic selection factor in this group, based on the decision to enter and remain in an industrial research setting.

The adolescent more and less creative groups are also found to be reliably differentiated on the same three personality factors that differentiated the more and less creative adult groups. Again, despite the fact that personality differences were found among the various vocational interest groups, no significant interactions were found between creativity and vocation. In effect, the personality differences associated with the two levels of creativity are replicated in the various vocational groups. The stability of these findings gives added weight to the observation that the direction of the discrimination between more and less creative groups on Disciplined Effectiveness is reversed for the adult and adolescent samples. These findings suggest that the personality dimensions of Adaptive Autonomy and Assertive Self-assurance, which similarly discriminate the more and less creative groups of adults and adolescents, may indeed be predisposing to creative functioning; however, such characteristics as are described by low scores on the personality factor, Disciplined Effectiveness—unconventional behavior, lack of concern with maintaining the good opinion of others, and relative lack of inhibition—do not appear to be predisposing to adult creativity but may instead be a consequence of recognition or maturity. The data do not preclude the possibility that the creative adults achieve their most creative works and conceptions at a relatively young age, when their personality style was in fact more similar to that of the adolescent in this study than that of the adult. The reduction of concern with Disciplined Effectiveness may be associated not with an increase in creativity but with a decrease. That is a possible but not probable inference.

Creative performance among the adolescents appears to be facilitated rather than inhibited by a measure of social skills and self-control. This finding is consistent with the view that the adolescent, unlike the adult, is faced with the practical necessity of coping with the world in which his success is, in fact, dependent upon his ability to maintain a reasonable degree of cooperation with those who control the resources he needs to achieve his goals. Society is understandably more tolerant of the demands, eccentricities, and flouting of convention of adults who have demonstrated their creativity than it is of similar behavior from the brash, unrecognized adolescent. The adolescent who is unwilling to make appropriate allowances for this reality may perforce have to devote much of his effort to struggle and rebellion, with a resultant loss of energy which might otherwise have been available for creative productivity. An additional consideration is the possibility that the performance of the potentially creative adolescent may be facilitated rather than hindered by a self-discipline that permits him to learn principles, heuristics, and basic information, which he may then proceed to reintegrate and reorganize in a constructively creative fashion. It is important to recognize that although the more creative adolescent is sensitive to the feelings of others and aware of environmental constraints, he does not let such considerations limit his pursuit of an important goal; instead he may use such knowledge to work more effectively in achieving his ends.

The projected longitudinal study of the adolescent sample will provide an opportunity to test whether the more creative adolescents will in time relax their concerns with social acceptability while their seemingly less conforming and less creative colleagues will tend to become more staid, circumspect, and conforming. To develop and then to "outgrow" a capacity to adapt to the requirements of society is a far different maturation process from that in which such discipline was never developed or was excessively delayed.

Although it is recognized that the current cultural pressures on adolescents may differ from those which operated on contemporary adults during their adolescence, it is difficult to attribute the discrepancy observed on Disciplined Effectiveness to such cultural changes. The current emphasis on nonconformity, self-actualization, and self-expression is hardly consistent with the direction of discrimination in the adolescent sample.

A further implication of these findings is that in contrast to popular belief, the creative adolescent is able to combine (1) the exercise of self-discipline, (2) a reasonable circumspection in his dealings with others, and (3) a capacity to exercise autonomy and independence of thought and to make novel and effective integrations of ideas. Since it may be assumed that these personality dimensions have been affected by experience, it is apparent that independence of thought is not stifled by life experiences which led the individual to behave in a rather socially adept manner. It must be recognized that the degree of conformance is not high in terms of the general population norms for the CPI, but high only in relation to general adolescent norms (Gough, 1957).

Generalizations based on the adolescent groups must be restricted to those who have decided upon a career in science, have made this decision at a relatively early age, have demonstrated great competence and are willing to participate in a nationwide competition. That these findings may possibly apply to nonscience groups is suggested by the fact that groups of more and less creative adolescents who do not plan to make science their careers showed personality differentiations consistent with the science sample. . . .

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Development of Scientist *Patterns of Interest in Boys*[1]

Leona E. Tyler

PROCEDURE

On the basis of their final performance on the Strong Vocational Interest Blank, two contrasting groups of boys were selected. To be included in the *Scientist* group, a subject was required to show a *primary* pattern in Strong's Group I, Group II, or in both. Seventeen boys qualified according to this criterion. To be included in the *Nonscientist* group, a subject was required to show a *reject* pattern in Strong's Group I, Group II, or in both. There were 22 boys in this group. Strong's Group I consists of the scales for biological or human scientists: Artist, Psychologist, Architect, Physician, Osteopath, Dentist, and Veterinarian. Strong's Group II consists of the scales for technical scientists: Mathematician, Physicist, Engineer, and Chemist.

The concepts of *primary pattern* and *reject pattern* are elaborated in detail in the monograph by Darley and Hagenah (2). As applied in this study, *primary pattern* means a preponderance of A or B+ scores in a group. *Reject pattern* means a preponderance of C scores or scores below the chance level indicated on the profile sheet.

The procedure was to compare the two groups on each of the kinds of measured characteristics at each of the developmental levels. Two broad general questions governed the tracing-back procedure: (*a*) At what developmental stage did evidence for the differentiation between the two groups first appear? (*b*) To what aspects of the subjects' personality, experience, and background is the differentiation related?

Besides testing group differences for statistical significance, we also inspected individual records to try to ascertain how common or universal were the developmental sequences reflected in the mean differences.

Because of the unavoidable gaps in the data, the *N*s for the comparisons between the groups are often less than 17 and 22. It seemed advisable to utilize all the cases available for a particular comparison rather than to restrict the samples to those cases for whom data were complete.

COMPARISONS OF SCIENTIST AND NONSCIENTIST GROUPS ON INTEREST TESTS

A comparison of Strong scores obtained during the two previous years in high school shows that the *Scientist vs. Nonscientist* differentiation was clearly established by the tenth grade. Table 1 gives the results. In studying them, one must remember that the groups

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[1] EDITOR'S NOTE: These data are part of a small-scale longitudinal study set up in 1946 for the purpose of tracing the early development of differentiated interest patterns that one can identify in adults by the Strong Vocational Interest Blank. The plan was to assess at several selected developmental levels the interests, special abilities, and personal-social characteristics of each subject. Such assessments were made in Grade 1, Grade 4, Grade 8, and Grades 10 through 12.

Two kinds of analyses were planned. At each level relationships between interests and other psychological characteristics were to be determined. Also in the 10th, 11th, and 12th grades, Strong test scores were related to interest scores that the subjects had obtained at earlier periods of their lives. Only the segment of the original monograph dealing with interests of male subjects is included in the present selection.

were chosen on the basis of twelfth-grade scores. The fact that differences show up so clearly and consistently in the eleventh-grade and tenth-grade results suggests that the scientist-interest pattern must have been crystallized by the age of 15. Boys in the *Scientist* group score significantly higher on almost all the occupational scales in Groups I and II and

Table 1. Mean Scores of 11th- and 10th-Grade Strong Vocational Interest Blank for Boys Classified in 12th Grade as Having Scientist and Nonscientist Interests

	Eleventh grade		*Tenth grade*	
	Mean		*Mean*	
Scale	*Nonscientist* *N = 13*	*Scientist* *N = 16*	*Nonscientist* *N = 11*	*Scientist* *N = 11*
Group I (Human science)				
Artist	22.15	35.75*	27.45	33.73
Psychologist	19.92	31.88‡	18.36	27.91†
Architect	19.31	39.13*	25.81	38.36‡
Physician	21.54	41.31*	28.45	41.73*
Osteopath	24.31	33.00†	28.45	34.73†
Dentist	20.92	38.94*	26.09	38.27†
Veterinarian	25.85	27.19	28.27	30.18
Group II (Technical science)				
Mathematician	13.92	29.25*	15.82	29.36†
Physicist	11.15	34.13*	14.91	35.09*
Engineer	23.23	42.44*	23.27	40.36*
Chemist	19.85	44.75*	22.18	43.45*
Group III				
Production manager	33.77	38.75	26.64	36.18†
Group IV (Nonprofessional)				
Farmer	39.77	44.88	39.64	47.91‡
Aviator	35.23	45.44‡	33.64	51.45*
Carpenter	27.08	36.00	25.55	40.55*
Printer	42.38	45.19	34.36	44.64†
Math.-Sci. teacher	34.69	40.13	27.27	39.91*
Indust. arts teacher	20.31	29.19	14.55	32.00*
Voc. agric. teacher	31.00	30.19	27.00	33.27
Policeman	35.92	35.06	32.55	37.73
Forest service man	21.69	28.63	20.27	33.73*
Group V (Social service)				
YMCA physical director	28.54	20.81‡	26.73	21.45
Personnel director	30.23	20.13†	20.55	18.82
Public administrator	29.77	26.63	25.00	25.09
YMCA secretary	27.46	12.06*	21.91	10.91‡
Social science teacher	35.77	23.38*	28.55	21.18
City school superintendent	20.38	17.00	19.36	15.45
Social worker	27.69	21.19	24.82	20.91
Minister	18.54	15.94	15.55	9.73
Group VI				
Musician	37.15	43.50†	36.91	40.64
Group VII				
CPA	21.31	20.38	22.45	19.00
Group VIII (Business detail)				
Senior CPA	36.69	34.69	31.27	37.64
Accountant	29.69	25.13	25.18	24.45
Office man	40.15	26.50*	31.45	27.91
Purchasing agent	32.92	24.88†	33.63	31.73
Banker	37.69	22.50*	34.18	25.55†
Mortician	37.62	25.63*	38.09	27.45†
Pharmacist	35.00	34.44	37.09	36.18

Table 1 (Continued)

	Eleventh grade		*Tenth grade*	
	Mean		*Mean*	
Scale	*Nonscientist* *N = 13*	*Scientist* *N = 16*	*Nonscientist* *N = 11*	*Scientist* *N = 11*
Group IX (Business contact)				
Sales manager	34.31	24.19*	34.18	24.18*
Real estate salesman	42.38	34.56†	43.73	34.36*
Life insurance salesman	31.92	23.25†	34.27	21.73*
Group X (verbal)				
Advertising man	33.62	33.13	34.73	28.27‡
Lawyer	28.92	24.63	32.36	24.00*
Author journalist	30.23	36.63	32.45	32.00
Group XI				
President, mfg. concern	27.31	30.25	30.18	29.36

* *t* test shows difference significant at the .001 level.
† *t* test shows difference significant at the .01 level.
‡ *t* test shows difference significant at the .05 level.

score significantly lower on the sales occupations that Strong includes in Group IX.

A careful scrutiny of the data in Table 1 —with careful note being made of the differences statistically significant at the tenth- and the eleventh-grade levels—suggests that some kinds of distinction between the groups had become sharper and others less sharp as time passed. At the younger age, boys in the *Scientist* group are characterized by a predominance of (Strong's) Group IV interests. Group IV consists of an heterogeneous set of occupations mainly at the technical or skilled level rather than at the professional level. According to Darley and Hagenah (2), it is the one most characteristic of high-school boys in general. It may be that *Scientist* types of boys tend to develop such nonprofessional interests early, but tend to lose them as time passes; while boys in the *Nonscientist* group tend to maintain their nonscientific interests or even tend to increase them. (Conclusions like this are doubtful because of the difference in *Ns* at the two levels. What looks like a developmental trend conceivably could be a sampling effect.) Conversely, on the scales in Group V on the Strong Vocational Interest Blank (the personal- or social-service occupations), our two groups did not differ much when they were tenth graders, but by the eleventh-grade level the *Nonscientist* boys showed more interests in personal- or social-service occupations than the *Scientist* boys did. A trend in the other direction is suggested by the means for Strong's Group X, the verbal or linguistic occupations. On the scales in Group X, the *Scientist* boys scored significantly low as tenth graders, but did not score significantly lower than the *Nonscientist* boys when they reached the eleventh grade.

Thinking about these clues, tenuous as they are, suggests a hypothesis that two kinds of polarities may be involved in what is labelled *interest in science*. One is a "things *vs.* people" distinction (Groups I and II *vs.* Group V); the other, a "things *vs.* words" distinction (Groups I and II *vs.* Group X). It may be that those who become scientists learn to make both distinctions. On the other hand, it may be that some scientists think in terms of the "things *vs.* people" distinction and that others think in terms of the "things *vs.* words" distinction.

Let us now consider the interest data obtained from the subjects when they were in eighth grade to see whether the *Scientist vs.* the *Nonscientist* distinction could have been made at that time. Table 2 shows the mean scores on the nine scales of the Kuder Preference Record. The reason that the *Ns* are so drastically reduced is that, in a considerable proportion of the cases, the validity scores are outside the critical points that Kuder has specified as acceptable. It seemed advisable to draw inferences only from those cases with adequate validity scores. Even with the reduced *Ns*, two significant differences appear.

The *Scientist* group scores significantly higher than the *Nonscientist* group on Kuder Scale 3 (Scientific) and scores significantly lower on Kuder Scale 8 (Social Service). This looks like the "things *vs.* people" distinction mentioned earlier. The directions of the fairly large but not quite significant differences in mean scores on Scale 0 (Outdoor), Scale 1 (Mechanical), and Scale 4 (Persuasive) also suggest the possibility of a "things *vs.* people" distinction.

Table 2. Mean Scores on Kuder Preference Record in 8th Grade for Boys Classified in 12th Grade as Having Scientist and Nonscientist Interests

	Scale	*Nonscience* $N = 10$	*Science* $N = 10$
0	Outdoor	41.50	47.70
1	Mechanical	37.80	43.30
2	Computational	27.30	25.10
3	Scientific	40.90	48.90*
4	Persuasive	43.30	35.50
5	Artistic	27.20	30.90
6	Literary	17.40	17.90
7	Musical	10.80	13.10
8	Social service	38.90	29.80*
9	Clerical	50.50	44.50

* *t* test shows difference significant at the .05 level.

Because it had not proved feasible to develop scoring keys for the Dreese-Mooney Interest Inventory, except for the masculinity-femininity and occupational-level scales, the performance of the *Scientist* and *Nonscientist* groups was studied by item analysis. Of the 237 like-dislike items, only 30 showed differences great enough to be significant at the .05 level, with the small *N*s of 17 and 22. For convenience, the nomographs prepared by Appel (1) were used in testing the significance of the differences between proportions, even though 17 is a little below the lowest *N* indicated on this scale. But a high degree of accuracy about the probabilities involved does not seem necessary.

The first impression one gets in considering the results is that 30 items is a rather low yield in view of the fact that the separate analysis of like and dislike responses to 237 items involves 474 comparisons. One might thus dismiss the obtained differences from further consideration and conclude that the differentiation between scientific and nonscientific varieties of interests had not appeared when the subjects were in fourth grade. To dismiss the differences, however, would be to ignore some bits of data that may constitute important clues in our attempt to understand interest development.

Close scrutiny of the item tabulations suggests that the differences that appear are not really chance differences. For one thing, most items show either a close correspondence between the two groups or a wide disparity. Thus, had the significance level been set at .10 rather than at .05, only nine extra items would have reached the criterion. Further, there are not many items that look as though they might have been significant had the number of cases been larger.

Let us look, then, at the items that yield differences significant at the .05 level. Things that the *Scientist* group liked less or disliked more than the *Nonscientist* group are the following [the F or M indicates whether the item is scored as an M (masculine) or F (feminine) response on the special masculinity-femininity key]:

> Part 1, Books: (F) Read mystery stories. Part 2, Movies: (M) Love stories; (M) Movies about books you have read; (M) Movies about rich people; (M) See the same movie twice. Part 4, Games and toys; (M) Hopscotch; (M) Jacks; (M) Playing school; (M) Riding in an automobile. Part 6, Things to own: (M) Party clothes; Part 7, School subjects: (M) Music; Part 9, Occupations: (M) Nurse; (M) Preacher or religious worker. Part 10, Activities: (M) Go to Sunday school or church; (M) Make costumes for a school play; (M) Write the play.

Things that the *Scientist* group liked more or disliked less than the *Nonscientist* group are the following:

> Part 3, Radio: Quiz contests. Part 4, Games and toys: Hide-and-seek; (M) Playing cowboys; (M) Playing war. Part 5, Hobbies: Clay modeling; Nature Study; (M) Sports. Part 6, Things to own: Dog. Part 7, School subjects: Art (drawing and painting); Science. Part 8, People: Clever people. Part 9, Occupations: (M) Forest Ranger. Part 10, Activities: Go on trips.

The first feature of the pattern that one can see in the differences is that the responses of the *Scientist* group, particularly responses of rejection or aversion, seem to reflect a masculine orientation. The only exception to the general trend is the item "Read mystery stories." The tendency not to enjoy this kind of reading matter is feminine rather than masculine. Otherwise, all the signs point in the same direction.

In view of the foregoing, one might expect that if the two groups were scored by the special MF key the difference between them would be significant. The mean for the *Scientist* group is 30.35; the mean for the *Nonscientist* group 19.09. The difference leads to a *t* of 2.54, significant at the .05 level. It would appear that 10-year-old boys who are developing the interests of scientists are more likely than other boys to give masculine responses to like-dislike items.

Over and above this masculine tendency, other characteristics may be involved in the significant item differences. Relating this analysis to the previous analysis of the Dreese-Mooney test results (8), one finds that a few of the items differentiating the *Scientist* from the *Nonscientist* group are items with high rotated-loadings on the factor earlier labeled *awareness of inappropriate activities* or on the factor earlier labeled *anti-sissy* and described as "the rejection of a tendency to get pleasure out of make-believe."

Both factors are related to the masculinity variable already discussed. Beyond this, without stretching the data too far, it is possible to see in the preferences of the *Scientist* group (for modeling, nature study, art, and going on trips) a tendency to turn toward the real world for satisfaction rather than to fantasies. The liking for quiz contests and clever people may express the beginnings of a tendency to take pleasure in ideas. The difference on the science and nature-study items may represent the beginnings of specialized science interests. These differences, and the direction of the differences on a number of other items, suggest that the boys later to be classified in the *Scientist* group had begun to differentiate themselves from the others even at this early age.

In summary we can say that, while it would not have been possible to identify clear-cut patterns of scientific interests among 10-year-olds, there is some evidence for what might be called *precursors* of scientific interests—a clear awareness of and preference for masculine activities, a turning in the direction of reality rather than fantasy and a tendency to like science courses.

Although there were only 11 of the *Scientist* group and 13 of the *Nonscientist* group who had taken the interest test at the first-grade level, the two groups separated clearly on masculinity, with the *Scientist* group scoring significantly higher. In fact, the difference was sharper at this level than at the fourth-grade level, in spite of the small *Ns*. The difference in means is significant at the .01 level. Furthermore *all* of the 11 boys later classified in the *Scientist* group had scored above the median of the other group for masculinity.

There were no differences that reached the .05 level of significance on the other first-grade interest scales. The only difference significant at the .10 level was on C, *Helping adults with work*. On this scale, the boys in the *Scientist* group averaged a little higher than the *Nonscientist* boys. It is possible that the direction of this difference fits in with the tendency hypothesized from the fourth-grade item differences—the tendency to prefer the real world to the world of fantasy.

COMPARISON OF SCIENTIST AND NONSCIENTIST GROUPS ON ABILITY AND ACHIEVEMENT MEASURES

The most striking thing about these comparisons is the almost complete lack of evidence for differences between the groups at any level. Starting with the final high-school grade-point average, the mean for the 20 *Nonscientist* boys who graduated was 2.50; for the 16 *Scientist* boys who graduated, it was 2.66. The difference did not approach statistical significance. In the *Nonscientist* group, one person moved away and another dropped out before graduation. The *Scientist* group also had one dropout. Seven persons in the *Nonscientist* group and three in the *Scientist* group failed to make a C average. Except for two boys in the *Scientist* group who had unusually high averages (above 3.6) the distributions look much alike.

In the eleventh grade, students in Eugene high schools took The Iowa Test of Educational Development. Table 3 shows the

Table 3. Mean Scores on 11th-Grade Iowa Tests of Educational Development for Boys Classified in 12th Grade as Having Scientist and Nonscientist Interests

		Mean	
	Test	*Nonscientist* *N = 14*	*Scientist* *N = 14*
1	Understanding of basic concepts in social science	19.29	21.84
2	Background in natural science	20.00	23.36*
3	Correctness and accuracy of expression	17.17	18.23
4	Ability to do quantitative thinking	18.14	21.86*
5	Ability to interpret reading materials in social science	20.62	20.21
6	Ability to interpret reading materials in natural science	17.93	20.31
7	Ability to interpret literary materials	17.29	17.62
8	General vocabulary	17.86	18.77
C	Composite score (Tests 1–8)	19.75	21.58
9	Use of sources of information	20.38	21.79

* *t* test shows difference significant at the .05 level.

means on the separate tests for students in the two groups. The only significant differences are on the tests on which knowledge is directly related to scientific interests.

On The Henmon-Nelson Tests of Mental Ability, administered by the school in ninth grade, the 14 *Scientist* boys for whom scores were available obtained an average *IQ* of 115, the 14 *Nonscientist* boys, 110. The difference is not statistically significant.

On the SRA Primary Mental Abilities tests given in the eighth, fourth, and first grades, there are no significant differences on any factor score at any level. The numbers are small, so that it would of course be difficult to establish a difference as significant, but a scrutiny of the distributions suggests no trends. The distributions overlap almost completely.

In general, the most reasonable tentative conclusion is that the differences producing the Strong-test interest patterns under consideration do *not* arise from differences in level or pattern of mental abilities. . . .

GENERAL OVERVIEW

The paper began with two kinds of question about interest development: (*a*) At what age do specialized patterns of interests emerge? (*b*) To what other psychological characteristics are such interest patterns related?

What answers to these questions have been suggested by the group comparisons? The answer to the first question would seem to be that science interests crystallize in most cases before the age of 14 and that they tend to persist once they are established. Even at the age of 10, some boys seem to have taken a direction of development that would lead to clear-cut science interests later. As fourth graders, these boys were more "masculine" in their likes and dislikes, particularly in those activities that they *ruled out*. This difference in "masculinity" was apparent even in first grade. Whether such difference means that potential scientists are more clearly aware of what is expected of them, whether it means that they reach this stage of awareness sooner, or whether it means that they are more inclined to accept limitations when they understand them cannot be determined from our data, but the problem certainly warrants further study.

While the *rejected* activities characteristic of the *Scientist* group at the fourth-grade level express mainly "masculine" choices, the *positive* reactions suggest a pleasure in real experience, an interest in ideas, and the beginnings of a favorable attitude toward the subject matter of science.

These findings suggest that a practical evaluation of interest patterns might be possible at the junior-high level. Indications of specialized science interests at the fourth-grade level are not clear enough to warrant practical selection procedures, but research investigation of *precursors* of science interests and inquiry into the important four-year period from age 10 to age 14, a period for which this study provides no data, would seem to be in order.

To determine how feasible it would have

been to identify at 14 the persons who were going to develop *Scientist* interests, the Kuder Preference Record profiles of the 15 members of the scientist group who had taken the Kuder test in the eighth grade were examined. (In this inspectional analysis those with questionable validity scores were included along with the others.) If the rule of thumb that Kuder recommends in his manual had been used and if scores above the 75th percentile had been considered evidence for the possession of scientific interest, 10 of the 15 boys would have been typed as "scientists" at the age of 14. Only two of the 15 had scores below the 50th percentile.

One could not have predicted as accurately which of the subjects would develop "reject" patterns for science. Four of the 16 *Nonscientist* boys for whom Kuder Preference Records are available had scores above the 75th percentile on the Scientific scale; nine of them scored above the 50th percentile. Only five out of the group of 16 who took the Strong Vocational Interest Blank in the tenth grade had shown clear "reject" patterns for science at that time.

It may be that a considerable number of high-school boys begin at the preadolescent stage to develop interests and attitudes characteristic of scientists. The findings of this study suggest that about two-thirds of them persist in this direction; about one-third shift away from it. Only a small minority *begin* to show scientific interests after the eighth grade.

So far as the second question is concerned, the results suggest that the main characteristic that distinguishes between scientists and other people on the Strong Vocational Interest Blank may be an actual interest *in* scientific activities. The only measures showing consistent differences between the *Scientist* and *Nonscientist* groups are other *interest* scores on previous Strong tests and on the Kuder test. The interests of scientists do not seem to be based on factors such as special talent or family background and they do not seem to reflect very clearly the kinds of general attitude and personal relationships that are measured by other personality tests or by the ratings given by classmates. The only exception to this statement is the variable that is measured by the *masculinity* scales. The MF scores are based on interest items, so that they too constitute a kind of interest measure. But do these differences, appearing most clearly at the *youngest* age levels studied, furnish a clue to some sort of developmental variable broader in its scope than likes and dislikes for specific activities? Are there temperamental differences during the earliest years that underlie the emergence of particular kinds of likes and dislikes? To this question, unfortunately, the present study affords no answer.

So far as the specific information these comparisons have contributed to what we know about *scientists* (rather than to what we know about *interests*), it can be said in general that most of our findings are in agreement with the results of previous research, if we take into consideration one difference between this study and most of the others. In the previous tracing-back studies, the subjects have been scientists or students who had already chosen a science career. In the present study, the subjects are persons who show the interest patterns characteristic of scientists, but they may or may not achieve scientific careers. The choice that a subject would follow his interests rather than let other factors determine his career was an extra component built into the experimental groups studied by Roe (4) and Terman (7). Such choice was not built into the present study.

A convenient summary of the fairly extensive work on scientists and scientific careers can be found in the monograph by Super and Bachrach (6). So far as interests are concerned, the present study corroborates what other studies have shown: namely, that scientific interests emerge during early adolescence or even in the preadolescent years. The failure to find differences in patterns of ability, personality qualities, and family background between *Scientist* and *Nonscientist* groups seems at first to contradict findings from several previous studies. It may be, however, that all of these factors are related not to interests but to the actual choice of a scientific career.

A study by Rowlands (5) presents some evidence to the effect that interest and choice are not the same thing. A group of 654 English school boys, 15-year-olds from six grammar schools representing the full range of economic levels in the London area, were questioned about their interests and plans. Interest in science was not related to family status or to type of school attended, but to level of aspiration and to plans to go to a university.

In another study—reported on the same symposium devoted to children's science concepts and interests—Kelly (3) questioned 117 13-year-old boys in an East London grammar school. He compared those who had chosen a science course with those who had chosen not to specialize in science. The results showed that the boys chose science for positive reasons: enjoying scientific activity, admiring scientists, liking the thought of a scientific career. Boys who had chosen the nonscientific educational program were less likely to mention its attractiveness or its advantages and were more likely to express doubts about their own attainments and abilities. Kelly's results, like those reported earlier in this chapter, suggest that the core of the interest pattern characterizing scientists may be an intrinsic interest in the things that scientists do. This may not be true of the motivational patterns underlying other occupations, as reflected in scores on such tests as the Strong.

Perhaps the most significant thing the present study adds to previous studies of scientists is the evidence for early development of masculine choices as a matrix out of which scientific interests crystallize. This warrants further study of a more discriminating sort than our data permit.

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Development of a Realistic Vocational Choice

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Ginzberg, Ginsburg, Axelrad, and Herma (1951) observed a trend for reality considerations to take priority over subjective considerations as a basis for vocational choice with advances in age during adolescence. From this evidence, they hypothesized that reality factors, such as opportunities for education and employment, become the predominant factors upon which vocational choices are based in adolescence, replacing such subjective factors as interests, capacities, and values (Ginzberg et al., 1951). Small (1953) did not find the hypothesized relationship between age and reality of vocational choice in his sample of 100 boys from age 15 to 19, "but much evidence that reality factors and fantasy drives operate simultaneously at all ages in the selection of a

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vocational goal [p. 17]." The reality-deviation score which Small computed has been criticized by Beilin (1955) as being conceptually different from Ginzberg's "reality factors as a basis for choice," presumably because it is based on an evaluation of the vocational choice, instead of an evaluation of the basis for the vocational choice. However, Beilin notes that it does follow logically that vocational choices which are increasingly based on reality factors should become more realistic with advancing age.

Several other studies have eaxmined the hypothesized relationship between realism and age. In general, they have not found a relationship (Lockwood, 1958; Stubbins, 1948; Super, 1961). Gribbons (1964) did find an increase in an interview-rating factor entitled "Occupational Choice—awareness of factors to consider" when the eighth-grade students in his sample were reinterviewed 2½ years later, in the tenth grade. Methodological problems such as unreliable ratings and restricted age ranges may have reduced the probability of finding a relationship in the earlier studies.

DEFINITIONS OF VOCATIONAL CHOICE REALISM

Three methods have been used to evaluate the realism of vocational choices. Sometimes census statistics were used to compare the distribution of workers in occupational groups with the distribution of the vocational choices of groups of students (Byrns, 1939; Coxe, 1930;. Miller & Form, 1951; Myers, 1947; Shosteck, 1955; Sisson, 1937; Stephenson, 1957). Other investigators have had judges clinically combine available data on the student and rate realism (Gonyea, 1963; Gribbons, 1964; Hewer, 1966; Hoyt, 1955; Pool, 1965; Small, 1953; Stubbins, 1948; Wright, 1963). A third group of researchers has explicitly established the criterion by which the individual's vocational choices were evaluated (Carp, 1949; Milliken, 1962; Super, 1961). The first method does not evaluate the individual potential of each student, nor does it always recognize that students are a select group in the adult population. The second method introduces the possibility of unreliable ratings. The third method seems to be the most reliable, particularly if objective and reliable evaluations are made of the work environment as well as the individual student.

PROBLEM

Are there increases in the realism of vocational choices with advancing age? It was hypothesized that there would be increases in the percentages of realistic vocational choices with advances in both age and grade during adolescence.

METHOD

The third method was used in this study to evaluate the realism of the level of vocational choice. Choices were obtained in response to a modification of a question used by Trow (1941): "What occupation do you plan to enter? Be as specific as possible. If you have no occupational choice, then put 'undecided.'" The choices were then classified into one of the six levels of Roe's two-dimensional system (Roe, 1956). Moser, Dubin, and Shelsky (1956) have presented evidence that occupations can be reliably classified using this system. Data from studies by Stewart (1947) and Wolfle (1954) were used in estimating the distribution of intelligence within each occupational level. The student's intellectual capacity was estimated using the composite score of the Iowa Tests of Educational Development (ITED, 1963) or the Iowa Tests of Basic Skills (ITBS, 1956). An estimate of the score needed to rank at the twenty-fifth percentile within each occupational level was used as the minimum intellectual capacity for operationally defining a realistic choice at each level (Super, 1961). A student with a score below that minimum has an *unrealistic* choice. Similarly, a maximum intellectual capacity was used to define *an unfulfilled choice*, a category of choice which indicates intelligence greatly in excess of that required by the occupation. Since a possible outcome of implementing an *unfulfilled* choice is job dissatisfaction, estimates of the percentage of job dissatisfaction at each occupational level were used to establish the upper limits (Centers, 1948). The upper and lower limits of aptitude at each occupational level are shown in Table 1.

Sample

The sample consisted of public school sixth- through twelfth-grade students from

Cedar Rapids, Iowa, who stated a vocational choice during the 1962–63 academic year. All students in the seventh through twelfth grades were questioned. Sixth graders in a sample of elementary schools which was representative of the city in terms of rental areas were also included (Crites, 1965). Of 7,397 students questioned, vocational choices from 463 students could not be used because aptitude test scores were not available and 2,318 students did not have a vocational choice, leaving 4,616 students. The distribution of the 4,616 students with vocational choices by grade, age, and aptitude level is shown in Table 2.

Table 1. Percentile Rank Ranges for Defining the Realism of the Level of Vocational Choice

	National norms			
Level of choice	*AGCT Adult*	*ITBS Grade 5–8*	*ITED Grade 9–12*	*ACT College freshman*
1	99	99	99	99
	95	95	93	77
2	98	98	98	95
	80	80	73	41
3	95	95	93	77
	60	60	42	14
4	85	85	78	47
	45	45	22	04
5	75	75	64	29
	25	25	07	00
6	55	55	32	08
	00	00	00	00

Note—The Army General Classification Test (AGCT) scores were used as an intermediate step in establishing the limits at each level; American College Test (ACT) scores were not used in present investigation, but are included for the use of other investigators.

Analyses

The realism information was treated as categorical data. The unfulfilled choices were placed in the same category as the unrealistic choices, because the unfulfilled choices were not present in all cells and where present the numbers were quite small. The combining of unfulfilled and unrealistic choices is further justified inasmuch as the theoretical questions are concerned with realistic choices.

The analyses relating the realism categories to age and grade were computed with a control on intellectual capacity provided by grouping the students within scholastic aptitude quartiles. This was done because the manner in which realism was defined created an artifactual relationship between aptitude and realism. Eight chi-squares were computed, each one being based on a 2 × 7 table within an aptitude quartile, with two realism categories on one dimension and age or grade on the other dimension.

RESULTS

Table 2 also shows the percentages of realistic choices and the values of chi-square for each aptitude level for both grade and age. The percentages were based on the total number of students stating a vocational choice. The values of chi-square testing the significance of the realism relationship with *grade* were all significant at or beyond the .05 level. Three of the four chi-squares testing the *age* relationship were also significant. The significant values of chi-square show that, in general, realism of vocational choice was related to both age and grade.

The overall trends were increasing ones, as hypothesized by Beilin. Of the 14 decreases in percentage of realistic choice, 6 occurred where 1 of the percentages was based on an n of less than 100. The decreases were on the average small, the median decrease being 1.15 percentage points and the median increase 5.00 percentage points. Only in the second quartile in the grade analyses did the number of decreases equal the number of increases.

DISCUSSION

Certain limitations are associated with the definition of realism, however, they are defensible. The artifactual dependence of realism on intelligence, for example, is to be expected logically, since there are more occupations open to those persons of greater intellectual ability and thus there will be greater percentages of realistic choices among more intelligent students. Equating ability levels across vocational fields presents special problems for a field such as arts and entertainment. However, these fields frequently require advanced education for the higher levels today, particularly in artistic and creative writing fields; therefore, tests of scholastic aptitude probably provide one criterion, admittedly incomplete, for evaluating the realism of the level of choice in these fields.

Table 2. Total Number of Students, Percentages with Realistic Vocational Choices within Each Aptitude Quartile, and Values of χ^2 for Grade and Age Analyses

Scholastic aptitude quartile	*School grade*							*Total n*	χ^2[a]
	6	*7*	*8*	*9*	*10*	*11*	*12*		
76–99									
n	30	159	182	271	296	240	165	1,343	
percentage	63.3[b]	62.3	65.9	69.0	64.2	70.8	78.8		14.894*
51–75									
n	40	213	196	225	246	159	171	1,250	
percentage	52.5	52.1	49.0	56.9	62.6	58.5	65.5		16.317*
26–50									
n	60	191	169	129	230	182	116	1,077	
percentage	30	34.0	33.1	43.4	56.5	60.4	60.3		62.954‡
01–25									
n	56	165	175	190	178	88	94	946	
percentage	3.7	2.4	1.7	34.7	38.8	45.5	56.4		183.060‡
Total *n*	186	728	722	815	950	669	546	4,616	

Scholastic aptitude quartile	*Age in years*							*Total n*	χ^2[a]
	12	*13*	*14*	*15*	*16*	*17*	*18*		
76–99									
n	83	180	231	387	259	156	27	1,323	
percentage	61.4	61.1	68.8	66.9	68.3	79.5	81.5		17.561†
51–75									
n	126	223	194	304	194	153	34	1,228	
percentage	46.8	53.8	57.7	60.2	57.7	62.7	67.6		11.165
26–50									
n	115	182	161	233	198	113	48	1,050	
percentage	33.0	32.0	37.9	53.2	57.6	63.7	64.6		62.110‡
01–25									
n	88	151	200	210	138	88	42	917	
percentage	9.1	1.3	15.6	22.9	42.8	45.5	53.7		137.173‡
Total *n*	412	736	786	1134	789	510	151	4,518[c]	

[a] $df = 6$.

[b] Percentage of realistic choices within upper quartile in the sixth grade.

[c] Small numbers of 11-, 19-, and 20-year-old students necessitated dropping them from the analysis.

* $p < .05$.

† $p < .01$.

‡ $p < .001$.

Theoretical Implications

The exact form of the relationship between realism of vocational choice and age was not specified by either Beilin (1955) or Ginzberg et al. (1951). The reversals in percentage within these data, where the expected cell frequencies were large enough to permit testing with chi-square, were not reliable. Although there may be plateaus where there is no increase in realism, the general trend during adolescence is one of increasing realism with advances in age or grade. The systematic increase in realism is particularly evident at the lower aptitude levels, where the students are not multipotential.

Ginzberg et al. proposed that a period of realistic choice begins between the ages of 16 and 18. If the beginning of a period is defined as the largest single increase in the percentage of realistic choices, then these data indicate that the period of realistic choice begins between the ages of 14 and 16 in the ninth grade. There is a change from the ITBS to the ITED as the student goes from eighth to ninth grade, but it seems unlikely that changing the test would alter the student's percentile rank within his class enough to produce this effect, since in an unpublished study the test-retest correlations between the eighth-grade ITBS composite and the ninth-grade ITED composite was .93 for 500 students.

In a cross-sectional design such as this one, age differences are confounded with differences in date or period of birth, and in a longitudinal design age differences are confounded with times of measurement (Schaie, 1965). However, even were these confoundings not present, the relationship between age and other variables would be purely descriptive and not explanatory (Townsend, 1953). For this reason, school grade seems preferable to age as an index of development in adolescence.

Life experiences and developmental tasks are shared by students in the same school grade, particularly within one educational system, and these common academic and social learning experiences suggest where one might look to find the influences which produce group differences in both cross-sectional and longitudinal studies of development.

Further Research

Tyler (1964) reports that the occupational criterion which intelligence tests predict best is occupational level. Since intelligence test scores are highly correlated with scholastic aptitude test scores, counselors should be able to assess whether a student will later be able to enter an occupation at his chosen level. Hewer (1966) found judges' global ratings of realism of vocational choice were not related to eventual occupational entry for 80 college students contacted 8 years later. Rather than judges' ratings, a more explicit definition of realism, limited to the level of vocational choice, would be more likely to be related to eventual occupational entry. Longitudinal validity evidence provided by a study predicting occupational entry could be of use to both counselors and investigators. Numerous investigators have used realism of vocational choice as a criterion for evaluating vocational counseling (e.g., Gonyea, 1962; Hoyt, 1955; Pool, 1965; Wright, 1963). With additional validity evidence and measurement refinements, the realism definition proposed in this study could be used as a more reliable criterion than judges' ratings.

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Explorations of a Theory of Vocational Choice
Part IV: Vocational Daydreams[1]

John L. Holland

. . . A group of 360 boys and 278 girls were polled by mail just before they entered college. Students were obtained from a pool of 7,000 volunteers of high aptitude students, who were National Merit Finalists and Commended students. No case can be made for the sample as representative of any well defined sample. They are described best as students of superior scholastic aptitude who come largely from families in which the father's occupation is managerial, semi-professional or professional.

Students filled out a questionnaire which included questions about their vocational choices, an adjective check-list, self-ratings, multiple choice questions about how they cope with problems of living, and sentence stems about vocations. Students also took the Vocational Preference Inventory, fifth revision [1]. The Realistic, Intellectual, Social, Conventional, Enterprising, and Artistic scales were scored and used to determine a student's resemblance to each type in the theory.

RESULTS

History of Vocational Daydreams

In previous studies, we have discovered that some personality types are more likely to change their vocational choices than others [3, 4]. Specifically, we found that among boys the Realistic and Intellectual types were more stable, especially the Intellectual type, while the remaining types, especially the Social types, were less stable. The unsocial, independent, unconventional character attributed to the Intellectual type and the social, oral dependent character attributed to the Social type appear consistent with these earlier empirical results.

For girls, we found that students resembling the Social type were more stable than any other type. In this instance, stability in vocational choice appeared to be associated with a feminine choice. These results suggest that the Social type for girls needs revision. In the present study, it appeared useful to learn if these differences in stability for students of different types at the college level would also be demonstrated in students' reports of daydreams about vocational choices for the adolescent and childhood years.

A retrospective report of all the vocational choices a student could remember was obtained to test the hypothesis that Intellectual and Realistic types among boys will have histories of tentative vocational choices with less variation in types of choice than will students resembling the remaining four types. For girls, Social types will have the most stable history of vocational choices. "Stability" in these hypotheses means choices falling in the same group of occupations assumed to represent each type. The information for testing these hypotheses was obtained from the following item.

> List below all the vocations you have ever considered in thinking about your future. List the vocations you have daydreamed about as well as those you have talked to others about. Try to give a history of your tentative choices and

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[1] EDITORS' NOTE: This investigation is one of the several studies performed to examine the usefulness of a theory of vocational choice perhaps best characterized as a heuristic theory of personality types and environmental situations.

[2] The theory postulates six model types (realistic, intellectual, social, conventional, enterprising, and artistic) and relates each type to certain hypothesized vocational preferences. Previous data from this study has shown that students of superior aptitude perceive occupations in stereotyped ways which tend to be consistent with the theoretical formulations for each type and that vocational preferences generally are associated with self-conceptions and favored mechanisms for coping with stress as expected. For a complete account of this study the reader should also see Part I: "Vocational Images and Choice" (*Vocational Guidance Quarterly*, 1963, *11*, 232–239); Part II: "Self-Descriptions and Vocational Choice, (*Vocational Guidance Quarterly*, 1963, *12*, 17–21), and Part III: "Coping Behavior, Competencies and Vocational Preferences," (*Vocational Guidance Quarterly*, 1963, *12*, 21–22).

Table 1. Vocational Choice at High School Graduation (Present Choice) and its Relation to a Student's Recollections of Earlier Vocational Choices (Boys)

Vocational choices	*Present choice is Realistic Distributions of earlier choices*							*Present choice is Intellectual Distributions of earlier choices*							*Present choice is Social Distributions of earlier choices*						
	1[a]	*2*	*3*	*4*	*5*	*6*	*7*	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*1*	*2*	*3*	*4*	*5*	*6*	*7*
Present	100								85								49				
Seventh	36	29	11	3	9	9	3	18	25	17	1	16	4	4	8	16	6	2	11	6	
Sixth	37	26	9	1	12	2	13	22	26	17		12	6	2	9	16	3		10	3	6
Fifth	31	14	15	2	4	4	30	21	13	11	1	8	8	23	13	10	4	1	5	3	13
Fourth	22	12	7	1	8	2	48	24	15	2		4	3	37	7	6	6		1	3	26
Third	15	4	6	1	2		72	13	8	3		5	2	54	7	3	2		2	2	31
Second	9	4	1			1	85	7	3	1		2	1	71	8	2	1				38
First	3	2	2		1		92	7	4				1	73	6					1	42
	Present choice is Conventional							*Present choice is Enterprising*							*Present choice is Artistic*						
Present				9								72								31	
Seventh	1	2	1		5			7	11	16	2	27	6	3	4	6	2		9	10	
Sixth	5	1		1	2			18	8	14	1	20	6	5	6	6	8		4	6	1
Fifth	4		2	1	1		1	18	8	15	3	11	7	10	4	9	7		2	4	5
Fourth	2	1	1		1		4	20	9	7		10	6	20	5	6	4		2	3	11
Third	2						7	15	6	3	1	6	2	39		4	4		1	3	19
Second			1				8	11	6	4		2	2	47	4	4					23
First		1					8	5				3	1	63	2	1			1		27

Note: In this table and in Table 2 the rows for each kind of *present* choice are the distributions of vocational choices at different age levels. The "first" choice is the distribution of choices at the earliest age level. The second choice is for the next earliest age, and so on. Notice that students are increasingly unable to remember tentative choices as the number of recalled choices increased. Very few students can recall 5–7 choices.

[a] 1 = Realistic, 2 = Intellectual, 3 = Social, 4 = Conventional, 5 = Enterprising, 6 = Artistic, and 7 = No Response.

Table 2. Vocational Choice at High School Graduation (Present Choice) and its Relation to a Student's Recollections of Earlier Vocational Choices (Girls)

Vocational choices	*Present choice is Realistic Distributions of earlier choices*							*Present choice is Intellectual Distributions of earlier choices*							*Present choice is Social Distributions of earlier choices*						
	*1**	*2*	*3*	*4*	*5*	*6*	*7*	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*1*	*2*	*3*	*4*	*5*	*6*	*7*
Present	1								42								82				
Seventh			1					2	17	10	1	5	6	1	4	17	29	4	10	14	4
Sixth			1					1	14	11		1	8	7	2	8	32	4	6	20	10
Fifth			1					1	12	10	2	1	7	9		15	22	4	7	18	16
Fourth							1	3	2	11	2	4	3	17	3	12	18	5	5	11	28
Third							1	2	4	8			3	25	3	6	14	1	1	8	49
Second							1	3	3	4			2	30	1	5	16	1	3	1	55
First							1	1	2	1			2	36		2	5		1	5	69
	Present choice is Conventional							*Present choice is Enterprising*							*Present choice is Artistic*						
Present				8								44								81	
Seventh	1	1	1		1	4		1	12	15	1	6	9		2	15	26	4	11	22	1
Sixth				4	1	1	2	1	13	14	1	2	12	1	4	21	15	3	7	25	6
Fifth	1	1		2	1	1	2	3	14	14		3	4	6	4	14	16	4	6	21	16
Fourth		1	4			1	2	1	10	9	2	3	4	15	7	10	17	2	3	20	22
Third			1	1		1	5	4	5	6	2	2	3	22	3	7	15		4	18	34
Second	2			1		1	4	1	1	5			3	34	4	7	9			11	50
First							8	3		1			2	38	2	3	2	1	1	8	64

* 1 = Realistic, 2 = Intellectual, 3 = Social, 4 = Conventional, 5 = Enterprising, 6 = Artistic, and 7 = No Response.

daydreams. Put your present choice on line 1 and work backward to the first vocation you ever considered.

	Vocation	At about what age?
1.		
2.		
3.		
4.		
5.		
6.		
7.		
8.		

Student vocational choices were coded by the classificatory scheme outlined in Table 1. Students were then classified by their present vocational choice to form six groups, and distributions of vocational choices at younger ages were formed for each of these groups. Tables 1 and 2 summarize these results.

When Tables 1 and 2 are reviewed, several trends stand out. For boys with present choices of Realistic or Intellectual vocations, the modal choice in the past has always been Realistic or Intellectual. For boys with present choices of Social, Conventional, Enterprising, and Artistic vocations, the modal choice in the past has usually included Realistic or Intellectual choices *and* choices in the remaining classes.

This variation in daydreams about choice closely parallels the tendency to change field in the first two years of college for students with science and non-science majors and vocational choices; that is, stability in major field and vocational choice is greater for students with initial choices of Intellectual and Realistic fields [3]. And now we find less variability in the vocational daydreams of students with Realistic and Intellectual vocational choices than we do for students with non-science choices.

The evidence for girls reveals a different pattern of variation in daydreams. Girls with present vocational choices of a Social character have generally daydreamed about Social vocations; girls with choices in all other fields show without exception greater variability in vocational daydreams. (The seven successive modes in the distributions of Social choices have a probability of occurring by chance only once in 279,936 times.)

The trends in Tables 1 and 2 suggest several hypotheses: Stability in a student's choice of vocation is greater when his choice is appropriate to his sex role. Stability of vocational daydreams and in major field may be part of what we term emotional stability. That feminine boys and masculine girls are less stable emotionally is a common assumption which appears to parallel our findings for changes in vocational choice both in the present study and other reports [3, 4]. The present findings also give strong support to the importance of sex typing and vocational choice discovered earlier by Tyler [8].

DISCUSSION

The use of a sample of students of superior scholastic aptitude limits the generalization of the findings so that the results may not be applicable to students of lesser aptitude and socio-economic status. These selection biases were also accomplished by a biased distribution of vocational choices which differs sharply from the distribution of choices for high school seniors generally. Similar studies using more normal student and adult populations are needed to extend the usefulness of the present study.

The chief value perhaps of this investigation is tentatively to outline some of the developmental history and the processes of vocational choice with which our earlier studies did not cope. Although most of the present findings were obtained with an inexact method, they imply that students acquire vocational images which have some validity, that students select vocations both to engage in attractive activities and roles, and to avoid activities and roles which they see as distasteful, and which they believe they are incompetent to perform.

Further, it seems clear that students make choices in terms of the kind of person they believe themselves to be. This formulation for the results is consistent with the first statement of the theory. The advantage of the present, primitive theory is that it outlines *how* a particular cluster of self-conceptions and personal traits are related to a particular class of vocational choices. The present results are also consistent with the formulations by Super [6] and Tiedeman and O'Hara [7] that "vocational choice is the implementation of a self-concept." Unfortunately, their broad statements do not lend themselves to explicit test, since they do not specify what self-conceptions lead to what kinds of vocational choices. Of equal importance, the present theory is not limited to the shortcomings of a self-concept theory: motivational influences are not always conscious, self-concept may be a methodological artifact [5] and self-reports for some aspects of personality and performance have little or no validity.

The occupational images of our sample of talented students imply the need for the development of more differentiated images. For example, although researchers are needed by most occupations, this role is found largely in the image of the physicist and engineer. Although some teachers are well paid, the image is that all teachers are underpaid, and so on. The development of more differentiated images might permit students to see themselves in more diverse vocations and roles within an occupation, a situation which might serve to attract talented students into more diverse occupations, and occupations in need of people of high scholastic aptitude. . . .

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Persistence of Vocational Choice of the Merit Scholarship Winners

Aubrey L. Forrest

Recent studies have reported the effectiveness of efforts to recruit top high school graduates into so-called "critical fields," namely, engineering, scientific research, teaching, and medicine. . . . Only a few studies, however, have attempted to examine the persistence of vocational choice of highly talented youth.

At least one study indicates that 50 per cent of the normal college population will change fields of major during the four years of undergraduate study [3]. . . . In a study based upon a questionnaire completed by scholarship winners in the 1956 National Merit Scholarship Program at the end of their freshman year [4], Thistlethwaite found a tendency for Merit Scholars to shift from science to nonscience major fields of study and from applied to theoretical fields.

These . . . studies raise questions of interest to both the recruiters and counselors of talented youth. To what extent do changes in

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major field of study indicate changes in vocational choice? How persistent are talented youth in their vocational choices? What are the patterns of change, if any? If patterns exist, what appear to be the motivations for change?

METHOD

In both the Iffert and Thistlethwaite studies, a single questionnaire was used and the subjects were asked to recall their original choice of major field. The present study was based upon two questionnaires completed by 507 of the 555 Merit Scholars selected in the 1956 National Merit Program. During the fall semester of the 1955–1956 school year, as high school seniors participating in the first Merit Program they were asked, "Look ahead fifteen years. Describe in a paragraph of not over one hundred words what you hope to be doing." A second questionnaire administered at the end of their third year of college asked, "What do you hope to be doing five to ten years after graduation?" Both questions related to approximately the same time period, 1965 to 1970.

Selected primarily on the basis of competitive tests from among the top one or two per cent of the nation's high school graduates, these Merit Scholars have been enrolled in 160 different colleges and universities throughout the country. Not included in this study are the 48 Scholars who had not completed three years of undergraduate study. Data are included for 368 males and 139 females.

In tabulating responses to both questionnaires, responses were included under each category only if it were clear that the respondent had decided upon a particular vocational area. Answers which were ambiguous or undecided were included in a category labeled "Undecided." Many of the female Scholars included marriage and family plans as primary or secondary preoccupations during the time period with which this study was concerned. These were disregarded and the part-time or full-time vocational choice was tabulated.

All categories were considered to be mutually exclusive. "Research" includes employment for pure research purposes, in private and governmental settings, including medical research. "Engineering" includes consulting, planning and practical employment in private and governmental enterprises. "Business" includes all employment in private practice at a management level, excluding those who are interested in purely legal aspects, but including those who may have scientific backgrounds, but specifically state their goal as being that of administration and management of personnel. "Military" includes only career officers, not engaged in scientific research.

The National Merit Scholarship Corporation receives from Scholars regular annual reports on which they inform the Corporation of their present major field of study. The following categories are used to classify major fields of study: literature and language; social sciences and history; physical science; engineering; art; music; business administration; speech and drama; mathematics; and philosophy and religion.

FINDINGS

Data were tabulated on the changes and non-changes of major fields of study and vocational choice for the 292 males and 120 females whose first choice was classifiable. While 50 per cent of the males who had a specific first vocational choice changed their choice during the three and one-half year period, only 32 per cent changed major field of study. Among the females, 54 per cent changed their original vocational choice, while 39 per cent made a change in major field of study.

Of the 94 males who changed major field, 79 per cent changed also their vocational choice; of the 47 females who changed major field, 85 per cent changed also their vocational choice. Of the 198 males who made no change in major field, 64 per cent also made no change in vocational choice; while of the 73 who made no change in major field, 66 per cent also made no change in vocational choice.

These data would indicate that although a change in major field is usually accompanied by a change in vocational choice and that persistence in one's major field is usually accompanied with persistence in vocational choice, there may exist a substantial number of exceptions to this tendency. This suggests another measure of stability of interest among talented youth.

It might be useful to study independently persistency in major field of study and persistency in vocational choice. We would, then, hypothesize that the interests of some students revolve around a major subject of interest, while the interests of others revolve around a vocational goal. If the data are examined in this light, we find that among the 292 males who had a classifiable choice of vocation and major field, 75 per cent maintained a preference for either their first field of study or their first vocational choice. Of the 120 females who had a classifiable choice of vocation and major field, 67 per cent maintained their interest in either their first choice of major or vocational goal. This indicates for both sexes a higher degree of stability of interest than would be indicated if we considered either major field of interest or vocational choice alone.

Of the 368 males responding to both questionnaires, 328 had a specific vocational choice on one or both questionnaires; and 261 indicated specific choices on both. . . . Of the 292 having a specific choice on the first questionnaire, 73 per cent chose the scientific fields of science teaching, research, engineering and medicine; 50 per cent chose the theoretical fields of teaching and research. Of the 297 indicating a specific vocational choice in the second questionnaire, 58 per cent chose scientific fields; 55 per cent chose the theoretical fields.

Although these data indicate a tendency for male Scholars to move from scientific to non-scientific vocational fields and from applied to theoretical fields, the trends are not clear cut. Fields of engineering and medical practice showed slight increases, and the area of science teaching indicated a substantial increase. The major loss in the scientific area was in the field of research. The major increase in the non-scientific field was in non-science teaching. Also of interest was the trend shown for those originally undecided to go into the three theoretical fields of science teaching, non-science teaching, and research. There was a marked trend for those with a specific first choice, but who later became undecided, to move away from fields of science teaching, research, and engineering. This kind of change probably represents a period of transition in which these Scholars will follow their colleagues into non-science vocational fields. There is also some indication that there was a trend to move away from the business management field to more theoretical fields.

Perhaps a better indication of trends is to examine the 261 who had specific vocational choices at both times during the study. Of these, 114 changed their vocational choice. Of the 114, 51 per cent changed to more theoretical fields; 24 per cent changed to less theoretical fields; 25 per cent changed to fields in which the work was at about the same level of abstraction. Of the 114, 7 per cent changed from non-science to scientific fields; 43 per cent changed from science to non-science fields; and 55 per cent did not change from science to non-science or vice versa.

Of the female Scholars, 139 responded to both questionnaires; 132 had specific vocational choices on one or both questionnaires; and 107 indicated a specific choice on both questionnaires.

. . . Of the 120 who had specific vocational choices in the first questionnaire, 47 per cent chose the scientific fields; 50 per cent chose the highly theoretical fields. Of the 119 having a specific vocational choice in the second questionnaire, 35 per cent chose scientific fields; 66 per cent chose the theoretical fields.

The number of girls selecting science teaching remained stable, while non-science teaching showed a substantial increase. Research, engineering, and medical practice showed slight decreases.

Of the 107 who had vocational choices on both questionnaires, 53 changed their vocational choice. Of the 53 who changed, 47 per cent changed to more theoretical fields; 13 per cent changed to less theoretical fields; and 40 per cent changed to fields which were at approximately the same level of abstraction. Of the 53, 9 per cent changed from non-science to science fields; 34 per cent changed from science to non-science vocational fields; and 57 per cent did not change from science to non-science or vice versa.

DISCUSSION AND SUMMARY

In this study of vocational plans of 507 Merit Scholars, it was found that about one-half changed vocational choice between their senior year in high school and the end of their junior year in college. Female Scholars demonstrated a slightly higher rate of change than male Scholars.

It was further demonstrated that a

change in major field was closely related to change in vocational choice. In 80 per cent of the cases in which there was a change in major field, there was also a change in vocational choice; while in 65 per cent of the cases where there was no change in major field of study, there was also no change in vocational choice. This suggests that a strong tendency exists for a change in major field of study to result in a change in vocational choice, while a change in vocational choice is less likely to result in a change in major field. Perhaps this is because a vocational choice, at this point in a student's life, is easier to change, or at least to declare, than the major field of study choice. It was also found, however, that three-fourths of the males and two-thirds of the females maintained their interest in either their original choice of vocational field or major field of study.

The present study indicates that the major patterns of change were from science to non-science fields and from practical to theoretical fields. The former trend seems to show that the efforts to recruit talented students into scientific fields have not been as successful as first thought on the basis of studies of original choices of major fields of study. Insofar as the critical field of teaching is concerned, indications are that more and more talented youth will tend to switch to the field of teaching as a part of the general trend from applied to theoretical fields.

One interpretation of the science to non-science trend would be that some high school students who are not really interested in science are being recruited by current efforts to increase the supply of scientists. There is also the possibility that the hope of winning a scholarship has played a role in influencing the stated preferences of vocational choice for these Merit Scholars. Because of the nature of some scholarship sponsors, a student seeking financial aid may feel that his chances of success are better if he states that he proposes to enter a science occupation.

Patterns of change in vocational choice of Merit Scholars were found to be different from those of the normal college population studied by Iffert. Merit Scholars are seemingly less concerned with "bread and butter considerations" in making occupational changes. This would suggest that the motivations or values of talented youth differ from those of the normal college population in that talented youth perceive certain benefits to be found in the more theoretical pursuits which are not to be found in the practical fields; or, such values or motivations may be the result of the high socio-economic status of Merit Scholars. The same observations may be made in comparing the attractiveness of scientific with non-scientific fields.

One further interpretation of the data would include the possibility that the changes in vocational choice are related to the college experience *per se*. A recent study of Thistlethwaite suggests that college faculties in science and non-science areas differ significantly [5]. Thistlethwaite found that "faculties in the arts, humanities, and social sciences tended to exhibit an image which is more friendly, and enthusiastic, and supportive than that presented by faculties in the natural and biological sciences. The former impressed students as exerting more press for independence and less press for pragmatism and compliance." Thistlethwaite's analysis also pointed out certain pedagogical techniques which differentiated the science from the non-science faculties.

These interpretations have important implications for those who are concerned with the guidance and counseling of talented students, as well as for those who are seeking to recruit talented personnel for the critical occupational fields. Changes in vocational choice among talented youth appear to be motivated by desires to seek vocational areas which give more stress to humanistic values and to achievement in areas stressing or requiring work at higher levels of abstraction. Trends toward teaching may involve a desire for greater independence or other needs. It would appear then that recruiting efforts will fall short of success if they are limited to financial inducements alone.

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Career Changes in College

Charles E. Werts

In studying factors related to changes in career plans during college, Davis found evidence of a trend toward "social homogeneity, the tendency for 'birds of a feather to flock together.' Regardless (almost) of personal characteristics or occupational field, the 'deviants' switch out and students with traits characteristic of the field switch in. Career decisions in college tend to accentuate the occupational differences already present at the beginning of college."[1] This result complements Wolfle's finding that intelligence differences between those entering different vocational fields are relatively constant at all educational levels, viz., high school juniors, college undergraduates, graduates, graduate students, and employed college graduates.[2] An attempt will be made here to replicate Davis' findings using data on career changes in the freshman year of college. This study is limited to eight groups of college freshmen, those who indicated career choices of: physicist, chemist, physician, engineer, lawyer, teacher, accountant, and businessman.

Two hypotheses are proposed. The first hypothesis derives from findings, both of the present study and Davis, that freshmen who want to be physicians and lawyers come from relatively high SES families,[3] physicists and businessmen from intermediate; and teachers, engineers, chemists, and accountants from relatively low SES families.[4] In line with Davis' findings, this hypothesis suggests that those switching out of medicine and law should be of lower SES than those who remain; those switching out of teaching, engineering, chemistry, and accounting should be of higher SES than those who remain; and no clear-cut SES differences between those who stay and those who leave should be found for physics and business. Previous studies[5] agree that the mean ability levels of students wanting to be teachers, accountants, and businessmen are low compared to the other five career choices. This yields the second hypothesis: those students switching out of physics, chemistry, medicine, engineering, and law should be of lower ability than those who remain, while those switching out of teaching, accounting, and business should be more able than those who remain. In both SES and ability, those who switch into another field as sophomores will be like those persons in the field who did not switch out.

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[1] James A. Davis, *Undergraduate Career Decisions*, Chicago: Aldine Publishing Co., 1965.

[2] Dael Wolfle, *America's Resources of Specialized Talent*, New York: Harper, 1954. Psychology, the social sciences, and physical education were exceptions to this rule. Those employed as psychologists and social scientists are more selected than students in these areas, with the reverse being true of physical education. Davis also notes that the social sciences are exceptions to the general principle, "people-oriented" students tending to shift out, even though initial choice of the social sciences is associated with a "people orientation."

[3] EDITORS' NOTE: Socioeconomic status (SES) is a measure of social class which in the present study is based upon the level of father's occupation and education.

[4] Charles E. Werts, "Career Choice Patterns: Ability and Social Class," *NMSC Research Reports*, 1966, 2, (3).

[5] Carl Bereiter and Mervin B. Freedman, "Fields of Study and the People in Them," in R. Nevitt Sanford, editor, *The American College*, New York: Wiley, 1962.

SAMPLE

Of 127,000 freshmen studied on entry to college, 30,000 were followed up to obtain the data for studying career changes.[6] The career choice data collected upon college entrance (fall, 1961) were compared to career choice data collected by mail survey one year later (summer, 1962).

For each career category, the following definitions will be used in discussing the results:

loyalists = those whose career choice codes were identical from the beginning to the end of the freshman year.

defectors = those who gave this career choice initially, but changed to any other code by the end of the freshman year.

recruits = those who gave any other career code initially and who gave this particular choice at the end of the freshman year.

To get reliable comparisons of loyalists versus defectors, only those careers with 150 or more defectors were studied. No analysis was possible for females because of the overwhelming choice of "teacher." For males, eight career choices had sufficient numbers of defectors to warrant analysis: engineer (N = 1999), teacher (N = 1816), physician (N = 1576), businessman (N = 928), lawyer (N = 869), chemist (N = 484), accountant (N = 420), and physicist (N = 391).

PROCEDURE

At the beginning of the freshman year, each student filled out a short information form which included the following questions:

1. Circle one: Male Female
2. Your high school average (circle one):
   D C C+ B– B B+
   A– A A+
3. Probable future occupation:
4. Father's education (circle one):
   Grammar school Some high school H.S. grad.
   Some college College degree Post-grad. degree
5. Father's occupation:

The following questions were asked in the mail follow-up one year later:

1. What is your average grade so far in college? (circle one):
   A A– or B+ B B– or C+ C
   C– or D+ D or less
2. What occupation will you pursue after you complete your education?

RESULTS

Table 1 shows mean father's education (FAED) for loyalists, defectors, and recruits, with careers grouped according to the first hypothesis. In this case, the original hypothesis appears to be completely confirmed: loyalists had more FAED than defectors among would-be physicians and lawyers; less FAED than defectors among teachers, engineers, chemists, and accountants; with little difference among physicists and businessmen. The findings for father's occupation were identical, as might be expected from the close relationship between occupation and education.

Three ability measures were available for study: high school grade average (HSG), freshman grade average in college (CG), and scores on the National Merit Scholarship Qualifying Test (NMSQT) taken during the student's junior year in high school. The NMSQT[7] data were not reported here because (a) scores were available on only two thirds of the sample, and (b) the results were identical with those for HSG and merely indicated that more objective ability measures give similar findings. Table 2 shows mean high school and college grades for loyalists, defectors, and recruits. For both measures, the comparison of loyalists and defectors for careers that initially drew high ability students (physicist, chemist, physician, engineer, and lawyer) showed that loyalists

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[6] Details of the data collection procedure were given in Charles E. Werts, "Social Class and Initial Career Choice of College Freshmen," *Sociology of Education*, 39 (Winter, 1966), pp. 74–85, reprinted in Section II B of the present reader, in which the same sample was used to study initial career choices in college.

[7] *National Merit Scholarship Qualifying Test, Spring 1966, Interpretive Manual*, Chicago: Science Research Associates, Inc., 1966.

Table 1. Father's Education of Loyalists, Defectors, and Recruits for Various Career Choices

Social class hypothesis	*Career choice*	*Sample size*			*Mean father's education*			
		Loyalists	*Defectors*	*Recruits*	*Loyalists*	*Defectors*	*Recruits*	*L-D*
Loyalists > Defectors (L-D = plus)	Physician	1152	424	502	4.3	4.0	4.1	+.3
	Lawyer	569	300	477	4.2	4.1	4.1	+.1
Loyalists ≅ Defectors (L-D = zero)	Physicist	159	232	153	3.7	3.9	3.8	—.2
	Businessman	467	461	780	3.8	3.7	3.7	+.1
Defectors > Loyalists (L-D = minus)	Teacher	1184	632	886	3.1	3.3	3.4	—.2
	Engineer	1282	717	511	3.4	3.5	3.6	—.1
	Chemist	199	285	175	3.3	3.7	3.4	—.4
	Accountant	240	180	204	2.9	3.3	3.2	—.4

Note: The hypotheses were derived from Davis' principle that the characteristics distinguishing loyalists from defectors are those which were associated with that field among entering students. For example, those choosing "physician" initially were high on father's education, thus loyalists should be higher than defectors on FAED. Students reported their father's education which was coded: 1 = Grammar school, 2 = Some high school, 3 = H.S. grad., 4 = Some college, 5 = College degree, and 6 = Post-grad. degree.

Table 2. High School and College Grades of Loyalists, Defectors, and Recruits for Various Career Choices

Ability hypothesis	*Career choice*	*Mean high school grade average*				*Mean freshman grade average*			
		Loyalists	*Defectors*	*Recruits*	*L-D*	*Loyalists*	*Defectors*	*Recruits*	*L-D*
Loyalists > Defectors (L-D = plus)	Physicist	6.6	6.4	6.2	+.2	3.8	3.0	3.4	+.8
	Chemist	5.9	5.7	5.9	+.2	3.0	2.6	3.3	+.4
	Physician	5.8	5.3	5.3	+.5	3.2	2.6	3.1	+.6
	Engineer	5.6	5.3	5.4	+.3	2.9	2.5	2.7	+.4
	Lawyer	5.3	5.1	5.2	+.2	3.1	2.9	2.9	+.2
Defectors > Loyalists (L-D = minus)	Teacher	4.7	4.9	4.7	—.2	2.8	2.9	2.6	—.1
	Accountant	4.6	4.3	4.6	+.3	2.8	2.4	2.5	+.4
	Businessman	4.2	4.3	4.5	—.1	2.5	2.4	2.3	+.1

Note: The hypotheses were derived from Davis' principle that the characteristics distinguishing loyalists from defectors are those which were associated with that field among entering students. For example, those choosing "physicist" initially were high in ability, thus loyalists should be higher than defectors in ability. See Table 1 for sample sizes. Students reported their letter grade average in high school which was coded: 1 = D, 2 = C, 3 = C+, 4 = B—, 5 = B, 6 = B+, 7 = A—, 8 = A, and 9 = A+. College freshmen reported their letter grade average which was coded: 7 = A, 6 = A— or B+, 5 = B, 4 = B— or C+, 3 = C, 2 = C— or D+, and 1 = D or less.

were more able than defectors. The case was not so clear for careers that initially drew low ability students (teacher, accountant, and businessman); since, contrary to the second hypothesis, among accountants, the loyalists were more able than defectors on all three measures of ability. Among businessmen there was no difference (in college grades) between loyalists and defectors, though the hypothesis was supported using the other ability measures (HSG and NMSQT scores).

Table 3 shows the percentage of sons who

Table 3. Percentage of Sons Choosing Father's Occupation among Loyalists, Defectors, and Recruits

Career choice	*Loyalists*	*Defectors*	*Recruits*	*Chi-square** *test of L > D*
Engineer	6.2	5.5	5.7	not signif.
Lawyer	14.1	9.4	6.7	(p = .06)
Physician	14.8	10.6	8.7	(p = .07)
Farmer	49.2	26.9	33.0	(p = .05)
Businessman	50.8	37.0	30.1	(p = .05)

Note: The career sample sizes are given in Table 1, except for Farmer which had 126 loyalists, 104 defectors, and 109 recruits.

* p = level of significance.

chose their fathers' particular occupations for those career choices having at least 50 such sons. If these sons were more stable in their choices, the percentage of such sons among loyalists would be higher than the percentage among defectors. For the five career choices—engineer, lawyer, physician, farmer, and businessman—shown in Table 3, the percentage of such sons among loyalists was greater than among defectors. The percentage of sons choosing their fathers' occupations among recruits indicated that the students who originally chose their fathers' occupations but decided to change by the end of the freshman year were replaced by those who changed their career choices to match father's occupation.

In general, the results confirmed Davis' finding that "deviant" students tend to switch their preferences to career choices more compatible with their personal characteristics. Both Davis and the present study find this principle valid for persons "deviant" on social class and on academic ability, even though the studies differ on: samples (recent college graduates vs. freshmen), period of time studied (changes from entrance to graduation vs. changes in freshman year), type of data (retrospective vs. prospective), particular indicators used (college grades adjusted for college vs. high school grades, unadjusted college grades and test scores), and career categories (self-coded vs. free response data).

In an earlier paper using the same sample, the marked tendency of sons to choose their fathers' occupations was noted.[8] The data in Table 3 indicate that such sons were somewhat less likely than others in a particular career to change their career plans. In a 14 year follow-up of a sample of Harvard students, McArthur and Stevens[9] also found that children who chose their fathers' vocations (or acceptable equivalents) were less likely to have changed from their initial expressed career interests.

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[8] Charles E. Werts, "Social Class and Initial Career Choice of College Freshmen."

[9] Charles McArthur and Lucia B. Stevens, "The Validation of Expressed Interests as Compared with Inventoried Interests: A Fourteen-Year Follow-Up," *Journal of Applied Psychology*, 39 (1955), pp. 184–189.

C. Scientists as Perceived Role Models: Precipitants of Choice

Introduction

Both perceptions of young people regarding the traits of scientists and the public's stereotypes of the "typical scientist" appear to be closely related to the problem of recruiting young people into science. Imagery about scientists is important for a number of reasons. First, the image delineates the student's belief about the scientist's personality and lifestyle. It suggests that the potential recruit must, if he elects to become a scientist, have certain types of personality characteristics and live a certain type of life. If the features of the scientist's personality and life do not mesh with the student's interests, beliefs, and values he is unlikely to commit himself to being a scientist. At the same time, stereotyped beliefs may lead the student who has chosen science to develop characteristics similar to the commonly accepted stereotype of the scientist. The perceptions of the scientist encourage recruitment of persons whose self-perceptions agree with their perceptions of the scientist's characteristics, thus limiting the number of potential recruits to science.

Evidence suggests (Mead and Métraux; Krippner) that young people and their parents may have generally unfavorable stereotypes of scientists and of science as a profession. The monetary and status rewards of science are not as high as such rewards of other occupations, e.g., law, medicine, and engineering, that also require advanced training. Therefore, young people may be loathe to become scientists unless they themselves have certain cognitive or personality characteristics that lead them to value and place emphasis upon the intrinsic rewards of being a scientist.

To determine how perceptions of the scientist are influencing entry into the scientific professions, it is first necessary to examine components of the image of the scientist. The first article in this series (Mead and Métraux) found that high school students have a generally unfavorable stereotype of the scientist. High school boys said they do not want to become scientists themselves and high school girls did not want to marry scientists. They seemed to sense a wide gap between the scientist's knowledge, values, and behavior and their own knowledge, values, and behavior. Mead and Métraux also found that the scientists' personalities became depicted in highly patterned and stereotyped ways by the students.

Beardslee and O'Dowd, Bendig and Hountras, Braun and others (e.g., Remmers and Radler, 1957; Holland, in Section III B) have used different data-collection methods to examine stereotyped images among college students. Rating scales, sentence completion, and forced-choice questions in which students had to answer questions like a typical scientist were employed. Yet the same stereotyped images about scientists were found. On the positive side, the similar results were that the scientist was seen generally as being high in intelligence, rationality, precision, objectivity, and orderliness and was seen as having a driving concern to extend knowledge and to discover truth. On the negative side, the scientist was seen as being disinterested in people, introverted, and unsuccessful in interpersonal relations. He was less charming, friendly, and self-confident in interpersonal relationships than men in other professional groups. He was also radical, unconventional, and "strange" or "odd" in some way. The image is a stable one shared by students of different types of colleges, sexes, classes, and socioeconomic background.

Data are also available on differences perceived between scientists and members of other specific occupational groups. The scientist is most similar first to the college professor and next to the engineer (Beardslee and O'Dowd). However, the scientist is seen as being less artistic, less interested in people, more withdrawn, and less popular than the professor and more intellectual, less conformist, more persevering, less sociable, and less wealthy than the engineer. Lawyers were rated slightly higher than scientists in number of socially desirable traits while engineers and businessmen were rated lower (Bendig and Hountras). Although the stereotype of the scientist may not be overwhelmingly favorable, it is more favorable than that of the engineer and businessman (Bendig and Hountras). The scientist and lawyer were viewed as having more intellectual traits than the other groups while the scientist and engineer had fewer interpersonal traits than the other groups.

The final selection is of interest because it attempts to relate perceptions of the scientist to interest and achievement in science, i.e., to future vocational choice. Krippner determined that junior high school pupils had less favorable occupational stereotypes regarding scientists than they did regarding most other professional groups. Few parents were reported to have advised their children to consider scientific careers, even when the children exhibited high academic ability. However, significantly more high-scholastic achievement boys and high-ability boys in an upper middle class school preferred scientific careers than did lower-ability and -achievement boys. It has been hypothesized (Krippner; Simpson and Simpson, 1960) that students who had high-achievement scores indicating success in academic areas might be more attracted to science or other professions on the basis of interest in the intrinsic nature of the work rather than on the basis of status and salary considerations, while low-achieving students were influenced by the intrinsic rewards of the job. Along these lines Currie et al. (1966) found that all common elements in the image of the college professor (e.g., intellectualism, esthetic sophistication, rejection of conventional values, and academic involvement) were predictive of college students' interest in college teaching, i.e., those students who wanted to become professors possessed the characteristics both they and other students perceived college professors to have. Both the Krippner and Currie studies imply that persons who go into science possess certain cognitive and personality characteristics that correspond to their image of the scientist, with their perceptions of the scientist similar to, but perhaps more favorable than, the average student's image.

The stereotyped traits of the students in some ways correspond to the "true" personality traits of the scientist as indicated on objective or projective personality questionnaires. Students see scientists as very intelligent, hard working, dedicated toward the search for truth, precise, logical, yet unconventional and, indeed, scientists as a group seem to actually possess a greater degree of these characteristics than the general adult male population. The students also perceive scientists as being introverted, lacking in interpersonal relationships, and less charming and friendly than persons in other occupations. While slight evidence exists to show scientists as introverted and as having so great an emotional investment in work that other relationships suffer in comparison, these traits cannot yet be called characteristics of scientists. Scientists are generally fairly well adjusted (Hall, 1962; Eiduson, 1962) rather than "strange" or "odd," as perceived by the students. The characteristics which scientists actually possess, may be distorted into pathological tendencies within the stereotyped perceptions of the students. While the students' perceptions contain a grain of truth and sometimes are closely aligned with the actual traits, they are, like most stereotypes, oversimplified and exaggerated. Thus, they are more, rather than less, consistent and homogeneous than true traits.

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Image of the Scientist among High-School Students
A Pilot Study

Margaret Mead and Rhoda Métraux

This study is based on an analysis of a nation-wide sample of essays written by high-school students in response to uncompleted questions. The following explanation was read to all students by each administrator. "The American Association for the Advancement of Science, a national organization of scientists having over 50,000 members, is interested in finding out confidentially what you think about science and scientists. Therefore, you are asked to write in your own words a statement which tells what you think. What you write is confidential. You are not to sign your name to it. When you have written your statement you are to seal it in an envelope and write the name of school on the envelope. This is not a test in which any one of you will be compared with any other student, either at this school, or at another school. Students at more than 120 schools in the United States are also completing the statement and your answer and theirs will be considered together to really find out what all high-school students think as a group of people."

In general, the study shows that, while an official image of the scientist—that is, an image that is the correct answer to give when the student is asked to speak without personal career involvement—has been built up which is very positive, this is not so when the student's personal choices are involved. Science in general is represented as a good thing: without science we would still be living in caves; science is responsible for progress, is necessary for the defense of the country, is responsible for preserving more lives and for improving the health and comfort of the population. However, when the question becomes one of personal contact with science, as a career choice or involving the choice of a husband, the image is overwhelmingly negative.

This is not a study of what proportion of high-school students are choosing, or will eventually choose, a scientific career. It is a study of the state of mind of the students among whom the occasional future scientist must go to school and of the atmosphere within which the science teacher must teach. It gives us a basis for reexamining the way in which science and the life of the scientist are being presented in the United States today.

OBJECTIVES

Our specific objectives in this study were to learn the following.

1) When American secondary-school students are asked to discuss scientists in general, without specific reference to their own career choices or, among girls, to the career choices of their future husbands, what comes to their minds and how are their ideas expressed in images?

2) When American secondary-school students are asked to think of themselves as becoming scientists (boys and girls) or as married to a scientist (girls), what comes to their minds and how are their ideas expressed in images?

3) When the scientist is considered as a general figure and/or as someone the respondent (that is, the student writer) might like to be (or to marry), or, alternatively, might not like to be (or to marry), how do (i) the positive responses (that is, items or phrases, not answers) cluster, and (ii) the negative responses (that is, items or phrases) cluster?

4) When clusters of positive responses and clusters of negative responses are compared and analyzed, in what respects are the two types of clusters of responses (i) clearly distinguishable, and (ii) overlapping?

5) Is a generally positive attitude to the idea of science, an attitude which we are spending a great deal of money and effort to create, any guarantee of a positive attitude to the idea of science as a career?

SELECTION OF RESPONDENTS

Two separate samples of respondents were used in the study: sample A, a nation-wide

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sample of high schools, and sample B, a sample of high schools with widely different economic and educational characteristics.

Sample A consisted of 132 public high schools (including one junior high school) that were selected from schools associated with the Traveling High-School Science Library Program sponsored by the National Science Foundation and administered by the American Association for the Advancement of Science. Of these, 118 were drawn from the high schools that participated in this program and an additional 14 from schools that qualified for the program but could not be included in it.

Sample B consisted of 13 special schools: four parochial schools, eight preparatory schools, and one public science high school. All these were from the eastern seaboard, selected to provide contrasts in educational and economic level to the smaller public high schools in the nation-wide sample (sample A). Sample B was collected after the homogeneity of the nation-wide sample had been ascertained.

The total enrollment of the schools participating in the study was 48,000. Schools with an enrollment of less than 300 students were asked to have each student complete one form; schools with an enrollment of more than 300 students were asked to complete 300 forms. The total sample (sample A and sample B) is drawn from the essays written by approximately 35,000 students, and the essays were kept together by the class, grade, and school from which the essays came.

The sample was randomized by drawing envelopes of these replies in groups that included three schools in one state, or three tenth grades, or all the separate classes in three schools, so that no essay was ever separated from the context in which it had been written.

DATA-GATHERING INSTRUMENTS

We asked each high-school student respondent to write a brief essay on a topic set by an incomplete sentence which was printed at the top of a page, on which provision was also made for giving the school, the grade, the class or section, the age and sex of the respondent.

Three different forms were constructed, each with a different incomplete sentence. Each of these three sentences was chosen to elicit one major aspect of the image of the scientist.

Only one form was used in any one school, but the forms were so distributed that each form was used by at least one school in each state. These three forms are as follows.

Form I

Complete the following statement in your own words. Write at least a full paragraph, but do not write more than a page.

When I think about a scientist, I think of

Form II

If you are a *boy*, complete the following statement in your own words.

If I were going to be a scientist, I should like to be the kind of scientist who

If you are a *girl*, you may complete either the sentence above or this one.

If I were going to marry a scientist, I should like to marry the kind of scientist who

Form III

If you are a *boy*, complete the following statement in your own words.

If I were going to be a scientist, I would not *like to be the kind of scientist who*

If you are a *girl*, you may complete either the sentence above or this one.

If I were going to marry a scientist, I would not *like to marry the kind of scientist who*

. . . The identification of the attitude pattern in any large sample of essays and of the cognitive and emotional processes which underlie the attitudes reported by individuals is best accomplished by trained behavioral scientists. Because any one analyst, no matter how well trained, may have some blind spots and biases, and because analysts differ in their types of disciplined perception, we had six different analysts work independently with six subsamples of the total sample drawn from different states. Because one kind of material may be more useful than another in outlining a given area, we used—in addition to the essay samples from the 35,000 students—a variety of other kinds of materials as well.

We are assured that we have identified important themes in the material by the multiplicity of independent analyses and by the use of a variety of data. We are assured of the validity of our conclusions by a comparison of the independent work of the analysts and by the agreement on materials from different parts of the country. . . .

THE COMPOSITE IMAGE

In reading the following composite statements, it is important to realize that they do not represent literary descriptions written by the analyst but rather composites of the responses made by the students in their essays, so that each "composite image" is to be understood as being something like a composite photograph which emerges from a very large number of superimposed photographs. Each phrase (response) both stands for a family of phrases (responses) which were used throughout the essays and is itself a recurrently used phrase (response). The phrases have been grouped in relation to themes, as they occur in the essays, but reference to the themes might occur in any order in the essays. It is important to realize that in organizing for presentation here the positive and the negative versions of the composite image of the scientist, the analyst has separated out from the answers the postive phrases (responses), on the one hand, and the negative phrases (responses), on the other hand, as an analytic device, whereas in the essays both occur—or may occur—together in a variety of combinations.

Before the image of the scientist is discussed, it will be useful to look at the way "science" appears in these essays. In the following composite statements, italics indicate the words and phrases (responses) used; detailed examples are given in parentheses, and explanatory notes in square brackets.

Science. Science is a very broad field which may be seen as a single unit (*science is very important*, or *I am not interested in science*), as a melange (*medicine and gas and electric appliances*), or as composed of entities (*biology* and *physics* and *chemistry* . . .) linked together by the personality of the *scientist*.

Science is natural science with little direct reference to man as a social being except as the products of science—*medicine* and *bombs*—affect his life. The subjects of science are *chemistry* and *physics* (*laboratories, test tubes, bunsen burners, experiments* and *explosions, atomic energy, laws* and *formulas* . . .), *biology-botany-zoology* (*plants* and *animals* [that is, as materials for laboratory work], *microscopes, dissection, the digestive system, creepy* and *crawly things* . . .), *astronomy* (*the moon, stars, planets, the solar system, outer space, astronomers, astrologers* [*sic*], *telescopes, space ships* . . .), *geology* (*the earth, rocks, mines* and *oil wells, out of doors* . . .), *medicine* (*cures for TB, cancer, heart disease,* and *polio, research, serums* . . .); *archeology* (*exploration, ancient cities, early man, fossils, digging* . . .). *Mathematics* is not a science but a tool and a measure of scientific aptitude.

The methods of science are *research* and *experimentation, invention, discovery, exploration, finding out new things* and *new ways of improving old ones*. Science means *doing* and *making: hard work*—not imagination—is the source of knowledge and the means of accomplishment.

The focus of science is upon the present. The past is important only as it is left behind (*without science we would still be living in caves*) and the future as a foreseeable goal (when we *find a cure for heart disease, see if there is life on Mars, discover new fuels* . . .). But as the past closes in behind us, the future opens to the curious (*there is still so much to discover*) into the yet unknown.

In thinking about science, different sorts of linked images occur which may be bracketed together when science is rejected or may be included when positive preference is expressed for one of a pair. So, science may be *theoretical* or *applied*, and either of this pair can be seen as more of a whole and be accepted (that is, the man in the laboratory is visualized as working through the whole problem; or the engineer can see the finished road), while the other is seen as partial and is rejected (that is, the engineer is visualized as working only on the end-product; or the man in the laboratory never sees the plan carried out). Likewise, science can be carried out *in the laboratory* or in a *far away place;* it may involve large-scale action (*traveling, digging, exploring, constructing, flying through space* . . .) or the skills of fine detail (*gazing through a telescope, poring over a microscope, dissecting, solving equations* . . .). The goals of science may be humanitarian (*working to better mankind, finding cures, making new products,* developing *programs for atoms for peace . . .*), or, in contrast, they may be either individualistic (*making money, gaining fame and glory . . .*) or destructive (*dissecting, destroying enemies, making explosives* that *threaten* the *home,* the *country,* or *all mankind . . .*).

Since, by implication, science is the source of unlimited power, its practitioners should have the highest and the most selfless motivations to use only its constructive possibilities—or its destructive possibilities only constructively—for the *welfare of their country* and the *betterment of people, the world,* and *all mankind.*

The Scientist, the Shared Image. The scientist is a man who wears a white coat and works in a laboratory. He is elderly or *middle aged and wears glasses. He is small,* sometimes *small and stout,* or *tall and thin. He* may be *bald. He* may *wear a beard,* may be *unshaven* and *unkempt. He* may be *stooped and tired.*

He is surrounded by equipment: test tubes, bunsen burners, flasks and bottles, a jungle gym of blown glass tubes and weird machines with dials. The sparkling white laboratory is full of sounds: the bubbling of liquids in test tubes and flasks, the squeaks and squeals of laboratory animals, the muttering voice of the scientist.

He spends his days doing experiments. He pours chemicals from one test tube into another. He peers raptly through microscopes. He scans the heavens through a telescope [or a *microscope!*]. *He experiments with plants and animals, cutting them apart, injecting serum into animals. He writes neatly in black notebooks.*

The image then diverges.

Positive Side of the Image of the Scientist. He is a very intelligent man—a genius or *almost a genius. He has long years of expensive training—in high school, college,* or *technical school,* or perhaps even beyond—during which *he studied very hard. He is interested in his work and takes it seriously. He is careful, patient, devoted, courageous, open minded. He knows his subject. He records his experiments carefully, does not jump to conclusions, and stands up for his ideas even when attacked. He works for long hours in the laboratory, sometimes day and night, going without food and sleep. He is prepared to work for years without getting results and face the possibility of failure without discouragement; he will try again. He wants to know the answer. One day he may straighten up and shout: "I've found it! I've found it!"*

He is a dedicated man who works not for money or fame or self-glory, but—like Madam Curie, Einstein, Oppenheimer, Salk—for the benefit of mankind and the welfare of his country. Through his work people will be healthier and live longer, they will have new and better products to make life easier and pleasanter at home, and our country will be protected from enemies abroad. He will soon make possible travel to outer space.

The scientist is a truly wonderful man. Where would we be without him? The future rests on his shoulders.

Negative Side of the Image of the Scientist. The scientist is a brain. He spends his days indoors, sitting in a laboratory, pouring things from one test tube into another. His work is uninteresting, dull, monotonous, tedious, time consuming, and, though he works for years, he may see no results or may fail, and he is likely to receive neither adequate recompense nor recognition. He may live in a cold-water flat; his laboratory may be dingy.

If he works by himself, he is alone and has heavy expenses. If he works for a big company, he has to do as he is told, and his discoveries must be turned over to the company and may not be used; he is just a cog in a machine. If he works for the government, he has to keep dangerous secrets; he is endangered by what he does and by constant surveillance and by continual investigations. If he loses touch with people, he may lose the public's confidence—as did Oppenheimer. If he works for money or self-glory he may take credit for the work of others—as some tried to do to Salk. He may even sell secrets to the enemy.

His work may be dangerous. Chemicals may explode. He may be hurt by radiation, or may die. If he does medical research, he may bring home disease, or *may use himself as a guinea pig,* or *may even accidentally kill someone.*

He may not believe in God or *may lose his religion. His belief that man is descended from animals is disgusting.*

He is a brain; he is so involved in his work that he doesn't know what is going on in the world. He has no other interests and neglects his body for his mind. He can only talk, eat, breathe, and sleep science.

He neglects his family—pays no attention to his wife, never plays with his children. He has no social life, no other intellectual interest, no hobbies or relaxations. He bores his wife, his children and their friends—for he has no friends of his own or *knows only other scientists—with incessant talk that no one can understand;* or else *he pays no attention* or *has secrets he cannot share. He is never home. He is always reading a book. He brings home work and also bugs and creepy things. He is always running off to his laboratory. He may force his children to become scientists also.*

A scientist should not marry. No one wants to be such a scientist or *to marry him.*

DISCUSSION

The "official" image of the scientist—the answer which will be given without personal involvement—which was evoked primarily in form I, but which recurs in the answers to all three forms, is a positive one.

The scientist is seen as being essential to our national life and to the world; he is a great, brilliant, dedicated human being, with powers far beyond those of ordinary men, whose patient researches without regard to money or fame lead to medical cures, provide for technical progress, and protect us from attack. We need him and we should be grateful for him.

Thus if no more than form I had been asked, it would have been possible to say that the attitude of American high-school students to science is all that might be desired.

But this image in all its aspects, the shared, the positive, and the negative, is one which is likely to invoke a negative attitude as far as personal career or marriage choice is concerned. While the rejection in the negative image is, of course, immediately clear, the positive image of very hard, only occasionally rewarding, very responsible work is also one which, while it is respected, has very little attraction for young Americans today. They do not wish to commit themselves to long-time perspectives, to dedication, to single absorbing purposes, to an abnormal relationship to money, or to the risks of great responsibility. These requirements are seen as far too exacting. The present trend is toward earlier marriage, early parenthood, early enjoyment of an adult form of life, with the career choice of the man and the job choice of the woman, if any, subordinated to the main values of life—good human relations, expressed primarily in terms of the family and of being and associating with the kind of human being who easily relates to other people.

To the extent that any career—that of diplomat, lawyer, businessman, artist, aviator —is seen as antithetical to this contemporary set of values, it will repel male students as a career choice and girls as a career for their future husbands. But it is important to see also the particular ways in which the image of a scientific career conflicts with contemporary values. It divides girls and boys. The boys, when they react positively, include motives which do not appeal to the girls—adventure, space travel, delight in speed and propulsion; the girls, when they react positively, emphasize humanitarianism and self-sacrifice for humanity, which do not appeal to the boys. The girls reject science, both as a possible form of work for themselves, concerned with things rather than with people, with nonliving things (laboratory animals, not live animals; parts of anatomy, not living children), and for their husbands, because it will separate them, give their husbands absorbing interests which they do not share, and involve them in various kinds of danger. In earlier periods, when career choices and marriages occurred later, the girls' attitudes might not have mattered so much; they are very important today, on the one hand, because girls represent a principal untapped source of technical skill, and, on the other hand, because, with present adolescent social patterns, paired boys and girls spend a great deal of time discussing the style of their impending marriage and parenthood and the relationship of the boy's career choice to the kind of home they will have.

The image of the scientist's relationship to money also presents a problem, in a period of full employment, to young people who think that an adequate income is something that should be taken for granted. The scientist is seen as having an abnormal relationship to money. He is seen either as in danger of yielding to the temptation of "money and fame," or as starving and poor because of his integrity. The number of ways in which the image of the scientist contains extremes which appear to be contradictory—too much contact with money or too little; being bald or bearded; confined work indoors, or traveling far away; talking all the time in a boring way, or never talking at all—all represent deviations from the accepted way of life, from being a normal friendly human being, who lives like other people and gets along with other people.

SPECIFIC INDICATIONS ABOUT THE TEACHING OF SCIENCE

From the standpoint of teaching, it is important to realize how the present image of scientific work lacks any sense of the delights of intellectual activity; the scientist works patiently and carefully for years, and only when he finds out something does he shout with joy. This lack of any sense that intellectual activity is rewarding in itself can be related to the lack of any mention of living things, plant, animal, or human, in the materials with which the scientist is believed to work. Plants and animals appear only as dead objects for dissection; the human body, as organs or systems studied in the laboratory and treated in medicine; whole human beings appear only as the dead denizens of dead and buried cities, and most of the scientists about whom they read are also dead. The lack of any sense of enjoyment can also be related to the central role given to mathematics as a tool, without any emphasis on the delights of observation, as in early natural history studies or in the perception of regularities and connections in the world around them, or between themselves and the world around them.

Because the materials were analyzed class by class and school by school, the study has also yielded, as a by-product, certain sidelights on science teaching: on the importance of participation as opposed to passive watching, on the role which the personality of the teacher plays in attitudes toward science, on the effect on the rest of the class of the presence in it of one type of exceptionally gifted child.

One of the most recurrent responses is an expression of active boredom, the phrase, "I am not interested in science," or in a particular science course (chemistry or physics), followed occasionally by highly emotional expressions of fury and hatred of particular activities which are being demonstrated. "Interest" and "active enjoyment" seem to be so closely related that the student seated in a classroom who has to watch things being poured from one test tube to another or listen to a string of unrelated facts becomes permanently alienated. General science courses seem to be the ones in which this attitude toward science is characteristically invoked, except when a gifted teacher gives it some special emphasis. When mathematics is seen as the key ability on which all future scientific work is based, not liking and not being able to do mathematics become a specially weak point in the circle of the students' interests.

In contrast, other activities are defined as nonscientific because they are absorbingly interesting: "watching things grow that I have planted," or "working on my hot rod car."

The role of the teacher—as reflected in the comments of a whole class—is an exceedingly interesting one. The disliked teacher is personalized and vivid, but the teacher who has obviously been very successful and has caught the imagination and enthusiasm of the whole class does not emerge as a person at all but, instead, sinks into the background of good classroom conditions, together with "good laboratory equipment." Special aspects of the disliked teacher are commented on in detail. He may be described as an outsider, a stranger, with unusual habits of dress and manner, who does not know his subject well, who cannot talk about anything but his subject, who lives alone without the slightest tie to the community, who is "stuck-up and who is too busy for anyone but himself." It is easy to see how the only male teacher in the school presents special problems to the boys, if he himself is a figure they reject, and how easily the sphere of work for which he stands may be rejected also. So one boy writes, "Anyone who digs our teacher's gab is a square as well as being queer." Some of these consequences undoubtedly flow from the convention in the United States that, ideally, science should be taught by men, with the result that men who might be more successful teachers in some other field are forced into teaching a subject which they dislike and in which they have no special competence. Similarly, foreigners and refugees—if male—may have a better chance to get positions as mathematics and science teachers than they have in other fields.

The significance of the lack of particular mention of the good science teacher is equally important, for it is related to the lack of invocation of authority by the students, who state their opinions about science—even those obviously related to a particular teacher—as their own. Only when they disagree, when they wish to attack the current image of science as a good thing from a minority position —that is, from the viewpoint of some fundamentalist religious position which they accept —do they invoke authority. It is related also to the situation in American culture where, through generations, there has been a break between immigrant parent and native-born child. In this new setting, the European tendency for children to identify with the personality and occupation of the parents has been replaced by a tendency to follow the style set by members of one's own generation, especially those in one's own local school clique.

In the classroom, a disliked fellow-student who is regarded as a future scientist may also be described in some detail, as students say they do not want to be the kind of scientists who "go about with their noses in a book, looking superior." But in those classrooms where everyone has been committed to the joy of some experiment or project, no individuals emerge: it is impossible to say what is the sex, age, nationality, and personality of the teacher.

In summary, it may be said that where science teaching is successful, the teacher has created a situation in which his or her (one does not know which) personality sinks into the background, and in which no one student stands out as so especially gifted and preoccupied as to rouse annoyance in the class. Students and teacher appear to have worked as a group, accepting science as a part of *their* lives, preoccupied with no specific identified individuals.

RECOMMENDATIONS

Mass media. Straight across the country there is a reflection of the mass media image of the scientist, which shares with the school materials the responsibility for the present image. Alterations in the mass media can have important consequences in correcting the present distorted image if such changes are related to real conditions. Attempts to alter the image, in which the public relations department of a particular company represents its research personnel with crew cuts and five children may improve the recruitment program of single companies, but do so only at the expense of intensifying the negative aspects of the image for the country as a whole.

What is needed in the mass media is more emphasis on the real, human rewards of science—on the way in which scientists today work in groups, share common problems, and are neither "cogs in a machine" nor "lonely" and "isolated." Pictures of scientific activities of groups, working together, drawing in people of different nations, of both sexes and all ages, people who take delight in their work, could do a great deal of good.

The mass media could also help to break down the sense of discontinuity between *the scientist* and other men, by showing science as a field of endeavor in which many skills, applied and pure, skills of observation and of patient, exact tabulation, flashes of insight, delight in the pure detail of handling a substance or a material, skills in orchestrating many talents and temperaments, are all important. This would help to bring about an understanding of science as a part of life, not divorced from it, a vineyard in which there is a place for many kinds of workers.

The schools. The material suggests the following changes which might be introduced in educational planning.

1) Encourage more participation and less passive watching in the classroom, less repeating of experiments the answers to which are known; give more chance to the students to feel that they are doing it themselves. A decrease in the passive type of experience found in many general science courses seems particularly necessary.

2) Begin in the kindergarten and elementary grades to open children's eyes to the wonder and delight in the natural world, which can then supply the motive power for enjoyment of intellectual life later. This would also establish the idea of science as concerned with

living things and with immediate—as contrasted with distant—human values.

3) Teach mathematical principles much earlier, and throughout the teaching of mathematics emphasize nonverbal awareness: let children have an opportunity to rediscover mathematical principles for themselves.

4) Emphasize group projects; let the students have an opportunity to see science as team work, where minds and skills of different sorts complement one another.

5) Emphasize the need for the teacher who enjoys and is proficient in science subjects, irrespective of that teacher's sex; this would mean that good women teachers could be enlisted instead of depending on men, irrespective of their proficiency. Since it would seem that the boys do not need to identify with an adult male as a teacher, this should leave us free to draw on women as a source of science teachers.

6) Change the teaching and counseling emphasis in schools which now discourages girls who are interested in science. This would have many diffuse effects: on the supply of women teachers and of women in engineering, on the attitudes of girls who are helping boys to choose careers, and on the attitudes of mothers who are educating their small children in ways which may make or mar their ability to deal with the world in scientific terms.

7) Deemphasize individual representatives of science, both outstanding individuals like Einstein—whose uniqueness simply convinces most students that they can never be scientists—and the occasional genius-type child in a class. (This type of child, who represents only one kind of future scientist and who is often in very special need of protection from the brutalities of his age mates, should probably be taken out of small, low-level schools, and placed in a more protected and intellectual environment.) Instead, emphasize the sciences as fields, and the history of science as a great adventure of mankind as a whole. (The monotonously recurrent statement "if it weren't for scientists we would still be living in caves" is an insult to the memory of millions of anonymous men who have—each in his way—made further advances possible.)

8) Avoid talking about *the scientist, science,* and *the scientific method.* Use instead the names of the sciences—biology, physics, physiology, psychology—and speak of what a biologist or a physicist does and what the many different methods of science are—observation, measurement, hypotheses-generating, hypotheses-testing, experiment.

9) Emphasize the life sciences and living things—not just laboratory animals, but also plants and animals in nature—and living human beings, contemporary peoples, living children—not the bones and dust of dead cities and records in crumbling manuscripts. Living things give an opportunity for wonder and humility, necessarily less present in the laboratory where students deal with the inanimate and the known, and contact with living things counteracts the troubling implication that the scientist is all powerful.

CONCLUSION

This report is not in any way a statement of the proportion of high-school students who will choose science as a career. It is a discussion of the state of mind of fellow-students, among whom the occasional future scientist must go to school, of the degree of personal motivation necessary to commit oneself to science, and of the atmosphere within which the science teacher must teach. Since most high-school students' attitudes closely reflect those of their parents, it is also an indication of the climate of opinion in which parents may be expected to back up their children in choosing science as a career, citizens may be expected to vote funds for new laboratories, and voters may be expected to judge Congressional appropriations for science education.

REFERENCES AND NOTES

1. When evolution is mentioned, it is mentioned negatively. It is impossible to tell what the absence of other than negative references to evolution means. In Remmers' study [H. H. Remmers and D. H. Radler, *The American Teenager* (Bobbs-Merrill, Indianapolis, Ind., 1957), p. 171] 40 percent of the teenagers checked "No," to the statement "Man has evolved from lower forms of animals"; another 24 percent checked "Don't know"; 35 percent checked "Yes."

The College-Student Image of the Scientist

David C. Beardslee and Donald D. O'Dowd

The image of the scientist among high school students has been studied in detail in recent years. Remmers and Radler (1) have reported on some beliefs of teen-agers about scientists, and Mead and Metraux (2) have summarized the image of the scientist revealed in essays produced by a large sample of high school students.

The beliefs of college students about the scientist are also of interest. Many students entering college seriously consider careers in science, and college students will eventually constitute an influential segment of the citizens whose views make up the public response to science.

Exploration of the college-student image of the scientist was initiated in a series of unstructured interviews with college undergraduates at Wesleyan University. In these interviews, students described the scientist as being dedicated to his work and carrying it out with heroic devotion at the expense of concern with public affairs and even family responsibilities. The scientist was described as unsociable, introverted, and possessing few, if any, friends. Some students referred spontaneously to his high intelligence: others were more impressed by the precision of his thinking and the objectivity (that is, lack of emotional involvement) with which he handles most personal and professional problems. Two telling comments represent the common response of undergraduate men to the scientist. One student volunteered, "I wouldn't care to double-date with a scientist," and another student commented, "maybe it's not a good idea for him [the scientist] to be married." A number of students were impressed by the scientist's apparent need to proceed in his work regardless of other demands on his time. In general, the college students revealed in these interviews beliefs similar to those found among high school students. The scientist, to use the student phrase, "is not well rounded."

In order to pursue further the subject of the student image of the scientist, a series of three successive questionnaires was designed and used in testing. A fourth version of the questionnaire was selected as the main instrument for an empirical study. It contained materials derived from the interviews and from standard questionnaires and scales developed in the earlier versions. In the questionnaire students were asked to indicate the appropriateness of a series of terms to each of 15 occupations, including that of scientist. The terms were arranged in two-ended, seven-point rating scales of the following form (3):

1. wealthy —:—:—:—:—:—:— not well-to-do
2. optimistic —:—:—:—:—:—:— pessimistic
3. excitable —:—:—:—:—:—:— calm

This design makes it possible not only to determine absolute values for characteristics attributed to the scientist but also to obtain an estimate of the standing of the scientist relative to individuals in other occupations.

The questionnaire was given to undergraduate men and women in four colleges in the northeastern United States: Wesleyan University, a second small and highly selective men's liberal arts college, a highly selective private women's college, and the college of arts and sciences of a state university. At each college, probability samples of freshmen and seniors were chosen. Over 90 percent of the students selected at three of the four colleges returned completed questionnaires. At the second men's liberal arts college, all of the freshmen but only two-thirds of the seniors completed the questionnaire. Data from these seniors were not used in many of the following analyses. In all, about 1,200 students were included in this phase of the study (4).

IMAGE OF THE SCIENTIST

It is possible to present a composite picture of the scientist from the responses obtained. Students from all of the colleges, both men and women, freshmen and seniors, were

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in sufficient agreement to justify a summary of the characteristics attributed to the scientist by all groups. There is clearly a well-defined stereotype of the scientist among college students as well as among high school students. In the following summary, the rating of the scientist relative to individuals in other occupations is considered.

The scientist, according to college students, is outstanding in several respects. Students see him most prominently as a highly intelligent person with a strong tendency to be both individualistic and radical in personal and social outlook. At the same time, the scientist is seen as socially withdrawn; he is indifferent to people, retiring, and somewhat depressed, and he rates low in social popularity. In overall sociability the scientist rates lowest among individuals in the 15 high-level occupations. It is therefore not surprising that he is believed to have a relatively unhappy home life and a wife who is not pretty. There is an air of strangeness about him; he is hard to like and comprehend. He is respected for his great contribution to society, but he is not the kind of person one can easily get to know.

The scientist is believed to be highly intelligent but not interested in art. He is both self-sufficient and persevering. He focuses his powers in a rational and sensitive pursuit of answers to nature's mysteries. He is rated as reasonably successful and as having ample opportunity to advance in his field. At the same time he is seen as having only modest wealth. It appears that the scientist could exploit his situation to secure wealth and status, but he is so devoted to his work that he is satisfied with a modest income.

The scientist is moderately confident, optimistic, and realistic in his approach to life. He has power in public affairs yet is given only a moderately high score on responsibility. When combined with his radicalism, this finding suggests that there are grounds for an anxious public to become suspicious of his loyalty. After all, he has few friends, great determination, and an unusual set of values.

Rather surprisingly, the scientist is scored relatively low on stability, caution, and calmness. It appears that he has difficulty controlling his impulses. This is consistent with the picture of his radicalism. He is coldly intellectual in some spheres of his life—mainly in his work—and he is emotional in his response to social and political appeals.

The complexity of the scientist's nature must account for his being considered mildly interesting and colorful. He is thought to be very valuable to society and to derive very great personal satisfaction from his work. If one were to study his recreational habits one would find him most frequently at chess, rarely playing bridge, and never playing poker.

In summary, there emerges a picture of the scientist as a highly intelligent individual devoted to his studies and research at the expense of interest in art, friends, and even family. The scientist derives great personal satisfaction, a sense of success, reasonably high status in the community, and a modest income from his work. He serves mankind in a selfless way, almost unaware that he is doing so; he serves others by serving himself.

In public matters the scientist is influential, but he may be somewhat naive. He is extreme in his views on social matters, and he tends to become emotionally involved with issues outside his realm of professional competence. The scientist is coldly intellectual in his professional area but excitable in the public political sphere. He is clearly an intellectual, but unlike "eggheads" in the humanities, he is characterized by a vigorous and directed use of his intelligence. The image conveys a sense of strength of personality, but it is a little extreme, a little strange, somewhat contradictory, and, therefore, hard to comprehend.

COMPARISON WITH IMAGES OF THE NONSCIENTIST

An estimate of the similarity of the scientist image with the images of individuals in 14 other occupations was obtained by correlating the mean scores obtained on 48 scales for the scientist and for people in these other occupations. The data from a subsample of the students tested were used to obtain the correlations presented in Table 1.

These data reveal that the scientist is believed to have much in common with the college professor. The similarity of ratings for the scientist and engineer was predictable, but the correlation with ratings for artist and school teacher had not been clearly foreseen. This correlation stems primarily from the students' grouping of all these roles as intellectual roles. It is clear that the students believe that scientists do not share many attributes with individuals in any of the business and industrial occupations.

Table 1. Correlation of the Profile of the Scientist with Profiles of Individuals in Other Occupations

Occupation	*Correlation*
College professor	+.77
Engineer	+.53
Artist	+.51
School teacher	+.49
Doctor	+.44
Lawyer	+.41
Social worker	+.30
Accountant	—.03
Business executive	—.03
Industrial manager	—.03
Personnel director	—.18
Sales manager	—.25
Office supervisor	—.29
Retail store manager	—.29

Comparison of the image of the scientist with that of the college professor reveals some interesting differences between these roles that are often filled by the same person. Both occupations are entered by men of high intelligence with personality characteristics represented by high scores on *self-sufficient* and *persevering*, middle values on *strong, active, confident*, and *self-assertive*, and low scores on *stable* and *adaptable in habits*. Both professions are believed to attract men who are, to a high degree, radical and individualistic. Members of the two professions differ in that the scientist is thought to lack the artistic interest, good taste, and sensitivity of the college professor. The scientist is not a cultured intellectual, while the college professor attains the highest score in this dimension. Moreover, the scientist is, to a striking degree, less interested in people and less sociable and popular than the college professor. The professor is interested in people and quite successful with them. The scientist is neither drawn to people nor socially attractive. Finally, the scientist is less interesting and colorful than the college professor. The scientist is scored above the college professor on two components of what might be called "material opportunity"—that is, wealth and the opportunity for advancement. The scientist has a more markedly active, persevering, and rational approach to life and work than the professor. In summary, the scientist has greater wealth and opportunity than the professor and a more forceful approach to intellectual problems. However, in the very important areas of social sophistication and esthetic interests the college professor leads the scientist by a wide margin.

When the full range of occupation profiles is considered, the scientist and the engineer have a good deal in common. In terms of strength and competence, as indicated by middle values on such items as *active, confident, strong, hard, self-assertive*, and *realistic about life*, they have very similar scores. Competence in either field connotes a reasonable degree of success, social status, and power in public affairs. The scientist differs from the engineer in that he is believed to be more intellectual and less conformist in personal behavior and political viewpoint. The scientist also is rated higher than the engineer in concern with esthetic matters, in spite of the relatively low rating of the scientist in the realm of cultural interests. The scientist is considered more persevering, self-sacrificing, and valuable to society, as well as more interesting and colorful. On the other hand, the engineer has two clear advantages over the scientist. First, the engineer is more concerned with people. He is a sociable, popular fellow as compared with the scientist. Secondly, the engineer is considerably wealthier, and he is a more "regular guy" than the scientist. This latter characteristic is indicated by the higher scores for the engineer on *clean cut, plays poker*, and *has good taste* (taste in clothes, house, car, and so on), and the engineer is believed more likely to have a pretty wife. In conclusion, then, the engineer is thought to be less of an "egghead" than the scientist. He is less intelligent, less nonconforming, less sensitive esthetically, and less valuable to society. At the same time, the engineer is a more normal, healthy American male, with somewhat the same traits of character as the scientist but with little of the scientist's tendency to go to extremes in behavior or emotional commitment. To summarize, engineers are "Simonized scientists," to bend a phrase recently reported in a national magazine.

RELATION OF EXPERIENCE TO IMAGE

The student responses were analyzed to determine whether the life experiences and current status of the students were associated with different beliefs about occupations. It was found in comparing the scientist image held by men with that held by women and the image held by students in private as against public colleges, by freshmen as against seniors, by students from different socioeconomic backgrounds, by students from professional as against business families, and by students from different types of communities, that these groups do not differ in their beliefs about the scientist. This is clearly a stable image that is shared widely among college students with varied histories and experience.

In a study parallel to the one under consideration, 41 entering Wesleyan freshmen who indicated an intention to become scientists were compared with all the freshmen who planned to be active in other careers. Those who intended to be scientists had a more favorable image of both the scientist and the engineer than the remainder of the newly arrived freshmen. The would-be scientists, as compared to the other freshmen, viewed the scientist as more colorful and interesting, of higher social status, more successful, more sensitive to art, and of a more sociable temperament. In absolute terms, the men wishing to enter the field of science rated the scientist quite high in material and social success and in esthetic interests, while they considered him moderately concerned with people. The scientist, as seen by these students, is interesting and colorful. Moreover, as compared with the non-science students, the science students had an image of the engineer that was closer to their image of the scientist. They viewed the engineer as more individualistic, persevering, and capable of deriving satisfaction from his work than did non-science students. In general, the engineer was seen as being more a man of parts by the pre-science students.

There is also evidence in the data that students on entering college have a more favorable view of the scientist than students who have already spent a semester in college. The new students have a more favorable view than second-semester freshmen of the intellectual ability, artistic concern, and success of the scientist.

FACULTY MEMBERS' VIEW

A group of 27 college teachers of science at Wesleyan University were asked to respond to the same questionnaire that was given to the students. These men were a random sample of the science faculty. It is quite clear that the word *scientist* has similar connotations for them and for students. There was a correlation of +.91 between the average values attributed to the scientist by the Wesleyan students and by members of the science faculty on a group of 21 scales to which responses were made by both groups. The main differences between the two groups were, first, that the students attributed much more influence in public affairs to the scientist than the science teachers did, and, second, that members of the science faculty saw the scientist as more interested in art. Otherwise, the two groups were in close agreement.

Within the ranks of college teachers at Wesleyan, members of science and of social-science faculties are in almost complete agreement on the scientist image. On the other hand, faculty members in the humanities are more complimentary to the scientist than are the teachers of science or social science. A random sample of 23 teachers of the humanities rated the scientist quite high in material and social success and considered him more calm and more sociable than the science teachers did. The worldly success of the scientist seemed more impressive to teachers of the humanities than it did to teachers of the sciences.

OCCUPATIONAL PREFERENCES

Students participating in the main study were asked to indicate the degree to which they would like to enter each of the 15 occupational fields if barriers related to expense, length of training, and native ability were removed. In other words, a male student was directed: "rate each occupational position in terms of how much you would like to be in it if you could be in any occupation you wanted." The data revealed that a group of four occupations—those of college professor, lawyer, doctor, and business executive—were considered most desirable, in that order. The occupations of scientist and school teacher came next in order in a second grouping, at some distance from the first. A rather large gap appeared between this and the next grouping, the occupations of engineer and personnel director. When women were asked to estimate the attractiveness of these occupations for men, they also ranked the scientist in the fifth position. However, when college women were asked to name the single occupation for a future husband that would be most pleasing to them, only 3 percent indicated scientist. Approximately 20 percent of the women wished their husbands would be doctors, and another 20 percent selected the profession of lawyer.

STEREOTYPES OF SPECIALIZED SCIENTISTS

In studies of the ranking in prestige of professions and occupations, the ranking of the term *scientist* differs from that of terms such as *chemist* or *biologist*, which describe scientists in specific fields (5). In view of this finding, an exploratory study was designed to elicit the images of biologist, chemist, and physicist. A small number of Wesleyan students were asked, in an interview, for their impressions of the personality, family life, status, social life, and motivations of men in each scientific field. Although the sample was small and unsystematically chosen, the agreement among students was so great as to suggest that the findings are of general significance. The stereotype of the specialized scientist in each case was more favorable than the image of scientist that was revealed in other interviews. According to these stereotypes the scientists in designated fields are more wealthy and successful, have richer social lives and more rewarding family lives, and are more pleasant and outgoing people than the "scientist" considered apart from his field. The biologist is the most normal of the scientists in the sense that he approaches most closely the American ideal, and to the physicist are attributed many of the negative qualities that emerged in the interviews concerned with the generalized "scientist." The chemist falls between the two extremes.

CONCLUSIONS

These data suggest that there exists among college students a readiness to respond to the word *scientist* in a complex and differentiated manner. There is wide agreement concerning the image of the scientist among various classifications of men and women students in the Northeast. Members of one college faculty share this image with their students. The image is the same for freshmen as for seniors. It is safe to assume that the outlines of the image are the same for students at many colleges and for many college-educated adults. It is quite likely that the image is shifted somewhat in the first few months of a student's college career, but it is obviously not markedly changed. The image of the scientist among college students resembles in many ways the image held by high school students, as reported by Mead and Metraux (2).

The specific features of the scientist image are important for several reasons. First, the image reveals the students' beliefs about the personality of the scientist and the style of life associated with a career in science. It means to the potential recruit that, if he selects science, he should have a certain set of personal qualities and can expect a particular kind of social life and certain types of personal associates, and it implies that the kind of life he will live is greatly limited by his work. If these features of the life of the scientist do not fit with the student's beliefs about himself or his hopes for the future, he is likely to be wary of committing himself to a career in science. At the same time, of course, the image influences the behavior of the student who has chosen science and leads him to develop those aspects of his character most in keeping with the stereotype of the scientist.

In short, the image has the effect of recruiting a certain type of person and discouraging others. This limits the range of people likely to consider the field, and it restricts the variety of basic talents available to science. Second, the public reaction to science, scientists, and the contributions of scientific research is likely to be colored by this image. This is particularly true in areas where arguments center around the generalized role of science. For example, the role of scientists in government or the advisability of admitting scientists to positions of high responsibility are issues frequently discussed in general terms. It may even be that the negative reaction of college students to courses in "general science" is attributable in part to the attitudes tapped by the word *science*.

The strong features of the image of the scientist are his high intelligence and his driving concern to extend knowledge and to discover truth. His work is of great value to mankind, and it brings him both a sense of satisfaction and a fair measure of success. The weaknesses in the image are many and disturbing. The scientist is seen as basically uninterested in people and unsuccessful with them. To the contemporary student, a person who does not care for people is suspiciously out of touch with life. The scientist is not interested in art—he has eschewed the life of the spirit that gives breadth and vitality to the life of the mind. Further, the scientist is a nonconformist and a radical, as well as a person with only moderate control of his impulses. These features suggest that college students possess beliefs that can easily be played upon to indict the scientist in times when loyalty is an issue of public concern. The undesirable aspects of this picture of the personal and intellectual life of the scientist make the role hard to accept in spite of the attractiveness of the work and the social contributions of the scientist.

The attractiveness of a scientific career in an abstract sense is clearly indicated by the high rank given it by men in statements concerning what they would like to be. Yet, surprisingly, few women wish to marry a scientist. It must be that, for men, the intellectual status, success, and material well-being of the scientist outweigh the many disadvantages of the scientist image. On the other hand, a woman married to a scientist must accept his personal qualities while benefiting very little in a direct way from the nature of his job.

Students clearly prefer the personality, social opportunities, and style of life of the college professor to those of the scientist. The scientist's only asset, by comparison with the professor, lies in the rewards associated with the work, and the differential is not great. The engineer and the scientist offer relatively interesting alternatives. The scientist is seen as an intellectual, with little capacity for social interchange; the engineer is a more normal "organization man," aiming at a nine-to-five existence, with an interest in good fellowship. It would seem that a student of science who could achieve the requisite training would be strongly drawn to college teaching with its richer, more humane connotations. On the other hand, the attractions of science and of engineering would seem to balance, with a person's view of himself playing an important role in his choice of one or the other.

It is interesting that students intending to pursue careers in science should have a more favorable image of the scientist than their colleagues who are planning other careers. It is not known whether committment to a field changes the image or whether those with a more favorable image are drawn to the field. Probably both of these processes contribute to this difference.

It is comforting to find that scientists who are identified with their particular specialties are perceived as relatively normal people. These findings indicate that monolithic "science" is a source of concern to many sensitive citizens. On the other hand, men with professional specialties are considered more human, loyal, and comprehensible than "the scientist."

SCIENCE AS A WAY OF LIFE

The standard contemporary response to the finding that a product presents a "bad" image to the public is to turn for assistance to a team of public relations men who are instructed to change the image. To change an image as well developed and as widespread as the image of the scientist appears to be a most discouraging undertaking. This image is embedded in a system of other stereotypes with which people, even highly educated people, structure their social world. To eliminate the unfavorable connotations from *scientist* would require a brilliantly conceived long-term campaign of confrontation through mass media and of educational innovation that is not likely to be undertaken. But is a massive campaign to alter this image appropriate? Scientists themselves, as well as their faculty colleagues, agree upon the essential features of the image. If it does represent, even in a distorted and exaggerated fashion, the characteristics of American scientists, it may be that to use publicity techniques would not only fail to hide the reality that lies behind the image but might also be dishonest.

Our studies give no data as to the actual (as distinguished from the perceived) characteristics of scientists. Yet C. P. Snow (6) has argued that indeed scientists *are* less interested than most educated men in esthetic matters and social affairs. Perhaps "the discipline" of science *does* narrow a man's interests, does create a group who do not meet the cultural ideal of the broadly educated man. If so, the "solution" is not to be found in an aping of Madison Avenue but, as Snow has also argued, in a more general appreciation on the part of the intellectual community of the demands the scientific mode of thought makes upon anyone, professional scientist or not, who seeks an objective understanding of the world around him. Perhaps, also, scientists have "over-conformed" to their own image of what a scientist is, and perhaps the reality can change as more of them develop the broader interests and cultural appreciation constantly called for by liberal educators.

A final stance for the scientist consists in recognition of the possibility that to be a scientist is indeed to be different. The studies of Roe (7) and of Thorndike and Hagen (8) have shown that scientists tend to have characteristic developmental histories and personality structures. It may be that in order to do their work, recruits to scientific careers require some of the qualities which, in extreme form, appear in the stereotype of the scientist. If so, cannot the scientist accept this and get on with his work?

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College Student Stereotypes of the Personality Traits of Research Scientists

A. W. Bendig and Peter T. Hountras

The extent of public support for scientific research and education is dependent upon the attitudes toward science and scientists which prevail in the culture. The development of these attitudes begins early in the elementary school (2). These attitudes are solidified by the time students reach the secondary school level (6, 8) where they influence the choice of a future career (7). The attitude of the public toward the current Man-Into-Space program is at least partially influenced by a general attitude of both respect for and a fear of the influence of scientific advances upon our so-

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ciety. Stated somewhat differently, this ambivalent feeling toward the research scientist means that he is simultaneously a "different" and perhaps slightly dangerous individual, but also a necessary and even useful member of our society. These attitudes would appear to be particularly significant as far as elementary and secondary school teachers are concerned since they are in daily contact with potential future scientists during the period when these attitudes are developing.

An indirect and somewhat disguised approach to *Ss'* attitudes toward scientists may be through a study of the consistency with which *Ss* attribute a syndrome of personality characteristics to the average scientist. It is clear that stereotypes of the personality traits of people in various occupations do exist among college students (1, 9) and identification of the specific traits that *Ss* believe distinguish the scientist from people in other occupations may provide an insight into their attitudes toward the scientist and permit the subsequent development of a relatively simple and objective assessment device to measure these attitudes. Terman's (10) investigation of intellectual and interest differences among four occupational groups, scientists, engineers, lawyers, and businessmen, suggests that comparisons among the personality traits attributed to these occupations might provide evidence as to the students' stereotype of the personality of the research scientist.

PROCEDURE

Traits. An original list of approximately 100 personality trait names was compiled from several published sources (1, 8, 9). Subsequently a list of 60 traits was selected on the basis of two criteria: (*a*) 30 traits that appeared on an a priori basis to be socially desirable and 30 traits judged to be socially undesirable, and (*b*) traits were selected that appeared representative of many significant dimensions of behavior including work habits, intellectual characteristics, and both the social and nonsocial aspects of personality. These criteria were employed (*a*) to minimize response bias in the subsequent ratings and to include a wide range of social desirability in the selected personality characteristics, and (*b*) to insure as far as possible an adequate sampling of many different areas of behavior. The trait names finally selected were: accurate, calm, clumsy, fearful, considerate, meddlesome, intellectual, economical, democratic, inept, egotistical, cruel, logical, mature, unsystematic, pessimistic, friendly, sarcastic, studious, alert, kind, disorganized, timid, critical, orderly, responsible, incompetent, impulsive, tactful, annoying, precise, sincere, humorous, unimaginative, reckless, irritable, persistent, stable, sloppy, nervous, sympathetic, shy, thorough, self-confident, generous, unproductive, miserly, fault-finding, industrious, dependable, inefficient, moody, tolerant, argumentative, capable, unreliable, rigid, poised, lonely, charming.

Forms. Four occupational titles were selected (research scientist, engineer, lawyer, businessman), similar to the comparison groups used by Terman (10), and six forms were prepared, one form for each of the possible combinations of two of the four occupations. On each form the *S* was requested to compare the average person in one (rated) occupation with the average person in another (reference) occupation and to make a judgment as to whether the first person would be most likely to have more, less, or an equal amount of each of the 60 traits than the person in the second (reference) occupation. For example, the significant portions of the instructions for Form C were:

> We all know that a person's interests, abilities, attitudes, and personality characteristics determine to a large extent what occupation he or she will select. For example, the average *research scientist* has more or less of certain traits than does the average *businessman,* although on other traits these two people will have the same amount of these particular traits. We are asking you to identify which of these traits distinguish the *research scientist* from the *businessman* and which traits they have in common.
>
> Below is a list of 60 traits to be identified. . . . Please indicate on your answer sheet your judgment for each of the traits using the following marking system:
>
> Column A: the average *research scientist* has *more* of this trait than the average *businessman.*
>
> Column B: both the average *research scientist* and the average *businessman* have about the *same amount* of this trait.

Column C: the average *research scientist* has *less* of this trait than the average *businessman*.

The combinations of occupations used on the forms are as follows: (*a*) Research Scientist vs. Engineer; (*b*) Research Scientist vs. Lawyer; (*c*) Research Scientist vs. Businessman; (*d*) Engineer vs. Lawyer; (*e*) Engineer vs. Businessman, and (*f*) Lawyer vs. Businessman.

A seventh form (Form G) was constructed that requested the Ss to rate each of the 60 trait names on a five-point scale of social desirability. No reference was made on this form to specific occupations, but the Ss were told that we were trying to obtain relative measures of the social desirability of a large number of personality traits. This last form was used as a check on our original dichotomization of the traits into socially desirable and undesirable groups.

Subjects. Form G was administered to 54 *Ss* (18 men and 36 women) enrolled in two sections of introductory educational psychology. Forms A through F were randomly distributed to 154 Ss in four other sections of the same course, each *S* receiving only one form. Sixteen Ss were discarded from this second group to insure that equal numbers of men and women *Ss* responded to each form. The discarding of *Ss* from each form-sex subgroup was random and the final group consisted of 138 Ss with 23 *Ss* (8 men and 15 women) recording their judgments on each of the six forms. The *Ss* were sophomore pre-education students who are required to take this course prior to admission to the School of Education.

RESULTS

The mean social desirability rating of each trait by the 54 Ss who received Form G was computed and no overlap was found in mean ratings between the 30 traits that had been a priori selected as socially desirable and the 30 traits selected as socially undesirable. Consequently, the original grouping of the items into these two classes was retained in subsequent analyses.

The answer sheets of the 138 *Ss* who responded to Forms A through F provided four separate scores: the number of desirable traits the rated occupation had more of (Desirable-More), the number of desirable traits the rated occupation had less of (Desirable-Less), the number of undesirable traits the rated occupation had more of (Undesirable-More), and the number of undesirable traits the rated occupation had less of (Undesirable-Less). Each of these four scores was then separately subjected to a two-criterion (sex and forms) analysis of variance. The results are reported in Table 1. Both the Desirable-More and Desirable-Less scores discriminated among the six forms at the .001 level of confidence, but neither the Undesirable-More nor the Undesirable-Less scores gave any evidence of significant differences among the forms. No statistically significant (.05 level) sex differences were found in any of the analyses. Apparently the *Ss* could consistently discriminate differences among the pairs of occupations with respect to the presence or absence of desirable traits shown by the average individual in the four occupations, but did not discriminate among the occupations as to undesirable personality traits. This suggests that

Table 1. Analyses of Variance of Four Personality Trait Scores Distinguishing Between Combinations of Occupations

		Desirable-More		*Desirable-Less*		*Undesirable-More*		*Undesirable-Less*	
Source of variation	*df*	*Mean square*	*F*	*Mean square*	*F*	*Mean square*	*F*	*Mean square*	*F*
Sex (Sx)	1	5.22	.29	38.74	2.77	32.36	2.13	36.27	2.05
Forms (F)	5	147.60	8.19*	138.06	9.87*	18.01	1.19	11.92	.67
Sx × F	5	16.67	.92	7.61	.54	17.46	1.15	24.53	1.39
Within	126	18.03		13.99		15.19		16.67	

* $P > .001$.

stereotypes concerning occupational personalities involve the presence or absence of socially desirable traits only.

The mean number of desirable traits attributed to each pair of occupations can be found in Table 2. The difference score between the "more" and "less" means can be viewed, in a sense, as a "favorability" score for the rated occupation when compared with the reference occupation. Both the sum of the "more" and "less" scores and the difference between these two scores were subjected to two-criterion (sex and forms) analyses of variance. The sums did not discriminate between the forms ($F = 1.23$) while the difference score showed form differences that were significant at the .001 level ($F = 14.75$). No sex differences were found in either analysis. Duncan's method was used to test the significance of the differences among the difference score means (4, 5, pp. 26–29) and it was found that the difference score means fell into four groups. The difference mean of Form D was significantly (.05 level) larger than the difference means of Forms A and B. Forms A and B were significantly different from Forms C and E, and the mean difference score of Form F was significantly lower than the mean difference scores of Forms C and E. The differences in means between Forms A and B and also between Forms C and E were not significant. The research scientist and the lawyer were quite similar in difference ("favorablility") scores as were the engineer and businessman. However, the scientist-lawyer pair of occupations were quite distinct from the engineer-businessman in mean difference scores.

Analyses were performed for the 30 desirable traits on each form to identify the single traits that distinguish between each pair of occupations. The "more" and "less" percentages ($N = 23$) were computed for each trait on each form and if either percentage was greater than 57 per cent the trait was selected as being a discriminating item. The results of these individual trait analyses can be found in Table 3. It seems apparent that stereotypes exist for all four occupations. The scientist and lawyer appear similar in the more intellectual traits, but the scientist lacks the warm social graces that characterize the lawyer stereotype. The engineer is a junior edition of the scientist while the businessman lacks the intellectual qualities of the lawyer, but shares many of his social traits.

Since our basic interest was in the stereotype of the research scientist, the traits that most consistently discriminated the scientist from the other three occupations are given in Table 4. Again the same stereotype pattern appears: the scientist, along with the lawyer, has more of the socially desirable intellectual and work habit traits than does the engineer and the businessman, while the scientist, like the engineer, has less of the social graces than does the lawyer and businessman. The 12 traits listed in Table 4 appear to constitute the core stereotype that the Ss had regarding the research scientist.

DISCUSSION

The evidence that our college Ss consistently attribute certain personality traits to the occupations of research scientist, engineer, lawyer, and businessman confirms the results of other studies (1, 9) where different occupations and different methodologies were used. The finding of most general interest was that

Table 2. Mean Numbers of Desirable Traits Attributed to Four Occupations in Six Combinations (n = 23, N = 138)

Form	Occupation rated	Reference occupation	Desirable traits More	Desirable traits Less	Desirable traits Difference
A	Scientist	Engineer	9.7	5.7	4.0
B	Scientist	Lawyer	6.7	7.3	—0.6
C	Scientist	Businessman	10.0	6.8	3.2
D	Engineer	Lawyer	5.8	10.7	4.9
E	Engineer	Businessman	7.0	6.9	0.1
F	Lawyer	Businessman	12.4	3.1	9.3

Table 3. Single Personality Traits Most Consistently Attributed to Six Combinations of Occupations

Occupations compared	*The first occupation is*		*Occupations compared*	*The first occupation is*	
	More	*Less*		*More*	*Less*
Research scientist vs. Engineer	Persistent Studious Intellectual Alert Thorough	Economical	Engineer vs. Lawyer	Precise	Poised Charming Self-confident Tactful Alert
Research scientist vs. Lawyer	Precise	Charming Poised Humorous Self-confident Friendly	Engineer vs. Businessman	Precise Accurate Studious Thorough	Tactful Humorous Poised
Research scientist vs. Businessman	Thorough Studious Precise Intellectual Orderly Persistent Accurate Logical	Charming Tactful Friendly Humorous Economical	Lawyer vs. Businessman	Studious Intellectual Poised Precise Thorough Logical Persistent Tactful Tolerant	Economical

Table 4. Percentages of Subjects Saying that Certain Socially Desirable Traits Distinguish the Research Scientist from Men in Other Occupations

Research scientist is more	*Than the average*		
	Businessman	*Engineer*	*Lawyer*
Intellectual	70	70	26
Logical	61	52	26
Orderly	70	48	48
Persistent	70	91	30
Precise	87	39	78
Studious	91	78	43
Thorough	96	65	39
Research scientist is less	*Than the average*		
	Businessman	*Engineer*	*Lawyer*
Charming	70	34	83
Friendly	65	39	57
Humorous	61	39	70
Poised	52	30	74
Self-confident	35	48	61

the Ss discriminated among the occupations only for socially desirable traits and not for socially undesirable traits. Whether this implies reluctance on the part of the Ss to say that one occupation has more of these undesirable traits, or hesitation in attributing relative freedom from undesirability traits to the paired occupation, cannot be determined from our data. The implications for research where the S is required to attribute personality traits to himself and also to other people are obvious.

The analysis of individual traits comprising the stereotypes of the personalities of these four occupations suggests that the occupations were discriminated along two relatively independent dimensions. The traits can be grouped into (*a*) those referring to intellectual and work habits characteristics and (*b*) those related to social personality traits that arise primarily in interpersonal relations. Research scientists are viewed as being high (having more of the traits) on the intellectual dimension and as being low (having fewer of the traits) on the social axis. Engineers are not as intellectual as scientists, but members of both professions are equally lacking in social graces. The lawyer is equally high on both axes, while the businessman is low on the intellectual dimension and high on social traits. Whether or not other occupations can be located within this two-dimensional system, or whether other dimensions would have to be added are questions for further study.

The 12 traits listed in Table 4 appear to offer a possibility of developing a short objective scale for measuring the extent of the stereotype of the research scientist held by individual Ss. The same procedure used in this study could be repeated by administering Forms A, B, or C to Ss and scoring each *S* as to how many of the first seven traits the *S* says the scientist has more of and how many of the last five traits he indicates the scientist has less of. Such a short 12-item scale may lack sufficient reliability for research purposes, but, if reliable, would offer a method of quantifying this aspect of attitudes for further research.

It is particularly interesting to note that the *Ss* displaying this stereotype of the research scientist were pre-education students who, in a few years, will be teaching children and adolescents from whom the next generation of scientists must be recruited. If this stereotype continues to be transmitted from teacher to student, the problem of interesting high school students in scientific careers will remain with us.

SUMMARY

Pre-education college Ss ($N = 138$) were asked to compare two of four occupations (research scientist, engineer, lawyer, businessman) as to whether the average members of the paired occupations would have more, less, or an equal amount of each of 60 personality traits. Equal numbers of Ss ($N = 23$) responded to each of the six possible pairs of occupations. The traits were evenly dichotomized into socially desirable and socially undesirable groups on the basis of an a priori selection which was validated by having another group of Ss ($N = 54$) rate the traits for social desirability. The number of socially desirable traits attributed to each occupation discriminated among the occupations (.001 level), but the socially undesirable traits did not. Analyses of the 30-individual socially desirable traits indicated that the Ss viewed the scientist and lawyer as having more of the intellectual traits while the lawyer and the businessman were perceived as having more of the desirable interpersonal traits. Both the scientist and engineer have less of the interpersonal traits and the businessman has few of the intellectual traits. The most consistent stereotype in this study regarded the research scientist, when compared with the other three occupations, as being more intellectual, logical, orderly, persistent, precise, studious, thorough, and also as being less charming, friendly, humorous, poised, and self-confident.

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Stereotypes of the Scientist as Seen with the Gordon Personal Profile and Gordon Personal Inventory

John R. Braun

INTRODUCTION

Bendig and Hountras (1) investigated the stereotypes of the personality characteristics of research scientists held by University of Pittsburgh students. Ss had to indicate whether typical members of various paired occupations (including research scientists, lawyers, businessmen, and engineers) would be equal or unequal on a number of characteristics. They found that the research scientist, as compared with the other three occupations was regarded as ". . . more intellectual, logical, orderly, persistent, studious, thorough, and also as being less charming, friendly, humorous, poised, and self-confident."

The present investigation dealt with the stereotypes of the personality characteristics of scientists held by Texas Christian University students in 1961. Instead of Ss making direct comparisons using trait labels, they responded to personality inventories in terms of their conceptions of how a typical scientist or typical business executive would answer.

METHOD

Ss were 42 male and female undergraduates in a child psychology class. Inventories used were the Gordon Personal Profile (GPP) and Gordon Personal Inventory (GPI).

Twenty-eight Ss took the GPP (2) under two sets. Under one set they answered as they thought a scientist would answer, and under the other set as would a business executive.

Fourteen Ss took the GPI (3) under these two sets. For both inventories the order of administration of the sets was counterbalanced. Significance of difference between the scores resulting from the two sets was evaluated by Wilcoxon paired-replicates tests.

RESULTS AND DISCUSSION

Tables 1 and 2 present the means and standard deviations of inventory scores produced by the sets. With the GPP, the business executive set resulted in significantly higher scores on Ascendancy and Sociability, while the scientist set resulted in significantly higher scores on Responsibility and Emotional Stability. The business executive set also resulted in significantly higher Total scores, which would be the extent to which complimentary rather than derogatory alternatives were chosen.

With the GPI, the scientist set resulted in significantly higher scores on Original Thinking and Personal Relations. There were no significant differences between the sets on Cautiousness and Vigor. This time Total score was significantly higher for the scientist set.

Based on these findings we may say that business executives were seen as quite ascendant and sociable, rather vigorous and energetic though cautious, and moderately responsible. They were seen as having much worry and

Table 1. Gordon Personal Profile Means and Standard Deviations for Scientist and Business Executive Sets (N = 28)

Variable	*Scientist*		*Business executive*	
	Mean	*SD*	*Mean*	*SD*
Ascendancy	—0.82	4.95	11.96†	3.24
Responsibility	13.43	3.99	6.32†	5.11
Emotional stability	6.29	5.41	0.57†	4.55
Sociability	—3.25	3.65	10.43†	3.65
Total	14.86	15.89	30.00†	8.72

† = Significantly different from Scientist Mean at .01 level.

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Table 2. Gordon Personal Inventory Means and Standard Deviations for Scientist and Business Executives Sets (N = 14)

	Scientist		*Business executive*	
Variable	*Mean*	*SD*	*Mean*	*SD*
Cautiousness	9.93	4.22	6.71	4.53
Original thinking	13.00	2.85	1.57†	6.70
Personal relations	9.57	3.31	—0.21*	4.11
Vigor	8.36	4.27	8.21	6.13
Total	30.86	7.77	16.29†	17.09

* = Significantly different from Scientist Mean at .05 level.
† = Significantly different from Scientist Mean at .01 level.

tension, not particularly concerned with ideas, and not having a great deal of faith or trust in people.

Scientists were seen as quite responsible, cautious, and concerned with ideas, rather vigorous and energetic, and rather high on degree of faith and trust in people. They were seen as having only an average amount of worry and tension and as being not at all ascendant and sociable.

In spite of differences in region, time, and technique of investigation, the results of the present study are consistent with those of Bendig and Hountras. The scientist is seen as high on intellectuality and low on sociability. Since adolescents place great stress on popularity it is easy to see why many may be reluctant to pursue a career in science. They see a scientist as one who lacks interest and skill in dealing with other people. Research into the most effective ways of modifying this stereotype is sorely needed. . . .

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Science as a Vocational Preference among Junior High School Pupils

Stanley Krippner

It is commonly observed that some careers have more respect, money, and prestige associated with them than do others. These occupations usually demand longer periods of education; material and non-material rewards are therefore needed to recruit new members [4]. As a result, high school students frequently have been reported to state interests in these high-level occupations despite the fact that relatively few will be capable of completing the required training [8].

Unfortunately, parents often encourage unrealistic vocational planning on the part of their children, indicating that they too have been influenced by the prestige and status of professional careers [5].

At the present time, the United States has an urgent need for qualified scientific personnel. Occupations in science are generally associated with intelligence and high professional standing. However, the research of Roe

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[14], Beardslee and O'Dowd [2], and Mead and Métraux [10] indicates that the stereotypes of scientific occupations contain unfavorable aspects which might deter young people from taking an active interest in these careers. This research reveals that scientists are considered "intelligent" but "unstable," "withdrawn," "irreligious," and the recipients of only "modest" incomes [2, 7, 13].

PROCEDURE

It was hypothesized that, in a well-to-do community where social status is of critical importance, junior high school pupils would receive parental direction toward careers other than science. It was further hypothesized, in view of the scientist's association with high intelligence, that virtually the only pupils expressing an interest in scientific careers would be those receiving high scores on academic achievement and mental ability tests.

RESULTS

Data were obtained from 351 seventh and eighth grade pupils in an upper-middle class Chicago suburban community during 1960. There were 189 boys and 162 girls in the sample. The Iowa Every-Pupil Tests of Basic Skills (IEP) and the Primary Mental Abilities Tests (PMA) were administered as well as a vocational interest questionnaire. . . . Responses to the following items on the vocational questionnaire were tabulated and compared:

1. What kind of work do you want to do when you finish school?
2. What kind of work does your father want you to do?
3. What kind of work does your mother want you to do?

High and low groups for the IEP were derived by finding the mean score for each sex and identifying those students falling one standard deviation above and below the mean. As no statistically significant differences were found between the two grades on the vocational questionnaire, the groups were combined.

When the IEP scores were adjusted, the mean girls' score was 8.7 with a standard deviation of 1.0. There were 30 girls in the high IEP group and 26 girls in the low IEP group. The mean boys' IEP score was also 8.7 with a standard deviation of 1.0. There were 26 boys in the high IEP group and 43 boys in the low IEP group.

The mean PMA score for the girls in the sample was 115.7 with a standard deviation of 14.4. There were 28 girls in the high PMA group and 21 girls in the low PMA group. The mean boys' PMA IQ was 113.1 with a standard deviation of 14.2. There were 28 boys in the high PMA group and 36 boys in the low PMA group. . . .

In response to the question, "What kind of work do you want to do when you finish school?" several vocations were elicited. The favored occupations of high-achieving girls were "teacher," "secretary," "airline stewardess," and "nurse," in that order. Low-achievers preferred "secretary," "nurse," "teacher," and "airline stewardess."

High-achieving boys cited "doctor," "scientist," "engineer," and "lawyer" while low-achievers also mentioned "doctor" more than any other vocation. Next in line, for the low-achievers, were careers in the skilled trades, aviation, business, and engineering. More low-achieving boys and girls were vocationally undecided than were high-achievers. This difference is statistically significant at the 0.01 level of confidence. Table 1 presents the occupational preferences of all four groups as well as the percentages of students who were undecided.

The occupational preferences of high-scoring boys were similar to those of low-scoring boys with the exception of science. Nineteen per cent of the high IEP group cited chemistry, physics, astronomy, and zoology as occupational goals. None of the low IEP boys mentioned scientific careers: this difference is significant at the 0.02 level of confidence.

Eighteen per cent of the high PMA boys preferred scientific vocations: once again, none of the low-scoring boys named careers in scientific fields. This difference is significant at the 0.05 confidence level.

These results confirm the hypothesis that pupils receiving high scores on achievement and ability tests would more often state an interest in scientific careers. The fact that approximately equal proportions of high- and

Table I. Occupational Preferences of High-Scoring and Low-Scoring Junior High School Pupils

Occupational preference	*High IEP boys*	*Low IEP boys*	*High IEP girls*	*Low IEP girls*	*High PMA boys*	*Low PMA boys*	*High PMA girls*	*Low PMA girls*
Armed forces	3.0%	2.0%	0.0%	0.0%	0.0%	5.0%	0.0%	0.0%
Arts	3	2	13	12	7	3	6	5
Aviation	0	9	10	12	4	11	4	10
Business, clerical, sales	3	9	13	27	7	13	11	5
Education	0	0	42	15*	0	0	39	24
Engineering	16	9	0	0	12	5	11	7
Farming and forestry	3	5	0	0	0	5	0	0
Housewife	0	0	3	4	0	0	0	5
Law	10	5	0	0	7	3	0	0
Medicine & nursing	24	16	10	15	20	16	6	24
Professional sports	6	5	0	0	7	3	0	0
Science	19	0†	3	0	18	0*	6	0
Skilled trades	0	16	0	0	4	7	0	0
Other occupations	3	2	3	0	0	10	0	0
Undecided	10	20	3	15	14	19	17	20
Total	100	100	100	100	100	100	100	100

* Difference significant at 0.05 level of confidence (Girls' $X^2 = 3.875$) (Boys' $X^2 = 4.456$).

† Difference significant at 0.02 level of confidence (Boys' $X^2 = 5.987$).

low-scoring boys preferred vocations in medicine, engineering, law, and business point out the uniqueness of scientific careers as having little appeal to low-achievers and low-scorers on mental ability tests.

Very little enthusiasm for vocations in science is noted when the boys' responses to items two and three on the questionnaire are examined. Of the high-scoring IEP and PMA boys, only five per cent reported that their fathers wanted them to become scientists. Only four per cent reported that their mothers had suggested scientific careers. Instead, parents were reported to favor careers in medicine, engineering, law, and business for their sons.

Twenty-two per cent of the high IEP boys and 18 per cent of the high PMA boys revealed that their fathers wanted them to become doctors. Mothers purportedly favored medical careers for 19 per cent of the high IEP boys and 25 per cent of the high PMA boys. The percentages were almost as large when the low-scoring boys' responses were examined. . . .

There was only one significant difference between high- and low-scoring girls. When the IEP groups were compared, 42 per cent of the high-scorers expressed an interest in teaching as opposed to 15 per cent of the low-scorers. This difference is significant at the 0.05 confidence level. Only four per cent of the girls (all high-scorers) expressed an interest in scientific careers.

None of the fathers and two per cent of the mothers reportedly suggested that their daughters consider scientific work. As a result, the hypothesis concerning parental direction is confirmed for both sexes.

DISCUSSION

Achievement, as measured by the IEP, and mental ability, as measured by the PMA, do not seem to be as influential determinants of medical, engineering, law, and business preferences as they do for vocational choices in education and science.

The medical profession has a high-prestige, high-status, high-income stereotype [2]. Doctors and physicians appear at the apex of occupational status rankings for both men's and women's jobs [1, 6]. Registered nurses also have a favorable stereotype and are third from the top in status ratings [6]. Engineers are thought to earn high salaries and are fifth from the top in status rank [1].

On the other hand, teachers are generally considered to be poorly paid. Elementary school teachers rank eleventh in status rating [6]. Although scientists have fairly high status ranks [1], other aspects of their stereotype are highly unfavorable [9].

Roe [14] points out that the public image of the scientist is not a flattering one. Studies of college students indicate that the scientist is thought of as "socially withdrawn," and so devoted to his work that he is satisfied with only a modest income [2]. Bendig and Hountras [3] discovered that college students looked upon scientists as "intellectual" and "studious" but lacking humor, poise, and self-confidence. Junior and senior high school students, in other studies, have described scientists as "odd," "dull," and "evil" [9, 13].

All of these professional fields are associated with intelligence and the need for specialized training. However, the stereotypes of teachers and scientists carry more unfavorable connotations than do the stereotypes of doctors, nurses, and engineers.

We might hypothesize that the higher a professional occupation is perceived by these students to rank in social status rewards, the higher will be the percentage of low-achievers among those preferring it. The bulk of these low-achieving students may be influenced in their choice by the extrinsic rewards of the job rather than by the intrinsic enjoyment promised by the work activity itself.

These results are similar to those recently reported by Moser [11]. He compared high school students' IQ's with their vocational interests and found the mean I.Q. of those preferring medical careers (doctor, dentist, nurse, etc.) to be 100. The highest mean IQ was 114 which characterized those students inclined toward teaching.

The findings regarding careers in education are important because teachers have even less favorable occupational stereotypes than scientists [6, 10]. This again demonstrates the negative effects of prestige considerations and the over-emphasis upon social status.

It should also be noted that academic achievement test scores revealed more dramatic inter-group differences than mental ability scores. This indicates that vocational preferences are more closely associated with "demonstrated achievement" than "potential ability." It is likely that the high-scoring pupils preferring scientific careers had experienced success in science courses. This success probably reinforced an intrinsic interest to such an extent that unfavorable aspects of the scientific professional stereotype were largely disregarded. The same behavioral pattern is reflected, to a lesser extent, in preferences attributed to the subjects' parents.

Additional research may demonstrate the validity of this hypothesis in other socio-economic groups and for additional vocations.

SUMMARY

Psychological and sociological research indicates that scientists have less favorable occupational stereotypes among Americans than do most professional workers. Vocational questionnaire responses of the sample investigated ($N = 251$, seventh and eighth graders) tended to confirm these findings. Significantly more high-achievement (on the EIP) and high-ability (on the PMA) boys preferred scientific careers.

In addition, few parents were reported to have advised their children to consider scientific careers, even when these pupils demonstrated high academic ability.

As the sample was drawn from an upper-middle class community, the investigator suspects that the unfavorable elements in scientists' occupational stereotypes led to a reduction of interest on the part of low-achievers. The students whose high achievement scores indicate success in academic areas might be more attracted to these professions on the basis of interest in the intrinsic nature of the work, not the extrinsic status and salary considerations.

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PART FOUR / PSYCHOLOGICAL ASPECTS OF PROFESSIONAL ROLE BEHAVIORS

A. Choice of Discipline within the Sciences

Introduction

The first papers in this section are characteristic of a number of studies aimed at determining the relationship of personality and biographical variables to occupational choice which have compared diverse groups of professions in hopes of finding clear-cut personality differences between professions (e.g., Siegelman and Peck (1960) on chemists, ministers, and military officers; Nachmann (1960) on law, dentistry, and social-work students). Despite some obvious weaknesses of these studies, they are included because they are among the few studies (together with Cattell and Drevdahl's in Section II E) that compare persons in the behavioral sciences with those in the physical and natural sciences.

The theoretical assumption adopted in the present selections is that not only is there a general fit between personality and occupation, but also, if our measuring instruments are sensitive enough, there must be found a more specific fit between personality characteristics and very specific occupational classifications within a general occupational classification, such as scientist. The background of such an assumption rests upon the developmental framework of vocational choice and its relationship to personality (discussed in Section III A and B) espoused by Roe, Holland, and Bordin, among others.

While the Cattell and Drevdahl sample was composed of three groups of scientists (physicists, biologists, and psychologists), differences between the behavioral and the natural and physical scientists are given little emphasis in their report. However, psychologists did appear to be more dominant, enthusiastic, and cheerful, and somewhat more adventurous as well as showing more Bohemian unconcern and unconventionality than the other two groups of researchers. In contrast, the Chambers selection in this section, using the same scale, the Sixteen Personality Factor Questionnaire (16 PF), found psychologists to differ from chemists only in Bohemian unconcern and unconventionality. These interdisciplinary differences have been pursued in a preprofessional population by Goldschmid (Section IV A) who hypothesized that significant personality traits will co-vary with the choice of major once the discipline has been located along a science-humanities continuum. When scores on paper-and-pencil personality and interest tests were administered to entering freshmen whose majors were recorded at the time of graduation, it was possible to identify the personality characteristics that were related to choice of major.

The next two selections help to fill out the picture of differences between the two general groups of scientists studied by Cattell and Drevdahl and by Chambers. These next selections are not the methodological equals of the above studies, since their techniques are descriptive and projective rather than providing "hard" data. However, they are included because they provide important comparison data to the first two studies, while utilizing somewhat different and unusual populations and more projective and open-ended data collection instruments. Galinsky's population consisted of forty graduate students in clinical psychology and physics studied through interview techniques. Although he found some distinct differences between groups, it can be argued that his findings cannot validly be applied for social scientists because clinical practitioners are very different from social scientists involved in research. In a pioneer effort, Roe, using projective techniques, compared a small sample of psychologists and anthropologists with biologists and physicists. The pattern which emerges shows that social scientists have a more marked interest in people (which agrees with Galinsky's data for clinical psychologists versus physicists) and have more dependent parental relationships, associated with guilt and unhappiness on the Thematic Apperception Test (TAT), than do physical scientists. Thus, the stereotyped portrait of the socially aloof, introverted personality of the scientific researcher may only apply to scientists in the so-called hard sciences. However, the data of Cattell and Drevdahl and of Chambers does not support this hypothesis. In fact, both studies could suggest that psychologists are more Bohemian, introverted, unconventional, and imaginative than physical and natural scientists (factor M on the 16 PF) and have more distant relationships with their parents.

Rossman, Lips, and Campbell's selection is one of the few dealing with interests of mature social scientists (see also Campbell and Soliman, 1968). It compares vocational interest patterns of Ph.D. and non-Ph.D. sociologists and the interest patterns of sociologists versus psychologists. It appears that sociologists were more oriented toward social service and cultural occupations, while the psychologists' interests were more like those of physical scientists and businessmen.

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Relating Personality and Biographical Factors to Scientific Creativity

J. A. Chambers

This study is concerned with the differences in personality and biographical factors between mature scientists who are highly creative research men and those who are much less creative in research.

One of the earlier and better studies of this problem (Visher, 1948) presented biographical factors of starred men of science. It was not until Guilford's presidential address to the American Psychological Association in 1950 on the topic of creativity, however, that systematic attacks on the problem began in earnest around the country. Summaries of the studies in this area to date may be found by consulting Anderson (1959), Roe (1953a), C. W. Taylor (1956, 1958, 1959), and Stein and Heinze (1960).

The studies most pertinent to the current investigation were those conducted by Roe (1951a, 1951b, 1953a, 1953b) and Cattell (Cattell, 1959; Cattell and Drevdahl, 1955; Drevdahl and Cattell, 1958). Both researchers chose as their subjects highly creative scientists; but whereas Roe selected a small number (64) of men and used personal interviews and projective techniques, Cattell chose to use a larger number of subjects and to depend on an objective paper-and-pencil test of personality. Roe's major finding was that these men were very strongly motivated, i.e., they were willing to work hard and for long hours. Cattell found his scientists to be cool and aloof, dominant, and introspective. Both of these researchers, however, in addition to their specific empirical findings, demonstrated several important things: (*a*) that creativity can be studied in a controlled manner; and (*b*) that highly creative scientists do have certain features in common. What these and other studies failed to show was that these factors are unique to creative scientists and do not describe scientists as a group. Cattell used no control groups at all, and Roe used only samples of college faculty members as controls with no regard for such variables as age, length of experience, and opportunity to do research.

This study attempts to improve on previous studies of creativity by using larger samples of subjects; by using control groups matched on the variables of sex, age, education, discipline, and opportunity to do research; and by using measuring instruments which have had the variables they purport to measure anchored by validity studies to outside criteria. . . .

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RESEARCH DESIGN

Subjects and Criteria

. . . Subjects were chosen who had given evidence of having produced one or more products which were new or novel to civilization. Essentially, persons were chosen who had achieved eminence as research scientists. In this way only creative persons should have been identified as subjects, although probably some creative persons were omitted since it is probable that not all creative researchers achieve eminence for their work, especially if it is done in an obscure or "unpopular" scientific area.

Four groups of male scientists were selected, all under 65 years of age and currently residing in the United States or Canada. Men responsible for the tests used in the study were excluded. Two groups (one of chemists, the other of psychologists) were chosen for their eminence as research scientists, and two control groups were chosen on the basis of their lack of eminence.

The eminent scientists were chosen from seven sources: (*a*) the membership roster of the National Academy of Sciences, (*b*) the membership list of the American Philosophical Society, (*c*) scientists starred in *American Men of Science* from 1903–43 as indicated by Visher (1947) in *Scientists Starred 1903–1943*, (*d*) the roster of selected general authorities from *Who Knows—and What* (1954), (*e*) *Who's Who in America* (1959), (*f*) the membership list of the Society of Experimental Psychologists (a society to which a limited number of psychologists is elected on the basis of distinguished work), and (*g*) the list of well-known psychologists identified and studied by Clark (1957). Equal numbers were selected in the areas of psychology (as representative of the social sciences) and chemistry (as representative of the physical sciences). The control groups were chosen from the membership lists of the professional societies of the two disciplines, with selection restricted to individuals who were not listed in any of the six sources from which the other groups were formed. Each control subject was chosen so as to match some eminent individual as closely as possible on the bases of age, amount of education, and opportunity to do research. The resultant lists of creative and noncreative scientists were submitted to a number of mature scientists within each discipline. These persons were asked to eliminate from the lists the names of those persons who, in their opinion, did not qualify to serve as a member of the group for which they had been chosen.

Creative and control groups each contained 200 individuals in the field of chemistry and approximately 170 each in psychology. The total sample thus consisted of about 740 scientists.

Measuring Instruments

Instruments used in the major study included an 81-item Biographical Inventory developed by the author (covering personal data; job-related behavior and attitudes; undergraduate, secondary, and primary school training; and home life in childhood and youth); Factors E, F, H, M, and Q_2 from Cattell and Stice's (1957) 16 Personality Factor (PF) Questionnaire; Items 51–75 from Maslow's (1952) Security-Insecurity Inventory; and the Initiative Scale from Ghiselli's (1954, 1955) Self-Description Inventory. . . .

Biographical Inventory. This was developed on the basis of the works of Roe (1953a), Visher (1948), Knapp and Goodrich (1952), and Terman (1954).

The 16 PF Questionnaire. Developed and studied extensively in recent years through factor-analytic techniques, this test consists of items measuring 16 factors, of which 15 are personality-type dimensions and 1 represents a measure of general intelligence. The factors of importance to this study are those designated E (Dominance versus Submission), F (Enthusiasm and Cheerfulness versus Seriousness and Introspectiveness), H (Adventurousness versus Timidity), M (Creativity versus Conventional Outlook), and Q_2 (Self-Sufficiency versus Group Dependency).

Security-Insecurity Inventory. A test composed of items of clinical derivation, its purpose is to detect and measure feelings of security. The complete test (75 items) correlates highly with the Thurstone Neurotic Inventory and the Bernreuter Measure of Neurotic Tendency.

Initiative Scale. This is an unpublished instrument consisting of 64 pairs of descriptive adjectives (32 positive and 32 negative) paired on the basis of social acceptability. The respondent is forced to choose one from each pair in the first 32 as the more descriptive of himself, and one from each pair in the latter 32 that is less descriptive of himself (Ghiselli, 1954). . . .

Procedure

. . . A personal letter was mailed several days ahead of the form to each individual. This letter explained the study, assured anonymity of response, requested cooperation, etc. Other procedures which were used to increase the percentage of returns included: (*a*) the enclosure of a post card with the initial letter, to be used if the person wished a copy of the results; (*b*) a stamped envelope (addressed to the investigator) to be included with the printed questionnaire; and (*c*) a follow-up letter sent 2 weeks later (Goode & Hatt, 1952). . . . Since returns were unsigned, some other method was needed in order to identify the group to which the returned questionnaires should be allocated. Color coding was therefore added.

RESULTS

Characteristics of the Responding Sample

A total of 438 forms was returned in usable condition, representing a return of approximately 60%. These forms were then grouped according to discipline, i.e., psychologists and chemists; and within each discipline creative was compared with control.

Throughout the study the various groups will be identified by letters, as follows: EP, creative psychologists; CP, control psychologists; EC, creative chemists; CC, control chemists; P, psychologists; and C, chemists. . . .

The characteristics of the various groups and of the total sample in relation to number of doctorates, age, and type of employment are given in Table 1. . . . It may be noted that almost all subjects hold the doctorate and that most are employed in an educational setting. . . .

Creative Compared with Control Scientists

Measures of central tendency, variability, and tests of significance on personality tests are given in Table 2. Tests of significance on biographical items appear in Table 3.

Personality Tests. Two major findings in this analysis are: (*a*) creative scientists are more dominant than control scientists (higher mean E score); and (*b*) they have more initiative (higher mean score on the Ghiselli scale). . . .

Biographical Items. Significant differences were found on 16 items, as reported in Table 3. Specifically, the creative scientists more often had fathers who were professional men. They graduated from high school at a younger age than control scientists, and later, more often achieved a straight A average in their major and in their over-all grade-point average both as undergraduates and as graduate students. They spent many more hours per week (in excess of 50) on study and research while in graduate school, published more articles then, and more often had their graduate school expenses met through scholarships and fellowships as opposed to part-time work.

As mature scientists, the creative men still show this strong motivation. They read more

Table 1. Number of Doctoral Degrees, Age, and Employment of Subjects

Variable	*EP*	*CP*	*EC*	*CC*	*P*	*C*	*Total group*
Doctoral degrees	109	102	104	116	211	220	431
Age[a]	49.5	50	56	54	50	55	53
Employment							
Educational	99	89	75	110	188	185	373
Industrial	4	2	20	6	6	26	32
Governmental	4	10	8	1	14	9	23
Other	3	1	3	0	4	3	7
Not listed	0	1	2	0	1	2	3
Total	110	103	108	117	213	225	438

[a] Figures in this row are medians.

professional journals and present more papers at conventions. They produce, of course, many more articles than the control scientists.

Several factors unrelated to ability and motivation also discriminated the two groups. The highly creative men in their current lives

Table 2. Comparison of Creative and Control Psychologists and Chemists on Personality Tests

	Psychologists					*Chemists*				
	Creative		*Control*		*t test*	*Creative*		*Control*		*t test*
Factor	CT^a	V^b	CT^a	V^b		CT^a	V^b	CT^a	V^b	
E	14.98	3.27	13.92	3.48	2.34*	14.15	3.52	12.91	3.89	2.53*
F	9.34	3.41	9.13	3.21	.47	9.14	3.17	8.66	3.44	1.11
M	12.84	3.43	12.25	3.40	1.27	11.80	3.05	11.61	3.02	.48
Q_2	13.63	2.60	11.92	3.35	4.14*	13.42	3.00	12.96	2.37	1.29
H	13.69	4.59	14.11	4.23	.70	13.65	3.79	12.99	4.21	1.26
S-I	6.50	4.03	6.00	3.05	1.35	6.00	2.97	7.00	3.06	1.00
Gh	34.44	6.68	28.06	7.68	6.63*	33.18	8.14	29.38	7.79	3.66*

Note—Abbreviations are as follows: from Cattell's 16 PF Questionnaire E—Dominance, F—Enthusiasm, M—Creativity, Q_2—Self-Sufficiency, and H—Adventurousness; S-I, Security-Insecurity Inventory; Gh, a measure of initiative from Ghiselli's Self-Description Inventory.

Median scores were computed for S-I; all other average scores represent means.

Semi-interquartile ranges were computed for S-I; all other such measures are standard deviations.

Median test used for S-I.

N varies from 213 to 225.

[a] Measure of central tendency.

[b] Variability.

* $p < .05$.

Table 3. Comparison of Creative and Control Psychologists and Chemists on Significant Biographical Inventory Items

Item no.	*Psychologists* χ^2	*Chemists* χ^2	*Item content*
3	17.36†	7.95*	My religious preference is: a. Protestant b. Catholic c. Jewish d. Other e. No preference
4	8.57*	23.15†	I would classify my interest in religion as: a. Strong b. Moderate c. Little d. None e. Opposed to religion
8	25.80†	10.52†	I would classify my commitments in civic and community activities as: a. Quite heavy b. Moderate c. Light d. None
10	15.01†	19.94†	My graduate grade-point average was: a. A b. A minus c. B plus d. B e. B minus or less
11	15.10†	10.46†	While in graduate school I devoted the following approximate amounts of time each week during the school year to my studies or to related research (include time spent in classes): a. 30 hours or less b. 31 to 40 hours c. 40 to 50 hours d. 50 to 65 hours e. 65 hours or over
12	22.53†	12.61†	While in graduate school, I had the following number of scientific articles published: a. None b. One c. Two d. Three e. Four or more
13	23.96†	15.85†	My expenses in graduate school were met largely through: a. Scholarships or fellowships b. Assistantships c. Own savings or part-time work d. Parents e. Other
21	19.98†	14.33†	My over-all undergraduate grade-point average was: a. A b. A minus c. B plus d. B e. B minus or less
22	18.99†	16.78†	My undergraduate grade-point average in my major subject was: a. A b. A minus c. B plus d. B e. B minus or less
23		10.74†	During college I was a member of: a. More than two honor societies b. Two honor societies c. One honor society d. No honor societies
26		24.14†	When I received my undergraduate degree I was: a. Over 22 b. 22 c. 21 d. 20 e. Under 20
29		6.00†	In high school I: a. Was not a member of any athletic teams b. Was active in one or two sports c. Was quite active in sports

Table 3 (Continued)

Item no.	*Psychologists* χ^2	*Chemists* χ^2	*Item content*
34		9.16†	My work in high school mathematics was considered: a. Outstanding b. Good c. Fair d. Poor
35	7.80*	16.81†	When I graduated from high school I was: a. 19 or older b. 18 c. 17 d. 16 e. Under 16
37		9.95†	My position in the family was: a. Oldest child b. Oldest son, but not oldest child c. About the middle d. Youngest child e. Only child
41	13.06†	9.59†	My father's occupation: a. Professional b. Business Executive c. Farmer d. Factory or office worker e. Other
47	7.88†		I would describe the family in which I was raised as: a. Closely knit b. Lacking in warmth c. Individualistic, i.e., each person went his own way
59		6.17†	I felt that my family: a. Was different from others b. Was somewhat superior to others c. Neither of the above
61	19.39†	7.84†	Concerning research as a career or major interest: a. I "drifted" into it b. I chose it
62	19.08†		I first chose or accepted research as a career or major interest: a. After leaving graduate school b. When I was in graduate school c. When I was an undergraduate d. When I was in high school e. Prior to entering high school
64	7.84*	18.81†	On the average, I keep up with the articles in: a. No scientific journals b. One or two scientific journals c. Three or four scientific journals d. Five or six scientific journals e. More than six scientific journals
65		35.16†	I am a member of the following number of professional organizations: a. None b. One or two c. Three or four d. Five or six e. More than six
66		27.30†	I attend the following approximate number of professional conventions each year: a. None b. One or two c. Three or four d. Five or six e. More than six
67	33.01†	21.30†	I deliver a paper at a professional convention: a. Rarely or never b. Every year or so c. At least once a year d. Two or three times a year e. Four or more times a year
68	6.04†		My opinion concerning most professional conventions is that: a. They are professionally stimulating and therefore of value b. They are sometimes interesting and sometimes a waste of time c. They are of primary value to the socially oriented individual and of lesser value to research oriented individuals
69		12.51†	I spend the following approximate number of hours weekly in connection with my work (including time spent both at my place of employment and elsewhere): a. 30 or less b. 31 to 40 c. 40 to 50 d. 50 to 65 e. 65 or over
71	11.25†		In relation to my work, I: a. Am completely happy only when working b. Get a great deal of satisfaction from it c. Get some satisfaction from it d. Am not too happy with my vocational choice e. Wish I had gone into another field
74		10.35†	Ideally, I think that the following number of hours per week should be spent in active research at the place of employment *if creative output is to be at a maximum:* a. Less than 20 b. 20 to 30 c. 30 to 35 d. 35 to 40 e. More than 40
76	29.98†	28.29†	Concerning professional positions, the most important one of the following factors, in my opinion, is: a. Opportunity for permanent work and advancement b. Stimulating associates and atmosphere conducive to research c. Opportunity to combine research work with teaching or administrative duties d. Opportunity to do really creative research and to choose problems of interest to me
80	61.17†	7.28†	I have the following number of scientific products to my credit: ________ number of published scientific articles (include joint publications)

Note—All item responses were tested for significance using the chi square except for Item 80, on which the median test was used.

* $p < .10$.

† $p < .05$.

show significantly more often either no preference for, or little or no interest in, religion and also make few or no commitments to civic and community affairs. A final finding, of possible interest to employers of research personnel, was that when seeking a position the less creative scientists are predominantly concerned with opportunities to combine teaching and administrative duties with research, while the overwhelming choice for the creative scientists is the opportunity to do really creative research and to choose problems of interest to them.

The creative research man thus emerges as the dominant, strongly motivated individualist who is self-propelled and whose interests are channeled away from social and civic activities and are directed towards his own individual research problems.

Creative Compared with Control Scientists within Disciplines

The findings so far have been common to both psychologists and chemists. However, creative psychologists differ from control psychologists in other ways not found for chemists. Creative psychologists prove to be more self-sufficient than their controls (Factor Q_2). . . .

There are also significant differences on four more biographical items, indicating that: (*a*) creative men in this profession more often came from individualistic families in which each person went his own way, (*b*) the creative man chose research as a career at a younger age than his less creative contemporaries, (*c*) creative psychologists place less value on professional conventions than their controls despite the fact that they present more papers, and (*d*) creative psychologists derive much more satisfaction from their work than their less creative peers.

Creative Chemists. Creative chemists differ from their controls on an additional 10 biographical items (there are no further differences in tests). These items indicate that as far back as childhood there were differences in achievements, feelings, etc., of the creative chemists as contrasted to the less creative ones. The creative chemists were usually the middle or older children. At relatively early ages these children considered their families to be superior to others. In high school these creative children exhibited their strong intellectual orientations by shunning sports and excelling in mathematics. This strongly expressed intellectual ability and motivation continued through undergraduate school as evidenced by membership in many honor societies and graduation at an early age. As adults these creative chemists are still exhibiting this exceptionally strong intellectual drive, now channeled into professional activities as evidenced by their membership in many professional organizations and attendance at many professional conventions each year, as well as spending long hours each week at their work. Further, many more of these men than their controls believe that 40 hours or more per week *should* be spent in active research if creative output is to be maximized. . . .

Psychologists Compared with Chemists

Although the major findings of the study have been presented in the preceding section, it was felt that an examination of the data relevant to the differences between scientists within disciplines and between scientists and other groups would be helpful since such information is necessary to the field of vocational guidance and strongly ties in with the early identification and encouragement of students having creative potential in the science area.

Personality Tests. Measures of central tendency and variability for both psychologists and chemists on the tests, as well as tests of significance, are presented in Table 4. As indicated

Table 4. Comparison of Psychologists and Chemists on Personality Tests

	Psychologists		*Chemists*		*Analysis of*
Factor	*CT*[a]	*V*[b]	*CT*[a]	*V*[b]	*covariance*
E	14.47	3.41	13.51	3.78	2.00
F	9.23	3.31	8.89	3.32	.04
M	12.55	3.42	11.70	3.03	4.76*
Q_2	12.80	3.10	13.18	2.69	2.27
H	13.89	4.41	13.31	3.96	1.75
S-I	6.00	3.00	6.00	3.00	.33
Gh	31.37	7.84	31.24	8.17	.04

Note—Median scores were computed for S-I; all other average scores represent means.

Semi-interquartile ranges were computed for S-I; all other such measures are standard deviations.

Median test used for S-I.

N varies from 212 to 220.

[a] Measure of central tendency.

[b] Variability.

* $p < .05$.

earlier, analysis of covariance was used rather than t tests, since significant differences in age had been found between these two groups. Analysis of covariance permitted the age variable to be statistically controlled. As may be noted, one significant difference was found, indicating psychologists to be more bohemian, introverted, unconventional, imaginative, and creative in their thinking and behavior than chemists (Factor M). . . .

Biographical Items. Table 5 indicates that significant differences were found on 36 items. These differences emerged as early as childhood years.

As children, more psychologists were reared in the northeast than the chemists, the latter more often being reared in the midwest or in a foreign country. As children, the psychologists felt there was less affection and understanding between themselves and their fathers than did the chemists, and further, that their parents were not as accepting of them. The psychologists tended to reject positive childhood images of their parents, instead recalling the rebelliousness they felt in regard to parental authority. In reference to values in the homes, too, psychologists' parents differed by more often placing little value on education and learning or by stressing their value in relation to achieving financial success and social

Table 5. Comparison of Psychologists and Chemists on Significant Biographical Inventory Items

Item no.	χ^2	*Item content*
3	23.66*	My religious preference is: a. Protestant b. Catholic c. Jewish d. Other e. No preference
4	39.52*	I would classify my interest in religion as: a. Strong b. Moderate c. Little d. None e. Opposed to religion
6	17.59*	When I get any free time: a. I enjoy watching or participating in sports most of all b. I enjoy outdoor activities (other than sports) most of all c. I enjoy indoor, individual activities most of all d. I enjoy social activities most of all
9	37.52*	My present feelings towards my parents (or feelings prior to their deaths, if deceased) could well be expressed as: a. Considerable love and affection b. High regard c. Sincere admiration for father, affection for mother d. Relatively indifferent e. Wish I could accept their behavior toward me with good grace but find it difficult
11	17.51*	While in graduate school I devoted the following approximate amounts of time each week during the school year to my studies or to related research (include time spent in classes): a. 30 hours or less b. 31 to 40 hours c. 40 to 50 hours d. 50 to 65 hours e. 65 hours or over
13	23.99*	My expenses in graduate school were met largely through: a. Scholarships or fellowships b. Assistantships c. Own savings or part-time work d. Parents e. Other
15	23.25*	Attendance costs at the school, in relation to other undergraduate schools, were: a. Quite high b. Fairly high c. Average d. Below average
18	18.21*	The department in which I took my undergraduate major: a. Had a departmental club, of which I was a member b. Had a departmental club, but I did not join it c. Had no departmental club
19	85.68*	In my opinion, my undergraduate department, as compared to other departments in the school, had: a. Severe requirements b. Fairly stiff requirements c. Moderate requirements d. Light requirements e. Very easy requirements
21	25.67*	My over-all undergraduate grade point average was: a. A b. A minus c. B plus d. B e. B minus or less
26	12.86*	When I received my undergraduate degree I was: a. Over 22 b. 22 c. 21 d. 20 e. Under 20
27	11.94*	During my high school years I spent the majority of my free time: a. Dating or running around with "the gang" b. Reading or studying c. Participating in various sports d. In connection with my hobbies e. Other
28	23.51*	In high school I participated in: a. No clubs b. One club c. Two or three clubs d. Four or more clubs
29	6.36*	In high school I: a. Was not a member of any athletic teams b. Was active in one or two sports c. Was quite active in sports
30	33.38*	I began dating when I was: a. 23 or over b. 20 to 22 c. 17 to 19 d. 14 to 16 e. 13 or under
33	72.60*	In high school my favorite subject was: a. Mathematics, chemistry, or physics b. English, foreign languages, or social studies c. Technical subjects d. Other

Table 5 (Continued)

Item no.	χ^2	*Item content*
34	26.16*	My work in high school mathematics was considered: a. Outstanding b. Good c. Fair d. Poor
44	17.39*	When I was a child, the house I lived in was located in the: a. Northeast b. Midwest c. South d. Far west e. Foreign country
49	9.76*	As a child, my parents: a. Dominated and/or overprotected me b. Encouraged me to do things on my own c. Were primarily concerned with their own affairs d. Did not accept me or give me as much attention as I felt I needed e. None of the above
50	12.46*	As a child, I felt that my parents: a. Loved me a great deal b. Were kind and considerate of me c. Were warm and affectionate towards me d. Were fine individuals, and that my father was a successful person e. None of the above
51	12.08*	When I was a child, I: a. Rebelled against my parents b. Accepted the discipline of my parents, but was inwardly rebellious against it c. Accepted the discipline of my parents without being too concerned about it d. Wished that my parents would be more strict with me e. None of the above
52	16.30*	In my childhood there was: a. More than moderate affection and understanding between my father and me b. Average affection and understanding between my father and me c. Less than moderate affection and understanding between my father and me d. Practically no affection and understanding between my father and me
56	10.38*	In the home in which I was reared, education and learning: a. Were not especially valued b. Were valued for their own sake c. Were valued as investments for future security d. Were valued in the light of the aid they provide in achieving financial success and/or social prestige e. None of the above
60	102.86*	I chose my profession: a. When I was in graduate school b. During my junior or senior year in undergraduate school c. During my freshman or sophomore year in undergraduate school d. When I was in high school e. Prior to entering high school
61	31.33*	Concerning research as a career or major interest: a. I "drifted" into it b. I chose it
62	27.40*	I first chose or accepted research as a career or major interest: a. After leaving graduate school b. When I was in graduate school c. When I was an undergraduate d. When I was in high school e. Prior to entering high school
64	9.63*	On the average, I keep up with the articles in: a. No scientific journals b. One or two scientific journals c. Three or four scientific journals d. Five or six scientific journals e. More than six scientific journals
65	11.76*	I am a member of the following number of professional organizations: a. None b. One or two c. Three or four d. Five or six e. More than six
68	19.51*	My opinion concerning most professional conventions is that: a. They are professionally stimulating and therefore of value b. They are sometimes interesting and sometimes a waste of time c. They are of primary value to the socially oriented individual and of lesser value to research oriented individuals
70	8.13*	I believe that I do my best research when: a. I work alone b. I work as a member of a group
71	13.95*	In relation to my work, I: a. Am completely happy only when working b. Get a great deal of satisfaction from it c. Get some satisfaction from it d. Am not too happy with my vocational choice e. Wish I had gone into another field
72	20.88*	Administrative aspects of scientific work: a. Interest me a great deal b. Are moderately interesting to me c. Are of little interest to me d. Are of no interest to me
73	17.21*	In research work, I believe that working hours should be: a. Standard, i.e., a regular eight hour day b. Flexible, so as to permit workers some freedom in choosing working hours c. Set by the individual alone
74	69.34*	Ideally, I think that the following number of hours per week should be spent in active research at the place of employment *if creative output is to be at a maximum:* a. Less than 20 b. 20 to 30 c. 30 to 35 d. 35 to 40 e. More than 40
75	14.54*	In research work, work deadlines: a. Should be set by a superior b. Should be set by the individual or group concerned c. Should not be set at all
80	8.83*	I have the following number of scientific products to my credit: ________ number of published scientific articles (include joint publications)

Note—All item responses were tested for significance using the chi square except for Item 80, on which the median test was used.

* $p < .05$.

prestige rather than as having value in their own right.

As adolescents, the psychologists continued their emotional pattern by expressing more often than chemists the feeling of being isolated or apart from others. Perhaps to compensate for this, psychologists during this period began dating at a younger age than the chemists and continued dating more actively than chemists during their high school years. They also were more active in clubs and in high school sports. Their favorite subjects were English, foreign languages, or social studies, while the chemists preferred mathematics, physics, or chemistry. The latter group also performed much better in mathematics than did the psychologists.

As undergraduates the psychologists more often than the chemists paid their own way through schools and attended ones with below-average costs. This financial problem may have retarded their progress somewhat, since chemists tended to be younger than psychologists upon receiving their undergraduate degrees. The undergraduate departments of psychology were more often characterized by respondents as maintaining moderate academic requirements and having no departmental club, while the chemistry departments were characterized by maintaining fairly stiff to severe academic requirements and by having a departmental club of which most respondents were members. The undergraduate grade-point averages of the psychologists also were considerably below that of the chemists.

In graduate school the psychologists continued to show the lesser intellectual motivation evidenced in comparisons with chemists at earlier ages, in that during graduate school they spent less time than the chemists each week in relation to studies and research. The chemists also seemed to "mature vocationally" faster than the psychologists, in that they made an earlier choice of career. Psychologists more often than the chemists "drifted" into research as a career.

As adults, the psychologists still manifest some of the individualistic, aloof behavior evidenced in their childhood years, maintaining feelings of indifference or passive hostility toward their parents, while the chemists have a high regard for theirs (this finding held up even when comparing experimental psychologists alone with chemists). Psychologists more often show no preference for or little interest in religion than the chemists, who profess a fair amount of interest, predominantly in Protestant faiths. The general social orientation of the psychologists continues to show itself in their greater preference for social activities during their free time.

In relation to the attitudes and behaviors of the two adult groups concerning their professions, chemists read and publish more scientific articles, but join fewer professional organizations. They also have a higher opinion of professional conventions; are more interested in group research and in administrative aspects of research than are psychologists; and they more often believe 40 hours or more per week should be spent in active research if creative output is to be maximized, while psychologists argue for 20 or 30 research hours per week. Also, chemists believe work hours should be flexible and work deadlines set by the group, while psychologists favor individual freedom in relation to both work hours and deadlines. Finally, the chemists are apparently more immersed in their work and gain greater pleasure from it than do the psychologists.

All of the findings pertaining to comparisons of psychologists and chemists relative to biographical items must be viewed with caution, of course, for as indicated earlier, median ages for the groups varied significantly and could not be controlled statistically.

Scientists Compared with College Students and with the General Population

As compared with students (see Table 6) and considering only scores of 4 or less, or 7 or more, as indicating a definite deviation from the average (Cattell & Stice, 1957, p. 7), the scientists as a group appear to be more silent and introspective (Factor F) and aloof and withdrawn (Factor H) than the "average" male college student as well as more self-sufficient and resourceful (Factor Q_2). Further, this pattern seems characteristic of all of the groups comprising the total sample.

In relation to the general adult male population . . . scientists differ in much the same ways as from the students, except that they appear somewhat less withdrawn than the student comparison showed them to be (Factor

H), but also more unconventional, imaginative, and creative when compared to the average man (Factor M).

DISCUSSION

Creative Compared with Control Scientists

Personality Factors. Results of the various investigations are presented in Table 7. As may be noted, Roe's (1953a) and Barron's (1959) findings have been supported to a greater extent than those of Cattell (1959) and Cattell and Drevdahl (1955).

Thus the creative scientist emerges as a strongly motivated, dominant person who is not overly concerned with other persons' views or with obtaining approval for the work he is doing (biographical factors and Factors E and Q_2). He is not the type of person who waits for someone else to tell him what to do, but rather thinks things through and then takes action on his own with little regard to convention or current "fashion" (Initiative Scale and

Table 6. Mean Scores on Cattell Factors Based on (Male) College Student Norms[a]

Factor	*EP*	*CP*	*EC*	*CC*	*P*	*C*	*All subjects*	*Normative M*[b]
E	6	5	5	5	5	5	5	(5.5)
F	4	4	5	4	4	4	4	(5.5)
M	7	6	6	6	7	6	6	(5.5)
Q_2	8	7	8	8	8	8	8	(5.5)
H	3	3	3	3	3	3	3	(5.5)

Note—Average scores on Factors F and H were corrected for age.

[a] Based on *N* of 364 men averaging 21 years of age (Cattell & Stice, 1957).

[b] Scores are expressed as stens, which are scales in which 10 equal points cover the population with mean of 5.5.

Table 7. Personality Characteristics of Creative Scientists

Characteristic	*Investigator(s) and measuring instruments*	*Present study and measuring instruments*
Strong motivation	Roe (1953a)—over-all assessment Barron (1959)—over-all assessment	Supported—biographical factors
High degree of initiative	Roe (1953a)—over-all assessment Barron (1959)—over-all assessment	Supported—Ghiselli Initiative Scale
High self-sufficiency and independence	Roe (1953a)—over-all assessment Barron (1959)—over-all assessment Cattell & Drevdahl (1955)—Factor Q_2, 16 PF Questionnaire	Supported[a]—Factor Q_2, 16 PF Questionnaire
High degree of dominance	Cattell (1959)—biographical data Cattell & Drevdahl (1955)—Factor E, 16 PF Questionnaire	Supported—Factor E, 16 PF Questionnaire
Many basic insecurities	Roe (1953a)—over-all assessment	Not supported—items from Maslow's Security-Insecurity Inventory
High on adventurousness	Cattell (1959)—biographical data Cattell & Drevdahl (1955)—Factor H, 16 PF Questionnaire	Not supported—Factor H, 16 PF Questionnaire
High introspectiveness	Cattell (1959)—biographical data Cattell & Drevdahl (1955)—Factor F, 16 PF Questionnaire	Not supported—Factor F, 16 PF Questionnaire

[a] Psychologists only.

Factor Q_2). He then is prepared to face the consequences of making unpopular decisions or of pursuing unconventional paths in his search for evidence relating to nature's laws (Factors E and Q_2).

Two other factors which have been hypothesized by various investigators as relating to creativity, i.e., persistence and energy level, apparently tie in with the above factors and appear promising as predictors when satisfactory instruments measuring these traits become available.

Returning again to the Cattell studies, the apparent reason for the discrepancies between the present study and the Cattell studies lies in the lack of appropriate control groups in the latter investigations. That Cattell's factors *do* distinguish creative research men from some segments of the population is borne out by reference to Table 6 (comparisons with student and adult general population norms), but apparently factors such as introversion-extraversion bear little relationship to achievement of creative research productivity, or so the results of this study imply.

The study also throws some light on another area of concern to many investigators, i.e., the relationship of mental health to creativity. Several investigators have been outspoken in their insistence on a relationship between these two concepts. Rogers (1959) and Maslow (1959), for example, have stressed "openness to experience" or "self-actualization" as basic for creativity, with both of these terms apparently referring to positive mental health. On the other hand, Roe (1953a) pointed out that many of the highly creative scientists she studied were experiencing rather severe emotional problems, and she therefore hypothesized basic insecurities as possible sources of the strong motivation to succeed in the lives of these persons. Mead (1959) also indicated a correlation of schizophrenic-like behavior with highly creative artistic productivity and well-adjusted, "happy" behavior with low-level creative artistic productivity in her South Pacific cultural studies. Historical studies of some highly creative artists, etc., also reveal a high incidence of neurotic or psychotic-like behavior among these persons.

The Maslow Security-Insecurity Inventory was included in this study in order to investigate the above, and, as noted in the preceding section, all groups were classified in the average range on Maslow's norms. Maslow (1952) stated, regarding his test,

> the purpose of the S-I Inventory is to detect and measure the feeling of security (which as defined here is one of the most important determinants of mental health almost to the point of being synonymous with it) [pp. 2–3],

and again

> security as defined here is almost synonymous with mental health [p. 7].

If this is accepted, then the results certainly offer no support for Roe's hypothesis or for the implications of Mead's studies in relation to creativity in science. But these findings give little support to the hypothesis that creativity is associated with the highest level of mental health, since average group scores were not in the "very secure" range, but rather were only average.

Biographical Items. Since there are so many findings to cover under this heading, only a brief comparison of the results will be given here.

First, in relation to childhood and family data, there were very few factors found to differentiate significantly the creative from the control scientists. Specifically, the socioeconomic class, being the first-born child, and similar factors previously found by Roe (1953a) and Visher (1948) to be characteristic factors in the backgrounds of creative scientists were not found in this study to be associated to a significant degree with achievement of "creative" status as research scientists. One factor identified by both Roe and Visher, however, relating to the high incidence of fathers of creative scientists being professional men, was supported by this study in relation to the chemists only.

A large number of factors differentiated the creative scientists from their controls concerning their activities from high school through adult life. On the whole, these supported Roe's (1953a) findings of early intellectual maturation (choosing their professions at an early age), strong motivation (as exemplified through studying long hours and making good grades as students, working long hours as adults, and producing many creative products), and strong work-oriented interests as adults, to the exclusion of religous, social, and community interests. No support was found for relating characteristics of undergraduate schools or departments to production of creative scientists (Knapp & Goodrich, 1952).

Special examination should be made, perhaps, of the religious factor, since this has been given so much attention by many investigators in relation to creativity in science. The finding most often made has been the preference for the Protestant religion on the part of the creative scientists, with very few eminent scientists showing a preference for Catholicism. This was noted by Roe (1953a), Knapp and Goodrich (1952)—in relation to production of scientists by Catholic institutions of higher learning—and others.

This study supports Knapp and Goodrich's findings in that only 1½% of all scientists included in the study attended undergraduate schools with Catholic affiliation. Further only 6% of all subjects came from homes in which the Catholic faith was preferred, while 77% came from Protestant homes. However, no relationship was found between achievement of creative status and religious preference. It appears, then, that religious preference is much more strongly associated with choice of science as a career than it is with achievement of highly creative productivity within a scientific discipline.

One final comment in relation to religion —Roe's findings relating to creative scientists' lack of interest in religion was supported. Even though their interest is small, however, it is interesting to note that most of them still classify themselves according to a particular religious preference.

Psychologists Compared with Chemists

Personality Factors. Table 8 summarizes the personality and biographical factors found by other investigators to be more characteristic of psychologists than of chemists. As may be noted, the majority of differences in personality characteristics found by Cattell and Drevdahl (1955) were not supported in this current study when the age variable was controlled. Since Cattell and Drevdahl did not control for age, the differences in results may be attributable to this statistical difference in the treatment of the data, especially since his groups tended to differ in age in the same direction as the groups in the current study. (The mean ages for Cattell's research groups were: psy-

Table 8. Comparison of Psychologists and Chemists: Findings on Personality and Biographical Characteristics

Characteristic	*Investigator(s) and measuring instruments*	*Present study and measuring instruments*
Personality		
Psychologists more dominant	Cattell and Drevdahl (1955)—Factor E, 16 PF Questionnaire	Not supported—Factor E, 16 PF Questionnaire
Psychologists more enthusiastic and cheerful	Cattell and Drevdahl (1955)—Factor F, 16 PF Questionnaire	Not supported—Factor F, 16 PF Questionnaire
Psychologists more adventurous	Cattell and Drevdahl (1955)—Factor H, 16 PF Questionnaire	Not supported—Factor H, 16 PF Questionnaire
Psychologists more Bohemian, introverted, unconventional, imaginative and creative in thinking and behavior	Cattell and Drevdahl (1955)—Factor M, 16 PF Questionnaire	Supported—Factor M, 16 PF Questionnaire
Biographical		
Psychologists more rebellious against parents	Roe (1953a)—biographical data	Supported—biographical data
Psychologists more often have feeling of family superiority	Roe (1953a)—biographical data	Not supported—biographical data
Psychologists more socially oriented	Roe (1953a)—biographical data	Supported—biographical data

chologists, 42.9; biologists, 48.7; and physicists, 51.4.) Also to be considered, of course, is the fact that Cattell and Drevdahl studied *only* creative scientists, while the comparisons between psychologists and chemists in this study included both creative and control subjects.

Biographical Items. Table 8 also summarizes Roe's (1953a) results characterizing social scientists as opposed to physical scientists. On the whole her findings of earlier and stronger development of social interests on the part of social scientists were upheld by this study. Whether or not this interest in people and social things on the part of psychologists is due to the lack of affection between them and their parents, and their consequent rebellion against their parents (as found by Roe and supported by this study), is unknown.

General Comments. It should be kept in mind that the central problem of investigation in this study as well as Roe's (1953a) and Cattell and Drevdahl's (1955) has been the measurement of differences between highly creative scientists and various control groups. Since less emphasis has been placed on measuring differences between scientific disciplines, less care taken in selecting samples, etc., the comparisons of the research findings relevant to this topic are less meaningful than those of creative compared with control scientists and should be viewed in this light.

Scientists Compared with the General Population

Table 9 lists the factors found by Cattell and Drevdahl (1955) to be characteristic of scientists when compared with the general United States' male population. The present study offers support to the findings that creative scientists are more introspective and more self-sufficient than the average man (Factors F and Q_2). The findings not supported may again be due to the reasons cited in the previous section.

Concluding Comments and Suggestions for Further Research

The studies to date indicate the typical creative scientist to be an extremely strongly motivated man (Biographical Factors) who needs no pushing but rather is self-propelled (Ghiselli Initiative Scale), dominating others to gain his desired outcome (Factor E) and being completely engrossed in his work to the exclusion of social and civic interests, with evidently no need for religion in his life (Biographical Factors). Yet this same man, who apparently is not "well rounded," is neither insecure nor unhappy (Maslow Security-Insecurity Inventory), but rather gains a great deal of enjoyment from his work (Biographical Factors). Personality differences between creative men in different scientific fields are less striking.

Research in this field is badly needed in the area of development and validation of tests of such factors as persistence, energy level, and other characteristics hypothesized by leading researchers in the area as possibly differentiating the highly creative from the less creative researcher. Intelligence, as measured by such global tests as developed by David Wechsler,

Table 9. Comparison of Scientists and (Male) Population Norms:[a] Findings on Personality Characteristics

Characteristic	*Investigator(s) and measuring instruments*	*Present study and measuring instruments*
Scientists more dominant	Cattell and Drevdahl (1955)—Factor E, 16 PF Questionnaire	Not supported—Factor E, 16 PF Questionnaire
Scientists more introspective	Cattell and Drevdahl (1955)—Factor F, 16 PF Questionnaire	Supported—Factor F, 16 PF Questionnaire
Scientists more adventurous	Cattell and Drevdahl (1955)—Factor H, 16 PF Questionnaire	Not supported—Factor H, 16 PF Questionnaire
Scientists more self-sufficient	Cattell and Drevdahl (1955)—Factor Q_2, 16 PF Questionnaire	Supported—Factor Q_2, 16 PF Questionnaire

[a] Cattell and Stice (1957).

should be further considered, since studies in this area to date have resulted in a great deal of confusion regarding the relationship of such factors to research creativity. Also, the possibility of combining validated measures such as the above into predictive batteries should not be overlooked, since there is a strong need for improvement in techniques in the areas of research grant awards, selection of scientists for research work in industry, and the like. Finally, every effort should be made to attack the central problem in this area—i.e., what are the wellsprings for the strong motivation of the creative researcher? . . .

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Prediction of College Majors by Personality Tests

Marcel L. Goldschmid

Although some relationship certainly exists between personality and the choice of a major, just as between personality and occupational preferences, studies of these phenomena have come to few conclusions.

The clarification of the relationship between vocational and educational choice and personality would enhance both the development of more adequate personality theories and our understanding of the counselee's problems in the areas of vocational and educational adjustment. With the continued expansion of education in all directions and at all levels, a better comprehension of educational choice might result in a lowered attrition rate in colleges and universities and heightened satisfaction with one's educational and vocational career.

The research reported here focuses on the forecasting of college majors along the two continua of the sciences and the humanities by means of personality tests. Specifically, an attempt was made to predict seniors' scores on the two continua using personality tests they had taken when they were freshmen.

PROCEDURE

Personality Tests

The five personality tests which were used included the California Psychological Inventory (CPI, Gough, 1957) Minnesota Multiphasic Personality Inventory (MMPI), Myers-Briggs Type Indicator (MBTI, Myers, 1962), Omnibus Personality Inventory (OPI, Heist, 1962), and Strong Vocational Interest Blank (SVIB). These tests were selected for their promise in identifying personality patterns related to educational choice.

The CPI, MMPI, and the SVIB are well known, but the other two require some brief description. The MBTI, which is based on a conceptual scheme modeled after Jungian typology, classifies people on the basis of their self-reported behavior, preferences, and value judgments into dichotomous categories along four dimensions: extraversion-introversion, sensation-intuition, thinking-feeling, and judgment-perception.

The OPI, developed by the Center of Higher Education at the University of California, Berkeley, has been devised primarily to measure personality characteristics of college students and includes 15 scales such as Thinking Introversion (TI), Theoretical Orientation (TO), Estheticism (Es), Impulse Expression (IE), and Schizoid Functioning (SF).

Specification of the Science and Humanities Continua

Dichotomous classification of majors into "sciences" and "humanities" would not only be difficult, but would also lose valuable information which could be retained if two continua of variation could be defined. The constant-sum method seemed the most appropriate technique for scaling majors along the two continua.

For our study, two rating forms were

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constructed, one for the sciences and one for the humanities. The two dimensions were anchored on physics and philosophy respectively; that is, physics was paired with each of the remaining 54 fields for the ratings on the science continuum, and philosophy was paired with each of the remaining 54 fields for the ratings on the humanities continuum. Students were asked to make 54 comparisons, each time assigning a total of 100 points to two majors, first to indicate the degree of science in each and, second, to weight each major according to its position as a humanity. The definition for science read as follows:

> A branch of study that is concerned with observation and classification of facts and especially with the establishment and/or quantitative induction of general laws verifiable by hypothesis and experiment.

The definition of the humanities was given as:

> The branches of learning regarded as having primarily a cultural character, and which are concerned, among other things, with the nature of knowledge and experience, and with the aims and purposes of man's being.

The 55 fields of study were chosen to represent a wide spectrum, including the major departments in most colleges or universities and consisting of fields in which both men and women tend to major.

To test the representativeness of the two scales, Rating Forms A and B were administered to 11 groups, composed of counselors, professors, and upper division students majoring in a wide variety of fields at the University of California. Both sexes were adequately sampled.

For each group, mean scores for each major (on sciences and humanities) were computed, and these group averages were correlated to reveal the degree of agreement among the groups of judges. There was extremely good agreement among the groups, all correlations being .90 or above. As expected, the ratings of the degree of science were negatively correlated with the ratings of the humanities, but in no case were the values greater than —.90 and in some cases as low as the upper 60s. The decision to use both continua independently was therefore vindicated.

To obtain the most representative and stable values for each of the 55 majors, the mean values provided by the total sample of raters ($N = 142$) were selected for subsequent analyses. These values are presented in Table 1. Inspection of this table reveals an inverse relationship in the ordering of the ranks for the two continua. On the science scale, physics, chemistry, biochemistry, bacteriology, and genetics received the higher scores; lower scores were obtained by classics, dramatic art, comparative literature, English, and journalism. On the humanities scale, phi-

Table 1. Final Scale Values of Fifty-Five Major Subjects, Classified in Order of Magnitude

Major subjects	*SVS*[a]	*Major subjects*	*SVH*[b]
Physics	10.00	Philosophy	10.00
Chemistry	9.93	Classics	9.23
Biochemistry	9.79	Theology	9.21
Bacteriology	9.34	Comparative	
Genetics	9.24	literature	9.05
Biology	9.19	Art	8.78
Astronomy	9.15	History	8.67
Zoology	8.97	English	8.65
Physiology	8.93	Dramatic arts	8.35
Mathematics	8.82	Music	8.22
Chemical		Anthropology	7.54
engineering	8.77	Psychology	7.48
Botany	8.76	Education	7.41
Medicine	8.58	Political science	7.20
Mineralogy	8.56	Sociology	7.19
Anatomy	8.47	Law	7.08
Electrical		Foreign languages	6.83
engineering	8.39	Social welfare	6.31
Geology	8.23	Speech	6.01
Oceanography	8.17	Journalism	5.78
Mechanical		Linguistics	5.75
engineering	8.06	Economics	5.62
Entomology	7.98	Architecture	5.54
Soil science	7.88	Criminology	5.39
Statistics	7.74	Public health	4.56
Civil engineering	7.63	Medicine	4.42
Animal husbandry	7.02	Nursing	3.98
Psychology	7.00	Home economics	3.97
Anthropology	6.72	Librarianship	3.96
Dentistry	6.64	Geography	3.88
Economics	6.13	Mathematics	3.65
Geography	6.00	Business education	3.46
Forestry	5.89	Genetics	3.27
Public health	5.65	Astronomy	3.00
Criminology	5.64	Biology	2.86
Sociology	5.46	Physiology	2.76
Architecture	5.38	Physics	2.74
Nursing	5.34	Zoology	2.72
Linguistics	5.07	Statistics	2.69
Political science	4.48	Geology	2.64
Social welfare	4.24	Forestry	2.60
Education	4.17	Anatomy	2.55
Law	3.99	Entomology	2.52
Business		Botany	2.34
administration	3.86	Dentistry	2.32
Home economics	3.76	Civil engineering	2.30
Philosophy	3.55	Bacteriology	2.22

Table 1 (Continued)

Major subjects	*SVS*[a]	*Major subjects*	*SVH*[b]
History	3.53	Animal husbandry	2.20
Music	3.38	Chemistry	2.17
Librarianship	3.05	Oceanography	2.15
Speech	2.63	Biochemistry	2.07
Foreign language	2.56	Soil science	2.06
Theology	2.16	Mechanical engineering	1.97
Art	2.02		
Journalism	1.97	Chemical engineering	1.96
English	1.92		
Classics	1.88	Mineralogy	1.94
Comparative literature	1.87	Electrical engineering	1.93
Dramatic arts	1.87		

[a] Scale values, science continuum.
[b] Scale values, humanities continuum.

losophy, classics, theology, comparative literature, and art were given the higher ratings; lower scores were given to electrical engineering and soil science.

A few fields, such as psychology and anthropology, were judged to be *high* on both scales. This may well reflect the fact that these fields represent in themselves a rather wide spectrum on these continua. For example, anthropology includes physical anthropology at one end of the science-to-humanities continuum and cultural anthropology at the other. In contrast to this, a few majors, such as business administration, librarianship, and home economics tended to receive *low* ratings on both scales, suggesting that the science and humanities continua are irrelevant with respect to these fields, their being neither science nor humanities. There may well be other dimensions upon which these majors might more adequately be classified.

Analysis of the Psychological Meaning of the Science and Humanities Continua

Two rating techniques were developed to differentiate between the psychological meaning of the science and humanities continua. The first technique consisted of 10 statements which the subject was asked to rank with respect to their importance for science and the humanities. The 10 statements sought to embody attributes which by hypothesis could discriminate clearly between the concepts.

A rank-order correlation of —.53 indicated a negative relationship between factors ranked as important for the sciences and the humanities. The rankings revealed that the humanities were seen as dealing primarily with people and abstraction; science was viewed as dealing primarily with theory, method, and least of all with people. These results support the idea of an underlying continuum going from impersonality at one end to an interest in people at the other. On this dimension science would be impersonal, the humanities the opposite.

A second rating technique chosen to elucidate the connotative meaning of science and the humanities was adapted from the semantic differential (Osgood, Suci, and Tannenbaum, 1957). The findings indicate that science was seen as more potent and active, but less personal than the humanities; it was also seen as more complex. The scores on the evaluative factor and on a transitory-permanent scale did not differentiate between the concepts.

To further clarify the location of the two concepts in the semantic space, Jenkins' (1959) "Atlas of Semantic Profiles" was consulted. This atlas presents the three factor scores (Value, Potency, Activity) of 260 words which were rated on the semantic differential by college students. An attempt was made to find words in the atlas which resembled science and humanities on all three factors.

Words of a "masculine" character were rated closer to science; words of a "feminine" nature paralleled the scores of the humanities. For example, the ratings of science approximate the scores assigned to "brother," "father," "man," and "scientist.' The humanities, on the other hand, were seen closer to "mother," "sister," and "woman."

Subjects

The size and choice of the samples were dictated primarily by three considerations. First, freshmen were wanted who had taken these tests at admission to a college or university and whose majors were known at the time of graduation. Second, to develop stable regression equations which could be cross-validated, samples of 100 or more subjects were required. Third, it was required that the samples be representative of students in general and not just of students at one university.

To find samples which met these requirements, other projects which had used the tests were examined. For the CPI, data were available on freshmen males entering in 1959 at Stanford University. Five years later, their majors were identified by checking their names against the registrar's list of graduating seniors. In this manner, science and humanities scores were obtained for all subjects.

The same procedure was followed for all

other tests. The MBTI, OPI, and SVIB scores of a sample of freshmen who had taken these tests at the University of California were available. The MMPI data were obtained from the University of Minnesota.

Data Analysis

The principal technique of analysis was to derive regression equations to forecast standing on the two continua of science and the humanities in an initial subsample. Both equations for each test were then cross-validated on holdout subsamples.

RESULTS AND DISCUSSION

The regression equations and the multiple *r*'s for both the original and cross-validation sample are presented for each test in Table 2.

The different predictions based on the five tests are in substantial agreement, in that they identify similar personality characteristics as being correlated with each of the two continua of science and the humanities.

Although the coefficients of most of the equations in both original and cross-validation samples are statistically significant beyond the .01 level, they are not large enough to lead automatically to operational application. However, the trends are clear and discernible, and given a favorable selection ratio, some of the equations (e.g., the humanities equation for males on the SVIB) could be quite effective in predicting future major.

There is a close relationship between the size of the original sample and the size of the coefficients on cross-validation. For those tests (e.g., CPI; OPI, males; SVIB, males; and MMPI, males) where the original sample was relatively large, the validities on cross-validation were better, whereas for those tests (e.g., MBTI; SVIB, females; MMPI, females) where the original samples were smaller, the cross-validation gave poorer results.

The longitudinal character of this study may also have affected the size of the validity coefficients. The predictions were made to the time of the student's graduation, 4 or 5 years after the testing which gave rise to the forecasts.

It should be emphasized that the results do not permit a comparison of the differential predictive power of the five tests used. The sample size varied for each test and so did the population, in one case consisting of Stanford students (CPI), in another of University of California students (MBTI), and still another of University of Minnesota students (MMPI). This study was not designed to contrast the predictive efficiency or the validity of these tests, but rather to identify personality characteristics that are associated with educational choice.

Having demonstrated the moderate predictive validity of these equations, the next question concerned their psychological meaning.

The findings of all five instruments were pooled in an attempt to evolve two global personality descriptions—one for students who scored high on the science equation, that is, freshmen who, at the time of graduation, majored in the sciences such as biology, chemistry, and physics and one for students who scored high on the humanities equation, that is, freshmen who, when they were graduated, majored in such fields as art, classics, and philosophy. These two descriptions are summarized in Table 3.

The personality characteristics which best identify each group of students seem to fall into five broad clusters: (*a*) orientation to life, (*b*) modes of adjustment and psychological functioning, (*c*) interest patterns, (*d*) behavior in social contexts, and (*e*) modes of cognitive functioning. The presentation of the two personality sketches in Table 3 is, therefore, organized around these five clusters.

With regard to previous research, results appear in line with those studies (e.g., Sanford, 1962; Tyler, 1964) which found that students in a particular major share certain personality traits which are significantly different from those in other majors. Previous investigations, however, tended not to specify the continua of variation, and many used rather small samples. Moreover, the use of regression equations allowed for a multivariate or profile combination, rather than the one-scale-at-a-time approach which is intrinsically less powerful. And, finally, the longitudinal character of this study has practical implications, particularly for workers in the field of educational counseling.

A follow-up study of the findings reported here is now in progress. Over 100 seniors majoring in a variety of fields at the University of California, Los Angeles, and Fresno State College were tested on the same five tests used in this study. In addition to the science and humanities continua these subjects' majors were also scored on a third continuum, the social sciences. Since all subjects have taken the same five tests, the predictive validity of the five tests

Table 2. Regression Equations, Ns, and Multiple r's for Each Test Predicting the Science and Humanities Scores

			Original sample		*Cross-validation sample*	
Test	*Sex*	*Equation*	*N*	*r*	*N*	*r*
CPI	m	*Science* = 75.013 — Dominance (.985) — Social Presence (.628) + Well-being (1.081) — Femininity (.714)	307	.34	200	.33
		Humanities = 5.355 + Dominance (.931) + Social Presence (.493) — Self-control (.673) + Femininity (1.653)	307	.37	200	.39
MMPI	m	*Science* = 54.826 + K (.335) — Masculinity-Femininity (.490) + Social Introversion (.137)	163	.40	82	.40
		Humanities = 56.202 — K (.437) — Hysteria (.321) + Masculinity-Femininity (.514) — Social Introversion (.314)	163	.50	82	.50
MMPI	f	*Science* = 49.637 + Hysteria (.443) — Masculinity-Femininity (.710) — Schizophrenia (.341) + Mania (.849) + Social Introversion (.397)	82	.52	33	—.26
		Humanities = 52.498 — Hypochondriasis (.490) — Depression (.303) + Masculinity-Femininity (.237) + Psychothenia (.441) — Mania (.430) — Social Introversion (.140)	82	.45	33	—.02
MBTI	m	*Science* = 58.685 — Extraversion (.662)	61	.21	—	—
		Humanities = 28.743 + Intuition (1.024) + Perception (.734)	61	.36	—	—
MBTI	f	*Science* = none of the variables significant	61	—	—	—
		Humanities = 62.304 — Sensation (.480) — Thinking (.917)	61	.29	—	—
OPI	m	*Science* = 43.283 — Thinking Introversion (.256) + Theoretical Orientation (1.057) — Impulse Expression (.145) + Social Introversion (.122) — Lack of Anxiety (.327)	209	.46	105	.50
	m	*Humanities* = 56.535 — Theoretical Orientation (.481) + Autonomy (.425) — Social Introversion (.294)	209	.41	105	.26
OPI	f	*Science* = 61.729 — Thinking Introversion (TI) + Theoretical Orientation (.976) — Estheticism (.365) — Nonauthoritarianism (.671)	105	.53	52	.10
	f	*Humanities* = 56.375 + Thinking Introversion (.393) — Theoretical Orientation (.759) + Developmental Status (.181) + Masculinity-Femininity (.279)	105	.54	52	.32
SVIB	m	*Science* = 63.892 — Artist (1.040) + Architect (.350) + Physicist (.712) — Policeman (.462) — Purchasing Agent (.392)	180	.63	90	.47
	m	*Humanities* = 51.575 + Dentist (.303) + Aviator (.223) — Mathematics-Science Teacher (.778) — Senior CPA (.304)	180	.63	90	.62
SVIB	f	*Science* = 70.161 — Librarian (.340) — Stenographer (.642) — Secretary (.642) + Business Education Teacher (.588) — Life Insurance Saleswoman (.723) + Lawyer (.373)	67	.61	36	.26
	f	*Humanities* = 54.024 + English Teacher (.310) + Dietitian (.551) — Mathematics-Science Teacher (.670) — Office Girl (.232)	67	.62	36	.32

Note—The equations for the SVIB were based on standard scores; raw scores were used for all other equations.

Table 3. Personality Characteristics Associated with Educational Choice

Category	*Characteristics of S scoring high on the science equation*	*Characteristics of S scoring high on the humanities equation*
Orientation to life	Prudent, conventional, energetic. Preference for overt action and a tendency to evaluate ideas on the basis of their practical and immediate application.	Participant, ambitious, wishes to do well and succeed in life. Emphasis on personal gain and pleasure, self-centered. Values personal independence, seeks freedom from rules and constraints.
Mode of adjustment and psychological functioning	Relatively free from self-doubt and disillusionment. Has sense of well-being. Tends toward strict control of impulses, denial of emotional.	Given to complaining about physical and psychological status. Impatient, demanding, difficulty in binding tension. Emotionally expressive, anxiety-prone.
Interest pattern	Restricted range of interests. Unlikely to venture into new and different pursuits. Interested in science and scientific activities, but little interest in people and social contacts.	Wide range of interests, e.g. literature, philosophy, art, religion, etc. Responsive to and involved in current, social, and political affairs.
Behavior in social context	Not assertive or dominant. Reserved, retiring, socially introverted, not spontaneous.	Ascendant, persuasive, clever, and imaginative in social situations. Outspoken, gregarious. Seeks social contacts and gains satisfaction from them.
Mode of cognitive functioning	Preference for logical, precise analyses. Problem-solving approach. Impersonal and critical habits of thinking. Values form and structure.	Preference for imaginative, even autistic thinking. Intuitive approach. Personal and subjective in evaluating ideas. Cathects innovation and ambiguity.

can be compared. The main purpose of this follow-up study is to further "cross-validate" the equations reported in this paper on seniors and to compare freshman and senior personality characteristics in these predictions.

To gain further clarity on these students' experience, satisfaction, and ability in their majors, a questionnaire was developed which was also administered to the students in the follow-up study. The results of this questionnaire will permit more subtle analyses, for example, contrasting students who are satisfied with their majors with students who are disappointed with their majors. Additional data from the questionnaire, such as factors of influence in choosing the major, parents' education, and vocational fields will further clarify the process of educational choice.

The most significant conclusion that may be drawn from the present results is that particular personality patterns are indeed associated with educational choice.

A possible consequence of persons of like characteristics choosing similar academic fields may be the eventual separation of these fields. That such may already be the case has been argued by Snow (1959). He believes that educational emphasis and biases have fostered a division between the sciences and the humanities which has served to hinder meaningful communication and understanding. Our findings partially support these contentions, but suggest that the split between the "two cultures" is the result of deep-seated and long-standing personality differences rather than that of attitudes or educational orientation.

A more appropriate conceptualization which our data supports and which warrants more optimism than Snow's formulations would permit is that a third force, namely the social sciences, exists. The social sciences may bridge the gap between the two cultures in that they share characteristics with the sciences and the humanities. While dealing with people and their cultural heritage, the social sciences are increasingly using technological and scientific methods to resolve human problems. It is hoped that our ongoing research and other projects will shed further light on these questions.

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A Psychological Study of Eminent Psychologists and Anthropologists, and a Comparison with Biological and Physical Scientists

Anne Roe

This is the third . . . in a series of clinical studies of research scientists. The series of studies was designed to investigate the existence of relationships between life history, intellectual functions or personality characteristics, and the selection and pursuit of a particular science as a profession. This has been the first series of its kind in this field, and hence the major approach has had to be observational and diffuse. In so complex a problem, the first need is to get some idea of the nature of the relationships, if any exist, the points at which a direct attack can be made, and the sort of tools to use. It was felt that no existent personality theory was sufficiently developed, or generally suitable for the derivation of hypotheses in advance. . . .

The subjects of the study are men who were selected for their eminence in research, as judged by their peers. The data comprise verbatim life histories, discussion of the work of the men, and results of three tests, a Verbal-Spatial-Mathematical Test, the Thematic Apperception Test, and the Rorschach. . . .

SELECTION OF THE SAMPLE

The sample restrictions imposed in the previous studies were also observed in this, so that the subjects are all men, under 61 years of age, American born, and currently engaged in active research. . . .

Although the data for psychologists and anthropologists are presented separately, these groups are combined for comparison with previous studies and together referred to as the social scientists. . . .

Excluding from each group those who were unable to cooperate because of illness or geographical location, the percentages of refusals were: biologists, 13%; physicists, 27%; social scientists, 22%. The higher rate of refusals among the physical scientists is, I believe, in considerable measure due to their heavier schedules.

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DESCRIPTION OF THE GROUP

The psychologists have an average age of 46.7, the anthropologists 49.4; for all social scientists the average age is 47.7. The physicists averaged 44.7 years and the biologists 51.2. . . .

Occupation and education of the fathers of the subgroups show that half of the psychologists and three of the anthropologists had professional fathers. Only 4 of the 14 psychologists came from families with rather good incomes, but 7 of the 8 anthropologists came from families that were well-to-do. Incidence of professional fathers in the other groups was 45% for the biologists and 73% for the physicists (experimentalists 50%; theorists 84%).

Number of children in the parental family and birth order of these subjects are given in Table 1. The distributions are similar to those for the biologists and physicists, and birth order for the total group of 64 scientists is also given. Comparison of the observed number of first-born with the calculated expected number shows the incidence of the first-born in these groups to be reliably greater than chance ($p < .01$). Cattell and Brimhall (3) also remarked on the high incidence of first-born in their sample of 855 scientists. For their group it was possible to check incidence of first-born in each family size from 2 to 7 and in all of them it proved to be greater than chance.

Of the 25 scientists in my group who were not first-born, 5 are oldest sons, and 2 of the second-born were effectively the oldest during their childhoods because of the death of older sibs, one at birth, one at age 2. Complete data are not available for 3, but for the others the average number of years between the subject and his next older brother was 5. The possible significance of this will be discussed later.

Marital Status

All of these men are married and most of them have children. . . . Average age at marriage of all three groups of scientists is rather late, which is doubtless in part connected with the long educational histories. The social scientists, however, differ greatly from the others in the permanence of their marriages. Among the biologists, there have been three divorces (15%); among the physicists, one (5%); but five of the psychologists (36%) and four of the anthropologists (50%) have been divorced, and of these several have been divorced more than once.

That the psychologists and anthropologists have a smaller average number of children than the other scientists may be related to the difficult marital histories, but the differences are slight. The sex ratios of the children are quite different in the different groups of scientists but this is probably chance: daughters are much more numerous for the physicists, sons for the biologists; for the social scientists, the difference is slight. . . .

EARLY HISTORY RELEVANT TO OCCUPATIONAL CHOICE

One of the striking differences between the social scientists and the others is the

Table 1. Number of Children in Parental Family and Birth Order of Subjects

No. of children, including subjects	*No. of subjects*		*Position in family*	*No. of subjects*		
	Psych.	*Anthro.*		*Psych.*	*Anthro.*	*All scientists*
1	3	3	1	6	5	39
2	1	2	2	3	2	13
3	5	2	3	2	0	3
4	4	0	4	3	0	3
5	0	0	5	0	0	2
6	1	1	6	0	1	2
			7	0	0	2
Average	3.0	2.4				
Average	2.8					

amount of material which was spontaneously offered in the interview. In part this may be due to the greater understanding among this group of the general problem and the relevance of details of personal history, but in perhaps larger part it is a reflection of their greater ease of verbalization. . . .

The Psychologists

This group came from lower to upper-middle-class backgrounds and the economic level varied from quite poor to well-to-do. Many of them had feelings of apartness relative to themselves or their families but it is rare for these to be colored with inferiority feelings. More than half of them had some definite sense of personal or family superiority, and family concern with social status, in one way or another (as striving, as recollection of striving in the parental generation, or as consciousness of belonging to the "best people").

The earliest vocational decision for any of this group was the sophomore year in college, and over half did not decide until after they had graduated from college, several not until they were part way through graduate school. Their earlier interests were oftenest English literature, although some had social interests and some began in chemistry or engineering. The final deciding factor was often the experience of doing research. . . .

Four suffered the loss of their fathers (at 8, 12, 14, and 17 years), one also his mother, but there were no homes permanently broken by divorce or separation. . . .

Inquiries about health during childhood and later uncovered a number of problems, of varying sorts. There is no record for one. Five stated their health had always been good but one of these developed migraines when about 25 and another developed a number of allergies in later life. Two have had hearing difficulties from an early age, and two have had some eye problems which have been corrected. None of these problems seems to have been an important early factor.

There were 4 who had quite serious problems which were constitutional. Two of these were abnormally small and 2 were abnormally tall. . . .

The Anthropologists

The average economic level of the anthropologists is clearly higher than that of the psychologists, and concern with the social status of the family or a firm conviction of the social superiority of the family is evident in all but one instance. This did not always result in the development of a definitely socially snobbish attitude in the subjects, but there is good evidence that most of them did consider themselves superior in one way or another. All but two of them went to private schools, either elementary or secondary, and this would certainly tend to foster these attitudes.

Two of this group decided on their vocation in high school, three in the later years of college, and three after graduating from college. Their earlier interests were somewhat varied. Four mention outdoor and athletic interests as having been of importance and a factor in the choice of a profession involving field work. Two with natural history interests, one with an interest in science generally, and three with interest in the classics could continue to find these interests in their profession, as could those whose earliest professional interests were sociology and law. Six of this group at one time or another had a special interest in literature and writing. . . .

Health during childhood and adolescence was apparently good for only three of them. Another had good health until an attack of rheumatic fever during secondary school, with some sequelae, which have not interfered in his field work. There are five who apparently had constitutional difficulties. Three were underweight or undersized, and in addition one of these had a number of allergies and the other had a number of serious illnesses, sufficient to have affected his early schooling. One was oversized. . . . Another was always the tallest in his age group which sometimes gave rise to awkward situations. . . .

As in the case of the psychologists, the importance of the discovery of the possibility of doing research as a factor in choice of vocation is clear.

Comparison with Other Scientists

Although there is not much difference in the general socioeconomic background of the different groups of scientists (except for the subgroup of theoretical physicists, 84% of whose fathers were professional men, as contrasted with about 50% in each of the other groups), there does seem to be a difference in their social attitudes. Among the biologists and physicists I encountered no direct expression of feelings of personal superiority, and there were very few by inference. One of the physicists did say that the family considered themselves extra privileged in spite of their extreme poverty; there are a few others who probably had some vague feelings of family superiority on one basis or another, and there are some who were conscious of their intellectual superiority, but they seem not to have translated this into social terms. . . . Among the social scientists, in at least half of the psychologists and in most of the anthropologists, a feeling of social superiority has definitely played a role in their development. . . .

In the matter of early interests (the term refers to spontaneous activities) this group differs markedly from the physical scientists, almost all of whom displayed early interest in mathematics, chemistry, physics, or gadgeteering, and very few of whom were ever interested in literature or the humanities. Two of the psychologists and one anthropologist began in chemistry but quickly shifted. Literature and the classics, and less frequently social welfare interests, were common among both anthropologists and psychologists, as were some natural history interests, particularly among the anthropologists. The biologists included men whose early interests had been in natural history, in literature, and in chemistry or physics, although the latter interest seems to have been aroused largely because these were the only sciences available in high school. In the histories of the social scientists and of the biologists the importance of the discovery of the possibility of doing research is highlighted, and this was often the factor that gave the final determination to their choice of vocation, or that fixed them in it once it was chosen. This particular aspect did not appear among the physical scientists, but this may well be because the difference between gadgeteering and experimental work is really a matter of degree and emphasis; the possibility of doing things yourself is obvious, whereas in the other fields it is not. It would seem that this may be an indictment of the pedagogical techniques in general use.

For the total group of scientists the median point of decision on a vocation is in the later undergraduate years, but it can be as early as high school and as late as postgraduate years. It is later for the social scientists and particularly for the psychologists. Psychology is encountered late in school, and lacks the popularization given King Tut and other archeological stories, and the adventure aura. But very few in the total group did any long range vocational planning.

Among the biologists, 5 lost father or mother before the age of 10, and the parents of two others were divorced (when the subjects were 9 and 16). Among the physicists, 5 lost a parent by death (at ages 5, 6, 9, 15, and 17) and the parents of one were divorced. There was only one divorce among the parents of the social scientists (and they remarried), but the mother of one anthropologist died at his birth and 4 psychologists lost their fathers by death (at 8, 12, 14, and 17) and one also his mother at 17. In the case of the biologists and physicists where the losses occurred very early, it seemed possibly to be a factor in the acceptance of isolation by the subjects, but among the psychologists and at least one of the physicists whose losses were later, the effect seems to have been more one of increasing the problems of adolescent reaction to authority, and this effect seems to have been greater in the case of the psychologists who have been more concerned with personal relations from the start.

A special factor, occurring generally only in the theoretical physicists, was the apparent effect to severe childhood illnesses which contributed to personal isolation. In all of the groups there are a number who had developmental problems related to constitution—abnormalities of size or general weakness. . . .

PSYCHOSOCIAL DEVELOPMENT

It is clear from interview data that patterns of family climate and discipline involving overprotection and firm, if not overt, control are very common in the group. They are commoner among psychologists than among anthropologists among whom there was more overprotection and more open hostility. Over half of this group reacted with more rebelliousness than is generally usual, and of these a number are still angry or rejecting or disrespectful of one or both parents.

Comparison with Other Scientists

The data on intrafamily relations are more complete than for the other groups, partly because of the fact that these groups are professionally more aware of the possible significance of such relations and are generally freer in such discussions. But there is additional, if inferential, evidence from the TAT protocols, and I think there is no doubt that the groups do differ in these respects. Both the physicists and the biologists early developed ways of life which involved very much less of personal interaction, and neither group shows anything like the extent of rebelliousness and family difficulty that the psychologists and anthropologists show.

There are also many more in the other groups who were isolates as children, or who had only one or two close friends, and the age of beginning heterosexual interests is very different. Among the biologists and physicists it is rare for there to have been any extensive dating in high school or early college. Half of the social scientists began dating in high school and dated happily and extensively from then on. . . .

THE VERBAL-SPATIAL-MATHEMATICAL TEST

This test (VSM) was compiled for the study by the Educational Testing Service. The verbal test contains 79 items in two sections, in each the task being the selection of antonyms. Time limit was 15 minutes for the two sections. The spatial test comprised 24 items, with a time limit of 20 minutes. The task was to select from four stimuli, the two views of the same figure. The mathematical test comprised 39 items, of mathematical reasoning. Time limit was 30 minutes. Examples are given in the earlier publications (13, 15).

Results are given in terms of sigma scores, in Table 2. The scores for the psychologists are based on the distribution of psychologists, for the anthropologists on the distribution of anthropologists. . . . The difference in means between psychologists and anthropologists is not significant for the verbal test, but t for the difference between means for the spatial test is 6.88 and $p < .01$; for the mathematical test, t is 6.68 and $p < .01$.

All but one of the experimental psychologists has a higher sigma score for either spatial or mathematical than for verbal. Two of the others have their highest scores on the spatial test and two on the verbal, the difference in one instance being very slight. No tendencies are evident among the anthropologists. . . .

Table 2. The Verbal-Spatial-Mathematical Test Sigma Scores

	Psychologists				*Anthropologists*		
Subject	*V*	*S*	*M*	*Subject*	*V*	*S*	*M*
Ps 1	+0.32	—0.55	+1.10	An 1	+0.57	—0.04	—0.67
Ps 2	—0.21	—0.55	+0.80	An 2	+0.77	+0.84	—0.06
Ps 3	+0.17	—0.78	+1.25	An 3	+1.22	—1.15	+1.15
Ps 4	+0.24	+0.41	—0.53	An 4	+0.26	—0.49	+0.85
Ps 5	—1.83	—0.07	—0.90	An 5	—0.14	—0.71	—1.58
Ps 6	+0.39	—1.26	—0.90	An 6	+0.55	+1.51	+0.24
Ps 7	+0.14	+1.36	—0.08	An 7	—2.03		
Ps 8	—2.05	—0.07	—0.68	An 8	—0.12		
Ps 9	+0.54	+0.89	+1.54				
Ps 10	+1.13	+1.84	+1.69				
Ps 11	+0.17	+0.65	—1.12				
Ps 12	+0.32	—1.02	—0.53				
Ps 13	+0.84	—1.50	—0.90				
Ps 14	+0.61	+0.65	—0.53				
Mean	57.7 ± 3.6	11.3 ± 1.1	15.6 ± 1.8		61.1 ± 3.2	8.2 ± 1.8	9.2 ± 1.4
SD	13.5 ± 2.5	4.2 ± 0.8	6.8 ± 1.3		8.9 ± 2.2	4.5 ± 1.3	3.3 ± 0.9

Comparison with Other Scientists

Table 3 presents the material for comparison with the other groups studied. The mathematical test was not difficult enough for the physicists. Differences between the means of the different groups are small and not significant. It should be noted that there is a large difference between the subgroups of physicists on the verbal test, the experimentalists averaging 46.6 and the theorists 64.2. On the spatial test, their averages are 11.7 and 13.8 respectively. If comparison is made by analysis of variance for five groups, experimental physicists, theoretical physicists, biologists, psychologists, anthropologists, *F* approaches the 5% level, even in these small groups. . . .

Table 3. Comparison with Other Scientists on the Verbal-Spatial-Mathematical Test

Test	*Biologists* ($N = 19$)	*Physicists* ($N = 18$)	*Social scientists* ($N = 22$)
Verbal			
N right, range	28–73	8–75	23–73
Mean	56.6 ± 2.8	57.3 ± 4.1	59.0 ± 4.2
Spatial			
N right, range	3–20	3–22	3–19
Mean	9.4 ± 1.0	13.0 ± 1.2	10.4 ± 0.9[*]
Mathematical			
N right, range	6–27		4–27
Mean	16.8 ± 1.4		13.7 ± 1.5[*]

[*] $N = 20$.

It is clear that the average ability of the scientists is very great. This is not surprising. On the other hand, it is surprising, and a matter of very considerable importance, that there are among the scientists a number who are not facile at the types of tasks presented by the VSM, but who have been able to make contributions of great value to society.

THE THEMATIC APPERCEPTION TEST[1]

The TAT is difficult to handle as a research instrument, since the scoring is not well codified, but it supplements the interview material elegantly for individual analysis. As in previous studies (11, 14), I have again followed Wyatt (17). . . .

. . . Half of the stories of the anthropologists and a third of those of the psychologists do not give an outcome, or give an unclear one. In both groups a successful outcome is predicted about one third of the time. Defeat and unsolved tension make up the remainder in about equal amounts. But of the stories to which an outcome is given, the anthropologists have a higher degree of certainty in their statements. . . .

In regard to the general tone of the stories, ones labelled unhappy, tense, and anxious are chiefly differences of degree, and incidence of stories in these groups is considerably higher for the psychologists than for the anthropologists, who have more recourse to the melodramatic and sardonic.

. . . For both groups, formal relations (e.g., father, son; husband, wife) are a little commoner than emotional ones (e.g., lovers, friends). There are some differences in presses, which do not quite reach significance at the 5% level (5 × 2 chi square table, with chi square 9.85). The anthropologists give more unfriendly and the psychologists more internal presses.

There are a few themes that seem to recur with considerable frequency, the most evident being one of general helplessness in the face of severe problems. This is sometimes very general and sometimes seems to be limited to the male figures in the stories in contrast to the female figures. There is also a considerable feeling of dependence on parent figures, and while there are some stories of successful rebellion without serious guilt feelings, there are more stories of characters who rebelled only with guilt and general unhappiness. At the same time there are a few who seem to have a strong sense of responsibility with regard to human relations.

The details on heterosexual relations are usually fairly full. This group of social scientists is not particularly conventional in its approach but is definitely much concerned with interpersonal relations and finds it relatively easy to verbalize them.

There are some among them who find contemplation of death a serious problem. In

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[1] Only 9 of the usual 20 cards in the series were used in this research. They are cards 1, 2, 4, 6, 7, 10, 13, 15, and 11, presented in that order, from the male series of 1943. The TAT was always given after the Rorschach.

two instances this concern with death may be a major factor in their professional activities.

Comparison with Other Scientists

. . . The average length of the responses to each card differs considerably, with the social scientists giving significantly longer stories ($p < .05$) than the other two groups, who do not differ materially from each other. This is in accord with the generally greater verbal productiveness of the social scientists and undoubtedly associated with the fact that more of them than of the other groups think verbally (12). The greater length of response may also reflect general testwiseness, but this is hard to check. There is practically no difference among the groups in the proportions of unusual stories or of unusual twists to common stories.

The full time range, which is significantly commoner among the social scientists (chi square for 3 × 2 table gives $p < .01$), may also be related to the social science group's willingness to verbalize at greater length. The biologists omit the past more than the others do. Description of the card or a story relating just to the immediate moment was the only response about a third of the time in all groups. The prodding possible in usual clinical practice is clearly inappropriate with these subjects.

There are no differences among the groups with regard to outcome—the proportions are remarkably similar. But there is a marked difference with regard to the certainty with which any outcome is predicted, the biologists being significantly more restricted in this regard than the others.

In all groups the major levels are concrete-factual and endopsychic, with the biologists giving more stories characterized by the former. This accords with other aspects of their general attitudes, such as their greater interest in form on the Rorschach and their generally better emotional control, or emotional flatness. There are no marked differences in tone, in personal relations, or in assignment of presses among the groups.

In these comparisons what is most noticeable is the great over-all similarity. However, analysis of content shows more striking differences. The biologists are the only group whose TAT protocols give any indication of particular meaningfulness to them of the paternal role. Both biologists and physicists are much less interested in interpersonal relations generally, and more inclined to handle them in distance-getting ways than are the social scientists, although many of these are uneasy about them. But the unease is of a different sort and a manifestation of a considerable concern with such relations, rather than a dislike for them. Both biologists and physicists show a considerable independence of parental relations, and without guilt, particularly in the case of the physicists, whereas the social scientists show many dependent attitudes and much rebelliousness, accompanied frequently by guilt feelings. The attitudes of helplessness so noticeable among the social scientists are much less common in the biologists and physicists. The biologists are definitely more restrained than the other two groups in their expression of aggressive attitudes; the social scientists are the freest in this respect.

What is most striking about these results, however, is the fact that the TAT rarely gives any indication that the subject is a man of considerable attainments. Sometimes, some amount of drive is shown, but for the most part this is not very evident in the stories, nor is there any clue in them as to what has made it possible for these men as a group to have achieved as conspicuously as they have.

THE RORSCHACH METHOD OF PERSONALITY DIAGNOSIS

The Rorschach was given and scored according to the directions by Klopfer and Kelley (6), and also by the Munroe Inspection Technique (9). The latter system makes the results easier to handle as a group and makes some allowance for variation in response total. . . .

Total number of responses (R) ranges from 10 to 186; the mean for psychologists is 66, for the anthropologists 69. These means are both very high, the usual total being 30 to 40 (2, 6). This group tends also to make more than usually rapid responses, time per response (T/R) averaging 33 seconds for psychologists and 39 seconds for anthropologists. Only one subject refused any card.

. . . There are no major differences between psychologists and anthropologists with respect to their use of locations in the blots, and a particular pattern seems to be characteristic of most of them. They produce an absolutely large, but relatively small, number of whole responses—they can deal with large concepts, can generalize adequately, if sometimes sweepingly, but are usually more interested in smaller, and less often noticed details. They are quite good observers, and tend to look at things which are not likely to strike most people. They are, however, quite casual and unsystematic in the way they go about things, sometimes to the extent of considerable disorganization in the approach. They are so productive, and so many responses occur to them so rapidly that they make no attempt to sort them out, nor do they need to rely upon any technique of procedure to stimulate further responses. . . .

All of the social scientists give an adequate number of popular responses. A number of them tend to considerable use of original responses, about which they are likely to be rather uncritical. Anatomy and sex responses are relatively common, and are used by almost half of the group to an amount considered excessive. The range of responses is a rather curious and interesting one, since it is customarily very broad in terms of number of categories, and at the same time may show stereotyping or restrictedness of some sort. This is more often in terms of particular individual perseverations of themes, but the restriction may be in terms of excessive use of animal and human responses. The wide range would be associated with their general productivity and must also indicate a pretty general receptivity. . . .

The content categories (human, animal, plant, geography, etc.) utilized by the two social science groups are very similar. A comparison of the number in each group using each of 26 different content categories yielded a rank correlation of + .88. . . .

From the quality of form responses it can be seen that psychologists are generally less concerned with what may be called external reality than they are with inner realities (e.g., motivation) and must often disregard the apparent reality and search further. (Is this rationalization?) This may be less true of anthropologists. Whatever the explanation, the fact is that social scientists are relatively unconcerned with formal qualities. This is also shown by the 15.1% average of responses which are not dominated by form.

. . . There are only scattered entries for excess of any particular variety of shading responses and these are not of special importance in the group picture. These groups seem to have fairly effective techniques for handling anxiety. . . .

The use of human movement in these subjects would indicate consistent interest in persons, but an interest which has been frequently restricted in some way and which is sometimes carried to extremes. The subjects tend to excess, rather than deficiency, in any movement category, but not to overall excess in the whole movement area. It is not the picture that they are generally self-absorbed, but rather that they have considerable empathic capacity.

. . . In general the picture for the social scientists is a well-balanced one (*FC* is usually larger than *CF* and there are very few *C* responses). The group as a whole shows a fairly rich reactivity to immediate external stimulation, with good emotional control and without impoverishment of reactivity. In individual analysis the content of the color responses is also of importance. . . .

Comparison with Other Scientists

The Rorschachs from these 22 social scientists can now be compared with those obtained from the 20 biologists and the 19 physical scientists previously studied. . . .

The great difference in average number of responses between the social scientists and the others makes direct comparison for mean frequencies of little value, and most percentages are also so affected by total number of responses that they can also not be fruitfully compared. The $F + \%$ (the percentage of responses which are good form) and the non-*F* dominated responses (total of *mF*, *cF*, *CF*, *C*, etc.) as well as the ITS are not so affected. Analysis of variance of these scores gives the following results:

	F	*P*
R	13.33	$<.001$
$F + \%$	.52	.10
non-*F* dominated %	5.84	$<.01$
ITS	9.58	$<.001$

It appears that the social scientists are significantly more productive on the Rorschach; that the biologists use relatively fewer responses not dominated by form than the others; and that the biologists, by Munroe's measure, are definitely the best adjusted. The two latter differences are certainly related, since the nature of the adjustment shown by the biologists is one in which rational control and caution are emphasized.

In Table 4 are listed the checklist entries which show some differentiation among these groups. Only entries for which chi square has a p of nearly .05 or less are given. Although the checklist scoring is adjusted for length of the individual protocol, some of the observed significant differences are related to difference in length. Succession cannot be scored in short protocols, and range is more likely to be great in longer protocols. Where very few M are given no entry for restricted M (r) can be made. The contrasts between physicists and social scientists in use of W, between the social scientists and the others in use of M, and between the physicists and biologists in use of CF do seem to be sustained by this analysis.

Differences between the scientist groups in content categories can be expressed over-all by the use of rank correlations (on 24 categories). These are: biologists-physicists + .739; biologists-social scientists + .713; and physicists-social scientists + .769. These are not high. Major differences are greater use by biologists of the categories science, animal anatomy, and abstract; by physicists of art and design and emblem; and by social scientists of clothes and food.

Some qualitative differences may be indicated also. The biologists are the least freely aggressive; the social scientists, particularly the anthropologists, the most so, and with greater likelihood of oral elements. There are great differences between the biologists and physicists in their handling of anxiety, but the social scientists show no consistency in this respect.

In the over-all picture the similarities are greater than the differences. This is to be expected from the fact that there is considerable heterogeneity within the separate groups, and from the fact that these men are all functioning adequately. . . .

DISCUSSION

The direct study of eminent men raises numerous and very difficult problems. One clearly does not have the complete freedom of a biographer writing centuries after the lifetime of the subject. But these difficulties are more than compensated for by the value of direct clinical and test data. . . . However, something should be said about the limitations of the study.

In the first place the sample is small in absolute numbers although relatively very large. The subjects are the best research men

Table 4. Comparison of Checklist Entries for Different Groups of Scientists (3 × 2 tables, except as noted)

	Frequency for				
Entry	*Biologists*	*Physicists*	*Social scientists*	χ^2	p
N	20	19	22		
$W+$	3	8	4	12.02[*]	$<.01$
$W-$	0	2	6		
Suc, I or II	6	7	17	11.05	$<.01$
$O+$	2	1	7	6.13	$<.05>.02$
Range+	1	2	13	19.36	$<.01$
F, BV	3	7	11	5.76	$>.05$
Mr	6	2	10	6.00	.05
$M+$	1	0	5	17.37[*]	$<.01$
$M-$	10	9	2		
$CF+$	0	7	2	14.28[*]	.01
$CF-$	7	1	2		

[*] W, M, and CF are checked by 3 × 3 tables, the rows being +, no entry, and —.

in each field and they comprise a high percentage of the men who could be so designated. The conclusions drawn, however, apply directly only to the first-rate scientist, and only indirectly, and with some qualifications, to scientists generally. . . .

We lack comparable groups in nonscientific vocations. A more serious limitation is the lack of any control group of relatively unsuccessful scientists, men who had the training and appeared to have the promise, but who have produced little or not at all in research. This is the next most important step and a prerequisite to the satisfactory development of hypotheses about choice of science as a vocation and success in it. One cannot always be certain whether the situations noted in this study refer to choice of vocation or to success, or to what extent they are affected by high frequency of a middle-class socioeconomic background.

As in all research with people, the complexity of the situations encountered makes the determination of direct causal relations practically impossible. What has been accomplished, however, is not only the accumulation of test data on a hitherto practically unstudied group, but also the identification of situations which recur with high frequency.

It is evident that the family backgrounds of the 64 scientists studied are by no means randomly selected with respect to the population at large. According to census reports for 1910, only 3% of the gainfully employed men in the country were professional men. In this group, however, 53% of the fathers of the subjects were professional men. . . . None of the scientists came from homes in which the father was an unskilled laborer and none came from families of very great wealth. . . .

What seems to be the operative factor here is that in practically all of these homes, whatever the occupation of the father, learning was valued for its own sake. Its concomitants in terms of possible better income or social position were not scorned, but it was rare for these to be the most important. This certainly was a major factor in the facilitation of intellectualization of interests. In my opinion this, rather than the probable associated intellectual levels, is the important aspect here. "Overintellectualization" may be a middle-class characteristic and it may interfere with libidinal development in other spheres, as some psychoanalytic writers have pointed out. Yet it seems to me doubtful whether one can develop the sort of intense personal involvement which is characteristic of these scientists without some degree of this, if a channeling of energy in one direction means a lessening of it in others. There is a serious problem here. Unquestionably overintellectualization is frequently a technique for escaping emotional problems, especially those bound up in interpersonal relationships, but it is not necessarily so. I believe it is possible to concentrate upon intellectual activities without having a relatively sterile life emotionally, but we certainly have not developed educational techniques which foster this.

I have reported a greater than chance incidence of first-born among these eminent men. The problem of birth order is an extremely tricky matter statistically, and I would not be inclined to pay much attention to this finding in a group of 64 were it not that Cattell and Brimhall reported the same finding in a group of 855 scientists. It could be argued that the point here also is that intelligence levels are higher in the first-born, for which there is some evidence, but it seems much more probable that both of these facts are results of the same cause, whatever that is.

Certain aspects of the data offer evidence on the basic importance of the need to achieve, or to keep independence, which is so well met by a career in research. There are no Catholics in the group. The Protestant churches to which all but five of the scientists' families belonged have varying degrees of insistence on the authority of the church over its members' interpretations of life, but all but three of these subjects have dismissed organized religion as a guide and usually had done so by late adolescence. In this respect, also, they have achieved independence. The dearth of Catholics in research science is corroborated in other studies (8, 16) and the Wesleyan survey found that production of scientists from Catholic institutions is uniformly low (7).

In the life histories of many of these men there are factors which indicate a feeling of apartness from others which takes different forms and seems to have a number of different causes. Ten of these men suffered the death of a parent before they were 10 years old, 7 others in their teens. Among most of those whose loss occurred early, this was apparently a factor in the acceptance of isolation. For several of these men, this early loss appears to have had an indirect effect upon vocational choice. There may have resulted an intensified problem over the acceptance of the inevitability of death. Study of life processes and study of ancient civilizations (reassuring in the continuity of mankind if not of a man) may be a technique, and an effective one, for coping with this. But not every biologist has strong death fears, nor is every archeologist concerned with survival problems.

Among the theoretical physicists, there was a very high incidence of severe childhood illnesses which certainly contributed to isolation. It was only among the social scientists that this feeling of apartness characteristically carried a tone of superiority. With the other groups it appears to be sometimes inferior, but characteristically neither. It is a related fact that the social scientists do not show the type of psychosocial development characteristic of the other groups—that is, a pattern of general avoidance of intimate personal contacts, a considerably later than usual development of heterosexual interests, or at least of their expression, and even at the present time, a decided preference for a very limited social life.

The biologists and physicists show a considerable present independence of parental relations, and without guilt for the most part. This has also been noted in business executives (5). The social scientists, on the other hand, are much less free of parental ties, in the sense that a number of them still harbor resentment and rebellion, even though they have achieved an outward independence. It is more than possible that this difference is a major factor in the choice of vocation. An unresolved conflict over parental relations could as easily be displaced to a concern with personal relations generally, as an unresolved conflict over death could lead to study of living processes.

More of these men than not, as boys, pursued rather independent paths—playing with one or a few close friends, instead of with a gang, following their own particular interests (shifting or not) with somewhat more than the usual intensity. There are some to whom this does not apply, but it is fairly characteristic, and such interests were more often intellectual than not, except among the experimental physicists and biochemists. It is, of course, true that their high level of intelligence would, in itself, have some of these effects.

There is no one general pattern by which they approached science as a career. The modal age at which the decision was made was during the last two undergraduate years, but in some cases it was made in early childhood or as late as the second year of graduate work. The introduction may have been through natural history interests, through gadgeteering, through interest in laboratory science as found in high school courses, or, for the social scientists, through dissatisfaction with literature as a means of studying the behavior of people, or through a service motivation. When the decisive point can be determined it was usually the discovery of the possibility of doing research, of finding out things for oneself. For some this was understood very early—as with those experimental physicists who spent much of their childhood playing with erector sets, radios, and all the other sorts of equipment that permit manipulation and construction. For others, it came as a revelation of unique moment. Once it was fully understood that *personal* research was possible, once some research had actually been accomplished, there was never any question. This was it. The educational implications are obvious enough. There has been no question since. From then on, absorption in the vocation was so complete as seriously to limit all other activity. In the case of the social scientists, at least for those for whom people themselves provide the data, this did not limit social participation; for the others it intensified an already present disinterest. Although a few of them have cut down somewhat on their hours of work as they have grown older, it is still the common pattern for them to work nights, Sundays, holidays, as they always have. Most of them are happiest when they are working—some only when they are working. In all these instances, other aspects—economic return, social and professional status—are of secondary importance.

Being curious plays a major role—a trait which many aspects of our educational practice tend to discourage. It is of crucial importance that these men set their own problems and investigate what interests them. No one tells them what to think about, or when, or how. Here they have almost perfect freedom. Their limitations are only those of equipment and time, and the limitations of their own understanding. (It is certainly true that the free flow of their work can be inhibited by emotional problems, but I believe that this could be dealt with directly. It would be worth while to try.) Certainly this is one vocation in which man can most nearly approach what he can be, and one that satisfies both autonomous and homonymous drives (1)....

The question also arises as to why one subject chose one field of science and others chose other fields. Apart from the often overlooked matter of necessary contact with the field, there is some further evidence from the study. The problem of coping with early affectional loss has been mentioned. It would also appear that there are some, particularly among the experimental physicists, who seem early to have formed direct relationships with objects rather than people, not compensatorily. In others, a generalized anxiety, of unknown cause, and possibly only an exacerbation of normal anxiety, is alleviated by concentration on a particular field....

The social scientists stand apart as having been more concerned at an earlier age, about personal relations (or as being willing to tolerate this concern as such, without translation). This may reflect an unconscious uncertainty over the consciously felt superiority that characterized half of the psychologists and most of the anthropologists. It is also certainly related to their difficulties in freeing themselves from their parents. The other groups seem to have been able, fairly early, to work out an adaptation not nearly so dependent upon personal relations, but rather strikingly independent of them. Certainly psychology to some extent, particularly social psychology, and anthropology to a large extent, particularly cultural anthropology, offer an ideal vocation to the person whose conviction of personal superiority is not accompanied by asocial characteristics; they permit a somewhat Jovian survey of their own society as well as others, and maintain the social scientist in a state of superiority just because he is able to make the survey. (This accounts nicely for the observation that some rather paranoid indications in the test material are not accompanied by forms of paranoid behavior, except perhaps as regards their own colleagues.) The experimental psychologists are generally less concerned with people as people, although this is by no means true of all of them. The further observations that a conflict over dominance and authority is common in the group, and that in a number of their homes the mother was dominant indicate the possibility of difficulties in achieving masculine identification.

In this respect it would seem very probable that the physicists, particularly the experimentalists, were able to identify more easily with their fathers than the other groups and hence to follow comfortably a science which has rather more of a "masculine" tinge in our culture than the others do.

It must be pointed out that it is likely that the kind of person who has gone into social science may have had a biasing effect on the theories produced by social scientists, particularly with regard to the desirable or the mature personality. Practically all current psychological theory of development stresses strongly the central importance in any life of the richness of personal relations as a basis for "adjustment." But the data of this study demonstrate, and it seems to me quite conclusively, that a more than adequate personal and social adjustment in the larger sense of an adjustment which permits a socially extremely useful life and one which is personally deeply satisfying, is not only possible, but probably quite common, with little of the sort of personal relations which psychologists consider essential. Many of the biological and physical scientists are very little concerned with personal relations, and this is not only entirely satisfactory to them, but it cannot be shown always to be a compensatory mechanism (nor are compensatory mechanisms necessarily undesirable). It can also apparently be satisfactory to others who are closely associated with them. That divorces are so much commoner among the social scientists is of interest in this connection. Problems with masculinity and dominance must be important here; but also, where much more attention and emotion are invested, demands are certain to be greater and more specific, and hence failure commoner.

Another finding of considerable importance is the differences of imagery which are associated with the different fields of science, and which accord with and perhaps explain some of the test data. Briefly, the biologists and experimental physicists tend strongly to dependence upon visual imagery in their thinking; the theoretical physicists and the social scientists, to dependence upon verbalization or similar symbolization in theirs. Nothing is known about the development of these modes of thinking, but it seems probable that they were developed early (they are associated with father's occupation) and played a part in the choice of a science. Further, it was shown that those scientists whose preferred mode of thinking differed from that characteristic for their science also differed in some aspects of their early history, and in the things they did or the ways they went about their work. (This is good reason for not using such a factor selectively—their contributions have a special place). The domination of the formal qualities of the blots in the biologists' Rorschachs, which the others do not show, is in accord with this, as is the generally much more fluid verbalization of the social scientists.

Doubtless, also, some intellectual factors enter. So far as the test used is a measure of these, it is clear that the theoretical physicists surpass all other groups on both verbal and spatial tests. The experimental physicists are high on the spatial and relatively very low on the verbal test. Psychologists are at about the mean for this total group on all three. Anthropologists are high on the verbal and lowest on both spatial and mathematical. These patternings are probably of importance in selection of vocation—particularly the relatively low nonverbal abilities of the anthropologists and the relatively low verbal ability of the experimental physicists.

I suspect that the verbalization so characteristic of the social scientist has also exerted some bias on his activities. This is probably most obvious in the field of testing where the emphasis still remains on verbal tests, although other tests have come into general use. But psychologists, and educators who are probably much like them in this respect, are in a position which makes possible the operation of this bias to keep out of college many adolescents who are verbally inept but have other capacities of equal value to society, and for whom college could be important. This bias may have affected the development of techniques of teaching and of therapy. The effect in the first is obvious. In therapy it may well be a factor in the common insistence on verbalization of insights as essential to therapy. . . .

The range of test intelligence in this group is also of importance. All of the evidence confirms Cox's remark: ". . . high, but not the highest intelligence, combined with the greatest degree of persistence, will achieve greater eminence than the highest degree of intelligence with somewhat less persistence" (4, p. 187). Portenier noted that "It would seem then that while there is a positive correlation between psychological test ratings and honor awards, the honor recipients are not limited to students with high psychological test scores, and many students who make high tests scores fail to win honors" (10, p. 499). Clearly a certain degree of intelligence is a necessary condition for a career in research science, but it is not a sufficient one.

The strength of the achievement drive which these men have shown is rarely reflected in the TAT in any direct way, and there are a number of Rorschachs which give no indication that the subject is capable of great accomplishment. Indeed there are a number of subjects for whom none of the test material would give the slightest clue that the subject was a scientist of renown.

There are Rorschach protocols which which would occasion no surprise in a clinic for the maladjusted. It is certainly true that those who work only with persons whose lives show considerable disruption seem to have no idea of the extraordinary range of tolerance of difficulty which "normals" show. A number of these men are particularly good examples. It should also be pointed out that for many of these subjects, the career itself has served as a technique for handling the personal problems. In some instances the basic problem has been, in a sense, extrapolated into a more general one, and the subject has then settled down to working on the general problem. This is a very neat and effective method. In other instances, absorption in the career has made possible the encapsulation of the difficulty in such a manner that it can be almost ignored by the subject. The price he may pay for this is another matter. There is nothing in these data to suggest that any measure from these or other projective techniques, or from intelligence tests, would be nearly so adequate in predicting their success as the fact that they worked long hours in graduate school, many more than the course reqirements, and that they preferred to work on their own. (But I do not know how many less successful scientists have worked hard and preferred independence.)

Most of these subjects were fortunate enough somewhere along the line to have found a teacher who induced them to find things out for themselves, or who let them do so, or who insisted that they do so because he did not want to be bothered. Once intellectual independence was really tasted, nothing else mattered much pedagogically; bad teaching then was only an irritation. But how many are there who have never learned to rely upon themselves, to find how valid their own thinking may be? Certainty of his own worth is any man's greatest need. Though some of them may find it only there, scientists do find this certainty in science.

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Personality Development and Vocational Choice of Clinical Psychologists and Physicists

M. David Galinsky

Studies by Segal (1961) and Nachmann (1960), utilizing psychoanalytic theory and job analyses, have shown that testable hypotheses about the relationship between personality and vocational choice can be generated, tested, and confirmed. In both of these studies hypotheses derived from psychoanalytic theory were tested. Segal studied creative writers' and accountants' current personality structure by means of projective tests, while Nachmann compared the life histories of lawyers with those of social workers and dentists, employing biographical interviews. Roe (1957) has offered some provocative speculations about the relationship between early experience, personality and vocational choice.

The study is grounded in Nachmann's assumption that occupations provide a variety of opportunities for impulse expression, utilizing defenses, and for organizing one's dealings with the world, as well as the further hypothesis that developmental experiences play an important part in predisposing one to choose a particular occupation. One would, therefore, expect to find that individuals who choose one vocation would have had different developmental histories from those choosing a vocation with rather dissimilar characteristics.

Working within the framework of psychoanalytic theory, specific hypotheses about the relationship among life history, personality, and occupations were developed in the following manner. The first step in the process was to become familiar with the demands of various occupations as well as the gratifications provided by them. Next, thinking in terms of psychoanalytic theory one could map out the personality traits which would be compatible with particular job demands and gratifications. Lastly, it was possible to cast hypotheses about the probable development experiences of individuals who choose various occupations.

THEORETICAL DEVELOPMENT

The present study grew out of an attempt to understand the relationship between curiosity and occupational choice. According to psychoanalytic theory, early sexual curiosity is the prototype for all later curiosity. There appear to be two elements in what has been called sexual curiosity—(1) curiosity about the body, and (2) curiosity about interpersonal relations—e.g. what things do parents do that the child does not understand and has difficulty finding out about. This study is concerned with curiosity about interpersonal relations, which is said always to be aroused in the course of development. Three fates are possible for such curiosity. As the child develops he (1) may have experiences which keep alive and further focus his interest on the area of interpersonal relations, (2) may have his curiosity channeled into other areas (displacement), or (3) may have experiences which lead him to abandon all attempts at having his curiosity satisfied.

Clinical psychology is an occupation in which curiosity about interpersonal relations is an integral part; Physics is one in which curiosity about the structure of and relationships among elements of the physical world is a major focus of interest. People who hope to find job satisfaction as clinical psychologists would be expected to have had experiences which focused their curiosity on interpersonal relations and left that curiosity relatively undisplaced. On the other hand, the experiences of physicists served to channel their interest into another area via displacement. Therefore, in their childhoods, clinical psychologists must have had their curiosity treated with greater tolerance by their parents, and have been disciplined much less rigidly when they persisted in being curious beyond the limits of parental toleration. Physicists, on the other hand, must have had their curiosity about interpersonal

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relations rebuffed, but have had the displacement of their curiosity to another area facilitated by greater intellectual stimulation in the home.

In a similar way other job requirements of the two occupations—requirements such as the ability to tolerate emotional expression of others, the capacity for nurturance, the necessity for working with people or for working alone—were considered in terms of the possible developmental experiences which would lead to adult compatibility with such job expectations.

Out of this occupational analysis there were developed hypotheses to be tested about differing developmental experiences in the two groups. The actual hypotheses tested will be stated in the results section.

SUBJECTS AND PROCEDURE

The subjects (Ss) for this study were 40 male graduate students in physics and clinical psychology at the University of Michigan. As was originally planned, one criterion for inclusion was that subjects must have completed their requirements for the Master of Arts or Science degree, but because there were not sufficient numbers available at that level, eight Ss with lesser amounts of training were included. Again because of the limitation of possible Ss, 12 Ss who were less than maximally satisfied with their choice of vocation were included in the sample.

The two groups of Ss were equated for social class of parents, which was measured by the method suggested by Allinsmith (1954), based on a weighted combination of father's occupation and education. Although it is not known whether equating for social class in the samples accurately reflects the distribution of social class in the population from which the samples were drawn, it was decided to equate the samples for social class lest the results be attributable to factors other than those which have been hypothesized. The groups were also equated for religious backgrounds of their parents, with the same numbers of Protestants, Jews, and Catholics in each group (listed in order of frequency). Here it is known that the sample does not accurately reflect the population distribution. In the population of physicists there is a much smaller percentage of Jews than in that of clinical psychologists. However, for the same reasons given for equating the samples for social class, it was decided to equate for religion.

The investigator was the interviewer in each case. The modal time of the interviews was about forty-five minutes. The interview was composed of a number of specific questions, each of which was asked in all cases. All of the interviews were tape recorded and verbatim written transcripts were prepared.

A coding system was devised from the predictions subordinate to each hypothesis in order to provide data in the form appropriate for the testing of those predictions. The coders worked from the transcripts of the interviews. In some instances the coder had only to attend to S's answer to a particular question, while in other cases he was required to form an impression from a number of S's replies. There were three coders employed in order to measure reliability. Coder 1 agreed with coder 2, 92 per cent of the time; coder 2 agreed with coder 3, 90 per cent; and coder 1 with coder 3, 90 per cent.

The chi-square test of independence was used for the analysis of the data. One-tailed tests of significance were used since the direction of differences was predicted in advance. Only differences at or beyond the .05 confidence level were considered significant.

RESULTS

Hypothesis I. As children, clinical psychologists had more opportunity to be curious about interpersonal relations than did physicists.

The specific predictions on the basis of which this hypothesis was tested are presented in Table 1. It may be seen that each prediction touches a separate but related facet of the development of concern about interpersonal relationships. This hypothesis was supported by the confirmation of all of the predictions that relate to it. Both from the side of hearing about the behavior of people outside the family as well as from having the opportunity to observe emotional interaction within the family, clinical psychologists had greater opportunity to be curious about interpersonal relationships than physicists did.

The results pertaining to the remaining

Table 1. Distribution of Experimental Groups with Regard to Interpersonal Curiosity

		Occupational group		
Item and prediction	*Response*	*Clinical psychology*	*Physics*	*Chi square*
The behaviors of people (a) were or (b) were not discussed frequently in the home. Clinical Psychology higher on prediction (a).	a b	14 6	2 18	12.60†
Emotional expression (a) was or (b) was not characteristic in the family. Clinical Psychology higher on prediction (a).	a b	13 7	3 17	8.44*
Frank discussion of people's behavior (a) was or (b) was not characteristic of family conversation. Clinical Psychology higher on prediction (a).	a b	9 11	0 20	9.18*

* $p < .01$.
† $p < .001$.

hypotheses will be summarized. The summaries are based on the outcomes of the testing of a number of specific predictions subordinate to each hypothesis as was illustrated in Table 1.

Hypothesis II. Fathers of physicists were more dominant and more clearly masculine figures than fathers of clinical psychologists.

The predictions dealt with the father's role as decision-maker, breadwinner, authoritative person. None of the predictions was confirmed, although all of the results were in the predicted direction. However, the picture that emerges is of the fathers' having been seen as masculine figures in both groups.

Hypothesis III. Mothers of clinical psychologists will be described as the dominant parent more frequently than physicists' mothers.

This hypothesis may be viewed as the obverse of Hypothesis II. Therefore, it is not now surprising that the predictions relating to it were not confirmed either. In both groups, Ss viewed their mothers as being authoritative about as frequently as they saw them as passive. It would appear from these data that maternal dominance in the family is unrelated to the choice of occupation of physicists and clinical psychologists.

Hypothesis IV. During childhood clinical psychologists had closer and warmer relationships with their mothers than did physicists.

The evidence was generally confirmatory of the fact that clinical psychologists had warmer relationships with their mothers than physicists did. However, it should be pointed out that the confirmation was based on inference. The data clearly indicated that mothers of clinical psychologists were the more expressive and demonstrative. Although it seems likely that demonstrative and emotionally expressive mothers have closer relationships to their sons than undemonstrative ones do, this hypothesis was confirmed only to the extent that the variables of demonstrativeness and close association between mother and child are related to one another.

Hypothesis V. Physicists more than clinical psychologists took their fathers as identity models (role models).

Only one of three predictions was confirmed, although all of the results were in the predicted direction. Coding was difficult for the prediction that the physicist's choice of occupation was more frequently similar to his father's in part because virtually all of the subjects were working toward occupations which required a far greater amount of training than their father's work necessitated. From answers to a question related to the second prediction, one got the impression that the most frequent first response of clinical psychologists was that they saw themselves as being similar to neither parent, while physicists usually settled on one parent or pointed to characteristics of both parents that they saw as also being present in their own make-up. Although the specific prediction was not confirmed, it would seem that physicists see themselves as having some continuity with their parents, while psychologists do not. Coupling this with the confirmation of the last prediction that physicists more frequently describe their fathers as persons to emulate leads one to conclude that in some not clearly specified way the physicist sees himself as wanting to follow in his father's footsteps, while the clinical psychologist does not.

Hypothesis VI. The home environments of clinical psychologists were less conventional than those of physicists.

None of the predictions which dealt with religious and political views was confirmed. While it was originally thought that conventionality was more of an issue for the clinical psychologist than for the physicist, it was also noted that anyone engaged in the enterprise of discovery must be free of rigid adherence to conventional ways of viewing the world. The physicist, then, was not seen as really being opposite to the clinical psychologist with regard to this hypothesis. Therefore, it is not surprising that this hypothesis was not confirmed.

Hypothesis VII. Physicists received more intellectual stimulation from their families than did clinical psychologists.

Two of the three predictions relating to this hypothesis were confirmed. In describing their parents, physicists more frequently characterized them as having intellectual interests. Intellectual stimulation appears, then, to have been provided the physicists by the behavior and example of their parents. In the second place, intellectuality was emphasized more directly in physicists' families in that family conversations turned more frequently to intellectual matters than in clinical psychologists' families. While such discussion had its positive features and stimulating aspects, it may also have served to avoid dealing with more personal matters which was the concern of Hypothesis I. A third prediction dealing with reading as a family custom was not confirmed.

Hypothesis VIII. Discipline of physicists was rigid, stressed obedience, and was meted out by their fathers; while discipline of clinical psychologists was flexible, stressed appeal to feelings and was meted out by their mothers.

There are three separate issues involved in this hypothesis: (1) the focus of discipline, (2) the degree of consistency of discipline, and (3) the source of discipline. The predictions relating to the first two of these were confirmed. Appeal to feelings as a means of discipline was reported by a majority of the clinical psychologists, but was mentioned by virtually none of the physicists. On the other hand, knowing and following the rules was more frequently stressed in physicists' discipline. Secondly, the discipline of physicists tended to be consistent and predictable, while the clinical psychologists' was more frequently found to be unpredictable. With regard to the third issue, the prediction that physicists' fathers were more frequently chief disciplinarians in the family was not borne out, although the results were in the predicted direction.

Hypothesis IX. Physicists had fewer but less stormy relationships with peers than did clinical psychologists.

Three of the four predictions stemming from this hypothesis were confirmed, supporting the original contention that physicists had few relationships with people throughout childhood and tended to spend much time by themselves, whereas clinical psychologists related to people a good deal, but more frequently had relationships that were conflictful. The prediction that physicists would have had more long illnesses than clinical psychologists was not confirmed. This fact does not detract from the support of the hypothesis, since the concern was not with the presence of illness *per se*, but the fact that long illnesses would have led to isolation from peers. The fact of physicists' greater isolation from peers was established by another prediction.

Hypothesis X. In adolescence clinical psychologists manifested more interest in the opposite sex than did physicists.

The data were in support of the hypothesis of greater involvement with members of the opposite sex by clinical psychologists than by physicists. The latter had fewer dates and were older at the time of their first sexual experiences. One prediction—that clinical psychologists began dating earlier than physicists —was not confirmed, although results were in the predicted direction. The coding was set up in such a way that more than three-fourths of all subjects fell into one coding actegory, which may have obscured a difference which actually existed.

Hypothesis XI. Clinical psychologists

more frequently than physicists had strong but conflictual attachments to their families.

Two predictions relating to conflict with family members and emotional expression in the home were confirmed. The prediction relating to continued involvement between the subject and his family fell just short of significance. The latter prediction was a particularly difficult one to code, since it involved both the amount of present contact between the subject and his family as well as the degree to which the subject still had an emotional investment in his family, a factor which was rather difficult to assess. The hypothesis is supported to the extent that clinical psychologists gave evidence of having had in the past stronger, but more conflictual attachments to their families than did physicists.

An examination of how successfully all of the predictions taken together differentiate the groups serves to provide an overview. To give a general picture of how well the predictions taken as a whole discriminate between the groups, the coded response of each S to every item was given a score of one if it was in the direction predicted for physicists and a score of zero if in the direction predicted for clinical psychologists. It was possible for scores to range from zero (all items coded in the direction predicted for clinical psychologists) to 36 (all items coded in the direction predicted for physicists). Actually the scores ranged from 6 to 31. The distribution of scores appears in Table 2.

Table 2. Frequency Distribution of Total Scores on All Predictions

Scores	*Psychologists*	*Physicists*
6	1	
7		
8		
9	1	
10	1	
11	2	
12	1	
13	3	
14	1	
15	1	
16	1	
17	2	
18	1	1
19	2	2
20	1	
21		2
22	1	1
23	1	2
24		2
25		1
26		3
27		1
28		1
29		3
30		
31		1

When the two groups are taken together, the median for the total distribution is 19.5. Only three of the twenty cases in each group fall on that side of the median on which the majority of cases of the other group appears. A median test applied to the data was significant beyond the .001 level of confidence.

DISCUSSION

The present study grew out of an interest in the vicissitudes of curiosity in the developmental process. The results point to several possible ways in which the socialization process may modify curiosity. Parental behaviors of various kinds can act as catalysts that intensify the interest of the child in certain areas of experience. At the same time, other parental attitudes can make clear to the child that there are certain things about which he is not permitted to be curious.

Discipline plays a part also. The more harsh it is and the more it emphasizes restrictions, the more likely the child is to avoid venturing into areas that parents consider taboo. The focus of the parent's discipline may re-emphasize to the child attitudes already expressed in another sphere. For example, in their everyday life the parents of clinical psychologists showed concern about the behavior and feelings of other people. This concern was reiterated by their appealing to the child's feelings as means of disciplining him.

A natural question in connection with this study arises from the fact that the data represent verbal reports of past events rather than direct observations. The question that must be asked is whether the data, in fact, present evidence about developmental history, or whether they only provide indirect evidence about current personality traits. The rationale for taking the latter position is that the individual has a distorted view of his past based on his current personality make-up, present values, or group norms. An additional question in the same vein might emerge because of the expectation that social scientists have a greater inclination and willingness to talk about "bad" relations with parents and with others than physical scientists do.

It is, of course, not possible to say definitively whether or not the information gathered reflects actual childhood experience. However, there is some suggestion that the data do reflect actual happenings. It is expected that the likelihood of distortion would decrease as the specificity of material is increased, except for the placing of events at specific times, which seems very much subject to inaccuracies of memory. It seems more likely that current attitudes would influence one's impressions of the past than that they could contribute to the inaccuracy of reports about actual events. The predictions of the study were based on both specific facts and diffuse impressions in about equal numbers. About half of those predictions which were based on specific information, presumably not subject to great distortion, were confirmed, supporting the contention that the information gathered does describe actual developmental experiences. Furthermore, an examination of the unconfirmed hypotheses reveals that their subsidiary predictions deal equally often with specific as with diffuse material. The latter fact supports the idea that diffuse material was distorted no more than specific material.

The results of this study are also of practical significance to the vocational counselor. If he has information on the developmental histories of various occupational groups, a counselor is better able to help a client assess the likelihood of satisfaction with his choice of a vocation. These data alone might prove useful only in the rather unlikely situation of a person trying to decide between physics and clinical psychology. However, Nachmann has already provided similar data on three other occupations. Further research in the area will add to the list. Future investigation will be able not only to amass a body of data useful to the vocational counselor, but also to increase our understanding of personality development. Further work ought to focus on the isolation of other personality dimensions which seem to be closely related to various occupations. The final aim is to have the occupational spectrum analyzed in terms of important personality and developmental characteristics, and at the same time to add to the body of knowledge about the process of personality development.

SUMMARY

This study grew out of an attempt to understand the relationship between certain personality characteristics and vocational choice. Specifically, it was concerned with the relationship between the desire to satisfy curiosity and the varying opportunities which occupations afford for its satisfaction. An additional concern was to learn more about the vicissitudes of curiosity in the developmental process. The occupations of clinical psychology and physics were chosen to be studied because of the contrast they provide in terms of different objects of curiosity offered by each—curiosity about interpersonal relations in the first and curiosity about the nature of the physical world in the second. Hypotheses were developed about the probable developmental experiences which would account for each group's personality make-up and occupational choice. Each hypothesis was tested by means of a number of specific predictions about aspects of life history, predictions which were derived from that hypothesis.

The life histories of 40 male graduate students at the University of Michigan—20 in physics and 20 in clinical psychology—were investigated by means of structured, tape-recorded interviews. The hypotheses were tested by comparison of groups on coded categories, derived from response to the interviews. Seven of the eleven hypotheses tested were in the main confirmed. All results were in the predicted direction. The effects of parental attitudes and styles of discipline on the development of curiosity in the child were discussed.

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Vocational Interests of Sociologists

Jack E. Rossmann, Orville Lips, and David P. Campbell

Counseling psychologists in the college setting are being increasingly called upon to discuss graduate school plans with undergraduate students. Whatever the motivations might be, a higher proportion of seniors is seeking admission to graduate school each year.

Some students know from the day they enroll as freshmen that graduate school is their goal. For them, the purpose of entering graduate school is the desire to pursue a particular academic discipline, and vocational choice is no problem. For others, however, the possibility of graduate school is not considered until they are well along in their college careers, and then this choice is probably based upon a feeling that they can best contribute to society by obtaining a graduate education, because they like the intellectual challenge of the academic world, or because they see few occupations at the BA level that interest them. These students who feel no particular commitment to any one academic discipline may want assistance in deciding what field they should pursue in graduate school.

Assuming, then, that some students do desire help in choosing fields of graduate study, can measures of vocational interests help this process? More specifically, can the SVIB be helpful to these *potential* graduate school students? Yes and no. The SVIB can be helpful to students in deciding between such varied occupational fields as biological sciences and natural sciences, between art and business, between social-welfare areas and linguistics.

While the Strong can be helpful in steering the potential graduate student toward a general academic area, a problem is encountered when attempts are made at finer distinctions. Of approximately 60 occupational scales which are presently scored on the SVIB, only four criterion groups have included a sizable proportion of PhDs (biologists, mathematicians, physicists, and psychologists), and only the psychologist key is composed entirely of PhDs. At present, therefore, the potential PhD can get only limited assistance from the SVIB in making discriminations among related academic fields.

If the SVIB is to be useful in deciding between possible disciplines for graduate study, there must be more research and scale development in areas that will enable differentiation among the various academic disciplines. This was the purpose of the project reported here.

METHOD

SVIBs were mailed in May of 1965 to 410 male regular and life members of the Midwest Sociological Society. Of the blanks, 179 (44%) were returned. The basic hypothesis tested was that interests of sociologists could be differentiated from those of the SVIB Men-in-General group. Four additional questions were studied: (*a*) Do the interests of PhD sociologists differ significantly from those of non-PhDs? (*b*) Do the interests of sociologists vary according to the type of institution with which they become affiliated? (*c*) Do the interests of sociologists who have entered the field within the past 10 or 15 years differ from those who entered the field earlier? (*d*) Do the interests of sociologists differ significantly from those of psychologists?

Gouldner and Spreke (1965), in a preliminary report of more extensive data on the attitudes and values of sociologists, concluded that they were a very diverse and complex

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group of professionals. In this initial report, however, no attempt was made to determine the basis of this complexity. Our questions remain: Does time of entry into the field have a bearing upon value orientation? Is institutional affiliation a crucial variable? Is possession of the PhD a critical factor?

Sibley (1963), for a study concerned with the education and training of sociologists, collected questionnaire data from over 1,000 sociologists in 1960–61. He concluded that the career patterns of PhDs differed markedly from those sociologists who held the master's degree. Of the PhDs, 70% were employed by academic institutions, whereas less than 15% of the MA sociologists were so employed. Will these differing employment patterns be reflected in vocational interest data?

Sibley's discussion of graduate education suggests an increasing emphasis during the past 10 years upon quantitative methods. Is this change in emphasis apparent in the interests of sociologists who have received their degrees within the last decade?

Sibley (1964) quoted one of his respondents, a PhD sociologist working in "human factors research" in industry as follows: "Somehow, I feel superior to psychologists generally—I feel I can look at life more broadly . . . and I suspect that I get along better with my fellow man than the average psychologist—yet I have gotten to the point of identifying myself as a psychologist [p. 173]."

Is it true, as this quotation would suggest, that in terms of a measure of vocational interests such as the SVIB the differences within the discipline of psychology or sociology are equal to or greater than the differences between the two groups? This study was designed to provide data related to these questions.

RESULTS

The responses of the 179 sociologists to the SVIB were compared with those of the 1965 revision of the Men-in-General group. The responses of these two groups can be differentiated quite well. On 74 items, at least one of the three possible response positions (like, indifferent, dislike) showed a 20% or greater response-frequency difference between sociologists and Men-in-General. (The total number of items was restricted to the 288 items retained in the revised form of the Strong.) The decision to select items at this level was based upon the work of Clark (1961), who has shown that scales with such characteristics are valid, reliable, and hold up well under cross-validation.

Differences among Sociologists

Three other kinds of data were collected from the sample members to answer the previous questions. Differences among the sociologists on three factors—age, institution, and degree level—were tested. . . . Significant differences suggest that the interests of PhDs more closely resemble scientifically and numerically oriented occupations, while the non-PhDs have interests more like those of the social-service occupations.* Significant *t* values were found between the two groups on 14 of the 55 scales tested. Eight of the differences were greater than 5 standard score points or one-half standard deviation.

Table 1. SVIB Scales on Which the Mean Scores of Sociologists and Psychologists Differ by More than 5 Standard Score Points

Occupation	*Sociologist*[a] *M*	*Sociologist*[a] *SD*	*Psychologist*[b] *M*	*Psychologist*[b] *SD*
Physicist	24.9	13.1	30.4	15.0
Chemist	36.6	11.7	42.1	12.9
Production manager	30.6	8.0	36.9	8.7
Social worker	44.1	10.2	38.2	11.5
Soc. science teacher	40.2	11.2	32.8	11.6
Minister	38.0	11.6	31.8	10.7
Librarian	47.5	11.2	39.7	11.0
Musician performer	47.1	9.4	42.0	9.5
Music teacher	44.3	10.0	37.1	10.6
Pres., mfg. concern	19.7	8.9	26.1	10.4

[a] $n = 179$.
[b] $n = 1{,}048$.

Analyses of variance were run among the four types of employing institutions—university, liberal arts college, state college, and nonacademic. Eleven of the *F* ratios were significant at the .05 level or better, with all of the differences between the two extreme means being greater than one-half standard deviation. On seven of these scales, those scientifically or numerically oriented, the univer-

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* EDITORS' NOTE: Tables elaborating the *t* values can be found in the original article.

sity group scored highest, the noncollege group lowest, with the liberal arts and state college faculty members in between.

The final comparison was between the interest scores of sociologists of different ages. The group was divided into those 40 years and under and those over 40. Significant values were found on only three scales, and all of the differences were less than 5 standard score points.

Since there are no indications of differing interests between these age groups in the areas usually associated with quantitative thinking, some questions may be raised about Sibley's (1964) assertions that there is an increasing quantitative emphasis on graduate training in sociology. If the assertion is correct, this shift in curricular orientation has apparently not as yet been accompanied by a measurable shift in the interests of individuals attracted to the field.

Psychologists and Sociologists

The overlap in the disciplines of psychology and sociology often appears great. Members of the two disciplines frequently attack similar research problems, and many share a basic social-service orientation. It might be predicted, therefore, that it would be impossible to differentiate between the interests of the two groups, and that any measure of the interests of psychologists would suffice for sociologists as well.

Since data for the psychologist SVIB criterion group were available at the Center for Interest Measurement Research at the University of Minnesota, it was decided to compare mean scale scores for the two groups. While the mean profiles "look" very similar, the two groups can be differentiated statistically. Statistical treatment of the data yielded differences, significant at the .05 level or better, on 29 of 52 scales. The large number of statistically significant differences may be partially explained by the size of the Kriedt (1948) criterion group ($N = 1{,}048$), which allowed small mean differences to be statistically significant. Of the means, 10 did differ by greater than one-half standard deviation, however (Table 1). These 10 scales indicated that sociologists were more oriented toward social-service and cultural occupations, while the psychologists tended to have interests somewhat more like those of physical scientists and businessmen.

SUMMARY AND DISCUSSION

There is clear evidence that the interests of sociologists can be differentiated from those of Men-in-General. That this difference can be of use to the counselor is much less clear, however. SVIB data collected from the 1965 class of 100 Danforth Fellows (first-year graduate students—43% in the humanities, 35% in the behavioral sciences, and 22% in the natural sciences) yielded a mean profile very similar to that of the sociologists and psychologists (Rossmann and Bentley, 1966). What may be appearing, therefore, is an intellectual or academic interest pattern.

If this is the case, the addition of more criterion groups and the development of scales in the academic area will prove fruitless. If future research in this area is to be meaningful, the development of academic Men-in-General or behavioral science Men-in-General groups may be a necessary next step.

Perhaps this is the direction which will increase our ability to assist the potential graduate student in his vocational choice.

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B. Career Activities

Introduction

During the course of his career, the scientist will adopt one or more of a number of possible work roles or work activities. The activity which he adopts, either volitionally or nonvolitionally, can influence his level of productivity and recognition. Andrews, examining the relationship between allocation of time to technical research, teaching, and administrative activities and productivity, finds that scientists who spent their full time on technical work performed more poorly than did those also involved in teaching and administration. The optimal division of time for Ph.D. scientists seemed to be 75 percent for research with the majority of the remaining 25 percent being time spent on administrative tasks rather than on teaching.

The roles which individual scientists assume are inherent in the social structure of the institutions and subcultures in which they normally function. The studies by Hargens and Crane discuss some of the situational characteristics affecting scientific interaction. Hargens finds strong inbreeding by individual institutions and strong intraregional mobility from doctoral institutions to first academic appointment, while prestige levels are not so pervasive.

Some types of social organization which exist in the culture of science are informal rather than formal. Crane obtained sociometric data, a rare technique in this area of inquiry, on different types of communication relationships between scientists who publish in one discreet problem area. She found that a relatively small number of scientists receive a large number of sociometric choices. Analyses of direct and indirect ties reveal that a tie with one or more highly productive scientists brought other scientists of less productivity into a large communication network. Most members in an area appear linked to each other only indirectly through these highly influential members.

The remaining selections consider roles or orientations that the scientist may actually adopt. Gough and Woodworth elaborate eight different research styles of scientists. A factor-analysis of a self-descriptive Q-sorting by 45 research scientists produced the eight factors. While other researchers (e.g., Stein in Section II E) have specified trait-type differences between scientists, Gough's differences among the eight types appear to be stylistic, showing that diverse styles of research methodology do exist among scientists and can be delineated. Further, these styles of work are related to personality characteristics as identified on the California Personality Inventory (CPI). The Gough statements on which the identification of factor types was based included not only activities and modes of work, but also work values. Barber (1952) has listed the moral values attributed to scientists, which are consonant with the goal of producing new knowledge. Few attempts have been made to discover whether a majority of scientists hold them as they are stated. However in his study of 57 academic scientists, West examined the extent of adherence to such values as impartiality, suspension of judgment until sufficient evidence is at hand, absence of bias, and group loyalty. Interestingly, creativity was unrelated to the acceptance of the classic morality of science.

The studies by Uyeki and Cliffe and by Eiduson are descriptive, rather than experimental. Each describes a particular role that a scientist may elect to adopt at certain stages of his career. The federal science administrators in natural and physical sciences studied by Uyeki and Cliffe had high career mobility (almost two thirds had worked for nonfederal employers) but fairly low interdepartmental mobility when in federal service. They came from diverse social backgrounds, and the top level of administrators resembled businessmen in social background, geographic origin, and educational background.

Eiduson longitudinally examined the commitments of 40 scientists, 18 of whom were engaged as federal advisors or consultants to specific programs or advisory boards. Prestige was the overriding motive for accepting such positions, although each said they hoped to really influence legislation and funding areas. As a group, they generally were taken aback by their ambivalent Washington reception. Particularly frustrating and distressing were the politicians' lack of awareness of the scientist; the scientist being asked for advice, but rarely listened to; the scientist's lack of influence; and the politics and red tape involved in the federal bureacracy. While each agreed that his full-time presence was necessary to make an impact, he felt that a full-time Washington commitment causes one to lose touch with the university and the current research scene, thus eventually weakening one's effectiveness in Washington. As might be predicted all but one scientist eventually left their Washington commitments disillusioned, and went back to the university where they could be productive and feel that they were making a significant contribution to science.

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Scientific Performance as Related to Time Spent on Technical Work, Teaching, or Administration

Frank M. Andrews

Many scientists and engineers spend full time on their technical work, that is, on research or development. Other scientists devote a portion of their time to teaching or administration. If research were similar to operating a machine, one would expect that the scientific contributions of those who spend full time on their technical work would surpass the contributions of those who spend only part time. But in many ways research is different.

In analyzing data from a nationwide study of 4,000 physiologists, Meltzer found that those who spent about three-quarters time on technical activities had the highest rate of publication and that full-time researchers published less. The same trend occurred within each professional rank.[1]

Could this trend be replicated? How is a scientist's or engineer's performance affected if he allocates part time to activities other than his technical job? If he has a choice, should he administer or teach? This article reports some findings on these questions.

METHOD

Subjects and Analysis Groups

Between 1958 and 1960, 552 scientists and engineers provided responses to a wide variety of questionnaire items inquiring about their work, laboratory organization, and amount of scientific output.

These respondents were employed in 11 different research sites: five industrial laboratories, five government labs, and one large university (seven academic departments).

These heterogeneous respondents were divided into the following five analysis groups:[2]

1. PhD's in research-oriented laboratories

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[1] This information comes from unpublished data; for other reports on this study see Leo Meltzer and James Salter, Organizational Structure and Performance and Job Satisfaction of Scientists, *American Sociological Review,* 27 (1962), 351–362; Ralph W. Gerard, *Mirror to Physiology: A Self Survey of Physiological Science* (American Physiological Society, Washington, D.C., 1958).

[2] For a description of motivations and attitudes in the five analysis groups, see Donald C. Pelz and Frank M. Andrews, Organizational Atmosphere, Motivation, and Research Contribution, *American Behavioral Scientist,* 6 (December 1962), 43–47.

(these labs were all in university or government; none of the industrial labs was research-oriented).[3]

2. PhD's in development-oriented laboratories (either industry or government).

3. Non-PhD research scientists who worked in research-oriented labs (all in government) which had few (under 20 per cent) PhD's.

4. Non-PhD engineers who worked in development-oriented labs which had few (under 20 per cent) PhD's. (Over half held degrees in engineering specialties.)

5. Non-PhD assistant scientists who worked in labs (either research or development) which had many (42 to 52 percent) PhD's. (Since all respondents were professionals, this group did not contain technicians or other subprofessional assistants.)

Independent Variables: Allocation of Time to Technical Work, Teaching, or Administration

The independent variables were derived from responses to the following question: "Of your total work time, about what proportion do you normally spend on the following types of activities?" Respondents indicated the percentage of their time spent on (1) teaching and training, (2) technical work other than teaching, and (3) administrative or other nontechnical work.

Technical work was defined to include for respondent's own research or development activity (including technical reading and writing), supervising the technical work of others, collaborating and discussing with colleagues, and consultation. Administrative work included: internal administration (expediting services, and so on); communicating with superiors, outside groups, or clients; or other nontechnical work.

Respondents who said they spent 81 to 100 per cent of their time on research or development activities were said to spend full time on their technical work. The remainder were divided into various groups depending on the proportion of time spent.

Using these definitions, approximately half of the non-PhD scientists and about three-quarters of the assistant scientists spent full time on their technical work. In each of the other groups, however, substantially less than half said they spent full time. Among the research PhD's, only one-fifth worked full time at research. The numbers of cases are presented in Table 1 below.

Only among the research PhD's (many of whom were located in academic departments) did teaching account for a substantial part of respondents' nontechnical work time. In the other four groups, teaching (if it occurred at all) was secondary to administration.

Dependent Variables: Measures of Scientific Performance

Measures of scientific performance were derived from two sources. The first was the scientist's own reports of his five-year output of patents, papers, and unpublished reports.[4] The second was judgments of each respondent's performance by peers and supervisors who felt knowledgeable about the man's work. Judgments were made with respect to the man's *scientific or technical contribution* and (separately) his *general usefulness to the organization.*

The self-reported outputs were transformed to log scores to produce more nearly normal distributions, and the evaluations were converted to percentile scores (within site).[5] All scores were then adjusted (separately for each of the five relatively homogeneous analysis groups of the sample) to hold constant four background effects which it seemed advisable to partial out: education, length of work experience (time since degree and time in di-

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[3] The characterization of a department as research- or development-oriented was based on an index of six items concerned with whether executives were believed to stress products or knowledge.

[4] For the engineers, patents have been used as the most relevant form of output; for the other four groups, papers were used.

A comparison of self-reported output and company records for one advanced research division of 28 members showed fairly good agreement; for patents, rho = .91 and for papers, rho = .82. Self-reported output of unpublished reports did not correlate with company records, but did correlate fairly well (rho = .58) with colleagues' judgments of scientific contribution.

[5] This conversion was based on a mathematical procedure proposed by L. R. Ford, Jr., in *American Mathematics Monthly*, 64 (1957), 28–33.

vision), type of research setting, and either time between BS and PhD or age at BS. These four factors, taken together, accounted for 5 to 38 per cent (median = 15 per cent) of the variances in the unadjusted scores, the exact value varying from group to group and measure to measure.

RESULTS

Relationship between Performance and Proportion of Time Spent on Technical Work

In Table 1 mean performance scores (after adjustments) are shown for each of the five analysis groups according to the proportion of total work time respondents spent on technical activities.[6] Results were clear: respondents who spent full time (81 to 100 per cent) on their technical work performed less well than those who spent only part time.

These results were consistent across all three measures of performance in all groups except the non-PhD scientists. Even for this group, the same trend was observed for evaluated performance if people spending 61 per cent or more time were compared with those spending less. In seven of the fifteen cells, the differences among means were statistically significant (at or beyond the .05 level); in no cell was the observed trend significantly reversed.

For PhD's and assistant scientists, scientific contribution was highest for those who spent three-quarters time (61 to 80 per cent) on their technical work. These results were consistent with Meltzer's findings on physiologists. For engineers and non-PhD research scientists, the maximum occurred at half time (41 to 60 per cent) or less.

Analyses to Check Stability of Findings

To be sure that the results shown in Table 1 were not due to some spurious relationship, it seemed wise to run three other analyses (data not shown).

Performance Unadjusted for Experience. What would have happened had the performance measures not been adjusted to hold constant the length of experience and the time between BS and PhD or the age at BS? When the analysis of Table 1 was repeated using performance measures from which these factors had not been partialled out, the results were similar to those in Table 1, but somewhat sharper.

This was not surprising, since the less experienced people (who showed lower performance) were more likely to spend full time on their technical work than scientists with longer experience. Thus, the low scores of the full-time people were *partly* due to a disproportionately large number of young people in the group. But differences in performance due to length of experience were partialled out of the scores shown in Table 1; so this factor did not account for the results observed there.

Supervisory Status. Could the results of Table 1 have been due merely to differences between supervisors and nonsupervisors? This seemed possible since supervisors got higher performance scores than nonsupervisors and also were less likely to spend full time on their technical work. As a check, the analysis of Table 1 was repeated for the nonsupervisors and supervisors separately.

Among the four nonsupervisory groups for which there were sufficient cases to determine a trend (Group 3, non-PhD research scientists, was too small to consider), the findings of Table 1 were not markedly altered.

Among the supervisors there were sufficient cases in Groups 1, 2, and 4—the PhD's in research and development and the engineers. The supervisory PhD's in development showed the Table 1 trend. For supervisory engineers, the proportion of time spent on technical work was virtually independent of performance. For supervisory PhD's in re-

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[6] In this table, Table 2 means were based on a weighted sample intended to be representative of the entire professional staff of the laboratories studied. Numbers of cases and significance levels, however, were based on unweighted data; they represent the characteristics of the 552 cases actually used in this analysis. (Use of the sampling weights in computing means actually made little difference.) The significance levels were based on F tests using unweighted means, by a one-factor analysis of variance. Significance levels are approximate and should be interpreted with caution. Readers will remember that large and important differences may be statistically nonsignificant if means were based on few respondents.

Table 1. Mean Performance (Adjusted) for Five Groups of Scientists, as Related to Time Spent on Technical Work[*]

Time spent on technical work	*Group 1 PhD's in research labs*		*Group 2 PhD's in development labs*		*Group 3 Non-PhD scientists*		*Group 4 Engineers*		*Group 5 Assistant scientists*	
	N	*Mean*	*N*	*Mean*	*N*	*Mean*	*N*	*Mean*	*N*	*Mean*
Scientific contribution										
0–40%	18	48.3	4	50.7	4	56.7	16	60.5	6	21.5
41–60%	17	64.8	21	47.6	9	58.8	48	59.9		
61–80%	12	66.1	31	64.3	7	42.9	78	53.7	16	60.3
81–100% (full time)	10	46.1	38	43.6	24	49.4	72	45.4	65	52.1
Stat. sig. of differences[†]		ns		.001		ns		.05		.001
Overall usefulness										
0–40%	18	52.7	4	66.2	4	66.7	16	63.6	6	54.4
41–60%	17	58.5	21	59.7	9	71.1	48	68.6		
61–80%	12	56.7	31	62.2	7	42.1	77	56.6	16	58.6
81–100% (full time)	10	39.5	38	38.6	24	44.1	72	43.4	65	49.9
Stat. sig. of differences[†]		ns		.001		.05		.001		ns
Papers or patents (logs)[‡]										
0–40%	30	2.94	7	3.22	4	3.35	16	2.91	5	2.52
41–60%	22	3.30	21	2.82	9	2.49	48	3.60		
61–80%	13	3.24	32	3.21	7	2.59	78	3.07	14	3.27
81–100% (full time)	11	2.80	38	3.09	25	3.28	73	2.82	65	3.01
Stat. sig. of differences[†]		ns		ns		ns		.001		ns

[*] Means are based on weighted data and numbers of cases, and significance tests are based on nonweighted data (see n. 6); for adjustment process, see text.

[†] Significance levels are approximate (see n. 6).

[‡] For engineers, log patents (past five years); for other four groups, log papers (five years). Arbitrary constants have been added to make the mean for each analysis group about 3.0.

search, peak performance occurred for those spending half time or less on technical work (a change from the peak at three-quarters shown in Table 1).

Thus, among all of the nonsupervisory groups examined, and among supervisory-level PhD's in research and development, differences in supervisory status did not account for the lower performance of the full-time people.

Total Time. A third possible explanation for the findings of Table 1 was examined.

Among the nonsupervisors it was found (data not shown) that those who spent only part time on technical activities actually spent more total hours on all work-related matters in a typical month than those who spent full time. Apparently, among non-supervisors time spent on training or administration was not fully balanced by reductions in time spent on technical activities. (Supervisors tended to spend more than average total time regardless of the proportion of time devoted to technical work.) Could the higher performance of the part-time nonsupervisors have been due to their having spent more total hours on their work?

When additional controls for total time on technical work and supervisory status were entered in the analysis of Table 1, the trends shown there were not greatly changed. It was concluded that differences in total time did not account for the differences in performance.

Effects of Allocating Nontechnical Time to Teaching or to Administration

The three check analyses just described provided some confidence that the higher performance of the part-time people (at least among the nonsupervisors) was not merely an artifact due to differences in length of experience, supervisory status, or total time spent on work-related matters. But if performance was enhanced by spending some time away from the bench, why? What occurred during the scientist's nontechnical activities which might have stimulated him?

There was no direct way to answer this question, but one thing which could be done was to ask whether performance varied according to the *kind* of nontechnical activity. Was there any difference, for example, between scientists who spent their nontechnical time on teaching or on administration?

Data were available to answer this question for three of the five analysis groups (most non-PhD scientists and assistant scientists spent full time on their technical work). Table 2 shows some of the results.

Among the PhD's in research, many of whom were in academic departments, those who spent their nontechnical work time primarily on teaching could be compared with those who gave equal or greater emphasis to administration. For the nonsupervisors, there was a clear—and surprising—tendency for those emphasizing administration during their nontechnical work time to perform at higher levels than those emphasizing teaching (statistically significant in two of the three cells). Among the supervisors this trend appeared for the measure of over-all usefulness, but it was reversed for the output of papers (neither trend statistically significant).

Several *post hoc* explanations for this surprising finding came to mind. Possibly administration really provided more stimulation for the nonsupervisors' research than did teaching. Perhaps administration, as done by these nonsupervisory PhD's in research, had more direct relevance for their on-going technical activities than did teaching. A third possibility was simply that the more able nonsupervisors elected administration. A fourth possibility was that those who taught were achieving results not measured by the performance measures used—perhaps they were producing scientists rather than research. A choice among these alternatives could not be made from the available data. These possibilities, however, pose leads for further investigation.

Among the engineers few people did very much teaching. The comparison for this group was made between those who did "some"

Table 2. Mean Performance (Adjusted) of Two Groups of Scientists Who Spent Nontechnical Time on Teaching or on Administration, by Supervisory Status*

Allocation of work time	*Judged performance*			*Papers or patents*	
	N	*Mean scientific contribution*	*Mean over-all usefulness*	*N*	*Mean*
Nonsupervisors					
PhD's in research					
Primarily teaching	15	45.7	37.3	25	2.83
Primarily administration†	8	64.6	59.5	11	3.50
Stat. sig. of diffs.		ns	.05		.01
Engineers					
Some teaching‡	27	55.8	55.8	28	2.05
All administration‡	33	47.6	47.0	32	1.86
Stat. sig. of diffs.		ns	ns		ns
Supervisors§					
PhD's in research					
Primarily teaching	7	65.6	58.4	9	3.32
Primarily administration†	17	67.7	76.4	20	3.19
Stat. sig. of diffs.		ns	ns		ns
Engineers					
Some teaching‡	24	70.3	76.8	25	2.34
All administration‡	52	57.7	70.7	52	2.54
Stat. sig. of diffs.		ns	ns		ns

* Same footnotes apply as in Table 1.

† Time on administration either equal to or greater than time on teaching.

‡ "Some" teaching was 6 per cent or more; "all administration" meant not more than 5 percent of time teaching.

§ In university departments, full professors were defined as supervisors.

teaching (6 per cent or more of their total time) and those who devoted all their nontechnical time to administration. Table 2 shows that among both supervisory and nonsupervisory engineers there was a slight tendency for those who did some teaching to perform at a higher level than those who did administration (the trend appeared in five of the six cells, nowhere statistically significant).

Data for PhD's in development labs are not shown in Table 2 but the same comparison was made for this group as for the engineers. Only slight (and inconsistent) performance differences appeared between those who spent nontechnical work time on "some teaching" and those who did "all administration." Neither teaching nor administration was superior for this group.

SUMMARY AND CONCLUSIONS

For five separate and quite different groups of scientists, those who spent full time on their technical work had lower performance than those who spent part time in administration or teaching. These findings, based on a wide range of scientists and performance measures, were in close agreement with findings by Meltzer for the publication rate of physiologists.

Among PhD's in research or development, and among assistant scientists, maxium performance was obtained from those who spent three-quarters time on their technical work. Among engineers and non-PhD research scientists, however, half or less time on technical work was optimal.

These trends continued to be present among the nonsupervisors even after several factors which might have accounted for them were controlled. Among the factors considered were length of working experience, supervisory status, and total time spent on all work-related activities.

If these findings continue to be supported by other research, their implications for the administration of research and development activities will be considerable. They suggest that scientists are not necessarily most effective if they spend full time on their technical activities.

Although the reason for the higher performance of those spending part time on their technical work could not be determined from the data available, a number of possibilities might be suggested. One is that nontechnical tasks were pushed upon high-performing scientists with the expectation that they would be the most effective administrators or teachers. Another is that high-performing scientists accepted nontechnical tasks as a means of getting others to work on their projects or ideas.

A third, and quite different, possibility is that the presence of diversity in scientists' work situations enhanced their performance. Pelz and Andrews have reported that scientists with several areas of specialization, and those performing several research and development functions, tended to perform at higher levels than those with just a single skill or function.[7] The findings reported here can also be viewed as supporting the diversity effect. Spending part time on activities other than one's technical work is one form of diversity in the work situation, and it was associated with above-average performance. Perhaps scientists confronted with diversity were exposed to more new ideas, problems, or people useful to their work than were those in homogeneous situations.

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[7] Donald C. Pelz and Frank M. Andrews, Diversity in R & D Tasks, *International Science and Technology*, 31 (1964), 28–36.

Patterns of Mobility of New Ph.D.'s among American Academic Institutions

Lowell L. Hargens

In their discussions of the institutions of higher education in the United States, sociologists have emphasized the fact that various colleges and universities commonly are ranked in a hierarchical fashion.[1] The basis of this ranking is usually expressed in terms of notions of institutional "quality" or "prestige," but it is clear that the hierarchy consists of more than existing social evaluations. For example, there is a high positive correlation between the rank of an institution and the average salary of its faculty members,[2] and recent studies suggest that, even when the scholarly merit of their work is the same, members of highly ranked institutions are more likely to receive formal honorary awards for their work than members of institutions of lesser eminence.[3] In fact, some investigators have asserted that this academic stratification system is even more than an unequal distribution of existing rewards and system of differential access to further rewards. Caplow and McGee employ a "major league-minor league-bush league" metaphor in their discussion of the hierarchy, and suggest that scholars are often enabled or condemned to spend their entire careers in the "league" in which they obtain their doctorates.[4] Scholars who hold positions in the highest levels of the hierarchy are sometimes pictured as forming inner circles which maintain close informal ties and control the distribution of rewards for the rest of their disciplines.[5] Thus, the academic community is portrayed as a set of vertically arranged strata, with little mobility between strata and élite control of the system as a whole.[6] These portrayals, of course, contradict the image of the academic community as a system wherein "universalistic achievement" norms are realized.

This general approach to the study of institutions of higher education in the U.S. has been especially popular among those who have studied the distribution of newly trained doctorates to positions in these institutions. Since informal contacts within one's discipline are a primary means by which scholars obtain new positions,[7] and since newly trained doc-

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[1] Early in this century, Veblen suggested that academic prestige might be viewed as a type of institutional "good will" which attracted money from potential benefactors and which could be unrelated to scholarly quality or achievement. Veblen saw American institutions of higher education as business enterprises which acquired prestige by improving and expanding their physical facilities. Thorstein Veblen, *The Higher Learning in America*, New York: Hill and Wang, 1957, pp. 98–107. More recent discussions have portrayed academic prestige in terms of the quality of scholars employed and trained at various institutions. For example, see Theodore Caplow and Reece McGee, *The Academic Marketplace*, Garden City, New York: Doubleday and Company, Inc., 1957; David Reisman, *Constraint and Variety in American Education*, Garden City, New York: Doubleday and Company, Inc., 1957; and Bernard Berelson, *Graduate Education in the United States*, New York: McGraw-Hill, 1960.

[2] Allan M. Cartter, *An Assessment of Quality in Graduate Education*, Washington, D.C.: American Council on Education, 1966, pp. 111–112.

[3] Diana Crane, "Scientists at Major and Minor Universities: A Study of Productivity and Recognition," *American Sociological Review*, 30 (1965), pp. 709–711. Stephen and Jonathan R. Cole, "Scientific Output and Recognition: A Study of the Operation of the Reward System in Science," *American Sociological Review*, 32 (1967), p. 390.

[4] Caplow and McGee, *op. cit.*, p. 193.

[5] *Ibid.*

[6] Although the American academic community often has been studied as a social stratification system, there have been no attempts to summarize the implications of such inquiries for more general theories of stratification. In particular, no one has attempted to show how the kinds of findings cited above might be explained by the widely discussed "functional theory of stratification." This is true both of those who have been severe critics of the operation of the system of rewards within the academic community (such as Caplow and McGee) and of those who have taken a more apologetic stance toward it (such as the Coles).

[7] David G. Brown, *The Market for College Teachers*, Chapel Hill, North Carolina: University of North Carolina Press, 1965, pp. 86–125; Howard D. Marshall, *The Mobility of College Faculties*, New York: Pageant Press, Inc., 1964, pp. 71–90.

torates are especially dependent upon their teachers in this regard, the pattern of acquisition of positions by new doctorates is likely to reflect the lines of social stratification within the academic community.[8] Thus, previous investigations have asserted that possession of a degree from a top university is almost a necessary condition for recruitment to a position in a top university, and that a scholar who obtains a degree from a minor university has a small chance of obtaining a position in a major university regardless of his competence.[9]

It will be the purpose of this paper to examine more closely the distribution of newly trained doctorates to positions in the academic community, noting whether or not the patterns of distribution do in fact follow the lines suggested previously, and suggesting some implications of the patterns which are found for the views of the academic community which have been outlined above.

THE DATA

In the spring of 1959, Bernard Berelson sent questionnaires to a sample of those who had received the doctorate in the United States during 1957.[10] Berelson received completed questionnaires from 61 percent of the 3,843 members of this sample, and has presented evidence that the resulting data are "reasonably representative" of the intended population.[11]

For my study, I have eliminated those respondents who received degrees in professional fields (such as business, education, engineering, and agriculture), and those who did not hold academic positions in 1959. Respondents who held academic positions outside of the continental United States also were excluded. Finally, I have eliminated respondents who held positions which are commonly of a temporary nature, such as post-doctoral fellows and instructors. Thus, only respondents with a rank of assistant professor or above have been retained. The resulting sample of academic scholars in the humanities and the physical, biological, and social sciences, includes 682 members.

The following analysis will examine the distribution of new doctorates both among universities and to colleges. Separate analyses also have been carried out for scholars in the physical and biological sciences on the one hand, and the humanities and social sciences on the other. The sample described above is distributed among these categories in Table 1.

The analysis will be divided into three general sections. The first section will be focused upon patterns of mobility of new Ph.D.'s among all universities; the second section upon patterns of recruitment of new Ph.D.'s to universities in the top prestige level; and the final section upon patterns of mobility among those respondents who obtained jobs in college settings.

PATTERNS OF MOBILITY AMONG RESPONDENTS WHO OBTAIN JOBS IN UNIVERSITIES

In order to obtain information about the general patterns of distribution of new doctorates to positions in the academic community, factor analyses were carried out on sociometric-type matrices describing the flow of new doctorates among the 72 universities most productive of new doctorates in this sample. Because these analyses were not carried out for all institutions of higher education represented in the sample of new doctorates (either as producers or as hirers), and because of the small number of cases upon which the analyses were based, it would be inappropriate to report, interpret and generalize the results in a detailed manner. It must suffice here to report that the factors yielded by the analyses

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[8] Hargens and Hagstrom have presented evidence which suggests that the correlation between the prestige of scholars' doctoral institutions and that of their present affiliations is highest for their first jobs and declines as their careers progress. Lowell L. Hargens and Warren O. Hagstrom, "Sponsored and Contest Mobility of American Academic Scientists," *Sociology of Education*, 40 (1967), pp. 34–35.

[9] Caplow and McGee, *op. cit.*, p. 193; Hargens and Hagstrom, *op. cit.*, pp. 31–32.

[10] For a more detailed description of this body of data and Berelson's analysis of it, see Berelson, *op. cit.* I wish to express my appreciation for Professor Berelson's kindness in furnishing me with the data.

[11] Berelson, *op. cit.*, pp. 278–279.

Table 1. Distribution of the Sample Among Subcategories of the Analysis

Present location of respondent	*Discipline of respondent* *Physical and biological sciences*	*Humanities and social sciences*	*Total*
University	155	209	364
College	104	214	318
Total	259	423	682

Type of Employer and Prestige Level of Positions Obtained by Respondents Who Held Temporary Positions in the Academic Community Two Years After the Receipt of the Doctorate

Type of employer and prestige level of temporary positions	*Prestige of doctoral institution* *1 (high)*	*2*	*3*	*4 (low)*
Universities				
1 (high)	34%	31%	25%	20%
2	8	9	16	7
3	9	9	13	5
4 (low)	22	14	9	20
Total universities	73%	63%	63%	52%
(N)	(58)	(22)	(20)	(21)
Colleges				
Top colleges	9%	14%	3%	2%
Other colleges	19	23	34	46
Total colleges	28%	37%	37%	48%
(N)	(22)	(13)	(12)	(20)
TOTAL	101%	100%	100%	100%
(N)	(80)	(35)	(32)	(41)

were, to a surprising extent, regionally homogeneous.[12]

However, it should be noted at this point that it would have been impossible to obtain factors which were homogeneous with respect to institutional prestige from this kind of analysis. Because the most prestigious universities tend to produce the greatest number of doctorates while at the same time tending to hire only a few new doctorates to positions of assistant professor and above, it could not happen that all doctorates from these institutions would also be hired by such institutions. Thus, the most prestigious universities must send some of their new doctorates to institutions of lesser eminence; of course, these latter institutions do not produce enough doctorates to supply themselves, so that there is a strong demand for doctorates from prestigious institutions. As a result of these circumstances, it would be impossible to find factors which are homogeneous with respect to prestige from a factor analysis of institutions which have the production-consumption pattern described above.

One must turn to another type of analy-

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[12] Actually, studies of scientific manpower have consistently found a fairly strong tendency toward intra-regional mobility in their analyses of the mobility patterns of Ph.D. holders in various fields. Such findings, however, have not been related to the concerns of this paper. For example, see Lindsey R. Harmon, *Profiles of Ph.D.'s in the Sciences*, Washington, D.C.: National Academy of Sciences, 1965, pp. 11–12; Alan E. Bayer, "Interregional Migration and the Education of American Scientists," *Sociology of Education*, 41 (1968), pp. 88–102.

sis to evaluate the relative adequacy of the view of the academic community as a prestige hierarchy in comparison to interpretations in terms of a set of regional groupings. Of course, these are not necessarily contradictory perspectives, and the following analysis will also attempt to measure the degree to which a combination of the two views can account for observed patterns of mobility of new doctorates.

Table 2 presents data in the form which will be used through the remainder of this paper for the distribution of new doctorates among U.S. universities. The first column of this table is divided into five components. The figure corresponding to each component represents the proportion of respondents who both received their doctorates and obtained their present jobs in a homogeneous manner with respect to that component. For example, Table 2 shows that 16 percent of the respondents received jobs in the same institution from which they took their doctorates. In addition, Table 2 presents patterns of *inter*institutional mobility, omitting those respondents who have been inbred into their doctoral institutions. Thus, 19 percent of those respondents who were not inbred obtained jobs in the same region as their doctoral institution but in a different prestige level, 29 percent in the same prestige level but a different region, etc. For the purposes of this analysis, institutions were divided into five regions: the East, the South, the Midwest, the Plains and Mountains, and the Pacific Coast. Except for the addition of the Plains and Mountains region, the assignment of institutions to regions is identical to that used by Cartter in his study for the American Council on Education[13]—assignments were not based upon the results of the factor analysis reported above. Prestige was measured by Berelson in a manner reported in his book *Graduate Education in the United States.*[14] Berelson's results are incorporated here but a new doctorate is said to be employed on the "same" prestige level as his doctoral institution if his institution is either on the same prestige level according to Berelson, or is on the next lower level. This decision was made because new doctorates tend to move down from the prestige level of their

Table 2. Mobility Patterns of Respondents Who Obtained Positions in U.S. Universities

Components	*Observed %* (*Po*)	*Expected %* (*Pe*)	*Po-Pe*
Inbreeding*	16%	1%	15%
Same region, different prestige level	19%	12%	7%
Different region, same prestige level	29%	32%	—3%
Same region, same prestige level	21%	10%	11%
Different region, different prestige level	31%	46%	—15%
Total N	364		
Total N less inbred	306		

* The Inbreeding component has been calculated on the base of the entire sample. The other components have been calculated on the base of the entire sample less the inbred respondents.

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[13] Cartter, *op. cit.*, pp. 129–131. Any regional classification, including the one used here, is more or less arbitrary and might be interpreted best as a crude method of measuring distance. Relations between physically proximate institutions are the center of concern in this paper, and no claim about the appropriateness of this particular classification is being made.

[14] Berelson, *op. cit.*, pp. 124–128, 280–281. It should be emphasized that the patterns of mobility found in the following analysis have no direct bearing upon hypotheses about the causes and consequences of these patterns. Only insofar as the observed patterns are or are not consistent with the patterns which would be predicted by such hypotheses can one make inferences about the validity of such hypotheses. Thus, in using the term "regionalism" in the following analysis, I refer only to tendencies for mobility within a given general geographical area, and not to the causes or consequences of these tendencies. Speculations on the latter topic will be found in the conclusion to this paper.

doctoral institutions during their early careers.[15] Thus, I am assuming that some downward mobility with respect to prestige is normal for new doctorates, but that if the prestige of one's doctoral institution really does determine his fate in the community, there is a lower limit beyond which other factors must be responsible for further downward mobility.

The second column in Table 2 consists of the percentage of respondents which would be expected for each component if there were no statistical association between the institution where a scholar receives his doctorate and the institution where he obtains a position. Thus, if a given institution produced one-quarter of all the doctorates in a given year and employed one tenth of all the doctorates, one would expect it to employ 2.5 percent (25% × 10%) of its own doctorates. Computing expected probabilities for moving from any institution to any institution for all institutions in the analysis, and summing with respect to each of the five components, one obtains the percentages in column two of Table 2.[16]

It is apparent that any measurement of the patterns of mobility within and between regions and prestige levels must take into account the fact that a process of random assignment of new Ph.D.'s to positions will produce a distribution for the five components in Table 2 which is not uniform for all components—and there is a possibility that this chance distribution might be misinterpreted as a reflection of differential patterns of mobility. For this reason, the third column of Table 2 presents a crude measure of the extent to which the observed values for each component deviate from those which would be expected on the basis of a random distribution model.[17]

All this is not to say that the absolute percentages reported in column one are meaningless. If 80% of those recruited to positions in universities were inbred into their doctoral institutions, this fact would have important implications for the nature of the academic community regardless of the percentage that one would expect to be inbred by "chance." Thus in the following analyses one must take into account the results in both column one and column three.

Examining the data presented in Table 2, we find that 50 percent of those respondents who were not inbred into their own doctoral institution obtained positions in the same prestige level (as defined earlier) as their doctoral institutions (29% + 21%). However, only 29 percent obtained positions which were like their doctoral institution with respect to prestige level only (same prestige level but different region), and this latter figure does not differ greatly from that which would be expected on the basis of a random recruitment model (32%). Thus, it would appear that two factors account for any nonrandom tendencies for new Ph.D.'s to obtain positions in the academic stratification system at the same prestige level as their doctoral institution. First, rates of institutional inbreeding are much higher than would be expected by chance, and second, a greater proportion of new doctorates than would be expected by chance stay within both the same region and the same prestige level as their doctoral institutions. These data suggest that American universities, rather than being structured in a rigidly stratified system of castes, form a more amorphous system wherein there is a tendency for each institution to rely, more than chance would predict, upon itself and institutions of similar location and prestige for the recruitment of

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[15] *Ibid.*, pp. 113–114; Caplow and McGee, *op. cit.*, p. 181.

[16] The expected frequency for the inbreeding component in Table 2 has been calculated from the observed institutional mobility matrix in the manner described above. Since I wish to keep the analysis of *inter*institutional patterns of mobility separate from the phenomenon of inbreeding, the expected values for the remaining components have been calculated according to a method presented by Goodman which assumes empty cells in the main diagonal of the mobility matrix (corresponding to the elimination of inbred respondents from this portion of the analysis). See Leo A. Goodman, "A Short Computer Program for the Analysis of Transaction Flows," *Behavioral Science*, 9 (1964) No. 2, pp. 176–186. Thus, the percentages for the last four components in Table 2 total to 100%. These general procedures are used throughout the rest of this paper.

[17] Since none of the values in Table 2 (and in similar tables throughout this paper) approach 100%, ceiling effects upon the Po-Pe measure are slight. For a general discussion of measures of deviation from "expected" mobility matrices, see Peter M. Blau and O. D. Duncan, *The American Occupational Structure*, New York: John Wiley and Sons, Inc., 1967, pp. 35–36, 90–97.

new doctorates. Thus, the selective tendencies displayed in Table 2 suggest that the academic stratification system is better represented as a set of regional hierarchies rather than a strict prestige hierarchy or only a set of regional groupings.

However, it should be pointed out that the data presented in Table 2 do not warrant the claim that the academic stratification system is *best* represented as a set of regional hierarchies. The fact that there is only a moderate difference between the observed and expected values for the "different region, different prestige level" component (this difference might be interpreted as the amount of nonrandom mobility which is not explained by region and prestige level) suggests that other variables in addition to region and prestige might serve as important parameters in a characterization of the academic stratification system. To point out variables with systematic deviations from a random mobility model is not to claim that models based on these variables alone necessarily fit the observed mobility patterns.[18] Indeed, comparisons of the observed interinstitutional mobility matrix (with inbred respondents removed) with that which would be expected on the basis of random assignment, and with several simple models based upon region and/or prestige alone, reveal that the random distribution model more closely approximates the observed mobility patterns than any of the other hypothetical models.[19] Nevertheless, it is clear that the selective tendencies reflected in Table 2 belie discussions of the academic stratification system which place primary emphasis upon the dimension of institutional prestige.

Since Table 2 is an aggregated presentation of the data, the possibility exists that the general patterns displayed in it do not characterize each region and each prestige level considered separately. In order to investigate this possibility, I have decomposed Table 2 into a series of presentations for each region and prestige level. These data are presented in Tables 3 and 4.

The most important data in Table 3 for the argument that selective regional tendencies are stronger than selective prestige level tendencies in the mobility patterns of new doctorates are found in the second and third rows of figures for each region. In four of the five regions, the observed percentage of respondents who remained in that region but moved to a different prestige level exceeds the expected percentage in this category. The one exception, the Plains and Mountains region, is characterized by high rates of inbreeding and remaining in the same region *and* prestige level as one's doctoral institution. In general, the results shown in Table 4 are consonant with those in Table 2, although the sizes of the differences between observed and expected percentages vary between regions. For example, unexpected tendencies toward regionalism appear to be strongest in the East (Po-Pe = 12%) and the Pacific Coast (Po-Pe = 9%), while the tendency to remain in the same region and prestige level as one's doctoral institution appears to be strongest in the South (Po-Pe = 29%) and Plains and Mountains (Po-Pe = 21%) regions. In a similar fashion, the percentage differences for respondents from each region who left the region but remained in the same prestige level are all negative in sign, with the lone exception of the Pacific Coast region.

The results reported in the second row for each prestige level in Table 4 show that universities in the lower prestige levels tend to send new Ph.D's to universities in the same prestige level but in different regions less than would be expected on the basis of a random model, but that this is not the case for universities in the higher prestige levels. Thus, the

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[18] In general, sociologists have devoted more attention to the discovery of deviations from random mobility models than to the construction of theoretical models which simulate observed mobility processes. *Cf. ibid.*, Ch. 2.

[19] This circumstance probably is due to two conditions. First, region and prestige level, in any model, are insufficient to adequately represent observed patterns of mobility. Second, even with only two general determinants such as region and prestige level, there is an infinite number of models which could be constructed (determinants do not unambiguously dictate a given model). Thus, perhaps models could be constructed which are based only on region and prestige and which fit the observed mobility patterns better than the few models constructed in this case. The models constructed were derived using the marginal totals from the observed matrix as constraints on the new expected matrices. A presentation of this sort of procedure will be found in the Appendix to C. Arnold Anderson, "A Skeptical Note on Education and Mobility," in A. H. Halsey, Jean Floud, and C. Arnold Anderson, editors, *Education, Economy and Society*, New York: The Free Press, 1961, pp. 164–179.

Table 3. Analysis of Regional Differences in Sending Patterns of New Ph.D.'s Who Obtained Positions in U.S. Universities

	Region														
	East			*Midwest*			*South*			*Plains and Mountains*			*Pacific Coast*		
Components	*Obs*	*Exp*	*Obs-Exp*	*Obs*	*Exp*	*Obs-Exp*	*Obs*	*Exp*	*Obs-Exp*	*Obs*	*Exp*	*Obs-Exp*	*Obs*	*Exp*	*Obs-Exp*
Inbreeding[a]	17%	1%	16%	16%	1%	15%	9%	1%	8%	40%	1%	39%	10%	1%	9%
Same region, different prestige level	27%	16%	12%	19%	13%	6%	5%	3%	2%	0%	2%	—2%	16%	6%	10%
Different region, same prestige level	25%	27%	—2%	29%	29%	0%	28%	50%	—22%	42%	57%	—15%	37%	28%	9%
Same region, same prestige level	14%	9%	5%	14%	8%	6%	55%	24%	31%	50%	12%	38%	13%	3%	10%
Different region, different prestige level	33%	48%	—15%	38%	49%	—11%	12%	23%	—11%	8%	29%	—21%	34%	63%	—29%
Total N		126			132			44			20			42	
Total N less inbred		105			111			40			12			38	

[a] The Inbreeding component has been calculated on the base of the entire sample. The other components have been calculated on the base of the entire sample less the inbred respondents.

Table 4. Analysis of Prestige Level Differences in Sending Patterns of New Ph.D.'s Who Obtained Positions in U.S. Universities

	Prestige Level											
	1 (highest)			*2*			*3*			*4 (lowest)*		
Components	*Obs*	*Exp*	*Obs-Exp*	*Obs*	*Exp*	*Obs-Exp*	*Obs*	*Exp*	*Obs-Exp*	*Obs*	*Exp*	*Obs-Exp*
Inbreeding[a]	13%	1%	12%	20%	1%	19%	17%	1%	16%	16%	1%	15%
Same region, different prestige level	25%	16%	9%	31%	15%	16%	7%	4%	3%	8%	8%	0%
Different region, same prestige level	16%	13%	3%	18%	21%	—3%	51%	61%	—10%	38%	50%	—12%
Same region, same prestige level	12%	6%	6%	8%	4%	4%	35%	20%	15%	38%	13%	25%
Different region, different prestige level	47%	65%	—18%	43%	60%	—17%	7%	15%	—8%	17%	29%	—12%
Total N		133			81			87			63	
Total N less inbred		116			65			72			53	

[a] The Inbreeding component has been calculated on the base of the entire sample. The other components have been calculated on the base of the entire sample less the inbred respondents.

summary figure reported in Table 2 for this component is not representative of each of the prestige levels. Also, in comparison to instituitions in the higher prestige levels, institutions in the lower prestige levels fail to send more new Ph.D.'s to different prestige levels within their own region than would be expected by chance. Two other features of the data in Table 4 will be noted. First, rates of remaining in the same region *and* prestige level as one's doctoral institution are highest in the lower prestige levels. Table 3 showed that these rates are highest in the South and Plains and Mountains regions, and it is these regions which are characterized by having universities which are quite predominantly in the lower prestige levels. Second, rates of inbreeding do not vary greatly over the prestige levels, with the highest level having a slightly lower rate than the other levels. However, since institutions in the higher prestige levels produce a larger number of new Ph.D.'s than institutions in the lower levels, similar rates across prestige levels will lead to a greater absolute amount of inbreeding in institutions in the highest prestige level. This will be shown in Table 6.

In sum, the results reported in Tables 2 through 4 suggest the following conclusions about the academic stratification system as a whole. First, unexpected (on the basis of the random distribution model) tendencies toward regionalism appear to be stronger than those toward prestige level homogeneity, but even the former tendencies are not sufficient to com-

Table 5. Mobility Patterns of Respondents Who Obtained Positions in U.S. Universities by General Disciplinary Area

	Physical and biological sciences			*Humanities and social sciences*		
Components	*Observed (Po)*	*Expected (Pe)*	*Po-Pe*	*Observed (Po)*	*Expected (Pe)*	*Po-Pe*
Inbreeding*	13%	1%	12%	18%	1%	17%
Same region, different prestige level	16%	9%	6%	22%	13%	8%
Different region, same prestige level	32%	35%	—3%	26%	30%	—4%
Same region, same prestige level	24%	11%	13%	18%	9%	9%
Different region, different prestige level	28%	45%	—17%	34%	47%	—13%
Total N		155			209	
Total N less inbred		135			171	

* The Inbreeding component has been calculated on the base of the entire sample. The other components have been calculated on the base of the entire sample less the inbred respondents.

Table 6. Patterns of Recruitment of New Ph.D.'s to Positions in Top Prestige Universities

Components	*Observed (Po)*	*Expected (Pe)*	*Po-Pe*
Inbreeding*	34%	3%	31%
Same region, lower prestige level	27%	16%	11%
Different region, same prestige level	39%	25%	14%
Same region, same prestige level	15%	11%	4%
Different region, different prestige level	18%	48%	—30%
Total N		50	
Total N less inbred		33	

* The Inbreeding component has been calculated on the base of the entire sample. The other components have been calculated on the base of the entire sample less the inbred respondents.

pletely account for the general patterns of mobility of new scholars among universities. Thus, interpretations of the academic stratification system as a fairly rigid prestige hierarchy would seem to be inappropriate. Second, the largest deviations from a model of random mobility patterns are found for the inbreeding and the "same prestige level-same region" components. The size of this latter factor, in conjunction with the unexpected tendencies toward regionalism alone, suggests that if any general model incorporating the element of institutional prestige is to be used in discussing the structure of the academic stratification system, it should be a model of regional prestige hierarchies.

Another decomposition of the data presented in Table 2 may be carried out by analyzing the patterns of mobility for the respondents in the sample who are in the physical and biological sciences as opposed to those who are in the humanities and social sciences. This is done in Table 5. The differences between the corresponding values for the two sets of respondents on each component are quite small. The humanities and social sciences exhibit a slightly higher rate of inbreeding than the physical and biological sciences, and a slightly lower rate of remaining in the same region and same prestige level, but otherwise no differences exist. Thus, no great exceptions to the general conclusions presented above occur when the sample is analyzed in terms of these two general sets of disciplines.

PATTERNS OF RECRUITMENT OF NEW PH.D.'S TO TOP PRESTIGE UNIVERSITIES

Up to this point in the analysis, I have examined tendencies toward regionalism, inbreeding, and prestige grouping in all U.S. universities (or in subsamples of them by region, etc.). In general, I have suggested that the portrayal of these universities as a simple prestige hierarchy is inappropriate. However, some of the strongest claims made by those who interpret the academic stratification system as a prestige hierarchy concern patterns of placement and recruitment in universities in the extremes of the prestige dimension. In particular, there have been claims that possession of a degree from a top prestige university is almost a necessary condition for recruitment into a top prestige university and that scholars from universities in the lowest prestige levels have little chance of obtaining jobs in higher levels, regardless of their competence. In order to evaluate these claims, data for recruitment patterns of top prestige level universities, and for the sending patterns of low prestige level universities to higher prestige level universities are needed. These data are presented in Tables 6 and 7.

Table 6 presents the percentages for each of the components for an analysis of the mobility patterns of new Ph.D.'s who were recruited to positions in top prestige level universities. The greatest difference between this table and earlier tables for all universities is the higher rate of inbreeding exhibited by top prestige universities. Second, there is a greater tendency for top prestige universities to recruit new Ph.D.'s from universities on the same prestige level but different regions than is the case for universities as a whole. Third, the tendency to recruit new Ph.D.'s from other top prestige institutions in the same region is smaller than is the case for universities as a whole. Thus, while universities in the highest prestige level do recruit new Ph.D.'s predominantly from top prestige levels, this is largely due to a much higher rate of inbreeding among top prestige universities than among other universities. A further analysis of these results by general academic discipline reveals two large differences between

Table 7. Patterns of Placement of New Ph.D.'s from Low Prestige Universities to Universities in Higher Prestige Levels (N = 53)

Components	*Observed* (*Po*)	*Expected* (*Pe*)	*Po-Pe*
Percent sent to higher prestige universities in same region	8%	8%	0%
Percent sent to higher prestige universities in different regions	17%	29%	—12%

the physical and biological sciences on the one hand, and the social sciences and humanities on the other. First, the high inbreeding rates observed in Table 6 are largely due to the respondents in the social sciences and the humanities (45% of those who are recruited to top prestige universities in the social sciences and humanities are inbred as opposed to 12 percent of the respondents in the physical and biological sciences). Second, top prestige universities are more likely to recruit from other top prestige universities in *different* regions in the physical and biological sciences (41%) than in the social sciences and humanities (18%). Values for the other components and expected values are quite similar for the two general disciplinary areas. Since these two differences largely balance each other, however, the over-all proportions of new Ph.D.'s from top prestige universities are about the same for both disciplinary areas.

Table 7 presents data for the sending patterns of universities in the lowest prestige level to higher prestige levels.

As is shown in Table 7, 25 percent of noninbred new Ph.D.'s from low prestige universities obtain positions in universities in higher prestige levels. However, this is smaller than the percentage which would be expected on the assumption of a random distribution model—the difference between observed and expected percentages being found in mobility to top prestige universities in different regions. Thus, the relative advantage of a new Ph.D. within his own region, and the disadvantage of his low prestige doctoral university origin are both shown in Table 7.

On the whole, then, it appears that new Ph.D.'s from institutions in the lowest prestige level are disadvantaged in comparison to their counterparts from institutions in the higher levels as far as recruitment to positions in high prestige institutions is concerned. These latter institutions depend upon themselves for new Ph.D.'s more than would be expected by chance, but general disciplinary area differences exist. In the social sciences and humanities, rates of inbreeding and recruitment from other high prestige institutions in the same region are high, while in the physical and biological sciences, rates of recruiting new Ph.D.'s from high prestige institutions in other regions are high.

PATTERNS OF MOBILITY AMONG RESPONDENTS WHO OBTAIN JOBS IN COLLEGES

Thus far I have examined the mobility patterns of new Ph.D.'s who have obtained jobs in university settings. Although there has been somewhat less speculation with regard to mobility patterns among those who obtain jobs in college settings, a few general notions will be examined. First, it might be expected that regional tendencies are stronger in the recruitment of new Ph.D.'s to jobs in colleges than is the case for universities.[20] Among the 318 respondents who obtained positions in colleges, 59 percent obtained positions in the same region as their doctoral institution, as opposed to an expected 25 percent. The corresponding figure for those recruited to positions in universities (with the inbreeding component, which has no counterpart among colleges, not included) is 42 percent, as opposed to an expected value of 32 percent (these latter values are from Table 2). Analysis of these results for the colleges by general disciplinary area and region reveal that although only slight differences exist between the two general disciplinary areas, larger differences exist among the various regions. The South (71%), and Pacific Coast (74%) exhibit fairly high rates of regionalism (the expected values in these three cases are around 22%), while the East (60%), Midwest (51%), and Plains and Mountains regions (13%) exhibit somewhat lower rates.

A second expectation about mobility patterns among those recruited to positions in colleges is that top prestige colleges recruit their faculty from the top prestige universities.[21] Table 8 presents the data bearing on this question.

The only large deviation from the component values which would be expected by chance is found in the "same region and top prestige level" component. Thus, top prestige colleges do recruit a slight majority (53%) of their new Ph.D.'s from top universities, but they do so within their own regions to a much greater extent than would be expected on the

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[20] For example, see Berelson, *op. cit.*, pp. 117–118.

[21] At least, Berelson shows that presidents of top prestige colleges believe that this is the case. *Ibid.*, p. 118.

Table 8. Patterns of Recruitment of New Ph.D.'s to Positions in Top Prestige Level Colleges (N = 30)

Components	*Observed* (*Po*)	*Expected* (*Pe*)	*Po-Pe*
Same region, not top prestige university	23%	19%	4%
Different region, top prestige university	20%	17%	3%
Same region, top prestige university	33%	10%	23%
Different region, not top prestige university	24%	54%	—30%

basis of Berelson's study of college presidents. Decomposition of Table 8 by general disciplinary area yields tables based on very small numbers of cases, but it is interesting to note that among the respondents in the social sciences and humanities (N = 20), 70 percent came from doctoral institutions in the same region as the college where they found positions (50% from top prestige universities and 20% from universities in lower prestige levels).

In general, then, colleges recruit new Ph.D.'s from universities within their own regions to a greater extent than would be expected by chance, and to a greater extent than the universities themselves do: and although top prestige colleges recruit about half of their new Ph.D.'s from top prestige universities, they recruit from their own region to as great an extent as colleges in lower prestige levels.

SUMMARY AND CONCLUSIONS

The data reported above are descriptive and cross sectional in nature, and, as such, allow no direct inferences either about the causes and consequences of the mobility patterns described above, or about trends in these patterns over time. However, on the basis of these data and findings reported by other investigators in this general area, a few tentative conclusions may be suggested. The overall picture of the academic stratification system presented in the above analysis is one of a set of regional hierarchies. In general, as one proceeds down the prestige dimension and into the ranks of the four year colleges, regionalistic tendencies become stronger. I will not attempt to speculate about the sources of these regionalistic tendencies here. Perhaps some combination of simplified explanations in terms of either the opportunities, or the desires of new Ph.D.'s with respect to job placement is the most appropriate approach. Nor will I speculate about the relative magnitude of these tendencies in comparison with points in time before and after Berelson's survey: perhaps the tendencies noted above are but dim survivals of earlier regionalisms, perhaps they have been fairly constant for some time. I would like to point out, however, that these tendencies are consistent with notions about regional schools in various disciplines. Although the existence of such schools is often asserted informally, there has been little study of their nature and extent. In his study of quality or prestige evaluations of U.S. universities, Cartter also has presented evidence which is consistent with notions of regional schools. Cartter compared the evaluations given by respondents in different areas and found a fairly consistent tendency for respondents to give highest average rankings to universities in their own region (of the four regions and four disciplines studied, highest average rankings were given to institutions in the same region in twelve of sixteen possible instances).[22] Cartter also reports that of the four disciplines studied in this manner (economics, English, physics, and political science), physics shows the least variation in ratings by region.[23] This is consistent with this paper's findings of greater tendencies toward regionalism in the social sciences and humanities as opposed to the physical and biological sciences. Of course, differential evaluations of institutional quality and patterns of recruitment of new Ph.D.'s have no necessary effects upon the intellectual styles and perspectives which are the usual objects of notions about regional schools. However, these kinds of differential tendencies serve both as possible sources of differences in intellectual perspectives, and as

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[22] Cartter, *op. cit.*, pp. 82–84, 90–91, 96–97, 102–103.

[23] *Ibid.*, p. 96.

possible indicators of the institutional structure of such differences.

Sociologists have devoted little attention to the possibility that phenomena such as those discussed above may also have some impact upon the structural cleavages within American society. Glenn and Simmons recently presented evidence which casts some doubt upon the argument that, under the influences of inter-regional migration, urbanization, and the mass media, the U.S. is becoming a "mass society" characterized by a homogeneity of attitudes and behavior on the part of its citizens. In addition to showing that regional differences in attitudes on issues in a variety of areas (religion, morals, politics, etc.) continue to exist, Glenn and Simmons suggest, on the basis of comparisons of age cohorts, that regional differences in many attitudes may be increasing.[24] Although Glenn and Simmons explain these differences in terms of such factors as differential impact of the mass media in different regions, regional tendencies among U.S. institutions of higher education may also have some impact.[25] Indeed, the data presented in this paper suggest that regionalistic tendencies may be strongest in the social sciences and humanities at top prestige colleges, and at colleges in lower prestige levels generally. Insofar as the former institutions are a primary source of higher education for upper middle class children, and the latter institutions are composed of four year teachers' colleges and other colleges with programs in education, populations which play an important role in the setting of regional cultural patterns may receive differential training.

These speculations clearly rest upon many as yet untested assumptions. Longitudinal studies of phenomena such as that reported in this paper, and studies of interpersonal relations among American academicians, would be relevant here. It is hoped that this paper will lead to further efforts along these lines.

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[24] Norval D. Glenn and J. L. Simmons, "Are Regional Cultural Differences Diminishing?" *Public Opinion Quarterly*, 31 (1967), pp. 176–193.

[25] Ladinsky has shown that academicians as a group have relatively high rates of geographic mobility compared to other professional groups. However, studies of the patterns of mobility of these important populations have not been carried out. Jack Ladinsky, "Occupational Determinants of Geographic Mobility Among Professional Workers," *American Sociological Review*, 32 (1967), pp. 253–264.

Social Structure in a Group of Scientists: A Test of the "Invisible College" Hypothesis

Diana Crane

One type of social group which has received relatively little attention from sociologists is the group comprised of scientists who work on similar research problems. This neglect is probably due to the amorphous character of this type of group. Its members are highly individualistic and widely separated geographically. Participation is voluntary. Turnover is very high; the majority of scientists have only one or two publications in any research area (Price, 1963). Even the boundaries of research areas are difficult to define since most scientific work can be classified in numerous ways, and, often, agreement among scientists regarding the categorization of certain work is far from unanimous. It can thus justifiably be asked whether anything resembling social organization exists in a research area.

Studies of informal communication among scientists indicate that scientists working on similar problems are usually aware of each other and in some cases attempt to systematize their contacts by exchanging reprints with one another (Libbey and Zaltman, 1967; Menzel, 1960; Paisley, 1965). Price has stated that some but not all scientists in a particular research area maintain a high level of informal communication and that information received in this manner is essential for the conduct of effective research (Price, 1963). The amount of material published in some fields is so large that it cannot be monitored effectively by any other

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means. Kuhn (1962) has argued that groups of scientists develop shared definitions of their work, paradigms which interpret findings and guide new research. In other words, scientists adjust to the problems of dealing with knowledge in their fields by forming social organizations of various kinds, based upon shared communication and shared interpretations of the situation.

However, the existence of "invisible colleges" has been difficult to prove. Scientists have many contacts with other scientists in their own research areas and in other fields, some fleeting, some lasting. If social organization exists in a research area, it is of a highly elusive and relatively unstructured variety. The present research was designed to discover if anything resembling social organization could be observed in a research area. For this purpose, a sociometric analysis of a group of scientists with common research interests was required.[1] The existence of social organization could be inferred (a) if scientists who had published in a particular research area had more social ties with one another than with scientists who had not published in the area, and (b) if scientists who had published in the area could be differentiated in terms of degrees of social participation within the area, suggesting the existence of leadership in the field.

THE MEASUREMENT OF SOCIAL TIES

If social organization exists in a research area, it is likely to be based on a number of different types of social ties. Informal communication regarding research findings, research-in-progress, and research techniques represents one way in which members of a problem area can be linked to one another. In the present study, information about informal communication was obtained only from those currently engaged in research in the field, a subsample of 52 members from a total of 147 respondents.

In addition to informal communication, several other types of ties between scientists exist. Collaboration occurs in several ways. . . .

Also, there are intellectual linkages represented by the influence of one scientist's work upon that of another. Citation references in journal articles are direct indications of such influences, and studies of citations have been made (Garfield, 1964; Kessler, 1965; Weinstein, undated). This type of relationship can also be measured by asking scientists to name others who have influenced them in their selection of problems and techniques. Questions of this sort do not elicit data identical to that obtained using linkages among citations. The latter method is more precise and provides more complete information. In addition, the question used in the present study deliberately restricted the scope of the information requested in order to simplify the task of the respondent. Respondents were asked about influences on the selection of problems and techniques but not about influences upon the course of the research. However, many articles include numerous citations, but the relative importance of each citation may vary considerably from a reference to a scientist whose work has had a very strong influence on the author to a scientist whose work is relevant only in connection with a minor point. The use of a questionnaire to elicit some of this information probably has the advantage of obtaining the most important influences, rather than a complete list of major and minor influences.[2]

Each of these several different types of

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[1] Although there have been a number of studies of informal communication among scientists, a sociometric study of communication patterns among scientists who are doing research in the same area had not been attempted before. Nicholas Mullins (1966) did a sociometric analysis of informal communication but did not focus upon a particular field. Several sociometric studies are currently in progress, for example, Lingwood (1968), APA Project on Scientific Information Exchange in Psychology (1968), and a dissertation by David Vachon (1969), who was the first to use the Coleman Sociometric Connectedness Program with this type of data. For analysis of problem areas using different techniques, see Fisher, 1966 and 1967, Mullins, 1968 and Russett, 1969. For analyses of scientific literature which relate to this problem, see Cole (1962 and 1963), Kessler (1965), Kossler and Heart (1962), McGrath and Altman (1966), Meadows (1967), Parker et al. (1967), Price and Beaver (1966), Stevens (1953), Stoddart (1967) and Weinstein (undated).

[2] A copy of the questionnaire which was used to elicit information concerning social ties in the area will be sent upon request. The question requesting names of scientists who had influenced the respondents' selection of research problems was not answered by 41% of the group. A similar question requesting names of scientists who had influenced their selections of research techniques was not answered by 59%. . . .

ties among scientists produces a somewhat different picture of the relationships within the problem area and between members of the area and outsiders. But the juxtaposition of these various indicators of linkage may provide an approximate picture of the degree of the relationship of each with every other. Combining several indicators into a composite index supplies further information about the group. . . .

. . . For the purpose of this study, it seemed preferable to rely on three methods. The first was simply an overall quantitative statement of the number of choices of various kinds directed both within and without the problem area.

The second was a method of assessing the extent of direct and indirect[3] relationships between members of the problem area. . . .

All techniques for assessing indirect relationships in groups other than analysis by manual procedures, which are impractical with large groups, are approximate (Coleman, 1964: 447). The technique which was used here was a modification of one which was devised by James Coleman (1964:444–455).[4] The choices of members of a group are arranged in matrix form where one axis represents choices made by a member of the group; the other axis, choices received by a member. Continuous multiplication of the matrix by itself provides a means of locating the indirect relationships between the individuals represented in the matrix. This analysis provides figures indicating the total number of persons to whom a particular scientist's choices directly or indirectly lead and the total number of persons choosing him directly or indirectly. In addition, it is possible to construct a measure of connectivity for a group as a whole. This is done by dividing the number of connections which occur (either from or toward individuals) by the total number of connections which could possibly occur.

Finally, direct and indirect relationships among members of different subgroups within the problem area were analyzed. For this purpose, members of the problem area were divided into five groups on the basis of productivity in and commitment to the area. The latter was measured in terms of continuity of interest in the area. Using three levels of productivity and two of continuity, it was possible to form six groups, of which five actually occurred in this problem area. Members of three subgroups indicated in response to an item on the questionnaire that they had continued to do research in the area: (1) eight High Producers, each of whom had published more than ten papers in the area; (2) eleven Moderate Producers, who had published four to ten papers in the area; (3) 33 Aspirants, who had published fewer than four papers in the area. Members of two subgroups indicated that they had not continued to do research in the area: (1) nine Defectors, each of whom had published four to ten papers in the area; (2) 86 Transients, each of whom had published fewer than four papers in the area. Every one with more than ten papers continued with his research in the area.

DESIGN OF STUDY

. . . At least four methods of locating members of a problem area are possible: the use of bibliographies, abstracting services, citation networks and sociometric data. Each of these methods has limitations in terms of locating a complete list of members. Lists of abstracts and bibliographies reflect the evaluations of relevance made by the compilers; citations reflect the evaluations of relevance of authors as do sociometric questions. In each case, some references or some names are likely to be omitted.

Although these problems cannot be solved completely, the availability of a bibliography, designed especially for maximum completeness and compiled by a highly productive member of a problem area, provided one means of dealing with the problem outlined above. Use of such a list made it likely that a very high proportion of members of the relevant group would be located.

The problem area selected for this study, the diffusion of agricultural innovations, was part of the research specialty—rural sociology. Since the "invisible college" hypothesis was developed from observations of the behavior of scientists in fast-moving specialties within

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[3] An indirect relationship is one in which A is linked to C because both are in communication with B.

[4] The program which was used here is a substantial revision and correction of the program which Coleman outlines in his book (Coleman, 1964:466–468).

physics, the selection of a research area in the social sciences might seem inappropriate. However, an analysis of the 403 papers published in the area between 1941 and mid-1966 revealed that it possessed several of the characteristics which have been found in the literature in the natural sciences (Price, 1961). . . . Although the field contained a larger proportion of single author publications (61%) than would be expected for a research area in the natural sciences, so many factors are believed to influence the amount of collaboration in a research area, and these factors are so imperfectly understood that this characteristic did not seem important enough to disqualify the area for a study of this type. . . .

Using the most recent bibliography compiled for the area (Rogers, 1966), questionnaires were sent to 172 of the 221 scientists listed, both junior and senior authors; 147 replies were received. Each respondent was sent a letter which included references to his publications as listed in the bibliography and was requested to respond to the questionnaire with respect to those publications only (rather than with respect to any other publications he might have produced).

The aim of the study was to obtain some information regarding the circumstances surrounding publication in the area from every scientist who had ever published in it. Since publications in the area began to appear in 1941, some respondents were describing events which had taken place many years ago. The majority of the publications in the area have appeared in the last ten years. . . . Even though information about publications written many years ago may not be as precise as information about recent publications, it seemed preferable to have information about both types of publications in order to understand the social organization of the area.

CHOICES WITHIN AND OUTSIDE THE AREA: THE GROUP AND OUTSIDERS

On every measure examined, respondents were about as likely to choose a scientist who had not published in the area as they were to choose one who had. Out of a total 1351 choices made by all respondents on all the different types of ties, outsiders were named 684 times (51%) and problem area members 667 times (49%). Does this suggest that a social group within the area did not exist? There are three reasons for arguing that these figures do not imply the absence of social organization within the problem area.

First, the majority of "outsiders" were selected only once; 84% were chosen no more than twice; twelve, or 3%, were named more than five times. Within the problem area, about half the members were never named; 78% were chosen twice or less. Most of these scientists had been relatively unproductive; many had only a brief contact with the problem area. On the other hand, 26, or 12%, were named more than five times. Fifteen, or 7%, were named more than ten times. Thus, the social organization of the problem area appeared to be centered around a small and relatively productive proportion of the total membership.

Since only one outsider was named more than ten times, there did not appear to be a group of outsiders whose influence had similar weight. The twelve outsiders who received more than 5 choices could be characterized in three ways. One was a scientist working in the problem area who had not yet published at the time the questionnaires were mailed but who published during the following year. Two others had published numerous articles in an adjacent problem area, the study of the diffusion of medical innovations. Most of the remaining frequently-chosen outsiders had achieved a high degree of eminence. . . .

There was some indication that the relationships between respondents and members of the problem area were different from their relationships with scientists who had not published in the area. Respondents were asked to indicate if they had any personal acquaintance with the scientists whom they mentioned as having influenced their selection of problems in the area. Of 246 choices of problem area members, 76% were designated as personal acquaintances; of 256 choices of outsiders, 57% were indicated to be personal acquaintances. This outcome suggests that to some extent the influence of outsiders was exerted through publications, while that of insiders was exerted through personal contact.

The problem area which included 221 scientists was able to compete with respect to choices with members of a very much larger population, the former receiving 49% of the total 1351 choices. The outsiders were drawn from several disciplines (unfortunately, this information was not available for many of the outsiders) which altogether include more than twenty thousand scientists. Similar distributions of choices have been found in the analysis of citation references (Swanson, 1966).

CHARACTERISTICS OF MEMBERS OF SUBGROUPS IN THE DIFFUSION PROBLEM AREA

Selection of Group Members versus Outsiders.—The five subgroups differentiated on the basis of productivity and commitment to the area were expected to exhibit varying degrees of linkage both with the members of the group and with those who had not published in the area. When all types of ties were considered together, one subgroup of highly productive members appeared to be equally strongly linked to the area and to outsiders. Another subgroup (one which was relatively unproductive) appeared to have a relatively peripheral relationship to the area as well as to outsiders. The other three subgroups ranged between these two in degree of linkage to the area and to outsiders. This suggests that both types of ties are characteristic of productive scientists in a research area.

Direct and Indirect Ties by Subgroups.—When direct and indirect choices of the members of the various subgroups were measured using the Sociometric Connectedness Program, the differences between the subgroups emerged more definitively. Among the subgroups currently doing research in the area, informal communication choices by the High and Moderate Producers linked them to more members of the group than did the choices of the Aspirants. In addition, a high proportion of choices by others led to the High Producers, placing them in the center of a fairly sizeable communication network. . . .

High Producers were more likely to be linked to others through communication ties which they considered to be very important for their research; moreover, a high proportion of choices of this type by others also led to them. The communication choices of the High Producers were more frequently reciprocated than were those of the other groups, again suggesting their central position in the group.

The High Producers were also linked through published collaboration to a large number of other members of the area, suggesting that much of the collaboration in the area had been conducted in association with High Producers. In addition, the High Producers had played an important role in the area as thesis directors.

The High Producers were not predominantly linked to others in the area through their own choices of influences on the selection of problems; however, they did receive a much higher number of direct and indirect choices with respect to this type of influence. They had apparently played an important role for many members of the area in the selection of research problems. The Defectors were the only other group that had played a significant role in this respect. However, comparisons of direct and indirect choices suggested that their role had been largely indirect. They had influenced the High Producers, who, in turn, had influenced many of the other members of the group. The role of the High Producers as influences on the selection of research techniques was less pronounced, although choices of members of the area were also more likely to lead to them than to members of other subgroups.

When all types of links were examined together, the High Producers, the Moderate Producers and the Defectors were linked to many members of the area through their own choices. The Aspirants were linked to fewer members and the Transients to even fewer. The High Producers and the Defectors also dominated on the receipt of choices. One hundred percent of the High Producers and of the Defectors had very high scores, much higher than any of the remaining groups. Thus, the High Producers, sometimes in company with the Moderate Producers, sometimes along with the Defectors, provided orientation for the other members of the area.

The group connectivity scores show the extent to which members of each subgroup had activated their potential relationships with each other and with members of other subgroups. This measure can be computed in two ways: (a) the number of choices of members of the entire group made by a subgroup in relation to the total number of possible choices and (b) the number of choices of a particular subgroup made by its members and members of other subgroups in relation to the total number of possible choices. The first measures how often members of a subgroup choose other members; the second measures how often members of a subgroup are chosen by members of the entire group. The High Producers' choices tended to relate them to more members of the area than did the choices of other subgroups. However, with respect to certain ties, they were chosen by members of other subgroups more frequently than they chose members of these subgroups. For example, 21% of the possible links between High Producers and other members of the area currently active in research were realized on the basis of the High Producers' own choices with respect to informal communication. Through the choices of others, 47% of the potential ties with members of the area were realized. With respect to influences on the selection of problems, only 4% of their potential links with other members of the area were filled as a result of their choices. On the basis of the choices by others in the area, 21% of these relationships were filled. A similar type of difference appears when all types of ties were considered simultaneously.

. . . Since the High Producers were more frequently linked to the group as a whole by the choices of others than by their own choices, it would seem that they had a high degree of visibility in the problem area. Many others looked to them for orientation. Thus the connectivity which developed in this area appeared to be at least partially the result of a large number of choices directed toward a few members. Anyone choosing even one of these individuals was brought into contact with a large network of individuals. This is a reasonable outcome in science where students or collaborators of very productive scientists are brought into contact, directly or indirectly, with many other scientists in the field. However, for some members, this contact had been very brief. Many of the Transients appeared to have had only peripheral contact with the area. . . .

CHANGES IN THE NETWORK OF SOCIAL TIES OVER TIME

So far, the group has been described without reference to the fact that members belonged to the group at different times and for different lengths of time. This approach is justified by the fact that, in a research area, scientists who are no longer working in a field continue in a sense to be the colleagues of those who are presently working in the field since their publications provide the basis for future research. However, as was described earlier, the amount of activity in the area steadily increased during the 25-year period examined. During the first ten years of activity in the area, only 5% of the members were active. Between 1948 and 1958, the number of authors entering the area doubled every three years. After 1958, the number of authors entering the field doubled every five years; the increase per year stabilized at approximately 17 authors per year. The trend for the number of publications was similar. Between 1951 and 1960, 49% of the authors entered the field, most of this increase occurring during the latter part of the decade. Another 46% entered between 1961 and 1966.

Since, as we have seen, the High Producers were very influential in the area, tracing the development of their interest in the field would help to explain the growth of the area. Two High Producers entered the field in the first decade, apparently influenced by the publications of a Defector and a Transient who had been active during that period. In the middle of the second decade, each of these High Producers had a student who also became a High Producer. Another thesis director and his student, both High Producers, entered the field at the same time along with a collaborator who also became a High Producer. By 1957, all of the High Producers had entered the field. Although almost two-thirds of the Aspirants and more than one-third of the Transients entered after 1960, only 11% of the more productive scientists (those with more than three publications in the area) entered the field in that period. Since at that time interest in the field was beginning to diminish relative to its earlier rate of exponential growth, this suggests that productive scientists may be especially sensitive to the potentialities for growth in a field in making their selection of research problems.

The influence of members who had published in the area steadily increased relative to those who had not published. . . . The influence of the High Producers also increased over time. None of the theses written in the area by scientists entering the field before 1956 were directed by High Producers. After 1956, 38% of the theses in the area were directed by High Producers, a subgroup representing 8 individuals. Twenty-seven percent of the theses were directed by members of the four remaining subgroups representing 139 respondents. . . .

Another development reveals the changes which were taking place in the field. On the basis of collaboration and student-thesis director relationships, members of the area were assigned to distinct subgroups of varying sizes (see Crane, 1968a; Price and Beaver, 1966). A scientist was assigned to a particular group of collaborators if he had a published collaboration with at least one of its members or had been the student or thesis director of at least one of its members. Before 1956, the group consisted of small groups of collaborators and student-teacher pairs and a number of isolates. After 1956, when the group as a whole increased in size, some of these small groups expanded. Two large groups emerged, with 27 and 32 members respectively, as well as several medium-size groups with five to 13 members. A number of new small groups with 2 to 4 members and numerous isolates appeared. Few of the small groups lasted more than a couple of years. Turnover among the isolates was also high. Medium-size groups, especially the relatively larger ones, were more durable. . . .

Six of the eight High Producers were members of the two large groups. The remaining two belonged to the largest of the medium-size groups. The large groups of collaborators appeared to reflect the way in which the High Producers exerted their influence upon the field, i.e., by surrounding themselves with students and collaborators among whom turnover was frequently high. Thus, these large groups of collaborators, under the direction of High Producers, provided continuity and structure in a situation which would otherwise have been amorphous due to the continual arrivals and departures of less committed scientists. . . .

CONCLUSION

As predicted, the social group which was described here was not one which was tightly knit or closed to external influences. "Outsiders" played an important role in influencing the activities of members of the group. That a social group within the problem area existed was principally apparent from the number of choices received by a relatively small number of members of the group. It appeared that members of the area were not so much linked to one another directly but were linked to each other indirectly through these highly influential members. The importance of these scientists was not obvious, except in terms of productivity, unless one examined their many and varied influences on the remaining members of their field, each of whom might be affected in only one or two ways and for short periods of time.

A question which remains to be discussed is how a group of this kind can best be conceptualized theoretically. Price has popularized the term "invisible college" which refers to an elite of mutually interacting and productive scientists within a research area. However, this conceptualization does not comprehend two aspects of the social organization of research areas which have emerged from the analysis presented here: (a) the interaction between the most active and influential members of the area and the "rank and file" and (b) the role of "outsiders" in the organization of the area.

Of the various types of social organization which have been identified by sociologists, the social circle would appear to be the one which best describes the social organization of a research area (Kadushin, 1966). The social circle is not well instituted, compared to the bureaucracy or even to less formalized entities such as the tribe or the family. Members come together on the basis of their interests rather than propinquity or ascribed statuses. Indirect interaction, interaction mediated through intervening parties, is an important aspect of the social circle. It is not necessary to know a particular member of a social circle in order to be influenced by him. Certain characteristics are more common to members of a particular social circle than to non-members, but members do not necessarily share all or even most of these defining characteristics. Each member is usually aware of some but not all other members. The exact boundaries of the social circle are difficult to locate. . . .

Future studies will be needed to perfect the methods for studying groups of this kind. For example, it would be useful to compare the results of different techniques for locating groups of scientists, such as the use of comprehensive bibliographies, abstracting services, citations, and sociometric data with "snowball" samples (Goodman, 1961; Mullins, 1966) . . . Kessler (1965) has shown that scientific publications form groups on the basis of common citation references but no attempt has yet been made to explore the social ties among authors drawn together in this fashion. Cluster analysis (Bailey, 1969) is being used to locate groups of scientists with similar interests on the basis of thematic similarities in the contents of research grant applications. This technique which could also be used to analyze the contents of abstracting services will be combined with analysis of social relationships among members of such groups.

Evaluation of different methods for analyzing this type of data is needed. The present study used Coleman's Sociometric Connectedness Program to examine the relationships between members of an entire group. Sociometric techniques for determining non-intersecting cliques are still inadequate (Lingwood, 1968: 20). However, if a group is actually a social circle, one would not expect to find distinct cliques based on sociometric ties. A true social circle should consist of a complex and undivided network of relationships. If such a group is to be meaningfully partitioned, it ought to be done on the basis of criteria other than social relationships. In the present paper, productivity and commitment to the area were used. Price and Beaver (1966) have suggested a method of locating cliques which utilizes ties based on published collaborations. Communication and influence ties between units formed on this basis have been examined (Crane, 1968a). Russett (1969) relied upon factor analysis to identify within a problem area subgroups who tended to cite each other's publications.

Finally, the points of intersection between problem areas should be examined. . . .

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Stylistic Variations among Professional Research Scientists

Harrison G. Gough and Donald G. Woodworth

PROBLEM

It is a matter of common observation in most fields of endeavor where high-level or professional talents are involved, e.g., in music, writing, artistic creation, athletics, etc., that stylistic differences as well as competence differences exist among practitioners. One would certainly expect to find similar stylistic variations among research scientists, although there seems to be very little research work by psychologists that one can turn to for evidence bearing on this anticipation. In our study at the Institute of Personality Assessment and Research of the research originality and general competence of professional workers in the physical sciences and engineering we therefore decided to attempt an exploration of this facet of the scientist's activity.

METHOD

Materials

The first task was to assemble a list of evaluative and descriptive statements which would encompass the range of approaches to research found among working scientists and which would possess the classificatory subtlety needed to delineate the differences between one man and another. In the spring of 1957 we spent a period of about two months working with consultants from physics and several engineering fields writing such material, and seeking to insure the inclusion of items possessing the required degree of relevance and diagnostic clarity. The preliminary list of statements was reviewed by members of the Institute staff and by others, resulting in the end in a set of 56 items suitable for *Q*-sorting or check-list usage, and for use by a scientist to describe his own work or that of another person.

The list of 56 items is presented in Table 1. For our study the items were used in *Q*-sort form. Each one was printed on a separate card with the subjects being asked to sort the deck of cards for personal relevance using the frequencies 5, 12, 22, 12, and 5. The task was found to be interesting by nearly all of the men examined, and typically took only 15 or 20 minutes to complete.

Subjects

The sample of subjects was comprised of 45 professional research scientists drawn from three industrial laboratories on the West Coast. These men were all volunteers for participation in a three-day program of assessment at the Institute in Berkeley, but each man had earlier been designated by the personnel director of his company as being employed in a scientific research capacity. Completion of the "Research Scientist Q-sort Deck" (*RSQD*) was one of many assessment tasks each man carried out while he spent his weekend at the Institute.

As might be expected, the sample reveals a preponderance of subjects with advanced educational training. Twenty-eight held Ph.D. degrees, four the M.S. degree, eight had had some graduate work, two held the B.S., whereas three had not completed a college degree. The age distribution was as follows: 25–29–five; 30–34–seventeen; 35–39–fifteen; 40–44–four; 45–49–three; and 50–54–one; the mean age was 35.69, standard deviation 5.46.

On the Terman Concept Mastery Test, Form *T* (Terman, 1956) the mean score was 118.2, standard deviation 29.4. This is fairly high average, as evidenced by these illustrative averages taken from Terman's *Manual:* Subjects of the Stanford Gifted Study $\overline{x} = 136.7$, $SD = 28.5$; applicants for Ford Foundation Fellowships, $\overline{x} = 117.9$, $SD = 35.1$; college graduates, $\overline{x} = 112.0$, $SD = 32.0$; spouses of gifted subjects, $\overline{x} = 95.3$; $SD = 37.0$; and Air Force captains, $\overline{x} = 60.1$, $SD = 31.7$.

Procedure

Various analyses of the *Q*-sort protocols could be conducted. To serve our present interest in the stylistic variations in approaches to

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Table 1. List of Items in the "Research Scientist Q-sort Deck"

1. Reacts quickly to research problems; immediately generates a great number of ideas.
2. Is somewhat deficient in his command of basic sources and technical literature in the field.
3. Pursues details and ramifications of research problems with great thoroughness.
4. His research interests lie within a rather narrow range.
5. Has exceptional facility in mathematical analysis.
6. Easily discouraged; needs help and encouragement to do his best work.
7. Is keenly interested in methodological aspects of research.
8. Prefers to work on problems which lend themselves to elegant and exact solutions.
9. Tends to slight the contributions of others; takes undue credit for himself.
10. Likes to play his hunches in research; is guided by his subjective impressions.
11. Is neat and orderly in his habits and manner of work.
12. Dislikes and avoids administrative details connected with research projects.
13. Is good at developing short-cuts and approximation techniques.
14. Frequently makes errors; his work needs to be checked for accuracy.
15. Prefers to work alone; is not a "team" research man.
16. Is erratic in his research output; varies from work of excellent quality to work of marginal or even inferior worth.
17. Indifferent to the practical implications of his own research.
18. Seldom comes up with a really new idea or suggestion.
19. Has exceptionally high standards of research performance for himself as well as for others.
20. Has a special talent for solving instrumentation problems.
21. Is thorough and patient in his approach to research issues; does not get upset if progress is slow.
22. Is a driving, indefatigable research man; cannot stop working on a problem until it is solved.
23. Is relatively uninformed on most subjects other than his research specialty.
24. Good at evaluating research; able to diagnose strong and weak points in a program quickly and accurately.
25. Is fiercely competitive; wants to be the best man in every research task that he undertakes.
26. Seeks out the help and advice of other people when he hits a trouble spot in his own research.
27. Makes a serious effort to read current publications and to "keep up" on the literature in his field.
28. Is creative in anything he tries, whether in science or not.
29. Has an orderly, well-organized approach to research; plans his projects and activities with great care and precision.
30. Gives freely of his own time and ideas to other research endeavors without asking for special credit or recognition.
31. Likes to talk out his research ideas and get other people's reactions.
32. Many of his ideas turn out to be impractical.
33. Is aware of his own professional limitations; does not attempt what he cannot do.
34. Has a quick tempo of thought and speech.
35. Prefers to think in analytical and mathematical ways, rather than in terms of physical and structural models.
36. Is never too busy to "talk shop" with other researchers.
37. Has an exceptionally good memory.
38. Has a knack for improvising quick solutions in research trouble spots.
39. Is a research perfectionist; devotes endless attention to matters of design, apparatus, procedure, etc.
40. Plays his cards "close to the vest"; prefers not to tell anyone about his research plans until his work is finished.
41. Is a creative and inventive researcher.
42. Is intolerant of metaphysical issues.
43. Is a talented "re-write" man; can take other people's ideas and hunches and fashion them into practical research designs and programs.
44. Has strong research biases; is vehement in his disapproval of certain methods and procedures.
45. Primarily an "idea man"; prefers to turn his hunches and hypotheses over to someone else for systematic experimentation and analysis.
46. Somewhat given to bluffing; claims to know more than he does.
47. Stimulating to other people; seems to catalyze others into more original and productive endeavor than they would otherwise achieve.
48. Tends to be sarcastic and disparaging in describing the work of other researchers.
49. Is intellectually gifted.
50. Has a lively sense of intellectual curiosity and inquiringness, a desire to know and to understand.
51. Is flexible and adaptable in his thinking; able to shift and to restructure easily.
52. Takes an esthetic view; is sensitive to matters of form and elegance in research problems.
53. Has a "sense of destiny" with respect to his own research career, an inner conviction of the worth and validity of his own efforts.
54. Enjoys philosophical speculation.
55. Lacks confidence, is afraid to strike out in new directions.
56. Subordinates everything to his research and scientific goals; puts scientific values above all others.

scientific and research activity it was felt that an inverse factor analysis, or factor analysis of "persons" would be the method of choice. The specific technique employed was a principal axis factor analysis, . . . followed by the varimax rotation for factorial invariance developed and programmed by Kaiser (1958).

Because the factor analysis program could accommodate only 36 persons, the calculations were first carried forward on the matrix of the first 36 names in an alphabetical list. From this matrix eight "person-factors" were extracted, which after rotation accounted for 22 per cent, 19 per cent, 14 per cent, 10 per cent, 9 per cent, 9 per cent, 9 per cent, and 7 per cent, respectively, of the analyzed variance. Approximately 71 per cent of the total variance in the matrix was included in the analyzed variance.

Following this, the three to five scientists (of the 36) having highest loadings on each factor were identified, and then work sheets were set up giving the average zero-order correlation of each of the 45 scientists with the composite of scientists chosen as the "marker" for each type. The purpose of this improvisation was merely to discover whether any of the nine men not included in the factor analysis had prominent enough correlations with any of the eight dimensions to justify being included in the reference or "marker" composite.

The next step was to select from this work sheet of average correlations the cluster of men to be used in deriving the reference *Q*-sort for each of the eight factor dimensions. From three to five men were again chosen to establish each composite. The individual *Q*-sorts for the men chosen were totaled and then re-sorted into *Q*-form. These eight new *Q*-sorts constitute the reference or defining *Q*-sorts for each of the factor types. Table 2 presents these reference *Q*-sorts for each of the eight types.

Table 2. Reference Q-sortings for the Eight Stylistic Types on the Research Scientist Q-sort Deck*

	Reference sortings							
Card	*I*	*II*	*III*	*IV*	*V*	*VI*	*VII*	*VIII*
1.	3	5	4	3	2	4	3	3
2.	2	3	2	3	4	3	3	4
3.	4	3	3	3	3	3	3	3
4.	3	2	2	2	3	1	3	3
5.	5	2	4	4	2	5	5	2
6.	2	2	2	2	4	3	2	2
7.	3	2	3	3	4	3	5	3
8.	5	3	3	3	3	5	4	3
9.	1	1	2	2	1	2	1	1
10.	3	3	3	3	3	4	3	4
11.	3	3	3	5	3	1	3	2
12.	4	3	3	1	3	4	3	5
13.	2	4	4	4	4	4	4	3
14.	1	2	1	2	2	4	2	3
15.	3	1	4	3	3	4	3	5
16.	2	3	1	2	3	3	3	3
17.	2	2	3	2	3	2	4	3
18.	1	2	1	3	3	3	2	2
19.	4	3	4	4	3	4	3	3
20.	2	4	2	3	4	2	1	4
21.	3	3	3	4	4	1	3	2
22.	5	3	3	3	3	4	3	1
23.	2	3	2	1	2	2	2	3
24.	4	4	5	4	3	3	4	3
25.	3	2	4	2	2	3	1	3
26.	4	4	2	5	4	2	4	4
27.	4	4	3	4	3	2	4	4
28.	3	4	5	2	3	3	4	1
29.	3	3	3	4	3	2	2	2
30.	4	5	3	3	5	4	4	2
31.	4	4	3	4	4	3	5	4
32.	2	3	2	3	1	3	3	3
33.	3	3	4	4	5	3	4	4
34.	3	4	3	3	2	3	3	4
35.	4	3	3	3	1	5	5	1
36.	4	4	3	4	5	4	3	4
37.	3	3	2	5	1	3	2	3
38.	3	4	5	3	4	3	2	4
39.	3	2	3	1	3	1	2	3
40.	2	2	3	3	3	3	1	2
41.	5	5	4	3	2	3	3	3
42.	2	2	4	5	1	2	3	3
43.	3	3	3	4	5	3	3	3
44.	3	1	1	1	3	2	1	3
45.	3	4	3	2	2	3	3	3
46.	2	3	2	1	2	2	2	2
47.	3	5	4	3	2	3	4	3
48.	1	1	1	2	2	1	2	2
49.	4	3	4	3	3	3	3	4
50.	5	5	5	3	4	5	4	5
51.	3	4	4	5	4	4	4	5
52.	3	3	3	3	4	5	5	2
53.	4	3	3	2	3	2	3	4
54.	2	3	5	4	5	4	3	5
55.	1	1	2	3	3	2	2	1
56.	3	2	2	2	2	3	2	2

* The higher the numerical value, the more salient or characteristic is the phrase indicated by the number of the card.

The intercorrelations among the eight composites are presented in Table 3. The range of coefficients is from +.12 to +.52, with a median r of +.31. These values seem low enough to warrant the inference that eight separate stylistic types are being defined by the scoring criteria (reference sorts).

In order to obtain a profile of "stylistic type" scores for each man, the individual Q-sorts of each of the 45 scientists were correlated with each of the eight reference sorts. (Mean coefficients for each of the eight reference dimensions were: .46, .45, .45, .37, .28, .32, .41, and .32). The correlation between the scientist's Q-sort and the reference sort was treated as his score on that factor. The 45 values for each factor dimension were then correlated with scores on other devices and with the ratings and evaluations made of each subject by the staff interviewers and observers.

DETERMINATION OF THE PSYCHOLOGICAL MEANING OF THE FACTORS

The next phase of the analysis was an attempt to determine the psychological meaning implicit in each of the eight stylistic types. To prepare such formulations a detailed study was made of the order of placement of all 56 cards in the specific composite, and also of the correlations between the "scores" described in the preceding paragraph and the other assessment data. The evidence here included various aptitude and achievement tests, situational procedures, psychodiagnostic life history interviews, experimental tasks, reports and comments made by staff observers, etc.

An example of one of the latter sources of data is a 50-card "interpersonal Q-sort deck" prepared by the senior author for use in describing the everyday, interpersonal behavior of a subject. This Q-sort deck was sorted in description of each of the 45 subjects by three persons: the psychologist who had seen him in the life history interview and two staff psychologists who had observed him during the various procedures and interactions of the three-day assessment program. Each of the eight sets of stylistic type scores was correlated with each of these 50 descriptive statements. Representative items revealing highest correlations with each stylistic type are listed below:

Highest Positive and Negative Correlations

> *Factor Type I. Positive correlations:* Tolerant, permissive, and benevolent; considerate and charitable (+.36). Has highly developed inner sense of ethics and morality; deeply humanitarian and altruistic (+.35). A conscientious and serious-minded person (+.25). *Negative correlations:* Wedded to routine; made anxious by change and uncertainty (−.28). Gets along well with others; able to "fit in" easily in most situations (−.43).
>
> *Factor Type II. Positive correlations:* Ambitious, likely to succeed in most things undertaken (+.43). Well-organized, capable, patient, and industrious; values achievement (+.40). Efficient; able to mobilize personal resources quickly and effectively (+.32). *Negative correlations:* Lazy, indifferent about duties and obligations; generally undependable and immature (−.50). Worried and preoccupied; tense, nervous, and generally upset (−.53).
>
> *Factor Type III. Positive correlations:* Is an effective leader; able to elicit the response and cooperation of others (+.31). Is forceful and self-assured in manner (+.26). Poised and self-confident; not troubled by pressure or criticism (+.26). *Negative correlations:* Shrewd and self-centered; inclined to be self-

Table 3. Intercorrelations Among the Scoring Keys for the Eight Stylistic Types on the Research Scientist Q-sort Deck

	I	*II*	*III*	*IV*	*V*	*VI*	*VII*	*VIII*
I.	—							
II.	.48	—						
III.	.48	.47	—					
IV.	.23	.31	.36	—				
V.	.16	.25	.22	.25	—			
VI.	.41	.30	.38	.16	.12	—		
VII.	.52	.42	.42	.39	.25	.45	—	
VIII.	.22	.31	.30	.17	.36	.17	.14	—

ish and opportunistic (—.36). Awkward and ill-at-ease socially; shy and inhibited with others (—.28).

Factor Type IV. Positive correlations: Is a conscientious and serious-minded person (+.33). Easily embarrased; feels inferior and inadequate (+.26). *Negative correlations:* Lazy, indifferent about duties and obligations; generally undependable and immature (—.33). Active and robust in manner; hard-headed and forthright in judgment (—.29). Coarse and vulgar; apt to behave in a crude and impolite fashion (—.28).

Factor Type V. Positive correlations: Honest and direct in behavior; mature and realistic in outlook (+.39). Gets along well with others; able to "fit in" easily in most situations (+.38). Observant and perceptive; quick to respond to the subtleties and nuances of others' behavior (+.37). *Negative correlations:* Given to moods; often difficult and recalcitrant (—.40). Headstrong, rebellious, and resentful of others; lacking in self-discipline; apt to behave in a rash or destructive manner (—.38).

Factor Type VI. Positive correlations: Lazy, indifferent about duties and obligations; generally undependable and immature (+.50). Undependable, poorly motivated; has difficulty in working toward prescribed goals (+.48). Clever and imaginative; a spontaneous and entertaining person (+.35). *Negative correlations:* Well-organized, capable, patient, and industrious; values achievement (—.59). Wedded to routine; made anxious by change and uncertainty (—.53).

Factor Type VII. Positive correlations: Tolerant, permissive, and benevolent; considerate and charitable (+.32). Has a talent for creative and original thinking (+.29). Given to moods; often difficult and recalcitrant (+.28). *Negative correlations:* Overly concerned with success; too dominated by own ambition and desire to win approval (—.38). Warm and unpretentious in behavior; a comfortable and uncomplicated person (—.31).

Factor Type VIII. Positive correlations: Active and robust in manner; hard-headed and forthright in judgment (+.41). Coarse and vulgar; inclined to behave in a crude and impolite fashion (+.31). Overly concerned with success; too dominated by own ambition and desire to win approval (+.28). *Negative correlations:* Worried and preoccupied; tense, nervous, and generally upset (—.29). Self-pitying and martyr-like; tries to make others feel guilty (—.29).

A second example of the kind of data utilized in reaching a formulation of the psychological meanings of each of the eight stylistic factors is taken from the personality inventory scores on the California Psychological Inventory (Gough, 1957). For brevity's sake, we are citing the findings only for the second factor.

Table 4 contains *CPI* data contrasting the scores of the 13 scientists having highest scores on Factor II with the 13 having lowest scores. Three of the differences seem to merit attention: the scientists high on the factor appear to be more dominant and forceful, less given to aggressive and rebellious behavior, and less governed by errant or erratic impulse.

These personality inventory findings are in accord with the previous interpersonal *Q*-sort data which showed Type II scientists to be ambitious, well-organized, patient, and generally free from problems of immaturity and nervous tension. Furthermore, in their own self-views of research activity (see Tables 1 and 2) they report that they react quickly to research problems, immediately generating a great number of ideas, that they give freely of their own time, that they consider themselves to be creative and inventive, and that they have a lively sense of intellectual curiosity. Utilizing these and other data in the assessment files we therefore decided to designate the Type II scientist as the "initiator."

PSYCHOLOGICAL DEFINITIONS OF THE RESEARCH TYPES

Our search for names and formulations for each of the eight types followed the lines suggested in the above material. It was systematic in that all of the available data (including clinical and life history data) were considered, and painstaking in that many hours of analysis were spent on the task. However, it has to be admitted that there is a great and imponderable element of subjectivity in the endeavor and that the formulations represent inductions arrived at by reflective and psychological processes rather than by means of formal statistical analysis. Our goal has been to delineate a brief but integrated psychological definition of the scientist typified by each of the eight stylistic factors; it is hoped that the formulations below represent at least partially valid steps in this direction.

Table 4. Comparison on the California Psychological Inventory of the 13 Highest-scoring vs. the 13 Lowest-scoring Scientists on the Second Stylistic Factor

	Highs		*Lows*				
CPI Scale	*M*	*SD*	*M*	*SD*	*diff.*	*t*	*P*
Dominance	33.69	3.40	30.31	4.66	3.38	2.11	<.05
Capacity for status	24.31	2.63	23.31	2.29	1.00	1.03	*
Sociabiilty	27.00	4.69	27.85	3.80	—0.85	0.51	*
Social presence	39.92	5.77	41.85	4.00	—1.93	0.99	*
Self-acceptance	23.46	3.93	24.23	3.09	—0.77	0.55	*
Sense of well-being	40.77	2.39	38.85	2.67	1.92	1.94	*
Responsibility	34.62	3.55	32.62	3.07	2.00	1.54	*
Socialization	39.38	3.48	32.85	3.78	6.53	4.60	<.01
Self-control	34.23	4.80	29.62	5.25	4.61	2.34	<.05
Tolerance	27.54	3.57	25.08	3.75	2.46	1.71	*
Good impression	19.92	4.50	16.46	5.36	3.46	1.78	*
Communality	25.92	1.12	25.46	1.39	0.46	0.92	*
Achievement via conformance	31.46	3.36	29.62	3.04	1.84	1.46	*
Achievement via independence	24.62	2.33	25.08	3.38	—0.46	0.40	*
Intellectual efficiency	43.46	3.07	43.62	2.99	—0.16	1.34	*
Psychological-mindedness	15.46	3.07	16.62	2.18	—1.16	1.12	*
Flexibility	11.54	4.12	13.92	4.13	—2.38	1.47	*
Femininity	16.54	2.44	17.69	1.97	—1.15	1.32	*

* *P* > .05.

Type I: The Zealot. This man is dedicated to research activity; he sees himself as a driving, indefatigable researcher, with exceptional mathematical skills and a lively sense of curiosity. He is seen by others as tolerant, serious-minded, and conscientious, but as not getting along easily with others and as not being able to "fit in" readily with others.

Type II: The Initiator. This man reacts quickly to research problems, and begins at once to generate ideas; he is stimulating to others and gives freely of his own time; he sees himself as being relatively free of doctrinaire bias—methodological or substantive—and as being a good "team" man. Observers describe him as ambitious, well-organized, industrious, a good leader, and efficient. They also characterize him as being relatively free of manifest anxiety, worry, and nervousness.

Type III: The Diagnostician. This man sees himself as a good evaluator, able to diagnose strong and weak points in a program quickly and accurately, and as having a knack for improvising quick solutions in research trouble spots. He does not have strong methodological preferences and biases, and tends not to be harsh or disparaging towards others' mistakes and errors. Observers see him as forceful and self-assured in manner, and as unselfish and free from self-seeking and narcissistic striving.

Type IV: The Scholar. This man is blessed with an exceptional memory, and with an eye for detail and order. However, he is not a research perfectionist nor an endless seeker for ultimates. He does not hesitate to ask help when blocked in his work, and feels that he can adapt his own thinking to that of others. He is well-informed in his field, and is not given to bluffing. Observers describe him as conscientious and thorough, and as very dependable, but as lacking confidence and decisiveness of judgment.

Type V: The Artificer. This man gives freely of his own time, and enjoys talking shop with other researchers. He is aware of his own limitations and does not attempt what he cannot do. He sees himself as having a special facility for taking inchoate or poorly formed ideas of others and fashioning them into workable and significant programs. Observers see him as honest and direct, getting along well with others, and as usually observant and perceptive and responsive to nuances and subtleties in others' behavior.

Type VI: The Esthetician. This man favors analytical over other modes of thinking, and prefers research problems which lend themselves to elegant and formal solutions. His interests are far-ranging, and he tends to become impatient if progress is slow or if emphasis must be put upon orderliness and systematic detail. His own view of experience is primarily an esthetic one. Observers see him as clever and spontaneous, but as undependable and immature, somewhat lacking in patience and industry and indifferent about duties and obligations.

Type VII: The Methodologist. This man is vitally interested in methodological issues, and in problems of mathematical analysis and conceptualization. He is open about his own research plans and enjoys talking about them with others. He has little competitive spirit and tends to take a tolerant view of research differences between himself and others. Observers characterize him as a considerate, charitable person, free from undue ambition; at the same time they report a certain moodiness and an occasional tendency toward complicated and difficult behavior.

Type VIII: The Independent. This man eschews "team" efforts, and dislikes and avoids administrative details connected with research work. He is not a driving, energetic research man, although he does have a lively sense of intellectual curiosity. He prefers to think in reference to physical and structural models rather than in analytical and mathematical ways. Observers describe him as active and robust in manner and hard-headed and forthright in judgment. He appears relatively free from worry and self-doubt, but inclined to behave impolitely or abruptly.

In Table 3 we presented the intercorrelations among the scoring *keys* for the eight stylistic types. One might well ask what would be the intercorrelations among the *scores* obtained by the sample of 45 scientists, and also whether this pattern of intercorrelations would be upheld in another sample. Table 5 contains data relevant to an answer to this question. The top line of correlations presents the results obtained with the initial sample of 45 professional scientists. The *RSQD* (Research Scientist Q-sort Deck) was also given to a sample of 40 honor students in the School of Engineering at the University of California, Berkeley. The Q-sorts of each student were correlated with the stylistic-type scoring keys, yielding eight coefficients or "scores" for each student. (Mean coefficients for the eight factor dimensions were: .35, .37, .30, .31, .24, .26, .29, and .26.) The 40 scores (coefficients) for each dimension were in turn intercorrelated, with the results shown in Table 5.

For the professional scientists, the range of coefficients is from −.32 to +.51, median +.12. For the honor students in engineering, the range is from +.25 to +.68, median +.27. These values are quite similar. The correlation between the two matrices is approximately +.87. The general pattern of relationship among the eight types would therefore appear to be relatively stable.

SUMMARY

A set of 56 diagnostic statements pertaining to scientific activity, values, and modes of

Table 5. Intercorrelations Among Scores on the Eight Stylistic Types of the Research Scientist Q-sort Deck*

		I	*II*	*III*	*IV*	*V*	*VI*	*VII*	*VIII*
I.	**The Zealot**	—							
		—							
II.	**The Initiator**	.47	—						
		.21	—						
III.	**The Diagnostician**	.64	.68	—					
		.47	.32	—					
IV.	**The Scholar**	.49	.44	.48	—				
		.13	.25	.25	—				
V.	**The Artificer**	—.25	—.08	—.24	.30	—			
		—.30	.13	—.22	.27	—			
VI.	**The Esthetician**	.19	—.02	.24	—.16	—.13	—		
		.30	—.23	.04	—.29	—.32	—		
VII.	**The Methodologist**	.59	.24	.44	.54	.11	.45	—	
		.51	.12	.27	.38	.00	.19	—	
VIII.	**The Independent**	—.02	.36	.17	.17	.32	.09	.13	—
		—.23	.12	—.12	—.12	.26	—.16	—.25	—

* Correlations in the first row are from the sample of 45 professional research scientists, in the second row from the sample of 40 honor students in engineering.

research activity was created and then given to a sample of 45 professional research scientists for self-descriptive Q-sorting. A factor analysis of the intercorrelations among scientists permitted the identification of eight factor types. The differences among the types seemed to be principally stylistic ones, and to warrant the conclusion that stylistic variations in scientific research methodologies do exist and can be specified.

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The Ideology of Academic Scientists

S. S. West

It is commonly believed that persons engaged in scientific research adhere to a set of moral values representing ideal types of behavior which facilitate the production of new knowledge. However, the many listings of these values which may be found in the literature range from intuitive to, at best, speculative. Few attempts have been made to discover whether a majority of scientists actually holds them as they are stated. This paper presents data obtained in interviews with academic researchers of faculty status, which outline the distributions of scientific values for one university. These men are engaged for the most part in basic research and are therefore representative also of the upper level of industrial research, upon which technological progress ultimately depends. Unless a research director understands the value-system of his staff, he may find it difficult to design an efficient organization and to staff it properly.

A representative statement of the moral values of science is given by Bernard Barber:[1]

1) Faith in rationality,
2) Emotional neutrality (as an instrumental condition for the achievement of rationality),
3) Universalism: in science all men have morally equal claims to the discovery and possession of rational knowledge,
4) Individualism (which expresses itself in science particularly as anti-authoritarianism),
5) Communality: private property rights are reduced to credit for priority of discovery; secrecy thus becomes an immoral act,
6) Disinterestedness: men are expected to achieve their self-interest in work-satisfaction and in prestige through serving the community interest.

The following additional values are also frequently imputed to scientists, either separately or together with those of Barber's list:

7) Impartiality: a scientist concerns himself only with the production of new knowledge and not with the consequences of its use,
8) Suspension of judgment: scientific statements are made only on the basis of conclusive evidence,
9) Absence of bias: the validity of a scientific statement depends only on the operations by which evidence for it was obtained, and not upon the person who makes it,
10) Group loyalty: production of new

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[1] B. Barber, "Science and the Social Order," The Free Press, Glencoe, Ill., pp. 86–92; 1952. See also pp. 267–268 for further references to this area.

knowledge by research is the most important of all activities and is to be supported as such,

11) Freedom: all restraint or control of scientific investigation is to be resisted.

This extended list is probably not exhaustive, nor are its items wholly independent. Emotional neutrality and impartiality differ chiefly in whether the focus of interest is the investigator himself or other persons with whom he may identify. Universalism, communality, and disinterestedness may be regarded as aspects of a requirement of free diffusion of scientific information. Freedom in research is implemented by group loyalty. Individualism is related to freedom, and its anti-authoritarian component is a component of absence of bias. As an added complication, one may note that faith in rationality is essentially irrational.

Because of this interdependence of values, it was decided that an adequate initial investigation of scientific ideology could be made on the basis of items 7 through 11 of the above list, plus one concerning free diffusion of information which would combine the essential content of items 3, 5, and 6. Accordingly, questions concerning these six principles were included in an interview schedule designed to explore also personal history of investigator, motivations for research, research process, and organizational interaction as related to productivity in research. Only the data concerning ideology will be discussed here.

Approximately one-third of the faculty members in six science departments of a midwestern university were interviewed, a total of 57 persons, each of whom held either a Ph.D. degree or an M.D. degree or both. Departments included were anatomy, biological chemistry, mathematics, physics, physiology, and sociology. In each department, members were dichotomized about median age and again about median age at receipt of doctoral degree, and names were drawn randomly in each of the four cells so defined, in proportion to the number of persons in the given department who fell in the given cell. There were thus included seven anatomists, nine biochemists, seventeen mathematicians, nine physicists, seven physiologists, and eight sociologists. It was found that the total sample so drawn was also distributed very nearly in proportion to the frequencies of the several academic ranks. Each interview required about 1½ hours and was tape-recorded for verbatim transcription.

FREEDOM IN RESEARCH

Information about the respondent's conception of freedom in research was provided by the following item of the interview schedule:

> "Item 17: Many researchers say that freedom in the research situation is important to them. How important do you feel it is to you? Why?"

In many cases, the answer to the first question of item 17 was "very important" or some equivalent phrase, but more often the respondent asked for a definition of freedom. Either at this point or after the second question, it was nearly always necessary to use the probe:

> If there is something that you would call freedom that is important to you as a research man, what should it permit you to do?

Only rarely were further probes required.

For these respondents, freedom was almost entirely a matter of choosing the problem to be investigated, on the assumption that the manner of investigation would be unrestricted. Table I shows frequencies of four degrees of of choice specified as necessary for maximum research effectiveness. Of the 57 respondents, 35 specified Level A of Table I as a minimum requirement, and 17 specified Level B. These are realistic requirements, because available evidence indicates that the respondents do actually have their stated degrees of choice. Moreover, although five persons specified lower levels of choice, three appeared to be working at Level A and the other two at Level B.

The amount of affect associated with the idea of freedom of choice was by no means the same for all persons specifying Level A. Of these 35 persons, eight considered freedom to be merely desirable, thirteen considered it quite necessary, but to only fourteen did it appear indispensable. Perhaps the strongest statement from this last subgroup was that of a mathematician, who said: "I think that the moment somebody walks in this door and says, 'Here is a problem that the department would sort of

Table I. Freedom to Choose Problem to be Investigated

Degree of freedom specified for effective research	*Number of respondents*
A) Unlimited choice of problem is:	
1) indispensable	14
2) quite necessary	13
3) merely desirable	8
B) Choice of problem may be limited by expectation of further support based on one of the following considerations:	
1) One should work only in the discipline represented by the department in which one is employed	6
2) Peers should judge the importance of one's work	8
3) Superiors should judge the importance of one's work	3
C) Choice of problem may be limited to certain general areas specified by superiors	3
D) The problem may be completely specified	2
Total:	57

like to have you think about. Think about it,' I would feel unhappy. My happiness would actually be shot to hell." Other statements were similar. A biochemist said, "I think in my own case that, if I were not permitted to decide what sort of problems I wished to investigate and how I might pursue them, I would not care for research at all, and would take up farming or something." A sociologist: "Freedom is a *sine qua non.* You just don't operate without it." (What should it permit you to do?). "To ruminate, on pay." A physicist: "Independence of what I could do has been a very important factor always. Independence in that I could go any place. I could refuse an offer. I could take up any problem that I wished, or drop it. This demand for personal independence applies to research, but it also applies to everything else, too."

Although restrictions on implementing such a choice are not directly relevant, the one most often cited is worth mentioning here. *Continuity of time* for research was felt by fourteen respondents to be of the utmost importance, as the following comments indicate: "Enough time is not enough. You have got to have time to relax your mind, and then, when your mind is relaxed, you think of something." "The chopped-up day is my idea of lack of freedom." "An hour at a time is no good. I get into a problem, and then I have to drop it. When I get back to it, it takes another several hours to get back to the place where I once was in my thinking." "You can't do research on a schedule. You do it when you feel like it."

Of those 17 persons whose responses corresponded to Level B of Table I, six considered that the researcher should be restricted to the discipline represented by the department in which he is employed, eight thought that the importance of his work should be subject to evaluation by his peers, and three thought that superiors should evaluate the importance of his work in comparison to that of others in the field. However, one physicist of some eminence, who considered that peers should evaluate, said also: "You must be free to do any type of research that you want, provided it is regarded as valuable by people in whose field it is." In one way or another, then, all persons in this category believe that a research contribution should be judged by others to determine further support of the researcher, whereas persons whose responses correspond to Level A, believe that the researcher himself is the only acceptable judge.

THE IDEOLOGY SECTION OF THE INTERVIEW

To minimize effects of anxiety and resistance, the other five values were examined in the final portion of the interview. Instructions in the interview schedule read as follows:

> Item 25: There exists a number of general principles in which scientists are said to believe. We would like to find out how universally these principles are held. I will state each one, and ask you whether you feel that it has any exceptions; that is, under what circum-

stances you would not wish to follow the principle. (Each of the following is on a separate card. Hand card to respondent, then read.)

(1) (IMPARTIALITY) Facts should be described impartially, without passing judgment on whether their consequences are desirable or undesirable.
(2) (SUSPENSION OF JUDGMENT) Conclusions should be suspended until enough facts are at hand for reasonable certainty.
(3) (ABSENCE OF BIAS) Statements of all persons with knowledge bearing on a given problem should be given equal weight in drawing conclusions about that problem.
(4) (DIFFUSION OF INFORMATION) Scientific information should be accessible to all persons who wish to have it.
(5) (GROUP LOYALTY) A scientist should support the interests of scientists before those of other groups.

(After reading each statement:)
Under what circumstances would you *not* wish to follow this principle?

Responses varied greatly in length, degree of organization, apparent insight, etc., but these characteristics will not be discussed here. An exhaustive analysis with respect to theme was made, and these themes were categorized according to their most clearly evident common dimension. This was not difficult, because there was rather a paucity of ideas even in responses which ran to several hundred words.

IMPARTIALITY

Table II shows the frequencies of the themes which occurred in responses to Item 25(1), concerning impartiality in describing facts. The order is that of increasing integration of individual with group—those persons who stated no exceptions representing the classical view that the producer of knowledge is completely neutral with respect to how that knowledge is applied. One-third of the respondents took this position. About one-fourth of the respondents modified this view by assuming completely separable roles, with impartiality still being maintained absolutely in one of these roles, but the antithesis was not the same for all: scientist vs citizen, scientist vs layman, isolate vs social man, researcher vs teacher, own research vs work of others, professional audience vs lay audience, each occurred from one to three times.

Table II. Themes in Responses to: "Facts should be described impartially, without passing judgment on whether their consequences are desirable or undesirable."

Theme	*Number of respondents*
No exceptions to principle	19
Separation of roles	13
Withholding of information	8
Exception for certain special cases	7
Consequences are always considered	10
Total:	57

As used in connection with the principle of impartiality, withholding of information had different meanings to different respondents. For four of them, it represented the only way in which judgment of consequences should be expressed, information being withheld if the consequences are expected to be bad. For two, it was a way of remaining impartial ("no exceptions" to principle) since no judgment need be made if the information is not permitted to have consequences. For two, it was a way of implementing a nonscientist role, in which one is permitted to control consequences by releasing or withholding information.

The ten respondents who said that consequences should always be considered were chiefly concerned with professional responsibility: "I think that it is our duty to describe the consequences and pass judgment on them in all cases." "If the individuals who gather the facts are sufficiently well-versed in evaluating them, then they should pass on the consequences." "The trained mind ought to be able to make objective and subjective judgments at the same time." "If one makes the statement that the consequences do not really matter, he is just playing games with the form of scientific experiment and refusing to face the implications of his results." Moreover, there are scientific as well as broader social consequences and judgment of these is often useful, as when one says: "This piece of mathematics is consequential, this piece of mathematics leads nowhere;" or, "This is a very nice theorem because it has the following implications."

SUSPENSION OF JUDGMENT

Table III shows frequencies of themes which occurred in responses to Item 25(2), concerning suspension of judgment. These themes were differentiated by the way in which the standard of "reasonable certainty" is thought to be set, the responses providing little or no information about the operational standard of certainty which is used in any given case. The twelve persons who cited no exceptions apparently assumed a fixed standard of certainty which would be regarded as reasonable by them or by other professionals in the same discipline. The fifteen who cited an exception of necessity to act (as in releasing unproven physiological research results for therapeutic purposes), thus implied the occasional substitution of a different standard, set by the extrascientific environment. Quite striking, however, is the large number of respondents (20 out of 57) who asserted the importance of publishing tentative conclusions, which the sociologist would call "hypotheses," the mathematician "conjectures," and the physicist "hunches." The following are representative comments.

Table III. Themes in Responses to: "Conclusions should be suspended until enough facts are at hand for reasonable certainty."

Theme	*Number of respondents*
No exceptions to principle	12
Exception for necessity to act	15
Exception to transmit useful data	4
Exception to transmit useful hypotheses	20
All conclusions are tentative	6
Total:	57

A sociologist: "I think it is our duty to try to be exhaustive in specifying the possible conclusions that can be reached from what limited findings we have, rather than leave the audience with nothing and say that we do not have enough data. This last is a very nice out for somebody who does not care to do some hard thinking." A biochemist: "Conclusions should not be held up until you have the complete proof, because other people might be able to supply proof even though you may not. The very important ideas sometimes have their merits without having proof." A mathematician: "In mathematics we do not publish results, of course, until we can prove them. We do publish conjectures. We do publish reasons for believing strongly in the conjecture." A physicist: "Information is never completely perfect. There are always probable errors, however small, on the data points. In physics, when one goes to a new energy or discovers a new particle, the first papers published always have very broad experimental errors, and the people who do this work are very proud of it because it represents a basic new information which had not been available. The indication from this experiment with its broad errors is that such-and-such a situation may pertain. This is an indication and not a conclusion. When later experiments have cleared the error, this indication may be reversed, which does not surprise the previous investigators at all, because they knew exactly what their error was and the probability of error in the conclusion, and so stated it if they were good investigators." A physiologist: "Eccles' recent work is some of the most stimulating stuff that has ever been published, but yet the reasonable certainty of the things that he is claiming as facts is very doubtful. I think they are sufficiently assured so that he can use them as working hypotheses, but he is pressing for their more or less complete truth as having been established by him, where in the eyes of most people there does not seem to be as great a certainty as he implies. But I think he is perfectly correct in this, and to be commended for publishing and giving them this tone of certainty, which will be useful for most people and will also be readily challenged by others."

The six respondents who regarded all conclusions as tentative were distributed among four departments. Their position is described by the words of one physicist: "The joker in this is the words 'reasonable certainty.' This can be anywhere from 1 per cent to 99 per cent certainty. Physicists in general do have to use and do use the process of making conclusions with an inadequate supply of facts. They take an incomplete set of facts and have to deal with them. Now whether they have enough facts for reasonable certainty, this is the man's opinion." Here we have essentially a complete rejection of the principle of Item 25(2), in that no universal standard of certainty is set, a

Table IV. Criteria for Weighting Scientific Statements Given in Responses to: "Statements of all persons with knowledge bearing on a given problem should be given equal weight in drawing conclusions about that problem."

Criterion for weighting statement	*Number of mentions*
A) *Characteristics of statement*	
1) Facts presented	16
2) Experiments from which the facts were obtained	5
3) Manner of presentation or kind of proof	7
B) *Directly observable personal characteristics*	
1) Experience: closeness to the facts, training in assessing observations, etc.	21
2) Record of verified statements	13
3) Record of research contributions	3
4) Status	3
C) *Inferred personal characteristics*	
1) Knowledge: breadth and profundity	11
2) Ability to integrate information: intuition, acuteness, imagination, understanding of complex phenomena	10
3) Bias: value-orientation, purposes, prejudice, theoretical commitment, fixation on certain aspects or variables	6
4) Honesty: veracity, intellectual integrity, absence of self-deception	5
5) Other characteristics: conservatism, precise and accurate reporting of facts, memory, concentration, good eyesight, involvement with problem, ability to understand one's ideas, etc.	14
Total:	114

prediction or conclusion may be made at any stage of an investigation, and the investigation is terminated on the basis of efficiency alone. As a physiologist put it: "Eventually comes a time when that particular piece of work is as near finished as it is ever going to be, at which time I draw whatever conclusions can be drawn from it, and at this time a paper is usually written and published."

ABSENCE OF BIAS

It was scarcely to be expected that no bias would be asserted with respect to statements of persons about a given problem and, indeed, the four respondents who did not state exceptions to Item 25(3) were merely at a loss to define their systems of weighting. Responses averaged two criteria of weighting per respondent, and these are summarized in Table IV. Of the 57 respondents, twelve gave only criteria for weighting based upon characteristics of the statement (that is, falling only in Section A of Table IV), 32 gave only criteria based upon characteristics of the person making the statement (nine in Section B, fifteen in both B and C, and eight in C), and nine gave a mixed response (four in Sections A and B, two in A and C, and three in A and B and C), while four were unable to formulate criteria. (See Table V.)

Table V. Distribution of Respondents with Respect to Class of Criteria Used for Weighting Scientific Statements

Class of criteria used for weighting	*Number of respondents*
Characteristics of statement only (A* of Table IV)	12
Characteristics of person only (B,* B and C,* or C)	32
Mixed response (A and B, A and C, or A and B and C)	9
No criteria given	4
Total:	57

* The letters A, B, and C designate the sections of Table IV in which criteria given by the respondent fall.

The classical position is an impersonal one, corresponding to Section A of Table IV. No biochemist or sociologist gave a pure type-A response, the twelve persons giving such a response being distributed proportionally among anatomy, physiology, physics, and mathematics. An example of a response of type A-1 is this statement by a physicist: "Persons have nothing to do with it. This man who signs his name on this paper is not a person to you, but you certainly have to give it weight if he has done it. You can easily check whether he is right or not. A scientific statement is independent of the person. It is true or false by itself, so the person does not really matter."

Experience (B-1 of Table IV) appears to mean practice rather than formal education to most of the respondents who named this as a criterion. One sociologist put it this way: "If Jim Bridger tells me in the 1830's that there are a lot of bears in those hills, and a Boston traveler says he did not see a bear, I am going to take Jim Bridger, education or no. After we leave this general formula, it becomes a wilderness of particulars: Are we talking about an isolated fact, are there bears or no bears, many bears or few bears, or are we speaking of a relationship like 'bears are found where there are pine cones.' Now somebody who had investigated the feeding habits of bears and then tells me there are no bears in this area because there are no pine cones is going to be making a more interesting statement to me than somebody who does not know anything about the feeding habits of bears. It is more than experience with the phenomenon. It is *systematic* knowledge, sophistication. The underlining is on systematic, not on knowledge. Exposure to the phenomenon, exposure to this class of phenomena and to the explanatory system involving this class of phenomena, and straight bias are the three elements that enter in."

It is worth noting that only six respondents appeared to be sensitized to the possibility of bias in scientific statements—fewer than might have been expected. As one sociologist said, "Some of us have a fixation on certain kinds of variables or things we look for in any kind of problem we investigate. So these people are likely to 'see' exactly that in anything they look at." Scientific orthodoxy also has a compelling influence on some persons. In the seventeen interviews in which the "flying-saucer" controversy was introduced as a concrete example, either by the respondent or by the interviewer, there were five respondents who asserted that they would not accept positive reports of such a phenomenon, no matter who made the observations.

DIFFUSION OF INFORMATION

The statement concerning free accessibility of scientific information, Item 25(4), suggested secrecy for military purposes to 40 of the 57 respondents; no other theme even approached this one in frequency, as Table VI shows. (Only two of these forty were opposed to all secrecy, although eleven considered the present degree of secrecy to be excessive.) Such secrecy was expressed usually in terms of "national security," less often, "national defense," occasionally, "classified information," and sometimes in descriptive terms. If illustrations were given, they were always the atomic bomb, the hydrogen bomb, or satellites.

The phrase "national security" appears to be a true stereotype in these responses. In no case was there evidence that the situation had been thought through to a rational judgment, or that effort had been made to assess objectively either the risks of alternative courses of action or the possible effect of secrecy upon the respondent as a scientist. As an example in which this was made quite explicit, consider the following interchange:

> (Should information having to do with any other matters than secret weapons be withheld?) "No. Certainly not. Well, secret weapons is not the right word. I would like to use the word national security. But this must not be restricted to what a politician thinks is security, such as public hysteria or his loss of office, but literally to things connected with national defense." (Does this cover only weapons of offense or defense, or other information as well?) "This includes all information relative to the defense of the country. I don't know what you mean by offense. I mean, if we have an ICBM, this is a defensive weapon. It has to do with national defense. Whether we launch the first attack or not is not the point. I still regard this as the defense of the country."

Admittedly, this is an extreme response, but it differs from the others mainly in its emotional intensity and explicit irrationality. Even those persons who were tentative in their orthodoxy appeared to be so more because of confusion than reasoned doubt—and sometimes said so.

Of the eleven persons who mentioned information which might cause hysteria or other public commotion, three favored complete

Table VI. Themes in Responses to: "Scientific information should be accessible to all persons who wish to have it."

	Number of mentions of indicated degree of access to information			
	++	+	*0*	*All degrees*
A) *Kinds of information*				
1) Information of military value	2	11	27	40
2) Information which might cause hysteria or other public commotion	6	2	3	11
3) Information from industrial research	0	4	0	4
4) Medical information about:				
a) Therapeutic agents and procedures	2	4	3	9
b) Contraception, abortion, or other sexual matters	0	4	2	6
B) *Kinds of persons desiring information*				
1) Dangerous persons	0	0	12	12
2) Untrained but curious persons	10	3	8	21
3) Scientific competitors	2	0	0	2
Total numbers of mentions	22	28	55	105

Designation of degree of access to information specified by respondents as desirable:
++ Full and unrestricted access
\+ Partial or delayed access
0 Completely controlled access

control of the information, two thought that delay or careful presentation is desirable, and the other six believed in complete accessibility. Said one physicist: "There is a big hassle on now as to the question of fallout and whether you should tell people or not. My answer is yes, you should tell them. You do not gain anything by hiding facts. You may make a few more neurotics in the world, but, hell, we've got a lot of them now. A few more is not going to hurt; in fact it might help." And a mathematician: "I think that the tendency of the populace to hysteria is greatly overrated. In connection with this atomic fallout information, I think that the general public has been, if anything, phlegmatic about it. People do not run through the streets when they see an eclipse any more. They have become very blasé about any sort of scientific information." In contrast to the amount of anxiety shown by the majority of the respondents in regard to external threats, their confidence in the internal stability of their culture is rather remarkable.

Mentions of withholding information from dangerous persons were concerned with poisons (four), with persons of "bad motivations," "psychotics," and would-be dictators (four), with antivivisectionists (two), with commercialization of medical research (one), and with bombs (one). Withholding of information from untrained persons was justified on the basis of inability to understand the information, inability to use it, possible harm to self or others, or arousal of anxiety, but only eight of twenty-one respondents who mentioned this situation took the extreme negative position.

On the other hand, ten of these twenty-one respondents either passively approved accessibility of scientific information to untrained but curious persons or showed an active interest in opportunities to pass on their special knowledge. One physicist said: "If a man stopped me on the street and said, 'Could you tell me the cross section for the production of a lambda-zero particle by pions or neutrons?' I would be delighted to tell him. As a matter of fact, I would be very touched that the public was taking such an interest in our work." And a mathematician: "Curiosity and scientific ability are the same thing as far as I know. One is pretty close to being a definition of the other."

Medical information represented an exceptional category to fifteen respondents, of whom seven considered that inadequate or misinterpreted information might result in undesirable self-treatment or unnecessary anxiety, two resented having medical information withheld from them, and six wished to restrict information on sexual matters. Only two persons mentioned scientific competitors, and both of these thought that only good could result from free interchange of information about research in progress.

Of the 105 mentions of possible withholding of information which are listed in Table VI, 22 favor full and unrestricted access, 28 partial or delayed access, and 55 completely controlled access. This is to say that only one-fifth of the mentions support the classical belief in full diffusion of information.

GROUP LOYALTY

Frequencies of themes in responses to the statement of Item 25(5) concerning group loyalty are shown in Table VII. Only eleven of the respondents expressed willingness to give scientists full support, but these were quite emphatic, as the following quotations indicate. A physicist: "I think that scientists must regard themselves as in a trade in the sense of a labor union or guild, and support their interests as other trades and professionals do." A sociologist: "If it is a question of conflict between scientists and nonscientists, then the scientists had better support the scientists, because who else is going to? I would even quote you a proverb on this score, a famous statement of Rabbi Hillel, who I think was first or second century B.C. He said, 'If I am not for myself, who will be for me? If I am for myself alone, what am I? And, if not now, when?' " A mathematician: "I would give this statement an unqualified yes, but to me it seems part of a larger statement. I look at a scientist as a man who has some academic pursuit on which he does research. Whether it is in the humanities or science is neither here nor there. And I want every one of these bastards to support this business of an academic man's being entitled to be supported in his research in all possible directions. I think I feel that strongly about it."

Table VII. Themes in Responses to: "A scientist should support the interests of scientists before those of other groups."

Theme	*Number of respondents*
Complete support of scientists	11
Qualified support of scientists	19
Role separation without conflict	5
Scientists should act as individuals	6
Society is not differentiated	16
Total:	57

Those nineteen persons who would support the scientist only in certain ways or to a certain degree gave many different reasons for restricting their support. They believed that strong partisanship is unseemly for a professional, that only certain issues are important, that other groups (such as political parties) are more important at times, that war or other emergency creates a unique situation, that "basic human rights" of nonscientists must often take precedence, that they would side with antivivisectionists or some religious group, etc. These persons feel some loyalty to scientists as a group, but apparently not enough to take risks, expend much effort, or sacrifice other values.

Five respondents took the position that a scientist can be defined as such only in the work-situation, the role of "scientist" being completely separable from the role of "citizen" or member of some nonscientific group. Six respondents asserted that scientists have no common interest (except possibly financial), each individual, supporter or supported, being unique.

Perhaps the most interesting category of response includes statements that society is not differentiated, that there are really no conflicts of interest between scientists and others, but merely misperceptions of interest. Such a response was obtained from about 10 per cent of respondents in physics or sociology, about 30 per cent of respondents in anatomy or physiology, and about 40 per cent of respondents in biochemistry or mathematics. Characteristic comments were: "I don't know what you mean by the interest of scientists. A scientist is no more or less of a citizen of the community than anyone else." "You can't be a good scientist if you are that narrow." "I don't believe in special privilege of any type." "I can't visualize another group that would not in some way be interlaced with science." "I believe that the interests of the scientific community are the interests of the community." "Groups opposed to scientists are not even acting in their own benefit. They think they are, but they are not." "What is good for the country is good for the area of scientific endeavor. Attacks on the scientific community are attacks on the community-at-large from the long-range point of view."

Responses which fall in this last category suggest that here feelings of guilt concerning differences of values and motivations between persons who produce new knowledge and persons who use it, or are affected by its use, have been dealt with in two ways. On the one hand, the differences may be denied, or, on the other, they may be rationalized as misperceptions which are projected upon nonscientists or upon other scientists.

These data may be compared with results published recently by Seeman[2] from forty interviews with assistant professors in social science and the humanities. His concern was with how the respondents dealt with their identity as intellectuals, and he found the dominant feature of his protocols to be the frequency and variety of minority-like responses. Defining a minority as a group against which categorical discrimination is being practiced, he distinguished five forms of minority reference: 1) direct acceptance of majority stereotypes, 2) concern with ingroup purification, 3) approval of conformity, 4) denial of group membership, and 5) fear of group solidarity.

Our respondents gave no evidence of reacting in the first two of these forms, but approval of conformity is very clear in the strong "national security" response of Table VI; denial of group membership corresponds to the theme described as "society is not differentitiated" in Table VII; and fear of group solidarity is the essence of the themes (Table VII) of "role separation without conflict" and "scientists should act as individuals." One should note, however, that less than half of our respondents exhibited these latter two forms of minority reference because persons who gave a "qualified support" response (Table VII) were affirming an active choice of other values rather than withdrawing support because of majority opposition.

RELATION OF IDEOLOGY TO RESEARCH BEHAVIOR

To test the hypothesis that creativity is directly related to acceptance of the values listed at the beginning of this paper, it was first necessary to decide on an approximate contrast between responses indicating acceptance and responses indicating rejection of each of the values presented to the respondents. Contrasts chosen were as follows, the classical position being included in the first of each pair of classes.

1) On *freedom in research:* Levels A-1 and A-2 of Table I vs Levels A-3, B, and C. (Actually most persons in our sample fall near the upper end of the continuum of choice, so this division around a median cannot be expected to discriminate very sharply.)

2) On *impartiality:* "No exceptions to principle" vs "consequences are always considered." (These are the extreme classes of Table II.)

3) On *suspension of judgment:* "No exceptions" plus "necessity to act" vs "to transmit hypothesis or data" plus "all conclusions are tentative." (This approximates a division around the median of the distribution of Table III, but the two contrasted classes of response are also logically consistent.)

4) On *absence of bias:* Weighting on characteristics of statement *only* vs weighting on characteristics of person *only.* (See Tables IV and V. The classical position is a completely impersonal one.)

5) On *diffusion of information:* Secrecy for military purposes partially or totally rejected or not mentioned vs secrecy for military purposes approved. (See Table VI.)

6) On *group loyalty:* Complete or qualified support of scientists vs role separation, "scientists should act as individuals," and "society is not differentiated." (See Table VII.)

Two measures of creativity were available. Item 13 of the interview schedule asked how many papers the respondent had published during the previous five years. Item 24 obtained judgments of strength of motivation by peers: from a list of all faculty members of the respondent's department, he was asked to select 1) persons very strongly motivated to do re-

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[2] M. Seeman, "The intellectual and the language of minorities," *Am. J. Soc.*, vol. 64, pp. 25–35; 1958.

search and (after completing the first selection) 2) persons rather weakly motivated to do research. If either group of selections was more than about one-fourth of the total membership of the given department, he was asked to select an extreme subgroup, although this was often done without its being suggested. A weight of 1 or −1 was assigned to "strong" or "weak" selections, respectively, which were not in the extreme subgroup, and a weight of 2 or −2 to the extreme selections, these weights then being summed over respondents from the given department, and the total for each man judged expressed in per cent of the maximum possible total. Thus from Item 13, one has a measure of productivity, and from Item 24, a measure of strength of motivation toward research.

Respondents were dichotomized about median rate of publication (six or more papers per five years vs five or less papers) and about median motivation score. For each of these measures and each principle, there was constructed a 2 × 2 table relating magnitude of measure to contrast of response defined above. Of the twelve such tables, none showed a degree of relationship even approaching significance by chi-square test, the smallest P being 0.28. (The largest ϕ coefficient was 0.24.) One may infer, then, that neither rate of publication nor strength of motivation toward research (as judged by peers) is more than quite weakly related to adherence to the classical ideology of science.

Furthermore, there is no ideological difference between prewar and postwar generations in the sample with respect to these measures. Dividing the respondents into those 40 years of age or younger and those 41 years of age or older, and examining the relation of age to each of the above-mentioned contrasts of response by means of 2 × 2 tables, one finds no significant difference between age groups for any of the six principles. Apparently, if there ever were a reasonably firm consensus with respect to scientific values, it was not maintained long after 1920. It may never have been more than a myth.[3]

It is possible, however, that ideology may be influenced by early socialization, and in this connection such variables as parental class and order of birth are relevant. Of our respondents, 30 had upper-middle or upper class fathers (Kinsey[4] class 6 or 7) and 27 had lower-middle or lower class fathers (Kinsey class 3, 4, or 5), for which division a 2 × 2 table was constructed on each contrast of response, no significant relation being found for any of the six principles. Next, the sample was dichotomized into the 30 respondents who were first-born and the 27 of higher birth-order, and the tests were made again. Although there was a tendency for first-born respondents to demand a higher level of freedom in research and to weight on characteristics of a scientific statement itself rather than on those of the person making it, even in these cases the chi-square probability exceeded 10 per cent, and it was greater for the other contrasts. Whatever the determinants of ideology, they appear to be more subtle and complex than is usually thought.

With an eye to the formulation of new hypotheses, we may also examine the nonsignificant differences between diagonals in the thirty contingency tables described in the preceding paragraphs. In seventeen cases, the absolute magnitude of the ϕ coefficient is less than 0.10, in only five cases is it greater than 0.20, and in no case is it greater than 0.31. In comparisons against age or class of origin, the ϕ coefficients are small, and there are as many differences in one direction as in the other. However, eleven of the twelve tables involving the two measures of creativity show differences in such a direction as to indicate that nonproductive and weakly-motivated persons tend to favor the classical ideology, and four of the six tables involving birth-order yield ϕ coefficients of magnitude 0.2 or greater and such a sign as to indicate that first-born persons tend to favor the classical ideology. If weak relations do exist, one may conjecture that they have these forms.

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[3] C. Arensberg remarks in a recent paper ("Values in action: a comment," *Human Organization*, vol. 17, pp. 25–29; 1958): Americans are especially prone to over-react to the general human experience, which always arises in public debate, that most large bodies of assembled persons do not have common values. They recoil from learning that, in a complex society like ours, people are not in fact in agreement about any, or certainly most, fundamental moral positions."

[4] A. C. Kinsey, W. B. Pomeroy, and C. E. Martin, "Sexual Behavior in the Human Male," W. B. Saunders Co., Philadelphia, Pa., pp. 77–79; 1948.

Now research begins with a need for closure and an assumption of causality, the researcher striving to achieve closure largely (but not wholly) by application of rational techniques[5] which have been found efficient for that purpose. These are matters which lie in the province of individual psychology, and we are here concerned with the social aspect of research. Development of the need for closure, habituation in kinds of thinking which are effective in producing it, obtaining information required to complete a conceptual scheme, and finding entrance to a situation in which problem-solving can be carried out are all results of social interaction, but only the latter two are influenced by the moral values of science itself. Moreover, the state of scientific knowledge depends far more on the existence of solutions for problems than on how these solutions were obtained, and a wide variety of situations, each associated with its own peculiar moral values, can supply the amount of information and degree of freedom from control which are necessary. . . .

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[5] As tabulated, for example, by D. B. Hertz, "The Theory and Practice of Industrial Research," McGraw-Hill Book Co., Inc., New York, N. Y., pp. 70–74; 1950.

The Federal Scientist-Administrator

Eugene S. Uyeki and Frank B. Cliffe, Jr.

A number of interesting questions can be asked about the scientist who is employed as an administrator by the federal government (*1*). Some of these are very practical questions. Others have relevance for theoretical concerns in the social sciences. For instance, exactly what is the professional distribution of scientists in government who hold supervisory and administrative positions? From what backgrounds do they come? What sorts of careers have they had, in and out of government? What is the correlation between the rank of the scientists and their fields of research? How does the scientific elite in government compare with other elite groups? Can any significant conclusions concerning the civil-service and the social function of scientists be drawn from this information?

To answer some of these questions, we have started to gather and analyze data for a random sample of natural scientists who were in the federal government in 1959 in administrative and supervisory positions. Our method was simple, though tedious and time-consuming. We began by taking a 1-in-10 sample from the alphabetical index of names in the *Official Register of the United States* (*2*). The *Register* is a virtually complete list of administrative personnel compiled by the United States Civil Service Commission from data supplied by the departments and agencies of the government. There are approximately 31,000 officials listed in the *Register;* we therefore secured some 3100 names by taking a 1-in-10 sample.

Each of the names in our sample was then checked against the listings in the tenth edition of *American Men of Science: the Physical and Biological Sciences* (*3*). Our study thus does not include social scientists. If the individual was listed in *American Men of Science,* we categorized him as a government scientist with administrative responsibilities (*4*). In our sample of 3100 administrators, we found 170 scientists (*5*).

The first question we raised was, What kinds of scientists are to be found in our sample? The kinds found are listed in Table 1. The largest group—over a third—received their formal training in the agricultural and life sciences.

Table 1. Professional Affiliation of Scientist-Administrators (N = 170)

Field	%
Agricultural and life sciences	38.2
Physical sciences	35.9
Engineering	14.7
Medicine	11.2

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Table 2. Professional Affiliation of Selected Groups of Scientists (20)

Category	*Scientists on the* National Register *(1956–58)* *(N = 124,036)* (%)	*All scientists in federal government* *(N = 56,888)* (%)	*Government scientists in R&D* *(N = 20,324)* (%)	*Scientist-administrators* *(N = 170)* (%)
Physical scientists*	72.1	39.9	74.1	50.6
Biological and medical scientists†	27.9	60.1	25.9	49.4

* Includes mathematicians.

† Includes agricultural scientists.

The physical sciences were nearly as well represented. Though the great majority of engineers and physicians had been excluded from our sample, representatives of these two disciplines comprised slightly over 25 percent of the sample. This distribution contrasts sharply with that for scientists in general.

As Table 2 indicates, physical scientists and mathematicians make up 72.1 percent of all scientists on the *National Register* of the National Science Foundation, but only 39.9 percent of all scientists in the federal government. The long-standing interest of the government in agricultural research no doubt accounts for this disparity. At the same time, if we consider only those scientists in the federal government who are engaged in research and development, the percentage distributions of different kinds of scientists are almost exactly those of the NSF *Register*. The federal government has three research and development people in the physical sciences for every one in the life sciences. Scientist-administrators are divided equally between the two main areas of natural science.

With regard to the scientist-administrators, two interesting conclusions emerge from the data. We found, as expected, that scientists tend to be better paid than most nonscientist-administrators. We also found that although the agricultural and life science group is numerically larger, there is a higher proportion of physical scientists in the higher echelons.

In order to make comparisons within our sample, we divided it into three salary groups, as shown in Table 3. Group 1, made up of individuals who earn $13,970 or more, includes the highest-ranking scientist-administrators—bureau chiefs and those in supergrades. Groups 2 and 3 are, respectively, in the middle and at the bottom of the federal scientist-administrator hierarchy. The first cutting point was chosen after inspection of the data. A number of scientist-administrators earned $13,970 in 1959; there was a considerable gap below that salary figure. We did not attempt to ascertain the significance, if any, of the gap; by choosing that point, we isolated roughly one-third of our sample for further study. The $11,000 cutoff between groups 2 and 3 was chosen because it is approximately half way between the top of group 2 ($13,969) and the bottom of group 3 (about $8000). This choice gave us a lowest group of about the same size as the top group.

Table 3. Salary Levels of Scientist-Administrators, and of All Administrators, in the Federal Government, 1959. Number of Scientist-Administrators, 170; of All Administrators in the Study, 310*

Group and salary	*Scientist-administrators* (%)	*All administrators* (%)
1 ($13,970 or higher)	31.2	13.9
2 ($11,000–$13,969)	40.0	47.1
3 ($10,999 or less)	28.8	39.0

* Federal salaries have been substantially increased since 1959. The positions which rated an $8,000 salary (GS 11) now begin at $12,600. A GS 13, who earned about $11,000 then now begins at $17,000. The $13,970 figure was for employees near the top of the GS 15 rating, now they begin at $24,251. The top supergrade, GS 18, has an annual compensation of $37,624 in 1972.

For purposes of comparison we checked the salaries for a 1-percent sample of *all* administrators in the *Official Register* and found (Table 3) that the percentages both for administrators in general and for scientist-administrators were higher for group 2 ($11,000 to $13,969) than for groups 1 or 3, and also that scientists tended to earn more than nonscientists. We also found (Table 4) that the distribution of scientists according to salary definitely favors the physical scientist.

We compared the three groups in other respects. We found (Table 5) that group 1 scientists apparently reach these positions at a fairly young age. Group 2 may include a number of scientists who will move to group 1, as well as many who have reached their highest position. We suspect that many of the scientists in group 3 (at least those to age 54) are likely to move up.

Table 4. Professional Affiliation of Scientist-Administrators by Salary Level. Number in Group 1, 53; in Group 2, 68; in Group 3, 49

	Group			
Field	*1* (%)	*2* (%)	*3* (%)	*Total* (%)
Agricultural and life sciences	9.4	36.8	71.4	38.2
Physical sciences	47.2	44.1	12.2	35.9
Engineering	26.4	13.2	4.0	14.7
Medicine	17.0	5.9	12.2	11.2

Table 5. Age of Scientist-Administrators. Number in Group 1, 53; in Group 2, 68; in Group 3, 49

	Group			
Age (*yr*)	*1* (%)	*2* (%)	*3* (%)	*Total* (%)
44 or less	28.3	23.5	24.5	25.3
45–54	54.7	38.2	49.0	46.5
55 and over	17.0	38.2	26.5	28.2

We also considered geographical origins and found that the sample as a whole was distributed as follows: northeastern United States, 29.9 percent; north central United States, 31.6 percent; the South, 19.9 percent; the West, 11.7 percent; and foreign, 7.0 percent. Group 1 has the highest proportion (15.1 percent) of scientists who are foreign-born (*6*).

We found the region of the undergraduate college for the three groups to be quite similar to the overall pattern for region of birth. In this respect there are no striking differences among the groups. But when we examine the type of undergraduate college (Table 6), we find some points worth noting. Group 1 has the highest percentage of individuals who attended private colleges, or universities. Of the total sample the majority attended a public college or university —a finding indicative perhaps of the social origins of most scientists—but the number decreases from group 3 to group 1. In effect, education at a private college or university seems to be associated, or to have been associated in the recent past, with later achievement of elite government-scientist status. There are no striking differences among the three groups so far as highest degrees are concerned. Of the sample, 24.1 percent have just the B.A. or B.S. degree, 16.5 percent have the M.S. or M.A., 41.8 percent have the Ph.D., 10.6 percent have just the M.D., and 2.4 percent have both the Ph.D. and the M.D. One person (in group 2) had not finished college. As a whole, then, this is a group of administrators with an unusual formal education.

Table 6. Type of Undergraduate Institution, by Salary Group. Number in Group 1, 52; in Group 2, 68; in Group 3, 47.

	Group			
Type of institution	*1* (%)	*2* (%)	*3* (%)	*Total* (%)
Private college or university	42.3	25.0	31.9	32.3
Public college or university	32.7	63.2	66.0	54.5
Technical; service academies	15.4	2.9		6.0
Catholic institutions	1.9	4.4		2.4
Foreign	7.7	4.4	2.1	4.8

The scientist-administrator also has an unusual degree of career-mobility. As Table 7 shows, almost two-thirds of the scientist-administrators had pursued a nongovernment career prior to joining the federal service. Of these, the largest percentage (19.4 percent for the three groups) had been employed in academic teaching before entering government service. In addition to this 19.4 percent, almost two-thirds of those with multiple positions (two or more positions outside of government) had held, or held concurrently, some academic position. More than 10 percent of the sample had come from industry; the proportions in this category decreased from groups 1 through 3.

Of the total sample, 8.2 percent had been previously employed in state or local government or had engaged in academic research or private nonprofit research. Thus, it may be con-

Table 7. Types of Nongovernmental Positions Held in the Past or Concurrently by Scientist-Administrators. Number in Group 1, 53; in Group 2, 68; in Group 3, 49

	Group			
Position	*1* (%)	*2* (%)	*3* (%)	*Total* (%)
None	28.3	26.5	55.1	35.3
Academic, teaching	20.8	19.1	18.4	19.4
Multiple positions (various)	15.1	19.1	12.2	15.9
Cyclical positions (various)	7.5	14.7	6.1	10.0
Industry	11.3	4.4	2.0	5.9
Industry research	9.4	4.4		4.7
State and local government		7.4	4.1	4.1
Private nonprofit research	5.7	1.5		2.3
Academic research		2.9	2.0	1.8
Miscellaneous	1.9			0.6

cluded that the scientist-administrator is drawn overwhelmingly from the nonbusiness sector of society.

Once in government service, the vast majority of scientists remain within a single department or agency. The percentages of such single-agency careers are 62.3, 75.5, and 91.8 percent, respectively, for groups 1, 2, and 3, and 76.5 percent for the total sample. Within the department and agencies (Table 8), scientist-administrators are to be found in large numbers in three departments (Agriculture, Interior, and Health, Education, and Welfare), in substantial numbers in three other organizational units, and scattered widely in the remainder. We were surprised to find the Army and the Navy so low on the list. A partial explanation lies in the reliance these agencies have placed, since World War II, on contract arrangements with private corporations and quasi-public agencies such as the Rand Corporation. But we know that the defense agencies employ thousands of scientists directly. The explanation of the low rating in our sample lies in the fact that, in the Army and the Navy, military career officers have, nominally, practically all of the administrative roles. Many of these men have received some engineering training by virtue of attending one of the Service academies but have not earned sufficient scientific status to be included in *American Men of Science*. Actual administrative responsibility may well be in the hands of a civilian scientist who reports to whichever career officer has been brought to the project through the circumstance of a change in station (7).

Table 8. Distribution of Scientists and Scientist-Administrators by Department and Agency, 1958–59 (see Table 2). Number of All Scientists in Study, 56,888; of Scientists in Research and Development, 20,640; of Scientist-Administrators, 170

	Scientists		
Department or agency	*All* (%)	*In R&D* (%)	*Administrators* (%)
Agriculture	34.2	19.6	31.2
Veterans	15.5	2.0	1.8
Navy	10.0	21.4	2.4
Army	9.9	16.1	0.6
Interior	8.3	13.4	21.2
HEW	7.7	11.4	14.1
Commerce	6.3	6.3	7.6
Air Force	3.6	6.3	4.7
AEC	0.8	0.9	4.1
NASA	0.6	1.3	1.8
All others	3.1	1.3	10.6

The career data permit us to make some generalizations.

1) Scientists occupy a considerable number of positions in the federal bureaucracy, including positions at the top of the various hierarchies. [In a recent study (8) it was ascertained that of 63 bureau chiefs in the federal government in 1958, nine had advanced degrees in the natural sciences and 17 others had come from an engineering or technical background.]

2) A higher proportion of scientist-administrators than of administrators as a whole receive high salaries.

3) The scientist-administrators come from a wide variety of geographical backgrounds, but the southern and north-central states are under-represented in relation to population.

4) The American administrative system is an open one at all levels, differing from the English and Continental pattern of entrance into government service only at the beginning of one's career (9). Almost two-thirds of the scientist-administrators of our sample had worked elsewhere prior to federal employment (20.2 percent in academic teaching and research, 2.3 percent with foundations), and 25.9 percent of the sample had multiple or cyclical careers.

5) Interdepartmental mobility was not common in our sample of scientist-administrators. Some students of public administration, in particular Leonard D. White, have argued the case for mobility, particularly at the higher levels. The vast majority of scientists, however, have worked for only one agency. On the other hand, interdepartmental mobility is much greater for group 1 than for groups 2 and 3. Thirty-eight percent of group 1, 24 percent of group 2, and 8 percent of group 3 have served in more than one department.

We can make a few tentative observations about the elite scientist-administrator. We recognize that the category of group 1 scientist-administrator is not synonymous with that of scientific elite. This latter category is broader; it includes advisers, part-time consultants, and nongovernment scientists. But there is some justification for considering the group 1 scientist-administrator part of an administrative elite. Comparison of the scientific-administrative elite with other administrative elite groups in our society—for instance, with business leaders, presidents of corporations, and flag officers in the traditional military services—is thus in order. It is a characteristic of members of these administrative elite groups that they have reached their top positions through performance and skills which have been rewarded by their superiors in the same fields. There are also political elite groups—congressmen, senators, governors, and so on. Such individuals are elected by a constituency, not by their colleagues or superiors. In Tables 9 and 10, data are given for a number of administrative elite groups and for one political elite group—U.S. senators.

The business leaders studied by Warner and Abegglen (*10*) include chairmen of the board, presidents, vice presidents, secretaries, treasurers, and controllers, from small companies as well as large. Hacker's group (*11*) deals only with the presidents of the 100 corporations that had the highest sales in 1958 (*12*). The group 1 scientists resemble the business leaders of the Warner and Abegglen study in geographic origin.

The group 1 scientist-administrators are the most highly educated of the various elite groups. There is not a single group 1 scientist-administrator who does not have at least an undergraduate degree. Of the three elite groups for whom we have data—U.S. senators, presidents of corporations, and scientist-administrators—the latter group has the smallest percentage of members who attended Ivy League colleges. The percentage is not much lower, however, than that for U.S. senators. The real difference seems to lie between these two groups and the presidents of corporations; the latter had almost twice the percentage of graduates of Ivy League colleges that either of the other two groups had. The percentages for undergraduate education entirely at private colleges (Ivy League or other) are highest for the scientist-administrators, next-highest for presidents of corporations, and lowest for U.S. senators. We noted earlier that of all the scientists in our sample the group 1 scientists had the highest percentage of members with private-college backgrounds (Ivy League and other). The finding suggests that this educational milieu is an important source of members of the administrative elite groups in our society. It is interesting to note that this also seems to be an important source of U.S. senators, inasmuch as 51 percent of the senators in 1949–51 had attended private colleges and universities (see *13* and Table 10). It should be noted, in qualification, that state universities also provide large percentages of elite person-

Table 9. Region of Birth of Several Administrative Elite Groups (10, 11, 14)

Region	*Business leaders* ($N = 7102$) (%)	*Presidents of corporations* ($N = 47$) (%)	*Senators* ($N = 95$) (%)	*Army* ($N = 166$) (%)	*Navy* ($N = 204$) (%)	*Air Force* ($N = 105$) (%)	*Group 1 scientists* ($N = 44$) (%)
Northeast	38	36	30	23	27	25	38
Northcentral	40	35	27	37	30	43	35
South	16	13	27	34	31	25	18
West	6	15	16	6	12	7	9

Table 10. Kind of Undergraduate College Attended by Members of Various Elite Groups (11)

College or university	*Senators* ($N = 100$) (%)	*Presidents of corporations* ($N = 95$) (%)	*Group 1 scientists* ($N = 45$) (%)
Ivy League	15	29	13
Other private	36	27	44
State university	40	31	42
No college	9	13	

nel, especially in the case of scientists and U.S. senators. As for presidents of corporations, almost one-third received their undergraduate education at state universities.

A number of studies have indicated a fairly sharp distinction in social origin between military leaders, corporation presidents, and U.S. senators, on the one hand, and business executives in general on the other. The scientific-administrator elite is clearly closer in this respect to the business executives.

The military elite, according to Janowitz (*14*), "has been drawn from an old-family, Anglo-Saxon, Protestant, rural upper middle-class professional background." Even though the base has broadened somewhat in the years since World War II, he says, "a strong emphasis on second- and third-generation, native-born, nonindustrial background still persists." The data for U.S. senators in 1949–51 suggest that these individuals are drawn from the upper levels of the society (*13*). And Hacker (*11*) suggests that the presidents of corporations, like the U.S. senators, are "grandsons of 'old American' families." In addition, his data on religion make it clear that 60 percent of the presidents of corporations belong to "high-status" Protestant churches.

Warner's data suggest that mobility among business leaders was greater in 1952 than it was in 1928, as reported by Taussig and Joslyn (*15*). Warner summarizes his data by stating (*10*) that "there has been not only an increase in the proportion of men who come from the lower ranks, but an accompanying decrease in the proportions of sons of highly placed men, particularly businessmen." We know from the Knapp and Goodrich study (*16*), as well as from the National Opinion Research Center (NORC) study of 1961 college graduates (*17*), that physical and biological scientists (except for physicists) tend to be drawn, to a disproportionate degree, from the middle and lower middle class. Thus, the business leaders and the group 1 scientist-administrators are drawn proportionately more from the lower middle class than the other members of elite groups are. It certainly seems that the occupational fields in our society which are most permeable, or in which social mobility is highest, are business leadership and science (*18*).

What, then, can we conclude about the federal scientist-administrator? It is important to emphasize again that our sample includes only natural scientists. The career of scientist-administrator is an open one in two senses. First, this is one of the sectors of our society in which social mobility is possible for individuals from a number of diverse backgrounds. The career of scientist-administrator is comparable to that of federal administrator in general, though probably the scientist-administrator has a higher level of education than the nonscientist administrator (*19*). Second, it is open in the sense that almost two-thirds of federal scientist-administrators have worked for another organization prior to joining the government.

As a group, the scientist-administrator elite contains people of diverse social backgrounds and bears interesting similarities to the business-leader and the nonscientist-administrator groups. Our data do not explain why the scientists become administrators. There is some evidence that money rewards for federal scientific-administrators are higher than for federal administrators in general. But these individuals were scientists before they became administrators, and a crucial question is, What induces scientists to move from scientific work to administration?

The role of government in science is still expanding, and it may well be that some other changes are in the offing—for instance, changes in the proportion of physical scientists, the entry of greater numbers of social and behavioral scientists, and changes in the proportion of scientific-administrators in the government.

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2. *Official Register of the United States* (Government Printing Office, Washington, D.C., 1959).
3. J. Cattell, Ed., *American Men of Science: The Physical and Biological Sciences* (Cattell Press, Tempe, Ariz., ed. 10, 1960–61).
4. We made, of course, a number of arbitrary decisions in choosing our sample. The 31,000 administrators listed in the *Register* probably do not include all officials who might appropriately be so categorized. However, with one important exception, discussed in the article, it seems safe to assume that the *Register* includes most federal administrators. Similarly, it might be argued that *American Men of Science* does not have data on all individuals in the United States who could be called natural scientists. The first four volumes include data on about 96,000 people, but there were doubtless some who did not reply to the questionnaires sent out by the editors. On the other hand, *American Men of Science* explicitly excludes most engineers and physicians. Roughly speaking, it includes engineers and physicians who have earned a Ph.D. degree or who have achieved comparable status in research or administration. For purposes of this article, engineers and physicians listed in *American Men of Science* were designated scientists.
5. Two "science writers" also turned up in the sample; we decided not to include them in our analysis.
6. Of the eight foreign-born scientists in group 1, four could be classified as directly engaged in some kind of defense work. Two more were employed in some capacity by the military departments.
7. A former Navy personnel officer told us of one scientist who was asked why he remained in the Navy. The civilian scientist replied that he knew he could always look forward to having a new boss in 2 or 3 years. An idea which could not be sold to one of the regulars could be put on the shelf and brought out again when a new officer came to the project. Admiral Rickover, on the other hand, has criticized the system strongly. "A constantly moving assembly line of officers is entering and . . . they automatically take charge. . . . The civilians have become dispirited" ["Organization and Management of Missile Programs." *Hearings Before a Subcommittee of the House Committee on Government Operations, 86th Congress, 1st Session* (Government Printing Office, Washington, D. C., 1959), p. 607].
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17. J. A. Davis *et al.*, "Great Aspirations: Career Plans of America's June 1961 College Graduates." *Natl. Opinion Res. Center Rept. No. 82* (1961), pp. 56–59.
18. We have evidence from a number of studies that there are some personality characteristics associated with occupational choices. It has been suggested [M. Rosenberg *et al.*, *Occupations and Values* (Free Press, Glencoe, Ill., 1957)] that natural scientists stress self-expression to a greater extent than individuals in business occupations, and that the latter place more emphasis on extrinsic reward than natural scientists do. Davis (see *17*) suggests that individuals who go into the sciences and engineering tend to be "low on extroversion and sophistication," whereas business attracts those who rate themselves "high on extroversion but neither high nor low on sophistication."
19. R. Bendix, thesis, University of Chicago (1947).
20. *American Science Manpower, 1956–58* (Government Printing Office, Washington, D.C., 1961), p. 3; *Scientists and Engineers in the Federal Government* (Government Printing Office, Washington, D.C., 1958), pp. 5, 8.

Scientists as Advisors and Consultants in Washington

Bernice T. Eiduson

This article, dealing largely with the relationship of the research scientist to Washington, is based on a follow-up investigation of a group of research scientists, carried out in 1964. The general aim of the follow-up was to study the course of their scientific careers, establish the main trends in development, and identify some of the factors influencing these trends.

Scientists: Their Psychological World (Eiduson, Basic Books: New York, 1962) describes the 1958–59 sociopolitical study of these forty research scientists. This investigation followed a psychological study of artists who had been compared with a group of non-artists (businessmen) acting as a control group (Eiduson, "Artist and Non-Artist: A Comparative Study," *Journal of Personality 26*, 1, March 1958). When this project proved successful in identifying a series of variables which distinguished the artists from the businessmen, the project was extended to the research scientist in an effort to determine whether these variables also described persons in other creative fields.

In the five years between the first study and the follow-up, it became apparent that the researchers were participating much more actively in government—in advisory or consulting capacities—than they had done previously.

The group had originally cited 33 government agencies with which they were involved as compared with 73 agencies in the 1964 study. While this work was distributed among 15 men before 1958—an average of two agency involvements per scientist—18 shared the government involvements in 1964. Thus, although the number who were serving as advisors or consultants had not greatly increased, those who were participating seemed to be doing so more actively.

A comparison of those who are participants at this time, with those who are not, showed a tendency for government to call upon older and more experienced men, their mean age being 52; average number of years beyond the PhD, 22.7. The nonparticipant averaged 43 years of age, and 15 years post-PhD.

There are three areas in which the scientists function: (1) as members of committees set up around specific projects within departments, or as consultants on specific research proposals; (2) as officially designated consultants with NASA, the NSF, the Office of Naval Research, the Air Force, or within National Institutes of Health divisions; (3) as advisors in part-time or close to full-time capacity for

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periods ranging from one to three years on such bodies as the President's Science Advisory Board, the National Academy of Sciences, or as members of permanent scientific committees. There was some mobility among these categories during the five-year period.

Data concerning the involvements with Washington were collected via interviews of approximately three hours, organized around a series of open-ended questions and supplemented by detailed curriculum vitae. These data supplemented detailed information about early development, personality make-up, motivations, and cognitive styles that were obtained during the original study. Of the 40 research scientists—12 chemists, six physicists, six earth-soil scientists, 16 biologists and zoologists—who comprised the original group, 39 were still living at the time of the follow-up.

The group was purposely not selected as homogeneous in regard to specific specialties, level of achievement, or status. Therefore, the results of the Washington experiences should not be considered representative for all research scientists. They are instead preliminary; their application to a larger body of research scientists needs still to be established.

SUBSIDY AND CREATIVITY

One factor serves as a common frame of reference for all of the scientists in the group: the federal government's role in subsidizing scientific research. The research of each of these men is primarily funded today by the federal government. In almost every case the support has been awarded individually, although in some instances it has come through academic departments which then make individual assignments.

In general, the response of this group to the heavy subsidy has been favorable. The subjects agree that more research production is possible than ever before and that the tempo has been hastened, with more technical workers hired; more post-doctorates supported; and more equipment made available. Scientists are already so accustomed to this new way of research life that few can fathom the idea of reverting again to the pre-government-subsidy era.

The scientists reported that the stepped-up program for which the government is responsible has influenced both the ways science is practiced and the quality of research. Certainly, the preliminary trends toward group research efforts noted five years ago have been swept into vogue. The scientists assert that the group research style comes from having funds pressed upon them, which encourages planning on a large scale, and has fashioned empire-builders from researchers. Some complain that empire-building has become so common in the university setting, that an extensive "organization" has become a *sine qua non* for the choicest academic plums. Furthermore, it is their conclusion that because empires demand administrators, scientists are to be found more frequently in desk chairs pulled up alongside the university accountant than on laboratory stools.

Yet the personal advantages of big science have apparently so outweighed the disadvantages that only a few decry these developments. However, there are some qualms about the effects of the large subsidies on the quality of research. Only 25 per cent of the group felt that money has actually improved the quality of research productivity. This minority takes the position that having review boards has served to raise standards for research projects; that protocol procedures serve as a stimulus to the scientist to organize his work at regular intervals, and place a premium on carefully conceived and well-documented ideas.

The majority of the group noticed a deterioration in research quality. They point out that more "hacks" have been drawn into the field, men attracted by the decent living and the prestige that science now offers. As one physicist said, "These hacks flock like locusts to well-funded areas and bring into science a level of ordinariness which previously would not have been tolerated."

The scientists also attributed certain bandwagon effects in research to the large-scale government subsidy. The government is accused of fostering fads and fashions in science. Although this is not consciously done, certain areas of inquiry do skyrocket with support, while others die on the vine. It is reported that this support has subtly encouraged scientists to affiliate their research interests with "hot" areas. Thus every research project is said to promise a crucial new kernel of information in the study of, for example, cancer or mental retardation; of course, in most cases, such relationships are extremely tangential. The stepped-up tempo of research is also blamed for heightening competition, and leading to premature publication.

Some scientists feel that the ready availability of funds is often wasteful, with more expensive equipment employed than is required. Funds are also felt to deplete manpower; as one man stated, "money has encouraged so much travel that nobody is staying home to work any more."

By and large, the majority of scientists could cite no negative effects of the heavy government subsidy on the creativity of the individual scientist. In fact, a few felt that because the government could afford to take more risks in the kind of projects that it supported, it actually encouraged more creative endeavors. However, others thought that the attendant cumbersome bookkeeping procedures promoted conservatism, since investigators felt bound to adhere to the supported programs.

One of the more vocal physicists, however, felt that the whole granting system was antithetical to creative work:

> I absolutely reject anything that has to do with government work. Various agencies send me projects to judge and ask me to decide whether a certain project should be supported or not. However, I won't answer the question of whether it's a good project, because I can't judge it. My feeling is, nevertheless, that every time a man can tell me how many years he is going to work and how many research assistants he's going to need, and how much equipment, he's not going to succeed. I can tell you if I'm going to work on gravity now, at this moment; but I don't know whether I'm going to have the answer to the problem that I want solved at the end of one week or four years. Therefore, how can I decide how many research assistants are necessary to help me with something I do not even know I'll be working on in the future? It's impossible! The whole idea is absurd!

This man, who has won some of the major scientific awards in this country, obtains his research funds by merely making his needs known to the university.

These, then, are some of the common experiences that have grown out of the contact with the government on the research scene, and which form the background from which scientists go to Washington for more direct confrontations.

THE RESPONSIBILITIES ANTICIPATED

Only eighteen of these thirty-nine subjects actually worked professionally in the capital. Some of the others, for whom such work promises to be a future experience, voiced their expectations of what the Washington experience would be like. Where they provide insight into preconceptions which may in fact condition some of the actual experiences, they have been included.

The total group, of course, anticipated that the use of scientists in Washington would continue to increase. More and more, decisions involve technical scientific considerations so that consultation becomes imperative. Scientists feel they have unique skills in this area that can be directed toward a number of purposes. Of chief importance is providing data to congressmen, embassies, and directors of departments, to inform them about the scientific aspects of their decisions. In addition, the subjects thought they could acquaint Washington with the kinds of information that scientists potentially could contribute. They felt that they were capable of, and had the responsibility for, showing the consequences of scientific developments, so that future problem areas could be anticipated by legislators. They hoped, too, to develop some genuine interest in science in Washington which might break down some of the emotional barriers that exist today and which inhibit a more routine rational approach to problems.

They anticipated that, in the long run, they even might effect a better liaison between government and the university. One aim in doing so would be to improve the balance of support among all fields of knowledge. They hoped, too, that they might be effective in broadening the base of the relationship between government and industry.

As educators, they hoped that one day they might even partipate in updating the training of scientists who are part of the regular technical cadre of the government. This might enable these civil servants to represent a very high level of scientific competence, which is not the case in all government departments today.

The researchers' feeling of responsibility grows out of appreciation of certain basic scientific skills and talents that seem appropriate to the tasks they could do in Washington. Included are such factors as intelligence; a strong technical background which has pertinence to current problems; an approach to problems which is rational and free from prejudice or prior commitment; scholarliness and attention to detail in pursuit of answers to questions; a problem-solving attitude which isolates what questions are appropriate and suggests ways to structure problems into solvable units; and the know-how or the skill to work on problems once they are posed. In addition scientists feel they are able to learn on the job and therefore can turn their problem-solving attitudes toward a variety of subject matters. Particularly germane to the scientist, too, is the high premium placed on imaginative and creative solutions to problems.

A few added that their administrative experiences or consultant work in other situations, such as in industry, or with special problems groups like UNESCO, had given them some prior experience for work in Washington.

THE MOTIVATIONS

With this imposing list of responsibilities and qualifications, it is perhaps not surprising to hear these scientists admit that their colleagues who go to Washington do not invariably measure up to the job, and are not always the most competent persons. The subjects who have not been chosen for these posts say that they often go to the more aggressive men, the more opportunistic, the ones who foster the "right contacts" while active in professional organizations.

The subjects who actually go to Washington agree that top scientists sometimes decline opportunities to participate for fear that their research careers would suffer from the necessary diversion. They feel, too, that the kind of scientists who enjoy Washington are those whose gratifications lie in organizational relationships and who recognize that in the capitol such needs can be readily gratified. Because it is possible for scientists, once there, to become remote from the university and laboratory, and to make a pseudo-career in science, or perhaps a career in pseudo-science, the most dedicated men, who search for an understanding of the world, are not likely to go. Only one subject said that researchers who go to Washington are the men with the highest public consciences, who sacrifice their research careers to serve their country. He added that these are often the busiest and most creative men who can move from one issue to another without any waste of time, and with unusual powers of concentration.

Our Washington-goers admit that they have been lured by a number of motivations. Some like "to be in the know" both in the university and in the political arena. They like to travel if this provides an opportunity for learning what goes on elsewhere, and for meeting interesting people.

All candidly state that prestige has been the strongest motivation for accepting such undertakings. They admit to the feelings of power, status, and pride these endeavors generate. One researcher elaborated it this way:

> Everybody loves to be needed and to have influence. For me it flatters my ego. There is nothing altruistic about it. It's pleasant to hobnob with generals and cabinet officers. I'm not doing this work out of duty but out of snobbery—out of a competition for prestige—a peculiar kind of prestige that's not scientific or public. It's stupid, perhaps, but it flatters my ego. It's like newspaper write-ups or appearing in *Life* magazine. It gives you a special kind of excitement.

Others spoke of the more altruistic aspects of their motivations. Some said that they have gone to Washington out of a sense of duty; one felt it important to do his share even though he did not particularly like being called away from the laboratory. Others were encouraged by their universities to try to play a significant role in shaping the ways governments might use scientists in the future.

THE WASHINGTON EXPERIENCE

Charging into Washington, then, with obvious pride, a glowing sense of responsibility and a long list of talents, the scientists found themselves generally taken aback by their very ambivalent reception. They anticipated being regarded with the same attitudes and esteem as they were at the university. These expectations had been encouraged by the knowledge that scientists were increasingly being beckoned to Washington, and even increasingly listened to. Many knew of the study by the National Academy of Sciences to determine the frequency with which advice from scientists is actually taken; this was showing a fluctuating but generally positively accelerating curve.

To be sure, the scientists reported that their experiences in Washington were not completely without satisfactions. Some found Washington an educational experience: it was interesting to see how committees worked, how different viewpoints were expressed, and to learn something about "the Washington game." Even being forced to make themselves understood in very specific nontechnical terms was cited as a good learning experience. Others felt that new sets of problem areas were opened to them, especially in social and life science areas. Their assignments included programs for control of narcotics; technical aspects of foreign aid; the Geneva test-ban agreement; ways to effect the exchange of scientists with bloc countries; manpower deployment to certain industries; assessment of the PhD market in future years; educational and scientific aid to underdeveloped countries; evaluation of drugs like thalidomide; and educational dropouts.

Yet, by and large, these positive experiences were outweighed by the frustrating and distressing ones. The scientists found that "it was still a political Washington." Uniformly they noted that there was very little awareness of the scientist, and very little indication that he was making any impact. As one said, "Now we are blasé and we do not regret what we've done; but we were certainly naïve to think that we could contribute anywhere near what we thought we could, and naïve to think that our services would be demanded by the total government." He had gone to Washington thinking that a great reservoir of talent existed in the universities that could be drawn upon by government, and he was interested in making a first stab at unleashing some of these forces. He found, however, that there were few in Washington who believed that scientists could make good and important decisions.

In general, the researchers found congressmen untrained in using scientists and uninformed on even popular scientific ideas, so that they were not interested in these intellectual men, and even shrank from them. A chemist said, "Congressmen are so used to getting advice only from lawyers and accountants that they feel intruded upon and afraid."

Perhaps more distressing were the experiences reported by five of the group who felt that they were "misused"—either played one against the other by politicians, especially if the politicians and scientists disagreed; or used as "whipping boys" to get votes by implying an unholy alliance between certain lobbies and certain scientists; or dragged across the country for committee meetings that would last only an hour, when other, equally competent scientists were available close by.

There were eight scientists whose contacts with congressmen led them to remark on their surprise at finding them very intelligent men; one said, "among the most intelligent men I have ever met." However, they were forced to recognize that legislators operate and think differently from the ways researchers do, with more attention to people and to the balance of factors. One physiologist was impressed with the legislators' understanding of areas in which they had no formal knowledge. However, another said, "All people who run federal projects and who appear before Congress are really nincompoops."

The greatest single source of frustration for the entire group lay in their being asked for advice, but seldom listened to. Some recognized, however, that decisions had to be based on other factors in addition to the scientific, and that these could not be knowledgeably evaluated by scientists. A number noted that scientific recommendations were often unsound in the light of the total picture. A retrospective comment pointed out that had some of the scientists' recommendations been taken, the consequences for the country might have been disastrous.

They also complained of being involved in sessions that were so bogged down by procedural detail and political maneuver that there was little opportunity to provide the scientific output that had been requested. In international conferences, they were particularly aware of the "tug of war between the United States and the big nations for the little ones," and sensed how insignificant scientific information was in this context.

They felt, too, that they had very little feedback from meetings in which they participated. Even members who had served on the President's Science Advisory Committee said that there was little evidence that any of the prepared reports were read or implemented. In other agencies, reports never moved to the top where they could conceivably have been considered. As a group, subjects remain unconvinced that advice offered is actually taken.

Another frustration was the slow pace at which Congress proceeds. Congressional action operated on a different time schedule from that to which scientists had been accustomed; it was necessary, therefore, to allow for a different time scale for change. While it might take a year or six months to "get an idea across," the subjects were generally in Washington for only a few days at a time. One scientist said, "Congress moves slowly and is quite unsympathetic to an intellectual approach. . . . I myself was very concerned about this, because of the way Kennedy was often stymied by this situation, for I was sure that his actions would be misunderstood. I found that I too was just not constituted for such a slow approach to problems."

Those in Washington for longer periods were disturbed because even the increased time gave them no chance to think. A chemist said, "No ideas or concepts are developed; instead, all the time is spent convincing people of things. There are lots of personal interactions; but there is no production of new ideas."

THE EFFECTIVE SCIENTIST

Because so many frustrations seemed to grow out of their feelings of ineffectiveness, it seemed appropriate to ask whether there were some scientists who were more effective than others. There was general agreement that some men were indeed effective and skilled in government matters, and a number of qualifications making for effectiveness were proposed.

Most important among these was the amount of time spent in Washington. There was unanimous agreement that full-time presence was necessary to insure competence. Since no one was professional in the kind of work required by this role, and no custom-made knowledge was ready at hand, researchers thought that learning on the job just took a long time. Being in Washington for longer periods also taught the scientist how to talk to congressmen. The greatest advantage, however, lay in being present at the critical time. As one geophysicist said, "The more you are in Washington, the more you are listened to, because you learn to get around, and you learn to put the right words in the right place in the bureaucratic maze. It is something you build up through experience. . . ."

This was discussed further: "If you are going to be there just a few months, the chances of your seeing a good idea come to fruition are very small. Eighty to ninety per cent of what I have seen come to fruition has taken six months to a year. I've taken an apartment in Washington and when I'm not around, I have an executive secretary there who serves as my eyes and ears. He lets me know when something important happens and alerts me when I've got to rush back; I couldn't be without him."

The scientists feel that full-time people have the best opportunity to learn the Washington game. One scientist was amused at the comment of a colleague who said, "When I get back to my university, my department will be putty in my hands. Compared to the ways I've learned to operate in Washington, my friends in the department are just amateurs." Full-time people can use this know-how to fight for their programs.

However, because full-time means becoming a career politician, scientists become wary of falling into a trap which might jeopardize their status. As full-time persons, they cannot climb out on limbs or advise "offbeat" solutions, because then they are liable "to get their heads chopped off." It is generally thought that this situation makes full-time people more conservative and less daring than the part-timers, who never lose touch with their universities and know they can always return.

It should be pointed out that the requirement of full-time participation causes one of the main conflicts among the scientists, for while desirable in some regards, it has great drawbacks. A full-time Washingtonian loses touch with his university and with research, and once this happens, he can no longer be considered a first-rate scientist. This weakens his effectiveness, and even his worth to Washington, for usually it has been his intimacy with the current research scene that has made him valuable in the first place. While effectiveness goes hand in hand with a strong scientific reputation, scientists also appreciate that the longer they stay in Washington, the more they lose their identity as scientists. To others, as well as to themselves, they become merged with the lawyers and politicians, and all the "advice-givers" who have preceded them.

A number of personality characteristics were additionally listed as important factors for effectiveness: sensitivity to other people's ideas; personal forcefulness—"If you're not forceful, you won't be listened to, because there are a lot of people in Washington trying to get their ideas across"; an ability to make decisions without worrying about too many details; an ability to "live with the consequences of decisions without becoming too obsessive and constantly debating with yourself about whether or not you have made the right decision"; an ability to arrive at solutions to problems now, not in ten or fifteen years, and without regard for whether the solution is going to be the most appropriate one for next month or next year. In addition, broad scientific as well as general interests; a commitment to work in Washington; and great powers of analysis, comprehension, tolerance, and understanding were proposed as valuable factors.

AFTERMATH

Perhaps the reactions of this group of scientists to their Washington experiences can best be summarized by pointing out that all but one of the scientists studied have resigned from the majority of their commitments. They have given up their posts and returned to the laboratories. The reasons for this retreat are twofold:

First, there is the severe disruption of research and university activities that the Washington work occasioned. While university presidents and chancellors were initially encouraging and agreeable to the Washington involvements, many did not appreciate the time that this would take and the difficulties it would occasion in conduct of the university. As professors, the scientists found that their graduate students would disappear during their absences. Professional associates resented the extra burdens they had to assume while the researchers were in the East. Travel and time grew to be severe pressures, but more disruptive was the consumption of emotional energies in trying to convince others of the scientific viewpoint. Furthermore, while important for their careers, the Washington experience did not prove to be fundamental; and most recognized that it was in fact keeping them from engaging in the very activities they hoped to promote by going to Washington.

The second reason for the retreat stems from the disappointments and unpleasantnesses in experience. This is summed up in quotations from two men who devoted large blocks of their time, for two-year or three-year periods, in Washington. One said:

> I was becoming a government committee figure. It was really big business. I found myself doing unpleasant things with people I despised and even before I knew it, I was finding myself trying to rival them. Then I realized that I was really selling out my soul to them; and that if I were going to do this, I might as well go into the Hollywood business, or do something else that would be more interesting and profitable.
>
> This attitude came to me one day when, at a committee meeting where we were discussing an unpleasant topic related to death, a man leaned over the table, and said to me, "Is this what we went to college for?" I felt nauseous, and almost had a blackout, and decided to get out right then and there. Now I see other kids doing what I was doing and I know very well that they are not doing it too well because they're too young—and I say, "Look at those sons-of-guns wasting their time the way I did in Washington." I ought to tell them not to do it—but I won't because somebody's got to do it, and this way it doesn't have to be me. It's an emotionally revolting and emotionally draining experience, and I've done my duty.

A second man reported: "I made a clean break after three years; otherwise, part-time availability would have called me up for one thing after another. . . . At first I was enthusiastic about it all. I thought it would be a great new avenue into which scientists could venture. I thought I would start and bring in my interested colleagues, but I learned that I was very naïve to think that we could play a significant role in Washington at this time."

It seems to me that these voiced disappointments reflect particularly the discrepancy between expectation and anticipation, and the realistic experience. Scientists thought that they would be cast into roles and responsibilities akin to those they undertook as researchers in the university laboratory. Their self-images as investigators had prepared them to act in Washington as they did at their home base: as "true" scientists who work logically, sort evidence systematically, weigh decisions carefully.

While these stocks-in-trade were not completely inappropriate to the Washington scene, they were at least insufficient for the kinds of functions the scientists had to assume.

Contributing, too, to the scientists' withdrawal from government work was Washington's lack of knowledge about how to use scientists, and lack of imagination about what scientists could do. The ambiguities and uncertainties which government agencies displayed here made their reactions to the scientists vague and lukewarm at best, and probably openly hostile at worst. Neither scientists nor the government realized that both sides were engaged in pioneering and unstructured efforts.

These first experiences compel a very critical examination of the future structure of the scientist-government relationship. The scientists themselves, troubled by the outcome of these preliminary encounters, have made certain recommendations for the future. They acknowledge that the situation in Washington moves so quickly and has so little stability that it is difficult to predict the kinds of arrangements that might be more workable. However, they are recommending that the problem might be constructively approached by experimenting with a number of models for using scientists in advisory and consulting capacities. Variables in these models would include such factors as the criteria for selection of participating scientists, their ages, backgrounds, and personality characteristics; arrangements with universities and industry in regard to the kinds of leaves and time commitments that are optimal; and the different capacities in which scientists might serve. In the light of this study, it might be well to include some experimentation with the kinds of preparation that might best reorient the scientists' thinking and preconceptions toward realistic assignments.

The group proposed, furthermore, that social scientists be called in to design some of these experiments, not only because of their skills, but also because such a task would provide an opportunity for social scientists to demonstrate their much-neglected resourcefulness. Quite conceivably, such projects might produce suggestions for using retired academic scientists or active industrial scientists more fruitfully than they have been used until now.

While a cabinet post for science has been popularly considered, this group has some doubts about whether such an office would provide an appropriate solution for the kinds of problems they have encountered. As yet, perhaps the functions of such a position are too ambiguous to presuppose that these problems might come under scrutiny. Many of the group question the desirability of giving science cabinet status. Their attitudes were expressed by one who said, "Let's remember that science is like the English language. It cannot be encompassed in one department. Science and a scientific approach should be used everywhere, at all levels, and in all departments of government. A scientific approach must be slowly diffused into everything that is done in Washington. Only then will science's real influence be felt."

C. Productivity

Introduction

How can we predict at what periods of their lives scientists will be most productive? The studies in the present section all relate to the problem of the correlation between aging and productivity. Some (Dennis, Lehman, Pressey) try to directly determine the relationship between these two events, while others (Roe, Cook and Hazzard) are primarily concerned with the effects of aging and only indirectly consider productivity.

It is well known that research scientists differ in productivity. The first Dennis selection examines the productivity of four groups of psychologists, active at different periods of time from 1887 to the present day. Within each of the four time samples, the publications of a relatively small group of individuals composed a large percentage of the total output of that sample and the majority of psychologists were relatively unproductive. For instance, the top 10 percent of the producers of each sample contributed from 37 to 47 percent of the total output of publications. Given these facts, Pressey advances an idea for specifically increasing the productivity of psychologists. While persons in the physical and natural sciences tend to select a career early in their lives, generally while still in their teens, only a minority of psychologists have decided on psychology as their life's work by the completion of their second year in college. If, as Lehman suggests, outstanding contributions in psychology tend to occur between the ages of 30 to 39, and sometimes even before, late completion of graduate training (i.e., on the average at age 31) limits both the quality and quantity of work. From an examination of the autobiographical material of 28 eminent psychologists, Pressey concludes that the main reason for the late selection of a career in psychology is that students were unfamiliar with psychology as a science and were often not offered psychology as a subject in school until the second or third year in college, a trend that is fortunately changing. Pressey advocates as a means of increasing the productivity of psychologists the teaching of psychology in high school and the selection of superior college undergraduates for accelerated programs for early completion of the doctorate.

As alluded to above, Lehman, through examination of several recognized published histories of psychology, found an age decrement in the productivity of psychologists after a peak at the 30 to 39 age range. However, psychologists do show their peak productivity at a later age than do scientists in the physical and natural sciences who generally show peak productivity at ages 20 to 29, according to Lehman (1960a; 1960b; 1962; 1966). While the Lehman interpretation is frightening at first glance because it implies that useful scientific life ends at the age of 40, the author recognizes that some invaluable contributions have occurred at age 70. Also, recent work by Bromley (1969), mentioned in Part I, has pointed to methodological shortcomings in Lehman's work, which make these findings doubtful.

The second selection by Dennis also contradicts Lehman's results. Dennis found that productivity of nineteenth-century physical and natural scientists is quite low between ages of 20 and 29, while in the 30s a high rate of productivity is achieved and this rate is maintained until the late 50s. In fact, more publications appeared while the scientists were in their 70s than appeared while they were in their 20s.

A limitation of the first four studies is that their results are based on study of nineteenth- or early twentieth-century scientists, gleaned through clever examination of biographical or autobiographical data, rather than study of the currently active, present-day scientists. The next two selections consider present-day scientists and scientific institutions. In a 15-year, follow-up study of a group of established scientists, Roe found that these men, aged 47 to 73 at the time of data collection, show few changes over the years unless they go into administration, although some exhibit changes in motivation and less energy, as did Eiduson's scientists (1970). Few retire before the maximum age and even after retirement they keep their offices and laboratory space and actively write or do research. Roe's eminent scientists (as those in the Dennis sample) have continued to contribute scientific articles and books to the scientific field in which they attained eminence.

Cook and Hazzard examine changes in the age distributions of scientists within a research laboratory as the institution itself matures. Generally, the older the research institutions become, the older the average age of the scientists working for them. Extrapolating another 15 years from present and past data, Cook and Hazzard predict that the typical 40-year-old research laboratory's scientists will be fairly evenly distributed over the 32 to 65 year age group with 80 percent over 38. The authors admonish both scientists and management to recognize the types of problems this older age distribution will encounter. If the data on peak periods of productivity are correct, publication rates of scientists in such laboratories may remain high, but significant scientific discoveries in the laboratories may decline.

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Productivity among American Psychologists

Wayne Dennis

That scientists differ greatly in regard to the number of their publications can readily be demonstrated. The question that we wish to examine here is whether the aggregate publications of any generation of scientists are made up primarily of the work of the highly productive minority or are composed chiefly of the contributions of the less productive majority.

Our data come from four groups of American psychologists. Because they were chosen

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from different periods of time, there is very little overlap in the membership of these groups.

GROUP I

From Murchison's *Psychological Register*, Vol. III, we obtained the names of all psychologists listed therein who were living in 1932. In order to render our subjects more homogeneous in regard to nationality and age, we chose for study only Americans and only those born prior to 1879. There were 160 such persons. The *Psychological Register* provided for each of these persons a bibliography of his publications through age 50.

The distribution of these bibliographies according to total number of publications is as follows:

No. of titles	*No. of persons*
130 and above	1
120–129	2
110–119	1
100–109	0
90–99	1
80–89	1
70–79	2
60–69	2
50–59	7
40–49	6
30–39	12
20–29	13
10–19	28
0–9	84

Clearly this is not a normal distribution. Rather it resembles the upper end of a normal curve. The most common degree of productivity is in the lowest category, namely, zero to nine publications, for the first 50 years of life. In the higher brackets of productivity, the number of persons becomes increasingly smaller.

Persons in Group I published a total of 3,056 titles. How were these publications distributed among persons of different degrees of productivity? To answer this question, the subjects were placed in deciles according to the size of their bibliographies, and the percentage of the total productivity contributed by each decile was found. The results are indicated below:

Decile	*Percentage of total titles contributed*
1	47
2	21
3	12
4	8
5	5
6	3
7	2
8	1
9	1
10	1

In each decile there are, of course, 16 subjects. The subjects in the lowest decile produced 4 papers, about one-tenth of 1 per cent of the total. In contrast, the top 16 subjects produced 47 per cent of the total. The lower half of the distribution contributed 7 per cent of the aggregate output of publications, and those above the median were responsible for 93 per cent. The most productive man published more titles than the 80 persons who made up the lower five deciles. These figures show unequivocally that in this group a minority of high producers was responsible for the bulk of scientific publication.

The reader may be interested in knowing the names of the 16 persons of decile 1 who wrote approximately half of the publications produced by their generation. They are, in alphabetical order: M. W. Calkins, June Downey, Knight Dunlap, C. E. Ferree, Shepard I. Franz, M. E. Haggerty, C. H. Judd, J. H. Leuba, Max F. Meyer, L. M. Terman, E. L. Thorndike, J. E. W. Wallin, H. C. Warren, Margaret F. Washburn, J. B. Watson, and R. M. Yerkes. If the reader wishes, he may compare his familiarity with these names with his familiarity with the names of persons in deciles 8, 9, and 10 by turning to Murchison's directory and finding men of this age group who have zero or one publication listed therein.

GROUP II

The second group is one studied by Fernberger in 1938.[1] He chose for study all persons who were members (not Associates) of the APA in 1937. They numbered 587. For each person he determined, by reference to the *Psychological Abstracts*, his total publications for the five-year period 1932–1936.

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[1] Fernberger, S. W. The scientific interests and scientific publications of the members of the American Psychological Association, Inc. *Psychol. Bull.*, 1938, 35, 261–281.

We have divided his subjects into deciles of productivity and have determined the proportion of titles which each decile contributed to the total. The results are as follows:

Decile	*Percentage of titles contributed*
1	37
2	21
3	14
4	10
5	8
6	6
7	3
8	1
9	0
10	0

Although Fernberger's data refer to a younger group of subjects than the persons in Group I and represent a 5-year rather than a 50-year span of time, they are very similar to those previously presented. The highest decile produced 37 per cent, the highest quintile produced 58 per cent, the most productive half published 90 per cent of the total publications.

GROUP III

Recently, under a grant from the National Science Foundation to the APA, Kenneth E. Clark has been engaged in studies of the scientific productivity of psychologists. He and his collaborators, by examining the *Psychological Abstracts,* have determined the total number of titles published by each psychologist who was a member of the APA in 1951 and who had received his PhD between 1930 and 1934. Thus, for each person the publication record covered from 16 to 21 years. Through the courtesy of Dr. Clark, we have been permitted to analyze these data by the method employed with Groups I and II. The results follow:

Decile	*Percentage of titles contributed*
1	42
2	21
3	14
4	9
5	6
6	4
7	2
8	1
9	1
10	0

The high agreement between these figures and those already presented will be noted. The percentages contributed by the first decile in the three groups are respectively 47, 37, and 42. The upper halves contributed 93, 90, and 92 per cent, respectively.

Dr. Clark has also collected data on persons receiving the PhD between 1935–1939 and 1940–1944. Analyses of the data for these groups are in substantial agreement with the results reviewed above.

GROUP IV

Group IV consisted of all persons who published original articles in the *American Journal of Psychology* between 1887 and 1900 and in the *Psychological Review* between 1894 and 1900. These were the only American journals devoted exclusively to psychology during this period, and they contain the major part of all American periodical literature in psychology published in the latter part of the nineteenth century. The articles in these journals were contributed by 229 persons. The percentage of the total publications contributed by each decile was as follows:

Decile	*Percentage of titles contributed*
1	37
2	21
3	11
4	9
5	5
6	3
7	3
8	3
9	3
10	3

The results are similar to those previously presented, except that there are no zero entries in the low deciles because only persons who had published at least one paper were included in this group. Each man in the last five deciles produced one title each.

It is interesting to note that in this group, as in Group I, nearly all persons in the first decile are known today although they published half a century ago. Some of these individuals, in alphabetical order, are: J. R. Angell, J. M. Baldwin, M. W. Calkins, J. McK. Cattell, H. H. Donaldson, G. Stanley Hall, J. H. Hyslop, J.

Jastrow, C. Ladd-Franklin, H. Münsterberg, E. C. Sanford, E. W. Scripture, H. M. Stanley, G. M. Stratton, E. L. Thorndike, and E. B. Titchener. In contrast, the 130 persons who contributed only one paper each are known to us in only a few instances.

FURTHER ANALYSIS OF GROUP I

The question which naturally comes to mind at this point is: are the works of the high producers of a quality equal to, higher than, or less than those of the less productive individuals? What is the relative importance to psychology of the men in various deciles of productivity? The relative familiarity of the names in the different deciles provides a partial answer to this question, but we have also attempted another measure. We believe that it may be assumed correctly that the most valuable contributions are those which survive in the scholarly literature of a field. That is, those publications which are described and discussed in advanced treatises and handbooks in psychology are believed to have contributed more than those which go unmentioned. We have examined several handbooks and advanced textbooks in psychology, and have determined for each of the 160 subjects of Group I the number of publications which appear in the bibliographies of these works.

The six volumes selected for the purposes of this study were Woodworth's *Experimental Psychology* (1938), Maier and Schneirla's *Principles of Animal Psychology* (1935), Munn's *Psychological Development* (1938), Greene's *Measurement of Human Behavior* (1941), Murphy, Murphy, and Newcomb's *Experimental Social Psychology* (1937), and Hilgard and Marquis' *Conditioning and Learning* (1940). These were chosen because they represent different areas of psychology and because they were published in the decade following 1928 or shortly thereafter. In examining the bibliography of each book, no items published after 1929 were counted in order that the references under study should belong primarily to the first 50 years of the life of each subject.

The total number of bibliographic items by our subjects cited in these books was 401. The proportions of these items which came from men belonging to the different deciles of Group I were as follows:

Decile	*Percentage of items contributed*
1	47
2	21
3	9
4	5
5	3
6	6
7	3
8	3
9	3
10	0

This table shows that the number of titles cited in scholarly works of psychology is almost directly proportional to the number of titles produced, as shown earlier. The top decile of this group produced 47 per cent of the total titles and also 47 per cent of the titles occurring in bibliographies. The agreement between the other categories of the two tables is good but not perfect. More extensive examination of data of these kinds is necessary before detailed quantitative statements can be made, but the data tend to show that the bulk of the cited references are the work of the more productive scientists.

Since recognition in science is based in part upon contributions made through publication, one would expect that those in the higher deciles of productivity would more often be recipients of various kinds of distinctions. We have tested this hypothesis for Group I with regard to three kinds of honors: being president of the APA, being starred in *American Men of Science*, and being included in the *History of Psychology in Autobiography*. The number of men in each decile receiving each of these honors is indicated below. The reader is reminded that there are 16 men in each decile.

	Number of persons		
Decile	*APA presidents*	*Starred scientists*	*Autobiographies in H.P.A.*
1	10	14	13
2	8	15	7
3	2	3	2
4	1	4	0
5	0	1	0
6	0	0	0
7	1	2	0
8	0	0	0
9	0	2	0
10	0	0	0

These figures scarcely require comment. Obviously, these honors go most frequently to the highly productive and seldom are given to those with a small number of publications. However, the relationship is not a perfect one. Some men in the top decile of productivity do not receive honors; some of those who are less productive, in a quantitative sense, do receive them.

SUMMARY AND DISCUSSION

We have examined the productivity of several groups of psychologists, representing periods of American psychology from 1887 to the present day. The total output of each of the groups is made up to a large extent of the publications of a relatively small number of productive individuals. The contributions of the most productive decile in the various groups range from 37 to 47 per cent of the total. In each population, the less productive half contributes 15 per cent or less of the total publications.

Further analysis of one group indicates that the more productive persons were also those whose work, on the whole, was most often cited in scholarly works in psychology. It was also shown that persons with high publication records are much more likely than persons with moderate or low records to be elected president of the APA, to be starred in *American Men of Science;* and to be included in the volumes of the *History of Psychology in Autobiography.* Thus, there is an association between quantity of output and professional recognition. Nevertheless, this correlation is not a perfect one, since some men in the high deciles of productivity received none of these honors and some whose quantity of publication was low did receive them.

Data from other fields of science, to be published elsewhere, show that the situation pictured above is a general one, not a peculiarity of psychology.

Toward Earlier Creativity in Psychology

S. L. Pressey

Clark states in *America's Psychologists* (1957, p. 109) that only some 4% of his significant contributors had thought of psychology as a career when in high school and only 23% when in the first two years of college. The first figure seems congruent with Visher's finding (1947, p. 527) that only 9% of the starred psychologists (those voted by a panel of their fellows as most eminent) in *American Men of Science* from 1903 through 1943 had before the age of 19 decided to specialize in their field; however, 59% of the starred chemists, 48% of the astronomers, and 31% of the zoologists had decided upon their specialization before that age—23% of the total group of starred cases had reached a decision to become a scientist before 15. Lehman (1953) reports median age of most notable contributions by 52 outstanding chemists as 34, and modal most productive 5 years 23–29; a few important chemical discoveries were by persons in their teens. However, 39 was the median age of 65 psychologists' most outstanding contributions, none of these appearing before the age of 25. Further, of chemists who made 21 or more contributions of some importance, average age of the first was 22 as contrasted with 36 for those making only one. In practically every major field, most notable work tended to be early. Conceivably the psychologists' late fruition had limited both the quantity and quality of their work. Anyhow, the situation seemed worth examining.

BEGINNINGS OF CAREER IN TWO FIELDS

For information regarding careers of notable psychologists, the four volumes of the *History of Psychology in Autobiography* (Murchison, 1932–52) seemed the outstanding source. In these volumes, only 28 Americans gave sufficient information about themselves

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to serve in the present study. Only one of these seemed to have made even a tentative decision to specialize in psychology before the junior year of college. Six did not so decide until after college graduation. So Thurstone entered psychology via engineering. Perhaps for him it was a desirable mode of development.

But for the most part the elementary cause of the late start seemed to be the fact that psychology was not usually offered before the junior year of college and was to some an unknown topic; thus, E. L. Thorndike declared that he had no "memory of having heard or seen the word psychology until my junior year at Wesleyan University when I took a required course in it." A few had earlier interests, but no courses were available. So Hunter at the age of 15 had purchased and read Darwin's *Origin of Species* and when 17 became interested in psychology after a preparatory school roommate lent him a text in the field. Pillsbury wrote that when 14 and in the second year of high school he had chanced upon a copy of Carpenter's *Mental Physiology* in his father's library, read it with great interest, and declared that he would like to specialize in psychology. However, not until upper class work with Wolfe at Nebraska did that interest really get underway. And Terman described observations he had made while still a boy regarding memory and afterimages.

The gist of it seemed to be that these psychologists did not start earlier because they did not know about the subject or could not because courses were not earlier offered. Probably some good people were thus missed; Harold Urey said in a recent telecast that he went to college planning to be a psychologist but changed to chemistry because no courses were available to him in the first subject before his junior year. The fewer reports, adequate for this purpose, of the European psychologists seemed largely to the same effect.

For a comparison, biographies of 20 leading chemists (as listed in a recent study by Lehman) were gone over. Before the age of 12, 7 of the 20 had already shown interest in science; only one had not shown such interest by the age of 18. By the age of 17, scientific interests of 6 had already been strongly stimulated by a teacher and 12 by a parent; 11 had done some work for a parent or teacher related to science—as helping a father in his pharmacy or a teacher to look after a school laboratory. By the age of 20, 4 had already published something. Clearly, both the generally recognized status of chemistry as a science, and both school and home influences in youth and even childhood, brought earlier interest in and preparation for a career in chemistry. That such early interest is now appearing in many sciences is illustrated by the winners in the last Science Talent Search. First was a 17-year-old boy who had in secondary school built a particle accelerator and decided on physics as his field. Second was a lad of the same age who had already done a bit of original research in zoology and as a child of five was reading in that field (*Science Newsletter*, March 14, 1959). And the question is as to suggestions obtainable, from past records and distinctive current school and college programs, as to desirable facilitations of psychological careers.

POSSIBLE FACILITATIONS OF CAREER

Jastrow, Judd, Hunter, and E. L. Thorndike obtained the doctorate when 23 and Washburne when 22; median doctorate age of the 28 American psychologists in the autobiographies was 26.8 which was early even for their times—the first 100 psychologists in the 1921 *American Men of Science* obtaining the degree between 1909 and 1920 did so at a median age of 29.5. And the first 150 pages of the 1958 APA *Directory* showed the 1,223 there listed as receiving the PhD between 1950–57 doing so still later, at a median age of 31.2 with 24% at 35 or over. However, similar samplings in *American Men of Science* for 1921 and 1955 showed median age of the doctorate in chemistry in 1909–20 to be 28.0; and in 1950–54, 28.7. As compared with chemists, psychologists thus tend to obtain the doctorate older—and increasingly so.

However, in spite of late choice of field, notable early American psychologists tended to get into career young: four obtained the undergraduate degree at 19, and the doctorate often took only three years, Judd taking only two. All this is very congruent with the finding of Lewis and Pressey (1958) that the 20 most prominent of 115 men who prior to 1947 had taken the doctorate in psychology at Ohio State University had obtained their undergraduate degree a year and a half younger, and the PhD three and a half years younger, than the 20 least prominent. And in correspondence Clark states that he found analogous differences between his "significant contributors" and psychologists in general: his monograph reports a median of four years from undergraduate degree to PhD for the first group as compared to seven for the second. It seems here also relevant that the last 20 presidents of the APA obtained their doctorates at a median age of 25.7—one at 22, one at 23, five at 24.

These various data surely suggest the desirability of completion of graduate training earlier than the thirties. Here apropos is a statement (Hobbs, 1957) of a recent committee of deans of graduate schools that now "generally the PhD takes at least four years to get, more often six or seven, and not infrequently ten to fifteen" and their recommendation that "except in most unusual cases, the whole program should not take more than *three* years of residence" [italics theirs]. However, simply to cut back to size a graduate program imported whole from Germany three-quarters of a century ago and not modified since—except for overextension—seems now not enough. As Oppenheimer has remarked (1958): "nearly everything that is now known was not in any book when most of us went to school; we cannot know it unless we have picked it up since." Margaret Mead (1958) has put it more strongly: "for those who work on the growing edge of science . . . only a few months may elapse before something which was easily taken for granted must be unlearned or transformed to fit the new state of knowledge." And she stresses the futility of trying to give initial professional training adequate into the indefinite future; rather, the great need is for methods of "lateral transmission" of knowledge as now commonly in business, where "refresher" programs at intervals bring a person once more somewhat up-to-date. But if postdoctoral and later short special programs become usual, even more the initial long stretch of full-time education can be reduced and career begun earlier.

A sampling of catalogs shows psychology now available earlier in curricula than 40 years ago, though not till the sophomore year in two famous universities and about half the arts colleges. But the most important career influence, stressed by 19 of the Americans and mentioned in every adequate autobiography, might be harder to get early now because of the great numbers in beginning courses and absorption of senior staff members in graduate work and research: frequent personal contact with an outstanding psychologist. And so might early contact with other able students of similar interests and enthusiasms once career choice has been made, an influence also mentioned in many autobiographies.

A program, to which an associate dean devoted all his administrative time, for superior students at the University of Kansas here seems relevant (Waggoner, 1957). Selected in high school as outstanding and most given scholarships, from entrance these students had as advisors major faculty members. Even in the freshman year they were in seminars of eight to ten of their group, taught by senior faculty, and were allowed to take some junior-senior courses (prerequisites not infrequently being bypassed). "Several of the students as freshmen and sophomores received small research assistantships in various departments," and a few teaching assistantships in the sophomore year; "the effects of these experiences were very noticeable, especially in directing the thought of the students toward research and university teaching." In this connection it may well be noted that 6 of the 28 American psychologists in the autobiographies mentioned doing some research while undergraduates. Encouraged to take placement tests and obtain credit by examination and to carry academic loads heavier than average, some of the Kansas students obtained an undergraduate degree in three years and at once went into graduate school, at least two into psychology. A follow-up 20 years after (Pressy, 1955) of a somewhat similar but less adequate program indicated, as only one outcome, a doubling of the number of students going on to obtain the doctorate, some of these being now outstanding in their fields.

All over the country efforts are being made to upgrade high school work, especially in science. Laboratory work is being stressed, also

> "opportunities for individual work . . . identification of the program with industry and with institutions of higher learning . . . course offerings or research opportunities for the talented during summer vacation months" (Conant, 1958, p. 111).

Advanced work anticipating college courses and permitting college credit therefore is being stressed by the College Entrance Examination Board. Notable programs are appearing (Brandwein, 1955) involving continuing selection, special courses, laboratories open even in leisure hours for companionship there in scientific interests with other students and a teacher specially chosen for his enthusiastic capability. Junior academies, science fairs, and such contests as the Westinghouse Talent Search are giving prestige in the community and the student group to superior accomplishment in secondary school science. Even more than shown in the biographies of chemists of a generation or more ago, it might be expected that all these programs would nurture early choice of and progress toward scientific career.

Might psychology well have some place in all this? As already mentioned, Pillsbury at 14 and Hunter around 15 were, on their own, reading in psychology; at this same age Cyril Burt was reading Ward's article on psychology in the *Encyclopaedia Britannica* and starting manuscript notes "which proposed to cover the whole range of human character-qualities." Indeed, Piaget (Murchison, 1932–52, Vol. 4, p. 238) published his first scientific article (in ornithology) at the age of 10. Some psychology is now being offered in some high schools (Engle & Bunch, 1956). For able secondary school students, offerings in psychology as substantial as now in chemistry would seem possible. Might occasional precocity, in psychology, have unique characteristics and contribute uniquely to the science? If psychologically sophisticated about his precocity, such a youth should both weather the hazards thereof especially well and have a very special vantage point for consideration of many psychological phenomena. Suppose (for instance) another Piaget should at the age of 10 read Piaget; might the boy contribute distinctive insights regarding the development of children's concepts?

In summary: data suggest that the present 31 is too old a median age for obtaining the doctorate in psychology, that earlier would increase and perhaps improve professional productivity.

Graduate school leaders are advocating that doctoral programs be shortened. Their remaking is urged by some distinguished scientists.

New programs for superior undergraduates involving first selection before entrance, throughout guidance and instruction by outstanding faculty members, skipping of repetitious freshman year work with admission even then to advanced courses, and research assistantships even in the second year give special promise of early finding and furthering creativity.

In the present great surge of improvement of secondary school science, psychology should be included, and in as substantial a form as the present best offerings in high school chemistry and biology.

Now in this country the development of excellence in athletics is a continuing process of selection and guidance and training, from the secondary school on. Something of this sort seems to be happening in Russian education, in science especially. The opinion is ventured that it should happen here—also that, in psychology, precocity might well occur and be uniquely valuable.

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The Psychologist's Most Creative Years

Harvey C. Lehman

It seems obvious that man's most creative years cannot be very adequately investigated under the restricted and relatively short-lived conditions of the laboratory. Just as in physics no single observation can yield a generalization applicable to all subsequent observations, similarly, in the heterogeneous field of creativity, and more specifically within the field of psychology, the chronological ages at which one individual displays his most important creativity provides little information regarding the ages at which others will do so.

My book, *Age and Achievement* (Lehman, 1953), contains information obtained from just one book that has to do with notable contributions to psychology, i.e., information from a book by J. C. Flügel (1933) who published a chronological table listing what Flügel regarded as "some major events in the history of psychology." By use of Flügel's chronology I found the names of 50 contributors and the dates when, according to Flügel, they made 85 important contributions. The maximum production rate for this group occurred at ages 30 to 39 inclusive.

Since, for all I know, Flügel's chronology was the product of only one man's evaluations, I decided to burrow further into the problem. The age curves that follow are based upon both a more recent publication by Flügel (1951) and also upon the thinking of a number of other historians of psychology.

Based upon information obtained from the most recent edition of Flügel's *A Hundred Years of Psychology* (Flügel, 1951), the solid line of Figure 1 presents by 5-year intervals the chronological ages at which, in Flügel's judgment, 117 important contributions to modern psychology were made by 79 individuals.

Notice that the solid line of Figure 1 shows again a peak production rate at ages 35–39 inclusive. In studying Figure 1 it should be borne in mind that it sets forth the *average* number of contributions to psychology per 5-year interval. Adequate allowance is thus made for the disproportionately large number of youthful workers that are always trying to see what they can do. A detailed explanation of the statistical procedure employed for obtaining maximum production rates has already been published elsewhere (Lehman, 1953).

In contrast to the solid line of Figure 1 the broken line of this figure sets forth the average number of contributions to psychology made by contributors who were still among the living in the year 1947. Although the second edition of Flügel's history was published in 1951, his revised chronology includes no contribution made subsequent to 1947. Therefore, in order to obtain sound arithmetical averages, in constructing the broken line of Figure 1, it was assumed that each of the then-living contributors had died in 1947. That is to say, regardless of when it was published, insofar as the present study is concerned, any book that includes no contribution made subsequent to, let us say, year X, has a terminal year of X.

Employment of the year of publication of Flügel's history (1951), rather than the terminal year (1947), for making the computations upon which the broken line of Figure 1 is

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based would have been unsound for my present purpose because such a procedure would have counted as unproductive several years during which production may have occurred but which could not have received due recognition in a chronology published in 1951.

Note in Figure 1 that: (*a*) Both curves attain their high points at the same age level, namely, at 35 to 39 inclusive; and (*b*) if some allowance is made for the relatively small numbers of cases spread over so many class intervals, the two curves are similar otherwise except that the curve for the still-living contributors (the broken line) terminates 5 years earlier than does its mate, i.e., at ages 70 to 74 instead of ages 75 to 79.

For constructing the two curves of Figure 1 the peak of each statistical distribution was arbitrarily assigned a value of 100% and the other averages within the same statistical distribution were then assigned proportionate percentage values. Since both modal values of Figure 1 were arbitrarily allotted the same value, it should be clear that the equal heights of the curves of this figure result from the way they were drawn, and this should not be taken to mean that the two production rates were the same numerically.

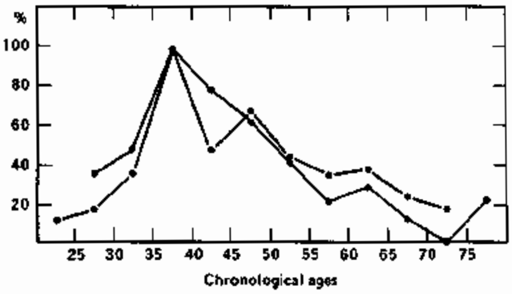

Fig. 1. Age versus contributions to psychology as listed by J. C. Flügel (1951). (Solid line: 117 superior contributions by 79 contributors now deceased and born subsequent to 1774—an average of 1.48 contributions each. Broken line: 62 superior contributions by 55 still-living contributors—an average of 1.13 contributions each.)

For constructing Figure 2 data were obtained from Hulin's *A Short History of Psychology* (Hulin, 1934). To permit concise expression and to avoid monotonous repetition, the expressions "still living" and "present generation" will be employed interchangeably hereinafter. Both of those terms refer to contributors who were still living at the time when the source book which cites their contributions was published, even though some of the contributors may have died since then. The word "deceased" will be employed similarly as a synonym for the expression "past generation." Notice that Figure 2 is like Figure 1 in that all four curves reach their maximum heights at ages 35–39 inclusive.

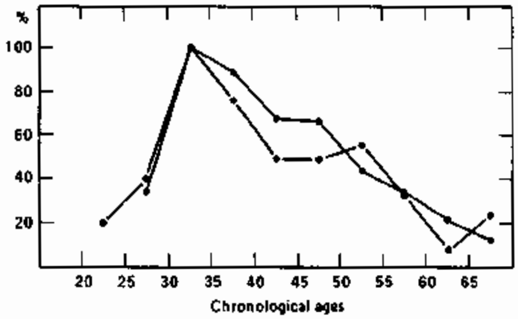

Fig. 2. Age versus contributions to psychology as listed by W. S. Hulin (1934). (Solid line: 112 superior contributions by 90 contributors now deceased and born subsequent to 1774—an average of 1.24 contributions each. Broken line: 98 superior contributions by 76 still-living contributors—an average of 1.29 contributions each.)

Because many histories of science cite the output of relatively few still-living contributors, the total number of contributions, and hence the average number per individual worker, tend to be larger for the past-generation than for the present-generation contributors (see Table 2). It might be supposed that, because of this, the production rates of the present-generation contributors are not commensurate. When, however, as in the present study, the output of reasonably large numbers of still-living contributors are studied, the production rates of the still living are comparable in size to those of past-generation workers (see Table 1).

It is true, of course, that all or almost all of the individuals whose production rates are shown in Figure 1 are still living and producing at the time of the present writing and, therefore, that Figure 1 does not take into account their entire lifework. It is also true, however, that in the years directly ahead, other contributors who were too young to have started making their contributions prior to 1947 are now making their contributions. Hence, it is not necessarily true that the *relative pro-*

Table 1. Average Number of Contributions per Five-Year Interval

Data used in	*Age interval (years)*														
	15–19	*20–24*	*25–29*	*30–34*	*35–39*	*40–44*	*45–49*	*50–54*	*55–59*	*60–64*	*65–69*	*70–74*	*75–79*	*80–84*	*85–89*
Fig. 1 (solid line)	—	—	.025	.034	*.071*	.055	.045	.030	.015	.021	.009	—	.015		
Fig. 1 (broken line)	—	.007	.011	.022	*.061*	.029	.041	.027	.020	.023	.015	.011	—		
Fig. 2 (solid line)	—	—	.020	*.056*	.050	.038	.037	.024	.019	.012	.008				
Fig. 2 (broken line)	—	.013	.026	*.067*	.051	.033	.033	.038	.022	.006	.017				
Fig. 3 (solid line)	.001	.022	.073	.085	*.111*	.088	.074	.062	.049	.039	.021	.019	.023	—	.020
Fig. 3 (broken line)	—	.016	.060	*.089*	.069	.043	.047	.016	.025	.015	.006	—	.012		
Fig. 4 (solid line)	—	.002	.032	.046	*.066*	.064	.057	.030	.035	.030	.014	.004	.012	.007	
Fig. 4 (broken line)	—	.008	.049	.063	*.073*	.059	.056	.034	.018	.006	.004				
Fig. 5 (solid line)	—	.012	.071	*.084*	.078	.068	.046	.035	.027	.016	.016	.004	.010		
Fig. 5 (broken line)	—	.016	*.075*	.067	.069	.056	.036	.017	.012	.004	.005				
Fig. 6 (solid line)	—	.015	.075	*.100*	.065	.059	.047	.036	.031	.022	.012	.011	.005	.015	
Fig. 6 (broken line)[a]	—	20%	73%	98%	*100%*	53%	58%	48%	32%	9%	8%				
Fig. 7 (solid line)	—	.027	*.093*	.077	.076	.058	.052	.017	.015	.005	.026				
Fig. 7 (broken line)[b]															
Fig. 8 (solid line)[a]	—	3%	48%	77%	*100%*	79%	66%	44%	34%	30%	19%	5%	19%		
Fig. 8 (broken line)[b]															
Fig. 8 (dotted line)[a]	—	17%	70%	92%	*100%*	68%	59%	46%	18%	14%	15%				

Note—The peak of each statistical distribution is in italics.

[a] It is explained in the text why the values in these rows are expressed not as decimals but as percentages, each of the maximum values being arbitrarily assigned values of 100%.

[b] Same as broken line of Fig. 6.

Table 2. Summary of Findings with Reference to Contributions to Psychology

Data used in	*No. of works*	*No. of men*	*Average per man*	*Median age*	*Mean age*	*SD of dist.*	*Years of maximum productivity*
Fig. 1 (solid line)	117	79	1.48	42.07	47.63	10.80	35–39
Fig. 1 (broken line)	62	55	1.13	41.63	44.15	11.23	35–39
Fig. 2 (solid line)	112	90	1.24	39.86	34.46	9.86	30–34
Fig. 2 (broken line)	98	76	1.29	37.08	39.41	10.24	30–34
Fig. 3 (solid line)	563	187	3.01	40.45	43.02	12.40	35–39
Fig. 3 (broken line)	120	68	1.76	36.00	45.63	10.51	30–34
Fig. 4 (solid line)	257	145	1.77	42.71	44.07	11.46	35–39
Fig. 4 (broken line)	295	185	1.59	37.81	39.03	8.44	35–39
Fig. 5 (solid line)	442	204	2.17	43.55	40.15	11.19	30–34
Fig. 5 (broken line)	248	159	1.56	34.96	35.92	8.40	25–29
Fig. 6 (solid line)	1,530	1,002	1.53	33.89	36.12	9.33	30–34
Fig. 6 (broken line)[a]							
Fig. 7 (solid line)	239	138	1.73	34.61	35.14	9.06	25–29
Fig. 7 (broken line)[b]							
Fig. 8 (solid line)[a]							
Fig. 8 (broken line)[a]							
Fig. 8 (dotted line)[a]							

[a] Percentage values only.

[b] Same as broken line of Fig. 5.

duction rates of the still-living successive age groups as revealed by the broken line of Figure 1 will change very greatly.

Consider the following illustration. A still-living Contributor X of, let us say, age 20 can add to his total output as long as he lives and he can thus increase both the total output and also the average output of any group to which he may later be assigned. But Contributor X can increase the production rate of age group 20–24 during the 5-year interval when X is of ages 20–24 and then only. Any contribution made thereafter by X can no longer be credited to age group 20–24 but must be allotted to the specific (and older) group to which he belongs at time of making his subsequent contribution(s). That is to say, when he has passed age 24, the present-generation contributor has already ended his work in so far as the production rate of age group 20–24 is concerned. Therefore, the trustworthiness of a production rate computed for any still-living group of ages 20–24 depends not on whether or not the contributors thereto have ended their entire lifework but only on whether a sufficient number of them have passed age 24 to yield a dependable average production rate. This statement also applies to each of the other age groups.

Data for Figure 3 are from E. G. Boring's (1950) *A History of Experimental Psychology.* Since, as has been shown elsewhere (Lehman, 1962), there is good reason for suspecting that age data for contributors born prior to about the year 1775 A.D. are not too trustworthy, in Figure 3 and in each of the other graphs presented herein, the curves for past-generation contributors include data only for individuals born subsequent to 1774. In constructing the two curves of Figure 3, the dates of achievement that were given in the notes at the end of each chapter of Boring's history were ignored because some of them merely repeat informa-

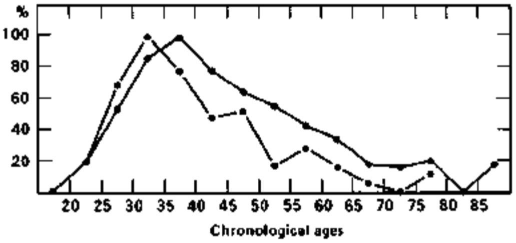

Fig. 3. Age versus contributions to psychology as listed by E. G. Boring (1950). (Solid line: 563 superior contributions by 187 contributors now deceased and born subsequent to 1774—an average of 3.01 contributions each. Broken line: 120 superior contributions by 68 still-living contributors—an average of 1.76 contributions each.)

tion found in the body of the text and others are references to secondary sources and to criticisms rather than references to primary sources.

Figure 3 differs from Figures 1 and 2 in that it covers a wider age range and the maximum production rate for the present-generation contributors occurs during the early thirties instead of during the late thirties. The late termination of the solid line of this figure may result in part from the fact that Boring pays much attention in his history to *personalities* as well as to their contributions. Thus, the rise of the solid line of Figure 3 at ages 85–89 is due solely to Boring's mention of the fact that Wundt wrote his life's psychological reminiscences at age 88.

Factual matter for drawing Figure 4 was found in Gardner Murphy's (1949) *Historical Introduction to Modern Psychology.* Although here too the highest production rate for both groups of contributors occurs at ages 35–39, that for the present-generation contributors covers a narrower age range, and the curve for the past generation workers is almost as high at ages 40–44 as it is at ages 35–39.

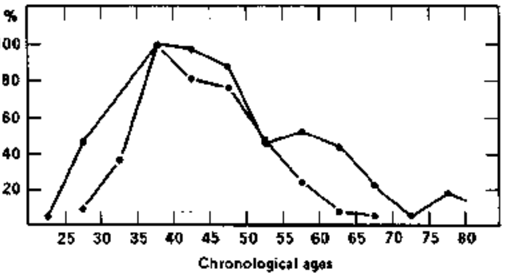

Fig. 4. Age versus contributions to psychology as listed by G. Murphy (1949). (Solid line: 257 superior contributions by 145 contributors now deceased and born subsequent to 1774—an average of 1.77 contributions each. Broken line: 295 superior contributions by 185 still-living contributors—an average of 1.59 contributions each.)

Figure 5 is based upon E. G. Boring (1942) *Sensation and Perception in the History of Experimental Psychology.* Because the evaluation of contributions to psychology differs somewhat with the individual who makes the appraisals, in the present study I have assembled the assessments of different scholarly historians whose books do not too greatly duplicate one another. But since one of Boring's histories (Boring, 1950) is an introduction to the whole field of experimental psychology, and the other (Boring, 1942) deals in detail with two topics only within that entire field, it seemed to me justifiable to employ both of these histories despite the fact that they were written by the same author. The legend of Figure 5 should enable readers to interpret it without further help from me.

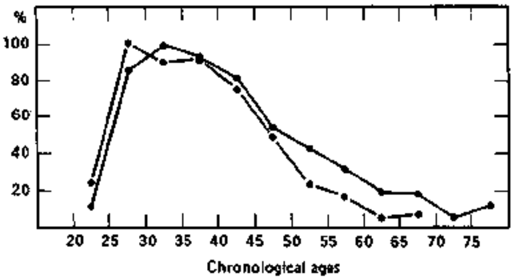

Fig. 5. Age versus contributions to psychology as listed by E. G. Boring (1942). (Solid line: 442 superior contributions by 204 contributors now deceased and born subsequent to 1774—an average of 2.17 contributions each. Broken line: 248 superior contributions by 159 still-living contributors—an average of 1.56 contributions each.)

If we may assume that the history of modern experimental psychology begins with Fechner's *Elemente* of 1860 and Wundt's *Beiträge* which appeared in 1862, the Woodworth and Schlosberg (1954) textbook, *Experimental Psychology* is not only a college text but also a history of *modern* experimental psychology. Based upon information obtained therefrom, the solid line of Figure 6 sets forth the ages at which 1,530 significant contributions to experimental psychology were published by 1,002 still-living contributors, and the broken line of this figure presents median percentage values for the five still-living groups of contributors as found in the five histories from which information was obtained for drawing the broken lines disclosed in Figures 1–5 inclusive.

To derive the median values used for preparing the broken line of Figure 6, the production rates for each of the present-generation groups of Figures 1–5 were first reduced to percentage values. If this had not been done, the evaluations to be found in one or two of the five histories might, because of their large size, have had an undue influence in determining the shape of the broken line. That outcome was one I wanted to avoid, and I avoided it by convert-

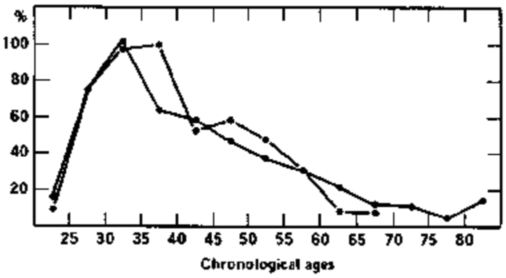

Fig. 6. Age versus contributions to psychology. (Solid line: 1,530 superior contributions to experimental psychology by 1,002 still-living contributors as listed by Woodworth and Schlosberg, 1954—an average of 1.53 contributions each. Broken line: median values—production rates—for the still-living groups as set forth in Figures 1–5 inclusive.)

ing the decimal fractions to percentage values prior to deriving the median values.

In view of the fact that the solid lines of Figures 1–5 inclusive are based upon information obtained from *histories,* and that the solid line of Figure 6 is based upon information obtained from a college *textbook,* the similarity of the two curves of Figure 6 is striking.

In the preface of one of his histories (Boring, 1942), the author makes the following statement:

> in this book, it has been my intention to slow down at 1920 and to stop at about 1930 except when the momentum of discovery is irresistible—as it was in the psychophysiology of hearing, when seventy-year-old problems began to come up for solution in the 1930's. Nevertheless, the reader should not trust me after 1930, since I do not trust myself. No man can see clearly so near his face, and writing history is a matter of selection [p. x].

Boring can say things with a nice flair. However, his foregoing observation raises the following question. Does the inability of the historian to place a firm evaluation upon each and every one of the most recent contributions to his field that he includes in his history invalidate the age curves for still-living contributors to psychology as set forth in Figures 1–5 of the present study? Research that has answered this query will now be described.

The broken line of Figure 7 is identical with the broken line of Figure 5. It reveals the ages at which 248 contributions to psychology were made by 159 contributors who were still among the living in the year 1941—the terminal date for Boring's (1942) *Sensation and Perception in the History of Experimental Psychology.* It will be recalled that, in order to obtain sound arithmetical averages for drawing the broken line of Figure 5, it was assumed that each of the 159 then-living contributors to psychology had died in 1941, the terminal date for Boring's book published in 1942.

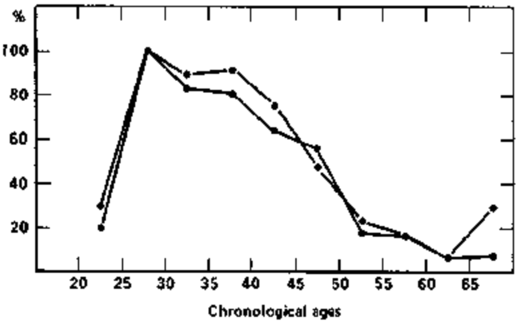

Fig. 7. The influence of "the newness" of contributions upon the shape of the age curve for still-living contributors as shown in Figure 5. (Solid line: 239 superior contributions by 138 still-living contrbutors who died—statistically—in 1921. Broken line: same as the broken line of Figure 5. 248 superior contributions by 159 still-living contributors who died—statistically—in 1941.)

The solid line of Figure 7 was drawn in the same way as the broken line, except that, in drawing the solid line of Figure 7, it was assumed that each of the then-living contributors had died 20 years earlier, i.e., they died (statistically) in the year 1921 instead of in the year 1941. This means, that in making the computations to be used later for constructing the solid line of Figure 7, everything published subsequent to 1921 by the contributors who died statistically in 1921 was ignored. It means also that no unproductive years were allotted to those same contributors subsequent to 1921. The solid line of Figure 7 thus reveals the shape that its mate presumably would have had *if,* in writing his history in 1942, Boring had included therein no contribution made subsequent to the year 1921.

Thus, despite Boring's greater confidence in his ability to identify the really important contributions made 20 or more years prior to publication of his history, the two curves of Figure 7 are much the same. They thus suggest that the historian's inability "to see so near his face" is relatively unimportant insofar as my age curves for still-living contributors is concerned. But, since Figure 7 is based upon information obtained from one book only, some additional evidence is called for.

Figure 8 supplies this additional evidence. In Figure 8 the solid line shows median percentage values for the five past-generation groups of contributors to psychology pictured in Figures 1–5 inclusive, and the broken line gives similar information regarding the five still-living groups of contributors shown in Figures 1–5 inclusive. The dotted line of this figure reveals the median percentage values of *five* different production rates each of which was derived in the same way as were the values employed for constructing the broken line of Figure 5.

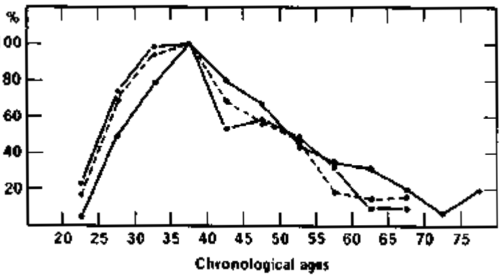

Fig. 8. Age versus contributions to psychology: summary of findings. (Solid line: median production rates for five groups of past-generation groups of contributors as set forth in Figures 1–5 inclusive. Broken line: median production rates for five groups of present-generation groups of contributors as set forth in Figures 1–5 inclusive. Dotted line: median production rates for five still-living groups of contributors that died—statistically—20 years prior to the terminal date of the book(s) which cited their several contributions to psychology. See text.)

That is to say, the dotted line of Figure 8 was obtained in the same way as was the solid line of Figure 7 except that, instead of presenting evidence procured from just one history, it sets forth the median percentage values obtained from five different histories and, instead of assuming that the present-generation contributors had died during the terminal year of each of the respective histories, it was assumed that they had died 20 years earlier. If the reader finds this explanation unclear, he should read once again the explanation of how the solid line of Figure 7 was made.

Granted that, in the days to come, some of the work of present-generation contributors (now cited and discussed in histories of psychology) may be overvalued, and granted also that the work of others whose findings are now ignored by historians will make their future entry into histories of psychology, it nevertheless seems probable that these conjectural changes will occur on a random basis *insofar as the ages of the contributors at time of making their contributions is concerned.* Since it is individual contributions and not the collective work of an entire age group that chroniclers choose for inclusion (or exclusion) in their histories of psychology, it seems probable that at least a rough proportionality will be maintained between the way in which our present-day historians evaluate the production of the successive age groups and the way in which future historians will do so.

Note that all three curves of Figure 8 attain their high points at ages 35–39 inclusive, and that the two curves which picture the production rates for the still-living contributors rise somewhat earlier and also terminate earlier than does the curve for the past-generation workers. This finding and the further fact that the production rates for the present-generation contributors are almost as large at ages 30–34 as they are at ages 35–39 may mean only that the recently born contributors have been more subject to "publish or perish" than were their predecessors.

It was said earlier that the Flügel chronology (see Figure 1) may have been the handiwork of a single individual. But it is only a remote possibility. It is almost inevitable that the competent historian's judgment is influenced by the opinions of others. For example, in a personal communication to the present writer an eminent historian who here will be nameless wrote as follows:

> I do not believe for a moment that historians sit down and read original papers and perceive their significance. I think they are led to the significance of papers by tracing back from later times to the earlier what people quoted, or what got people going, or what the later man thought was important. You couldn't possibly see any significance in Newton's analysis of the spectrum in 1672 just from reading it, nor from the unfavorable comment that was made about it. But when you know something about the development of that thing through the eighteenth century

> and then the nineteenth century—to know about the early nineteenth is enough—then you see its significance and out it stands, and that gets it established as a crucial turning-point in history.

The following points are also worthy of mention. In reply to a letter of inquiry a second historian remarked that, in preparing his chronology, he first consulted with more than 50 individuals whom he regarded as the most competent to advise him with reference to the contributions that should be cited in his forthcoming history. Another said that, prior to writing his history, he first wrote to a number of eminent workers in his field and made a statistical list of their recommendations for inclusion in his forthcoming book. Still a fourth stated that, when he prepared his history, he first reread the familiar histories in his field and also read some unfamiliar ones. In light of all this testimony, it is hardly conceivable that any standard history of science is solely the product of only one man's mind.

A few years ago the following advice was proffered to all young persons who wish to be creative workers (Woodworth & Marquis, 1947):

> the young should not delay putting forth their best efforts until they become middle-aged. If they have anything original in them it should begin to come out while they still look on the world with the clear eyes of youth, and while they still burn with the fire of youth [p. 302].

The foregoing advice should be of even greater interest to psychologists today than when first uttered because, if the findings here set forth are as trustworthy as I believe them to be, it no longer is necessary merely to *assume* that the relationship between chronological age and creativity is the same today as heretofore. On the contrary, if differences both in time lag and also in quality of output be taken into consideration, it may well be that there has been little or no noticeable change therein.

As I review what I have written here the following items seem to me to merit special emphasis.

1. This study concerns itself not with contributions to psychology that were merely "publishable," but rather with output that was of far more than average merit.
2. No attempt was made to determine the overall importance or the total value of what the successive age groups accomplished, but only with such of their work as subsequently found its way into our standard histories of psychology.
3. Whether or not the age decrements here described will persist in the years to come is something to which I do not pretend to have a trustworthy answer.
4. Since group means tell nothing at all about individual performance, since each of a large number of some one individual's contributions may possess far more merit than the masterworks of certain other individuals, since there are numerous heart-warming exceptions to the general trend, and since, moreover, life should consist of far more than the production of scientific contributions, the present study provides no good reason why anyone should feel at any age level that his usefulness is at an end.

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Age and Productivity among Scientists

Wayne Dennis

This paper is concerned with the output of scientific papers among a group of scientists all of whom reached the age 70 and many of whom lived to age 80 or beyond. Among the topics examined are the following: What is the relative productivity of a scientist at various decades of life? What percentage of his total bibliography is produced by age 30, age 40, and so forth? How many contributions are made during the additional decade that is allotted to the octogenarians?

Because my bibliographic source provided data only for the 19th century, it was necessary to choose for study scientists whose adulthood fell entirely between 1800 and 1900. In order to obtain subjects, I selected from the biographical directory of *Webster's New International Encyclopedia*, 1930 edition, each scientist listed therein who lived to age 70 or beyond and whose years of life from age 20 onward fell between 1800 and 1900. This procedure yielded the names of 156 scientists. Of these, 100 lived to ages 70 to 79, inclusive, while 56 lived to ages 80 to 89. The few who survived to age 90 or beyond are not treated in this report.

The 156 subjects belonged to a variety of scientific specialties. There were 17 astronomers, 24 chemists, 19 geologists, 17 mathematicians, 34 naturalists, 15 physiologists, and 20 physicists, while 10 fell into other categories or were difficult to classify. In general they were eminent men in their respective fields, and many are universally famous.

For each subject, a count was made of the number of his scientific publications per decade of life as listed in the *Catalog of Scientific Literature, 1800–1900*, prepared by the Royal Society of London. This catalog lists only papers published in scientific journals and in the proceedings of scientific societies. It does not list other publications, such as books, letters to editors, memorial addresses, obituaries, popular writings, and so on. Thus we are not dealing with complete bibliographies but only with scientific periodical literature. It is believed, however, that the major part of the bibliography of science consists of this kind of publication.

For convenience, the group living to ages 70 to 79 is called the 70-year group, and the remainder is called the 80-year group.

Table 1 shows the mean number of papers published per man, per decade, for each group and for the combined groups. This table indicates that productivity between ages 20 and 29 is quite low. The low productivity of this decade is the result, in large part, of the very low productivity between ages 20 and 24. Of the 156 subjects, 96 did not begin to publish until age 25 or later. However, even the second half of the decade of the 20's does not equal the record of later productivity.

In the 30's a high average rate of productivity is reached, and this rate is maintained for three decades. On the whole, there is little change in mean output of scientific articles between age 30 and age 59. The mean output of my subjects during this period approximates two publications per year. The rate of publication for the combined groups decreases about 20 percent in the 60's, and the 80-year group shows a still further decline in the 70's, although an appreciable amount of productivity is maintained. It will be noted that the number of publications appearing in the 70's is considerably higher than the number in the 20's.

Although the statements just made indi-

Table 1. Mean Number of Papers Per Person Per Decade

Decade	*20's*	*30's*	*40's*	*50's*	*60's*	*70's*
70-year group	9.1	20.1	21.8	23.8	18.1	
80-year group	6.9	21.9	24.7	18.5	17.0	13.1
Combined groups	8.1	20.7	22.9	21.9	17.7	

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cate the general trends, there are, of course, individual exceptions to these. The range of productivity, within each decade, and over the total life span, is great, even for this group of highly eminent men. The distributions are skewed to the left, resembling the upper end of a normal distribution curve. Because of the nature of the distributions, the usual measures of variability are not appropriate and, hence, are not presented.

Next, my data are treated so as to show the proportion of the eventual bibliography that is produced by the close of each decade. In the figures that follow, it should be noted that the bibliographies of the 70-year group were closed at age 70 and those of the 80-year group at age 80, as in Table 1. However, the percentages obtained would be only slightly altered if we based them on bibliographies at death rather than at ages 70 and 80. The results obtained are shown in Table 2. It will be observed that a very small part of the lifework of these men was completed by age 30. About one-third of their publications had appeared by age 40. It is notable that nearly one-half of their output appeared after age 50.

Table 2. Percentage of Total Output Completed by Various Ages

Age	*30*	*40*	*50*	*60*	*70*	*80*
70-year group	10	32	56	80	100	
80-year group	7	28	52	70	87	100

Between ages 20 and 70 members of the 70-year group produced a mean of 92.8 papers each. Between ages 20 and 80 the 80-year group published a mean of 102.2 papers per person. This comparison suggests that, for a man of the calibre with which we are dealing, an additional decade of life beyond age 70 results in the production of approximately 10 scientific papers.

Another way to examine this problem is to compare the record of the 80-year group at age 70 with its own record at age 80. Between age 70 and age 80, the mean bibliography of the 80-year group increased from 89.1 to 102.2, a gain of 13.1 papers.

In an earlier paper (*1*), correlations between the degrees of productivity in different decades were computed for two groups of scientists living to age 70. The same methods have been applied to the 56 octogenarians of the present group with the results shown in Table 3.

Table 3. Correlations between Degrees of Productivity of 56 Octogenarians

Age in decades	*Age in decades*				
	30's	*40's*	*50's*	*60's*	*70's*
20's	.57	.46	.46	.35	.33
30's		.49	.50	.47	.55
40's			.80	.75	.62
50's				.65	.61
60's					.84

The figures are in substantial agreement with those published earlier. The number of scientific publications in the 70's is closely related to productivity in the 60's. The data show also appreciable relationships between the 70's and decades prior to the 60's, the correlations becoming smaller as the decades are farther removed from the final decade.

Readers who are familiar with the various works of Lehman, recently assembled in a single volume (*2*), will note that my findings are at variance with the general tenor of Lehman's results. However, my findings do not necessarily contradict Lehman's. I have dealt with the total output of scientific articles, whereas most of Lehman's attention has been given to "significant" works. In the instances in which Lehman has analyzed the total output of scientists, each of whom lived to a specified age, he, too, finds that productivity persists in the later decades of life.

Finally, it should be noted that the data here presented on eminent 19th-century scientists are in agreement with those that I have previously presented for members of the National Academy of Sciences and for unselected American psychologists (*1*).

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The Productivity of Sociologists at 45 American Universities

Norval D. Glenn and Wayne Villemez

Two recent articles in *The American Sociologist* deal with the productivity of sociology departments in the United States (Lewis, 1968; Knudsen and Vaughan, 1969), and it is likely that measures of departmental productivity will continue to be published with some regularity. These measures are interesting and may be useful to professional sociologists, graduate students in sociology, and prospective graduate students. As Knudsen and Vaughan stress, they are a needed supplement to the subjective measures of departmental quality published by the American Council on Education (Cartter, 1966). Among other functions, they help promote rivalry among departments and aid prospective students in their choice of departments.

However, any index of productivity that can be devised without excessive effort and expense is at best a crude measure of the quality of the faculty or program of a department. For instance, the measures used by Knudsen and Vaughan, the best that have been devised, give equal weight to all theoretical and research monographs reviewed in the *American Sociological Review;* yet everyone would agree that the quality and importance of these books vary a great deal. Furthermore, articles published in journals other than the *American Sociological Review,* the *American Journal of Sociology,* or *Social Forces* do not contribute to any of the indices reported so far, even though an increasing proportion of high-quality sociological articles are published in other journals. The weights assigned to different kinds of publications by Knudsen and Vaughan reflect only their judgment rather than a consensus among sociologists on the relative importance of the different kinds of articles and books. It would therefore be unfortunate if anyone were to place much emphasis on any of the published indices in his evaluation of any sociology department. In fact, the utility of the published indices for the evaluation of the current quality of departments is minimal, because each index pertains to a period that ended with the calendar year 1964 or 1965 and reflects work completed even earlier.

The purpose of this paper is to report more recent measures of productivity that cover a wider range of publications than the previously published indices. The measures are for the calendar years 1965 through 1968 and thus indicate changes that occurred after 1960 through 1964, the period covered by Knudsen and Vaughan's indices of faculty productivity. For comparability, we have used the most comprehensive measure used by Knudsen and Vaughan, but we have also added a more comprehensive and refined measure. In spite of several refinements, our comprehensive measure is still crude, and by the time the index values are published they will be somewhat dated. Therefore, readers should keep in mind that these values, by themselves, are not an adequate basis for evaluating departments. They are not totally adequate measures of the quantity and quality of publications of the departments, and of course publications output should not be the sole criterion of departmental quality.

COMPUTATION OF MEASURES

One of our measures is based on books reviewed in the *ASR* and on articles published in the *ASR*, the *AJS*, and *Social Forces* and is approximately the same as Knudsen and Vaughan's most comprehensive index; the only major difference is that Knudsen and Vaughan did not count research notes in the *AJS* and *Social Forces,* whereas we counted them and did not distinguish them from articles. We made this change because we could discern no consistent or considerable differences in length and theoretical relevance between articles and research notes in those two journals. We counted a few long papers in the "Commentary and Debate" section of the *AJS* but did not count the shorter comments or any items in the "Communications" section of the *ASR* or in the "Commentary" section of *Social Forces.* In addition to this departure from Knudsen and

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Vaughan's procedures, we may have handled multiple authorships and editorships differently. We attributed the full value for each publication to each institution represented, whereas Knudsen and Vaughan do not report how they treated publications produced by more than one department. The fractional departmental index values they report suggest that they divided the points for each publication among all the institutions represented. Nevertheless, our measure that approximates the one used by Knudsen and Vaughan is identified in our text and Table 2 as the Knudsen-Vaughan Index.

We also computed a more comprehensive and refined measure, which is identified in the tables and text as the Glenn-Villemez Comprehensive Index. This index is based on all books reviewed in the *ASR* and on articles and research notes in all journals listed in Table 1. Since several of the journals make no formal distinction between articles and research notes, we did not try to distinguish between the two; rather, we assigned the same weight to all papers in each journal. In assigning the weights, we utilized the responses from a sample of sociologists who were asked, by means of a mail questionnaire, to assign weights to each kind of publication. The sample of 250 was randomly drawn from all professors and associate professors listed in the *ASA Guide to Graduate Departments of Sociology* (1969) who were in departments with Ph.D. programs. We reasoned that these generally active and experi-

Table 1. Weights of Types of Publications

Type of publication	*Mean weight assigned by sample of sociologists*[a]	*Weight used in Glenn-Villemez Comprehensive Index*
Research and theoretical monographs (books)	33.8[b]	30
Textbooks (including revisions)	18.1[c]	15
Edited books	11.2[d]	10
Articles in:		
American Sociological Review	—	10
American Journal of Sociology	9.6 (109)[a]	10
Social Forces	8.0 (107)	8
Sociometry	7.9 (99)	8
British Journal of Sociology	7.9 (96)	7
Social Problems	7.6 (98)	7
Public Opinion Quarterly[g]	7.0 (100)	7
Demography	7.3 (77)	6
Rural Sociology	6.7 (95)	6
Administrative Science Quarterly[g]	6.7 (85)	6
Journal of Marriage and the Family[g]	6.6 (94)	6
Milbank Memorial Fund Quarterly[g]	6.6 (83)	6
American Sociologist	6.3 (106)	6
Sociology of Education	6.2 (81)	5
Sociological Quarterly	6.2 (73)	5
Journal of Health and Social Behavior[e]	6.2 (72)	5
Social Science Quarterly[f, g]	6.1 (56)	5
Sociology and Social Research	5.9 (92)	5
Sociological Inquiry	5.9 (75)	5
Pacific Sociological Review	5.7 (82)	5
Sociological Analysis	6.0 (50)	4
Phylon[g]	4.9 (73)	4

[a] The number in parentheses after the mean for each journal is the number of sociologists in the sample of 109 who assigned a weight to articles in the journal.

[b] The median is 20.

[c] The median is 10.

[d] The median is 8.

[e] During the early part of the period covered by this study, the title of this journal was the *Journal of Health and Human Behavior*.

[f] During the early part of the period covered by this study, the title of this journal was the *Southwestern Social Science Quarterly*.

[g] In these journals, only articles authored by sociologists were counted.

enced sociologists could make more informed judgments of the kinds of publications than could a random sample of all sociologists. For this study, we tabulated the responses from the 109 persons who returned completed and usable questionnaires within three weeks from the time the instruments were mailed.

The respondents were told that a weight of 10 had been arbitrarily assigned to an article in the *ASR* and they were asked to assign weights to other types of publications using the average "importance to the discipline" of an *ASR* article as their standard. In other words, a type of publication judged to be only half as important, on the average, as an *ASR* article would be assigned a weight of 5, a type twice as important would be assigned a weight of 20, and so forth. The respondents were asked not to assign a weight to a journal if they did not know it well enough to judge the average importance of its articles.

The mean weights given by the respondents are reported in the first column of Table 1. We did not use exactly these values as weights for our index. Rather, we reasoned that the proportion of respondents who did not know a journal well enough to assign a weight should also be taken into account. A good article in a widely read and widely known journal is likely to have greater influence, and thus be more important to the discipline, than an equally good article in a little-known journal. Therefore we took the percentage of respondents who did not assign a weight (for example, 20 per cent), divided this percentage by half (=10 per cent), and reduced the assigned mean weight by the amount of the latter percentage. This reduced value, rounded to the nearest whole number, is reported in the second column of Table 1 and was used in computing the indices reported in Tables 3 and 4.

The means and medians of the weights assigned to journal articles were almost the same, but the means and medians for the three kinds of books differed considerably (see notes b, c, and d, Table 1). The means were influenced a great deal by a few extremely high values. Therefore, we used as the weight for each kind of book a multiple of 5 which fell below the mean but which was closer to the mean than to the median. The ratios of these weights to one another and to the weight for an *ASR* article are exactly the same as the ratios among the weights used by Knudsen and Vaughan.

The journal in which an article appears is a very crude index of its quality or its importance to the discipline; undoubtedly, some articles in 5-point journals are more important than some articles in the *ASR* and in the *AJS*. However, when the weights are used on aggregate data rather than to judge individual articles, errors in different directions tend to offset one another. Thus the article portion of our index should be fairly sensitive to variations among the departments in the quality as well as in the quantity of their publications. Without adjustments, however, the book portion of the index is strictly a measure of quantity, although the average quality of books probably varies appreciably among departments. Therefore, we estimated the quality of the books produced by each department on the basis of the mean weight of its articles. We divided the mean weight of each department's articles by the approximate mean weight of all articles we counted (7.0) to arrive at an "article quality factor" that ranged from .821 for Fordham to 1.134 for Chicago. We then multiplied this factor by the raw book score to arrive at an adjusted score. Both the adjusted and the unadjusted scores are reported in the last column of Table 3. We used the adjusted scores to compute our Comprehensive Index reported in the first column of Table 3. Readers who would prefer to use a comprehensive index based on the unadjusted scores can easily compute one by adding the unadjusted book scores to the scores for all articles (the fourth column of Table 3).

The journals used for our Comprehensive Index are not all those that publish a significant number of articles by American sociologists (perhaps each reader can think of another journal or two that he would have included), but they probably contain a very substantial majority of the significant articles. Original articles and essays published in anthologies are an important type of publication not represented in the index, but these publications are a relatively small percentage of the total.

Although our concern is with graduate sociology departments, our measures are actually for all sociologists at each institution, regardless of whether or not they were in the sociology department. We at first tried to base our indices only on publications by faculty members of sociology departments, but we found it virtually impossible to determine whether or not some authors were faculty members of sociology departments when the publications appeared. Only complete lists of the faculties of the departments for each of the four years would have allowed us to exclude all publications by nonmembers of the departments. We sent a few requests to departments to see if we could obtain these lists, but some departments did not have them on hand and, understandably, were unwilling to prepare them. However, we were able to keep separate the publications from different campuses of the same institution. For instance, the data for Illinois refer only to the Urbana campus, the data for Wisconsin refer only to the Madison campus, and so forth.

We also included a few articles by persons who are not sociologists, because we assumed that the authors of articles in the strictly sociological journals were sociologists, and in at least a few cases this assumption was not correct. In interdisciplinary journals, we counted only articles by persons identified as sociologists, and we counted books reviewed in the *ASR* only if the authors or editors were fellows or active members of the American Sociological Association.

Had it not been for difficulties that diminished its accuracy, the most useful innovation introduced by Knudsen and Vaughan would have been the measure of productivity per faculty member. It is important to know whether or not a high productivity index for a department results from the large size of the department and moderate per-person productivity or from very high per-person productivity. For instance, to sociologists and graduate students seeking stimulating colleagues and mentors, per-person productivity may be more relevant than total departmental productivity.

Unfortunately, it is very difficult if not impossible to compute a truly accurate index of per-person productivity without having accurate lists of the sociology faculty members at each institution for each year covered by the index. We estimated the average number of sociologists at each institution during the four years by computing the mean number of sociologists listed in the 1965 and 1969 editions of the ASA *Guide to Graduate Departments of Sociology*, and we computed a per-member index by dividing this estimated number into each institution's total points on our Comprehensive Index. Our estimates of the numbers of sociologists were subject to several sources of error, however. The 1965 edition of the *Guide* supposedly lists all sociologists at each institution (but in some cases does not), whereas the 1969 edition lists only those persons affiliated in some way with the sociology department or departments. At a few institutions, including especially Harvard and Columbia, there were several productive sociologists not affiliated with the sociology or social relations departments. Since our index is for all sociologists at each institution, we obviously needed to know the total number of sociologists at each institution, but that information was not available for 1969.

Another problem was that both editions of the *Guide* listed some people as "part-time." In most cases, these people were clearly full-time academicians, and a few were among the most productive sociologists at their institutions. In other cases, they appeared to be part-time academicians who only taught a sociology course or two. We assumed that anyone with a Ph.D. or with a rank of assistant professor or higher was a full-time academician and thus should be counted, and we counted a few persons who did not meet this criterion but who were in departments in which most of the "part-time" people did meet it. Some of the "part-time" faculty had visiting appointments, and these were not counted.

As if these difficulties were not enough, graduate students and faculty members in other disciplines made appreciable contributions to the indices of total productivity of some of the institutions but of course were not counted in the denominator for the computation of the per-person index. The result of this and other sources of error is that most of the per-person productivity index values overstate the actual productivity. This overstatement may be 50 per cent or more in the case of Harvard and Columbia, and it may be considerable in the case of a few other departments. Since the error is apparently positively associated with actual per-person productivity, the ranking in Table 4 is probably approximately correct, but the absolute differences between the top-ranked departments and the others are not as great as the data indicate.

Knudsen and Vaughan's per-person indices understate the productivity of sociologists at some institutions because anthropologists were counted in at least some of the departments of sociology and anthropology. We avoided this source of error by excluding anthropologists, and of course we counted only sociologists in such interdisciplinary departments as the Harvard Department of Social Relations.

Instead of including all institutions in the United States with sociology graduate departments (the 1969 *Guide* lists 184), we restricted our study to the forty-five institutions with the highest scores for 1965–68 on the Knudsen-Vaughan Index. No other institutions are considered in this paper, even though a few others would have ranked in the top forty-five on some of the indices we report. We considered restricting the study to institutions with Ph.D. programs in sociology, but we did not do so. Two institutions are therefore included that do not have Ph.D. programs in sociology: California State College at Los Angeles and City College of the City University of New York. Institutions with no sociology graduate programs were excluded even though the sociologists at two of them—MIT and the University of California at San Francisco—would have ranked rather high in productivity.

FINDINGS

The two rankings in Table 2—for 1960–64 and for 1965–68—reveal the changes that have occurred with regard to the relative productivity of sociologists at the forty-five institutions as that productivity is indicated by the combined weights for books reviewed in the *ASR* and articles published in the "big three" journals. In the 1960–64 period reported by Knudsen and Vaughan, five universities—Berkeley, Harvard, Chicago, Michigan, and Columbia—constituted an elite whose productivity far surpassed that of all other institutions. Among the elite, Berkeley exceeded the others by a wide margin, with an index value almost 50 per cent greater than that of second-ranked Harvard.

For 1965–68, the Knudsen-Vaughan Index also reveals an elite of five universities, separated from the sixth-ranked institution by a margin greater than that between any two universities within the elite. However, Wisconsin had replaced Berkeley within the top five, and Columbia had moved from fifth to first place. Chicago and Michigan had each risen one rank, and Harvard had fallen from second to fourth (Table 2). The movements, in opposite directions, of Berkeley and Wisconsin were more marked and dramatic than the changes in rank alone reveal. In 1960–64, Berkeley's score was 146 per cent of that of the number *two* university, whereas in 1965–68, it was only 55 per cent of the score of the second-ranked institution. In 1960–64, Wisconsin was in a rather distant seventh place, having a score only 35 per cent of that of the top-ranked university. By 1965–68, this percentage had risen to 81.

Below the elite universities, the scores of similarly ranked institutions were very close for both periods, and, therefore, as one would expect, there were more marked changes in rank among them than among the elite. Among the top five in 1965–68, the mean change from 1960–64 was only 2 rank positions, whereas for the next lower ten it was 6.6, for the next lower ten it was 16.0, and so forth. Only one institution was displaced from the top five, and it dropped only to sixth place. In contrast, three were displaced from the top ten, and these dropped to thirteenth, twenty-fifth, and thirty-fourth places. Upward movements were equally dramatic. The top twenty in 1965–68 included universities that had risen from thirty-first, thirty-fifth, fifty-second, and fifty-seventh places. In short, there was little movement into and out of the elite but a great deal of upward and downward movement below that level.

We have already noted that one reason for the considerable instability in ranks below the elite is that similarly ranked institutions differed so little in productivity that small changes in productivity could produce marked changes in rank positions. It is also our impression that the productivity of departments below the elite departments is more likely to result largely from the efforts of one, two, or a few persons, so that a small change in personnel may effect a large change in productivity. Furthermore, in departments in which most members publish infrequently, several members may by chance publish books or major articles within a short period of time, thus temporarily raising the department to a rather high level of productivity.

Table 2. Ranking of Sociologists at 45 Institutions on the Knudsen-Vaughan Index, 1965–1968

1965–1968 Rank	*Department*	*Index*	*1960–1964 Rank*[a]	*1965–1968 Rank*	*Department*	*Index*	*1960–1964 Rank*[a]
1.	Columbia	1544	5		Texas	340	16
2.	Chicago	1516	3	25.	Michigan State	332	10
3.	Michigan	1500	4	26.	Duke	328	19
4.	Harvard	1424	2	27.	SUNY at Buffalo	326	43
5.	Wisconsin	1248	7	28.	Stanford	324	17
6.	Berkeley	832	1	29.	Indiana	308	42
7.	Brandeis	632	18	30.	Southern Illinois	288	49
8.	Princeton	564	6	31.	Ohio State	284	21
9.	Illinois	548	20	32.	Florida State	272	27
10.	Washington (Seattle)	520	23	33.	Rutgers	264	15
	Yale	520	11	34.	Fordham	260	54
12.	Cornell	508	12		Northwestern	260	9
13.	UCLA	500	8	36.	Pennsylvania State	240	59
14.	NYU	488	31	37.	Pittsburgh	224	28
15.	Minnesota	472	13	38.	Nebraska	216	37
16.	Pennsylvania	456	35	39.	Syracuse	208	46
17.	North Carolina	408	24		Case Western Reserve	208	38
18.	Washington (St. Louis)	400	14	41.	Vanderbilt	204	33
19.	California at Santa Barbara	356	52	42.	City College, CUNY	200	not ranked
20.	New School	352	57				
21.	Johns Hopkins	348	45	43.	California State at Los Angeles	192	not ranked
22.	Washington State	344	36				
23.	California at Davis	340	not ranked	44.	Oregon	184	26
				45.	Colorado	176	58

[a] The 1960–64 ranks are revised in accordance with a note in the August 1969 *American Sociologist* by Knudsen and Vaughan which reports errors in the data in their article.

In contrast, in departments with many highly productive members, the output should be more sustained and stable.

Our Comprehensive Index, reported in the first column of Table 3, is, we believe, a more adequate measure of the quantity and quality of the productivity of sociologists at an institution than is the Knudsen-Vaughan Index, but we computed its values only for 1965–68. This more comprehensive and refined measure placed the same institutions in the top five and in the top six as did the Knudsen-Vaughan Index, but the rankings within the top five were different. Our index raised Michigan two places, to first, and Wisconsin three places, to second. It lowered Columbia from first to fourth and demoted Chicago and Harvard each one place. Our index, even more than Knudsen and Vaughan's, made it clear that Wisconsin had risen into the ranks of the elite institutions, so far as sociological productivity is concerned.

Once-elite Berkeley ranked sixth on both indices but was closer to the elite institutions on the Glenn-Villemez Index: 272 points below fifth-ranked Harvard and 369 points above seventh-ranked North Carolina. Since on our index the widest gap in scores between any two adjacent institutions was between Berkeley and North Carolina, and since North Carolina was more similar to forty-second-ranked Colorado than to Berkeley, one might well consider Berkeley one of an elite of six institutions. However, it would have to be considered a somewhat marginal member of the elite. Fifth-ranked Harvard was considerably closer to top-ranked Michigan than to Berkeley, and the range was only 207 among the top five but was 479 among the top six. Furthermore, Berkeley ranked only nineteenth in per-person productivity (Table 4), whereas all other elite institutions ranked tenth or higher, and all except Wisconsin ranked in the top five.

Below the elite, the ranks of the institutions on the Comprehensive Index differed to an important extent from those on the Knudsen-Vaughan Index. In twenty-one cases the ranks on the two indices for 1965–68 differed by more than three places, and in twelve cases the difference was more than five places. Institutions at which faculty members published mainly books ranked higher on the Knudsen-Vaughan Index than on our index. For instance, the New School ranked twentieth on the former but thirty-fifth on the latter, and Southern Illinois, Fordham, California at Davis, Washington University, and Brandeis each ranked seven to ten places higher on the Knudsen-Vaughan Index. Conversely, by taking into account most books but only a small percentage of articles, the Knudsen-Vaughan Index discriminated against institutions at which sociologists published heavily in journals. These included Northwestern, which ranked seventeen places higher on the Comprehensive Index, and Indiana, North Carolina, Texas, and Case Western Reserve, each of which ranked at least seven places higher on the Comprehensive Index than on the Knudsen-Vaughan Index.

We note above that our per-person index (Table 4) is subject to considerable error, and therefore its values should be interpreted with caution. The value for Harvard overstates by a considerable margin the per-person productivity of the nineteen Harvard sociologists we counted; although we were unable to determine the exact amount of the error, we are confident that the Harvard value should be no more than 45. There is much less error with most of the other universities, but there is probably enough that little confidence should be placed in differences in index values of no more than 3 or 4 points.

The rankings of the institutions in total and in per-person productivity did not closely correspond. For instance, Berkeley ranked thirteen places higher in total than in per-person productivity, and Wisconsin ranked eight places higher on the total index. It is clear that the elite status of Berkeley and Wisconsin was due to their having an unusually large number of sociologists. Their per-person index values were quite good, but not outstanding, and were exceeded by the values for such non-elite institutions as the New School, Princeton, Duke, Northwestern, and Brandeis. Even Vanderbilt, which ranked thirty-sixth in total productivity, almost equalled Wisconsin and exceeded Berkeley on the per-person index.

The per-person data throw light on productivity norms in the major departments, and to us the per-person productivity of sociologists at most of the forty-five universities seemed surprisingly low. Assuming that the Harvard index value should be no more than 45, the mean productivity at the institution with the most productive sociologists was exceeded by anyone who had a monograph reviewed in the *ASR* and who published an article in the *AJS* and an article in *Social Forces.* In all but the top seven institutions (and probably in all but the top two), the mean productivity for the four-year period was exceeded by anyone who had a monograph reviewed in the *ASR*, published three articles in the *ASR* or the *AJS*, published four articles in *Social Forces* or *Sociometry*, or published any combination of books and articles with a total point value of 30 or more. The mean productivity of persons in any institution below the top twenty was exceeded by anyone who published a 5-point article and had a textbook reviewed in the *ASR*, published two articles in the *ASR* or the *AJS*, or published three articles in 7-point journals. Below the top thirty-eight universities, one article in the *ASR* or the *AJS*, one edited book, or two articles in 5-point journals placed a faculty member above the mean. In other words, an article in a journal such as *Sociology and Social Research* or the *Sociological Quarterly* every other year was above-average productivity. It should be kept in mind, however, that many sociologists at these institutions had publications, such as original contributions to anthologies, that did not contribute to the index.[1]

The productivity of sociologists is often stated in terms of number of articles published per year. Although this is a much cruder index

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[1] The per-person index is the mean contribution of the faculty members to the total index points for the institution, and the number of points an individual contributed is not in all cases an adequate index of his personal productivity. This is true because we gave the full value of coauthored or coedited publications to each institution represented. Therefore, if a person coauthored an article in the *ASR* with someone at another institution, he contributed 10 points to his institution's index, but if he coauthored the article with someone at his own institution, his share of the contribution was only 5 points. In either case, of course, his personal producitivity was the same.

The examples above of the numbers and types of publications that would have exceeded the mean contributions of individuals at institutions at the different ranks are based on the assumption that the publications were not coauthored or were coauthored with people at other institutions.

Table 3. Ranking of Sociologists at 45 Institutions on Five Indices of Productivity, 1965–1968

Glenn-Villemez Comprehensive Index		*ASR articles*		*ASR, AJS, and Social Forces articles*		*All articles*		*Books (adjusted scores)*[a]	
1. Michigan	1374	1. Michigan	180	1. Wisconsin	440	1. Wisconsin	904	1. Columbia	798 (735)
2. Wisconsin	1356	Wisconsin	180	2. Chicago	394	2. Michigan	654	2. Harvard	761 (735)
3. Chicago	1314	3. Columbia	150	3. Michigan	330	3. Chicago	611	3. Michigan	720 (670)
4. Columbia	1269	4. Chicago	140	4. Columbia	262	4. Berkeley	539	4. Chicago	703 (620)
5. Harvard	1167	5. Berkeley	130	5. North Carolina	188	5. Columbia	471	5. Wisconsin	452 (420)
6. Berkeley	895	6. Harvard	100	6. Harvard	186	6. North Carolina	418	6. Berkeley	356 (375)
7. North Carolina	526	7. North Carolina	80	7. Berkeley	150	7. Harvard	406	7. Brandeis	329 (360)
8. Illinois	525	8. Washington (Seattle)	70	8. Texas	144	8. Texas	325	8. Princeton	309 (305)
9. UCLA	521	Washington State	70	9. Washington (Seattle)	130	9. UCLA	318	9. Yale	280 (280)
10. Cornell	510	10. Indiana	60	10. Vanderbilt	118	10. Northwestern	313	10. Illinois	267 (275)
11. Washington (Seattle)	488	Oregon	60	Washington State	118	11. Indiana	303	11. Pennsylvania	252 (250)
12. Minnesota	474	12. Cornell	50	12. Indiana	114	12. Cornell	280	12. NYU	244 (250)
13. Princeton	465	Texas	50	UCLA	114	13. Minnesota	268	13. Washington (Seattle)	242 (220)
14. Brandeis	444	UCLA	50	14. Cornell	110	14. Illinois	258	14. Cornell	230 (230)
15. Yale	434	15. Yale	40	15. Florida State	104	15. Washington (Seattle)	246	15. New School	220 (220)
16. Texas	420	Minnesota	40	Minnesota	104	16. Washington State	242	16. Minnesota	206 (210)
17. Northwestern	418	17. Brandeis	30	17. Oregon	96	17. Duke	202	17. Washington	
18. NYU	408	Michigan State	30	18. Johns Hopkins	88	18. Florida State	197	(St. Louis)	203 (225)
19. Indiana	406	Colorado	30	19. Illinois	82	19. Case Western Reserve	188	UCLA	203 (210)
20. Pennsylvania	365	Northwestern	30	20. Pennsylvania State	80	20. Michigan State	182	19. California at Davis	187 (180)
21. Duke	364	21. Vanderbilt	20	21. NYU	76	21. Johns Hopkins	180	20. California at	
22. Washington State	360	Southern	20	22. Northwestern	60	22. Vanderbilt	177	Santa Barbara	177 (175)
23. Michigan State	336	Illinois		23. Princeton	58	23. Oregon	171	Stanford	177 (185)
24. Johns Hopkins	334	Rutgers	20	24. Michigan State	56	24. Ohio State	168	22. Duke	162 (180)
25. California at		Princeton	20	25. Case Western Reserve	54	25. NYU	164	23. SUNY at Buffalo	155 (164)
Santa Barbara	333	Pennsylvania State	20	SUNY at Buffalo	54	26. California at		24. Michigan State	154 (160)

Table 3 (Continued)

Glenn-Villemez Comprehensive Index		*ASR articles*		*ASR, AJS, and Social Forces articles*		*All articles*		*Books (adjusted scores)*[a]	
26. Washington		NYU	20	27. Rutgers	52	Santa Barbara	156	Johns Hopkins	154 (150)
(St. Louis)	323	Florida State	20	28. Pennsylvania	48	Princeton	156	26. Southern Illinois	150 (160)
27. Stanford	318	California at Davis	20	Yale	48	28. Yale	154	27. Ohio State	133 (150)
28. Ohio State	301	29. Washington		30. Duke	42	29. Pennsylvania State	146	28. Fordham	123 (150)
29. Florida State	298	(St. Louis)	10	Nebraska	42	30. Stanford	141	29. Rutgers	119 (125)
30. SUNY at Buffalo	294	Syracuse	10	32. California at		31. SUNY at Buffalo	139	30. Washington State	118 (120)
31. California at Davis	289	SUNY at Buffalo	10	Santa Barbara	40	32. Rutgers	127	31. City College of	
32. Case Western Reserve	269	Pittsburgh	10	33. Brandeis	38	33. Washington		CUNY	116 (125)
33. Rutgers	246	Ohio State	10	California at Davis	38	(St. Louis)	120	32. North Carolina	108 (105)
34. Pennsylvania State	235	Nebraska	10	35. Ohio State	36	34. Nebraska	118	Pittsburgh	108 (130)
35. New School	220	Johns Hopkins	10	36. Colorado	30	35. Brandeis	115	34. Northwestern	105 (130)
36. Vanderbilt	219	Illinois	10	37. Stanford	26	36. Pennsylvania	113	35. Indiana	103 (100)
37. Pittsburgh	218	Duke	10	38. Southern Illinois	20	37. Pittsburgh	110	California State at	
38. Nebraska	207	Case Western Reserve	10	Washington		38. California at Davis	102	Los Angeles	103 (120)
39. Oregon	202	California at		(St. Louis)	20	39. Syracuse	69	Syracuse	103 (115)
40. Southern Illinois	196	Santa Barbara	10	40. Syracuse	18	40. Colorado	66	38. Florida State	101 (100)
41. Syracuse	172	40. California State at		41. Pittsburgh	10	41. Southern Illinois	46	39. Texas	95 (100)
42. Colorado	160	Los Angeles	0	42. Fordham	8	42. Fordham	23	40. Colorado	94 (90)
43. Fordham	146	Stanford	0	43. California State at		43. City College of CUNY	13	41. Pennsylvania State	89 (90)
44. City College of		Pennsylvania	0	Los Angeles	0	44. California State at		Nebraska	89 (105)
CUNY	129	New School	0	City College of CUNY	0	Los Angeles	12	43. Case Western Reserve	81 (90)
45. California State at		Fordham	0	New School	0	45. New School	0	44. Vanderbilt	42 (40)
Los Angeles	115	City College of CUNY	0					45. Oregon	31 (30)

[a] Unadjusted scores are given in parentheses.

Table 4. Ranking of Sociologists at 45 Institutions on the Glenn-Villemez Comprehensive Index of Per-Person Productivity

Rank	*Department*[a]	*Index*	*Rank*	*Department*[a]	*Index*
1.	Harvard (19)	61.4	24.	Indiana (23)	17.7
2.	New School (5)	44.0	25.	Minnesota (27)	17.6
3.	Chicago (33)	39.8	26.	Yale (25)	17.4
4.	Michigan (39)	35.2	27.	Washington (St. Louis) (20)	16.2
5.	Columbia (39)	32.5	28.	Florida State (20)	14.9
6.	Princeton (15)	31.0	29.	California at Santa Barbara (23)	14.5
7.	Duke (12)	30.3	30.	Case Western Reserve (22)	12.2
8.	Northwestern (16)	26.1	31.	Cornell (43)	11.9
	Brandeis (17)	26.1	32.	Pennsylvania State (20)	11.8
10.	Wisconsin (57)	23.8	33.	City College of CUNY (11)	11.7
11.	NYU (18)	22.7	34.	Pennsylvania (33)	11.1
	Stanford (14)	22.7	35.	Southern Illinois (18)	10.9
13.	California at Davis (13)	22.2	36.	Syracuse (16)	10.8
	Washington (Seattle) (22)	22.2	37.	State University of New York at Buffalo (28)	10.5
15.	Vanderbilt (15)	21.9	38.	Fordham (14)	10.4
16.	Washington State (17)	21.2	39.	Michigan State (35)	9.6
17.	North Carolina (25)	21.0		Oregon (21)	9.6
18.	UCLA (25)	20.8	41.	Colorado (18)	8.9
19.	Berkeley (44)	20.3	42.	Rutgers (28)	8.8
20.	Texas (21)	20.0	43.	Ohio State (36)	8.4
21.	Illinois (27)	19.4	44.	Pittsburgh (34)	6.4
22.	Nebraska (11)	18.8	45.	California State at Los Angeles (28)	4.1
23.	Johns Hopkins (18)	18.6			

[a] The numbers in parentheses are the estimated mean numbers of faculty members for the four years.

of per-person productivity than the index reported in Table 4, its greater familiarity may make it more meaningful and interesting to some readers. We estimate that there was an average of 1,060 sociologists at the forty-five institutions during the years 1965–68, and if this estimate is correct, per-person productivity at all institutions, considered as an aggregate, was .34 articles per year, or one article every three years. If articles published in journals not included in this study were considered, productivity might have approached one article per person every two years, but we doubt that article production was higher than that. In no institution was per-person productivity in the journals we considered as high as one article per person per year. The highest figure was .74, for Harvard, but we have already indicated that the per-person productivity data for Harvard are in error and are too high. The next highest figures were .73 for Northwestern and .67 for Duke. Chicago, North Carolina, and Texas tied for fourth place with .58, and Michigan and Wisconsin ranked seventh and eighth with .56 and .53. Only these eight institutions had per-person productivity of at least one article every two years. Seventeen of the universities produced less than one article per person in four years. These included several institutions with rather prominent sociology departments, such as Cornell, Yale, Michigan State, Washington University, and Pennsylvania. Of course, sociologists at some of these universities compensated for their low article production with a heavy output of books.[2]

When, in the near future, the next ranking of the quality of the faculties of sociology graduate departments is published by the American Council on Education, it will be interesting to compare that ranking with the productivity rankings reported here. However, there is little reason to expect that the two will closely correspond. The ACE began gathering data for its study early in 1969, whereas many of the publications that contributed to our indices appeared in 1965, 1966, and earlier. By 1969, some of the departments that ranked rather high on our measures had lost their most

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[2] For data on the productivity of a more representative sample of sociologists, see Babchuk and Bates (1962).

productive members; other departments had recently gained prominent members who had not been in their new departments long enough to contribute appreciably to our productivity indices. Furthermore, the reputations of some departments are based to a rather large extent on the prestige (and also on the continued competence) of faculty members whose major contributions were made prior to the period covered by our study. The ACE ratings and productivity indices should also differ somewhat because of the probable time lag between changes in the productivity of departments and changes in their reputations. Sociologists asked by the ACE to evaluate sociology departments in the spring of 1969 had not seen data such as those reported here, and they probably had no truly accurate impressions of the current and recent productivity of the departments. However, they had seen Knudsen and Vaughan's data for 1960–64 and Lewis's data for 1956–65, and they may have been influenced by those dated productivity rankings.

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Changes in Scientific Activities with Age

Anne Roe

In the years 1947 to 1949 I studied a group of eminent research scientists (*1*). My reasons for seeking them out then were to find out if scientists differ in any consistent ways from nonscientists, or if different kinds of scientists differ consistently from each other, and to find out why they became scientists rather than something else.

In 1962 and 1963 I interviewed these same men again. I was concerned to learn what changes had taken place in the nature or amount of their scientific work, in the pattern of their lives generally, and in their opinions about such things as the nature and management of research activities (*2*).

The highlights of the earlier study can be reviewed briefly. There were 64 men in the group, 20 in the biological sciences, 22 in the physical sciences, and 22 in the social sciences. They were selected by their peers for the excellence of their scientific contributions. At that time their average age was 48, all of them were married, and most of them had children. Five were from Jewish homes, one was from a home of free-thinkers, and the rest had Protestant backgrounds. Just over half of them had fathers who were in professional occupations; none were sons of unskilled laborers, and none were from very wealthy, aristocratic families.

There were some characteristic patterns in their early histories. Most of the social scientists were socially active from an early age. Most of the others were rather shy, socially late-maturing boys, with strong hobbies and noticeable persistence in them. With the exception of some of the experimental physicists, all of them were voracious, if unselective, readers through-

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out their childhoods. More of the natural scientists regarded their fathers with great respect but felt somewhat distant from them. More of the social scientists had had strong conflicts in the family.

These scientists are of extraordinarily high intelligence. On a verbal test, their median was about equivalent to an IQ of 166, with the lowest about 121. That seems to be about a minimum IQ for a research scientist; higher levels are no drawback, but above that level other aspects of the person's nature or endowment may become more important. Perhaps the most influential of these other aspects is motivation—the degree to which the individual's work is important to him. All of these men are, and have always been, so immersed in their work that other considerations play much smaller roles (3). They give a picture of hard-working, driven, and devoted men, but they are these by choice. For the most part they spend their time doing what they want to do, and they always have. It is this and the respect of their peers that repays them, for their financial rewards are far from commensurate with the contributions they make to society.

Of the 64 men originally studied, 54 are still living; two of the biologists, three of the physicists, one of the psychologists, and four of the anthropologists have died. Some of the 54 had retired,. some had moved to other institutions, but I was able to see all but two, who were in Europe at the time. From one of these I received a long report. Most of the interviews were tape-recorded.

Their present ages are from 47 to 73. There are 17 over 65, and 11 of these are biologists. The age difference is of more significance now than it was 15 years ago, because of the retirement issue.

Many scientists move about from one institution to another and stay for varying periods in various places, but, aside from wartime assignments and visiting professorships, 16 of these men (ten of them physical scientists) have stayed at one institution. Two biologists have been at seven different institutions, and one biologist and one social scientist, at six.

HONORS

Many honors have come to these men, in addition to the visiting professorships and lectures, which are in themselves honorific. The most obvious are medals, prizes and awards, honorary degrees, and membership in certain societies. Tables 1–3 give the data for this group as of July 1963. More honors have accrued to some of them since then.

It is noteworthy that the social scientists have received many fewer honors than the others, although they are as highly selected a group. Table 1 shows, for example, that there were 38 different medals, prizes, and awards given to the biologists, a number of men in the group having received the same award at different times. For the physicists there were 37 awards, but for anthropologists and psychologists, considered together under the heading "social scientists," only four different honors were available. In addition, some subjects in all groups received the Presidential Certificate of Merit for contributions to the war effort, but this is the only award common to all fields.

The situation with regard to honorary degrees (Table 2) is even more striking: only six of the social scientists have received any honorary degrees.

Membership in the National Academy of Sciences and in the American Philosophical Society is also commoner among the natural scientists (Table 3). Psychologists in these two

Table 1. Medals, Prizes, and Awards Received by the 54 Scientists of the Study

				Average		
Category	*No. in category*	*No. with no such awards*	*Range per person*	*For group*	*For those receiving awards*	*No. of different awards within category*
Biologists	18	6	0–11	2.88	4.33	38
Physicists	19	8	0– 9	2.26	3.91	38
Social scientists	17	6	0– 3	1.06	1.63	5*

* This number includes one award from a scientific organization in a field not connected with psychology in the usual sense.

Table 2. Honorary Degrees Received by the 54 Scientists

Category	*No. in category*	*No. with no honorary degree*	*Range per person*	*Average*	
				For group	*For those receiving awards*
Biologists	18	5	0–11	3.35	4.92
Physicists	19	3	0–16	2.94	3.50
Social scientists	17	11	0– 2	0.40	1.16

Table 3. Society Memberships of the 54 Scientists

Category	*No. in category*	*National Academy of Sciences*	*American Philosophical Society*
Biologists	18	17	14
Physicists	19	14	11
Social scientists	17	13	8

societies have always been selected almost entirely from the "experimentalists" (who now constitute a relatively small percentage of the American Psychological Association), and under present election procedures this imbalance is likely to continue indefinitely. It is somewhat surprising to find the psychologists in the American Philosophical Society no more broadly representative of the profession, since this society has members from the arts and humanities as well as the sciences.

These differences are interesting and somewhat curious. It is, of course, true that receipt of any of these honors tends to lead to the receipt of others, for it adds to the general visibility of the recipient. Also, the older sciences have undoubtedly accumulated a stock of medals and prizes, which continue indefinitely, but the differences in honorary degrees and in society memberships cannot be explained in this way.

RETIREMENT

Retirement policies vary from institution to institution, but for most of these men retirement from teaching or administration comes earlier than retirement from research. Few retire before the required age, which may be anywhere between 65 and 70 and seems to be usually earlier in private than in state-supported institutions. However, there are special appointments which are unlimited, and there is an occasional institution without a retirement policy (this often means without a good pension plan).

I encountered no one who did not have, or did not expect to have, the privilege of retaining office space, and frequently laboratory space if he remained in the same city after retirement. There *are* problems with regard to assistance, and one major lack is secretarial assistance. This can be serious for many of those whose correspondence has become very heavy over the years. Retired scientists who wish research grants seem to have no difficulty in securing them.

In general, the scientists' working habits have changed very little in 15 years, and they show very few signs of "disengagement"—at most, they go to fewer meetings and no longer work nights.

Nine of the biologists have formally retired, but one of these is continuing as a research professor, having retired at 66 from administration and teaching only, and another has continued research on a half-time appointment since retiring at 67. One retired from one university at 65 and has just retired from another at 70; he has given up experimental work but continues writing and working on data from other biologists. Three others retired at 67, and two at 68. All of them continue to be active, although in different ways. One, in the 7 years since his retirement, has divided his time about equally between work in a research institution and society administration. One has been involved in administrative and consultative activities with research foundations and professional associations. After retirement another spent a year and a half in administration in a different science and is now working actively on his own research, with adequate grants. One, who has never had teaching or administrative duties, is continuing in his laboratory, with no noticeable change in his activities.

Among the physical scientists only two (both experimentalists) have formally retired, one of them twice, at 65 and 70 from different institutions. He is continuing a full-time program of writing and lecturing. The other retired at 67 after devoting many years to full-time administration. He still retains an office at his university and has begun some experimental work again, but he spends the greater part of his time in maintaining an active consulting practice in his specialty. Another physicist, in his fifties, has made an unusual arrangement with his company to serve as a consultant 1 or 2 days a week, but he has otherwise retired.

Only two of the social scientists have formally retired. One psychologist retired at 65, but is continuing on a half-time appointment until he is 70. He devotes about a fourth of his time to teaching and the rest to research, and his productivity is at an all-time high. The other retired several years ago at 66, but, here again, there has been no noticeable change in his activities. He had not been engaged in either teaching or administration and continues his research as before, although the output in terms of number of papers has been steadily decreasing for some years.

One anthropologist will retire shortly, at 70. His teaching load is still a very heavy one, but he expects, after retirement, to spend 1 year giving a seminar at another institution and to devote the rest of his time to writing. He has done little research for some time, but has continued writing.

RESEARCH SUPPORT

The sources of funds which have been available to these men for the support of research throughout their professional lives are given in Table 4. This list may not be complete. Occasionally one of them would remember other (usually minor) sources after we had moved on to different questions, and there may be still other sources that they did not remember. Presumably these are few and not major ones. No source is listed individually in Table 4 if it was named by only three scientists. Subgroups of the physical scientists (theoretical and experimental physicists) and of the social

Table 4. Sources of Research Support, During Their Professional Lives, for the 54 Scientists

		Physicists		*Social scientists*	
Source	*Biologists*	*Theoretical*	*Experimental*	*Psychologists*	*Anthropologists*
None	2	7	0	0	0
Institution	6	0	4	4	4
Private	3	0	0	1	2
Ford, Rockefeller, Carnegie	8	2	1	8	0
NIH	4	0	1	9	0
NSF	7	0	2	7	1
AEC	4	0	4	1	0
Cancer, Polio, Heart foundations	4	1	0	0	0
Social Science Research Council, NRC, General Services Administration, American Philosophical Society	2	0	1	4	0
Office of Naval Research, Army, Air Force, NASA	0	1	5	11	1
Office of Education, Weather Bureau, Office of Vocational Rehabilitation, Office of Scientific Research and Development	0	0	1	5	0
Miscellaneous small foundations	5	1	5	5	2
Commercial	0	1	0	0	0

scientists (psychologists and anthropologists) are listed separately, since their patterns of support are very different. Research requirements for theoretical physicists may be only paper and pencil or blackboard and chalk, and it is only in this group that there are men who have had no research funds whatever; there are a few more who have had very limited funds, or funds for a very few projects.

The few men who have had no support other than the funds of their own institutions are generally proud of this. At the other extreme, there are a few who have very large grants from several sources and are responsible for several large studies—usually, but not always, closely related ones.

Very few of the 54 scientists had really adequate funds before World War II, and indeed for many of them the kind of research they are doing now, especially that requiring large funds, was not even thought of then. In some instances this is obviously due to changes in the science itself. In others, because large research funds were extremely rare outside of purely research institutions, large programs were simply not designed. The change is very largely due to research support from the federal government.

There is unanimous testimony that, since the war, none of these men have lacked the funds they have needed, although a few have had to put in a great deal of time securing them. These last have been chiefly men who were developing new programs in rather underfunded institutions. . . .

Asked whether or not their own research plans had ever been altered because of the source of funds which might be available to them, the general reply was an unqualified "No." Most of the men said quite flatly that they had never made any changes in what they wanted to do in order to get funds for it, but a few did admit to some (minor) influence from the source of support or stated that they know that such things have occurred with other people.

ADMINISTRATION

One of the men said, "Well, it is a fact of life that most professors who rise in the world have to take on administrative posts." As Table 5 shows, this has happened to the majority of these men. There are some interesting differences among fields, however. None of the social scientists has become administrative head of an institution, although a number of men in both the other groups have. It is also clear that, of the group in this study, administrative duties have been undertaken more often by the experimental physicists than by the theoretical physicists.

Most of those who are or have been administrative heads had been department chairmen, and sometimes deans, before reaching their latest positions. The amount of time required for administrative duties is extremely varied. A college president spends full time at it, but a museum director may not. There is full agreement, however, that any administrative position takes time away from research, and that the higher positions are likely to put an end to all research. The men who have gone from research to administration give different reasons for having done so. Very few of them have gone back to research. . . .

Table 5. Administrative Positions Held Now or Previously by 53 of the 54 Scientists. Numbers of Individuals in the Four Categories: Biologists, 18; Theoretical Physicists, 9; Experimental Physicists, 9; Social Scientists, 17. The Data Do Not Include Wartime Assignments but Do Include Government Posts Held Since World War II

Position	*Biologists*	*Physicists*		*Social scientists*
		Theoretical	*Experimental*	
Department chairman	7	2	4	5
Director of laboratory or large research project	2	2	4	6
Dean	1	1	1	2
Administrative head of institution	4	2	4	0
Other	3	1	1	0

PUBLICATIONS

There are various ways by which a scientist's output can be measured, but perhaps the easiest is to make some count of his publications. For 45 of the men interviewed, complete bibliographies were obtainable (2). The average yearly number of publications for these 45 ranges from 1.11 to 9.24, with an overall average of 4.20 per year. Peak production may come anywhere from the first to the ninth 5-year period after receipt of the doctorate, with modes in the second and sixth 5-year periods (so far as possible, allowance has been made for wartime activities). There is some, but not a consistent, tendency for straight research reports to diminish over time relative to other types of publication (books, chapters, symposium papers, and so on).

It appears from these records that there is clearly no justification for the sometimes expressed opinion that rapid advancement in the academic hierarchy will somehow reduce motivation to produce. Ten years after the doctorate most of these scientists were well up in that hierarchy, and their production was generally increasing.

But many of them do now notice changes in motivation and energy:

"My motivational system is narrowing. I'm not as charmed by giving speeches as I was once."

"I was already aware when we had that previous session that in my own case the originality or drive which leads to productivity in experimental research was slowing down. The work I did took a great deal of physical energy, and while as I got older I had more help, it still took daily attention and planning of a fairly arduous sort. After a while we had gotten to the end of what you could do in my field with the techniques I knew and I saw this coming. So I said to myself that since I had another talent I would quit doing experimental work and do something that I could do better than most."

"I have become considerably less productive. My main interest is the most advanced and difficult kind of mathematical physics, but for a long time I have not had the time or the energy to keep up with it enough to make any efforts of mine to publish in that direction any more than laughable."

"As you grow older you don't have patience or nervous strength to make long calculations in the way I did on my Ph.D. thesis. You have a certain amount of momentum and I hope to rewrite my book. That does not require quite the same creative energy as to plan a new research publication, but it requires a lot of energy at that. It takes a lot more effort to write a paper now, somehow or other."

Other kinds of changes are noted:

"I get along better with students than I used to, but I suspect it is because I'm not pushing as hard as I used to."

"I've become far less controversial. I can no longer think ideas as I used to be able to do. I now fall back more and more on mechanical ways of arranging materials and ideas. . . . There's a very, very clear-cut difference."

One of the scientists has quite a different sort of problem:

"You know this really is beginning to bother me—I'm having a hard time telling the difference between right and wrong. I always felt I really knew, and now especially for other people I just don't know whether they're doing right or wrong. The frameworks are so different from anything corresponding to the way I used to make judgments for myself, and then I used to make judgments about other people, too. But when I look at the different situations which they have faced and the different real situations in which they live, well, this is kind of upsetting. I've lost my sense of smell, my eyes are kind of feeble, my hearing is a little bit weak—when I can't tell right from wrong, I really had better retire, so it's got me worried."

SUMMARY

Continuous study of the lives of eminent research scientists shows that, in spite of some changes, these men have continued to contribute at an extraordinarily high level. For most of them, their contributions are still in the scientific fields in which they attained eminence. Others may contribute more significantly now in administrative posts, in facilitating the work of other men. All are happy in their work, and none regrets his choice of occupation. Their rewards have been in terms of inner satisfactions and recognition from their peers.

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1. Both the earlier study and the one reported here were supported by grants from the National Institute of Mental Health. A full account of the earlier study appears in A. Roe, *The Making of a Scientist* (Dodd Mead, New York, 1952); an Apollo edition appeared in 1961. A more complete report of the later study is given in A. Roe, "Scientists Revisited," *Harvard Studies in Career Development, No. 38.*
2. In addition I recorded material about the scientists' health, their children's occupational choices, their opinions of the effect of governmental support of research, and their opinions of women in science. I have also analyzed in some detail the nature and pattern of their publications. These data are reported in "Scientists Revisited," *Harvard Studies in Career Development, No. 38,* and in reports now in preparation. A brief paper on women in science will appear in the *Journal of Counseling Psychology.*
3. For a fuller account of the personality patterns of scientists, based on this and other research, see A. Roe, *Science 134,* 456 (1961).

Mature Research Institutions and the Older Scientist

Leslie G. Cook and George W. Hazzard

Scientific research as a full-time profession, instead of as merely part of an academic career, is a phenomenon largely of the last few decades. Since World War II the number of large industrial laboratories in this country has increased by 50 percent, and the total number of scientists in these laboratories has doubled (*1*).

The number of males who graduate from college with degrees in science and engineering has been increasing at the rate of 12 percent per year, as compared with 5 percent for college graduates generally and only 2 percent for people of age 22 in the total U.S. population (*2*). All in all, there has been a remarkable and disproportionate increase in the flow of young people into the science professions, matching the equally remarkable flow of money into research and development activities.

As a consequence, most scientists in professional research laboratories have thought of themselves as young people, in young organizations and with unlimited growth opportunities. Yet now, looking around, they are intuitively aware that, on the whole, they are working with older people. What exactly has been happening in these laboratories?

Data were available to us on seven long-established laboratories, six industrial and one governmental. While there are some variations in detail, all seven show the same essential trends in ages of the Ph.D. research staff. . . .

A few years after its inauguration, the typical laboratory would have a Ph.D. staff of 150. Usually, 20 percent or less of the staff is over age 38, with the majority of the members in the 27-to-35 age group.

During the next 15 years the laboratory typically doubles in size. . . . Perhaps 50 percent of the staff is over age 38, with a fairly uniform distribution in the 27-to-50 age group. Such is the approximate age distribution in the early 1960's for all the laboratories studied.

Extrapolating this trend another 15 years, one gets a new age distribution. We have assumed that the laboratory has ceased to grow, having reached the maximum size its parent organization will support. Because of this, new employment has been confined to replacements. Partly because of restricted additions of young staff members, some 80 percent of the staff will be over age 38 and distributed fairly evenly over the 32-to-65 age group. This trend is more than hypothetical. The General Electric Research Laboratory, founded in 1900, had come to just this kind of age distribution by 1939 on about the time scale mentioned.

Two peripheral points are worth noting. Over a 15-year period, attrition of newly hired younger people may be close to 60 percent. However, there is usually sufficient hiring at distributed ages to reduce the net attrition in

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the young group to about 25 percent. Actually, the chances are about one in six that a man hired under age 33 will remain until retirement, whereas they are one in two for a man hired over age 38. There also appear to be regular periods of higher employment rates, and the age waves thus created appear to progress through the age periods. They seem to be 6 to 7 years apart, with a 3- to 4-year half-width. Some relationship to a business cycle or to an assimilation period for new staff members may exist.

This growth in the proportion of older scientists is characteristic not only of these seven laboratories, but to some extent of the whole scientific community; it is, however, not indicative of what is happening in the U.S. population as a whole. Data (3) on the U.S. population from 1930 to 1980 (estimated) do not show an aging population. In fact, the ratio of U.S. population in the 50-to-60 age group to those in the 20-to-30 age group is predicted to decrease substantially.

Data on the scientific community are complicated by the problem of deciding whom to include. . . .

Below age 32 there will be many potential scientists who are still students, but by age 32 this complication can certainly be neglected. These curves indicate that the rapid increase in the number of those who entered the science professions in the past, which shows up in the 37-to-50 age group, is tapering off. This, perhaps, was inevitable, and will tend to bring the science community more into age balance with the population as a whole as time goes on. It seems as if the whole science profession will more and more face the same preponderance of older scientists as is now developing in the research laboratories examined.

This increasing preponderance of older scientists in professional research laboratories will render some questions and problems of increasing significance, both for the individual scientist and for the research manager or director.

One of these problems is the mundane but essential one of cost. A laboratory of the sort considered, which has stabilized at about the maximum size that its parent organization will support, must expect its overall real personnel costs to increase by about 1 percent a year because of the increasing maturity of its staff. Although this easily gets lost in much larger increases brought about by present inflationary pressures, nevertheless, over a period of 10 to 20 years it makes its cumulative contribution to financial headaches.

This leads directly to the question of productivity, demonstration and measurement of which become more and more important to the laboratory and to the individual as costs increase. The maturing scientist wants to feel sure that his increasing cost is being justified by an increasing contribution to the parent organization. The research director wants to feel sure, too, to say nothing of the parent organization itself.

Despite many studies and hypotheses, simple and quantifiable indicators of research productivity are not reliable. Physical energy and published output seem to be fairly constant for those who remain practicing scientists for their whole working careers. Yet scientific insight and selectivity, or breadth of technical contact, can increase greatly with age. Perhaps the best measure for the research director is how well these abilities are utilized for the attainment of common goals. Thus, in a maturing research laboratory, the question of relations and shared responsibilities with its parent organization becomes of increasing importance.

There are other and more personal problems, too, which are scarcely apparent in a young laboratory, but which become increasingly pressing in the maturing laboratory. For example, the mobility of personnel drops substantially after age 40, which means that the individual implicitly makes a new type of long-term commitment to the laboratory, and the laboratory to him, at about this age. Both are naturally anxious to know exactly what this commitment is or what it is likely to become.

Such commitments in a maturing laboratory are different from those in a young laboratory. Whereas, for example, the successful research scientist in a young laboratory may be reasonably certain of an opportunity to assume substantial research-management responsibilities should he wish to develop his career in that direction, in the maturing laboratory he will be much less certain of such an opportunity. There will be an increasing number of qualified candidates for every opening in research management. In the maturing laboratory, the older scientist becomes more and more committed to a continuing career as an individual contributor to research.

This in turn raises other questions. The maturing scientist who is becoming committed to a life career as an individual contributor must pay special attention to the problem of his own technical obsolescence. Too much concentration on problems special to his parent organization may result, after a few years, in a scientist's finding himself hopelessly outside the mainstream of science. A compromise must be developed by each individual scientist, who consequently becomes anxious for reassurance that the compromise he is developing is appreciated by the laboratory and is contributing to his parent organization. This may create a need for some kind of organizational recognition of his special role, a need which as a young man he did not feel, and which the maturing laboratory may find it singularly difficult to fill.

From now on, the annual number of experienced and capable scientists crossing a decision threshold around age 40 will increase steadily. This growing number of individual decisions by scientists should be regarded as an opportunity for a research laboratory and its parent organization rather than as a problem. It would seem as if the scientist is to mid-20th-century society what the lawyer was to 19th-century society. The knowledge and skills of a scientist in a technically based society can well be used in decision-making by all kinds of modern organizations. Once a scientist sees a second career in administration, management, or public affairs as a logical extension of his previous experience, he becomes part of a valuable supply of talent for business, education, or government. The attraction and use of this increasing supply of talent is both a challenge and a responsibility for research management.

If management does not meet this challenge and responsibility—and perhaps even if it does—there is likely to be an increasing spillover of the talent of mature and successful scientists into administrative and management careers in business, government, and education.

We draw the following conclusions:

1) From now on research scientists reaching maturity and the age of 40 will be well advised to examine carefully their career plans for the next phase of their working life, for the competitive situation they will face will be quite different from that which their predecessor faced during the last two decades.

2) Research-laboratory managements and their supporting organizations face a new challenge and opportunity, that of making full and proper use of the increasing flow of mature and capable scientists of age 40 and over.

3) Business, education, and government should be alerted that this flow of mature talent is at hand and that tremendous advantages could come from attracting some of it into their activities.

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D. Achievement of Eminence

Introduction

Although eminence and recognition within a scientific field is thought to be directly proportional to the quality (and to a lesser extent to the quantity) of scientific productivity, other psychological and sociological variables interact with productivity to determine not only the attainment of eminence, but also the effects of eminence upon the eminent. Among the important influencing factors discussed in the articles in this section are personality traits, type of work pattern, location within the stratification system, prestige of university, and patterns of collaboration.

Wispé compares trait ratings of 239 eminent psychologists and 136 noneminent psychologists through factor-analytic techniques. Results suggest that eminent psychologists rate high in research and conceptual skills and in professional aspiration (i.e., a willingness to assume professional obligations in order to further one's career) but low in altruism (i.e., a generalized concern for the welfare of others). Wispé hypothesizes that psychologists may achieve eminence by different routes. The "idea man," high in research skills, embodies accepted scientific values, while the "professional aspirant" is extremely active in supporting his professional association and supporting and advancing its values; it is this aspect of his professional role that eventually allows him to become eminent.

The next two selections (Cole, and Zuckerman) examine the effects on the researcher of having attained a high degree of recognition within the social stratification system of science. Cole examines a phenomenon known as the Matthew Effect (Merton, 1968) which occurs when scientists receive differential recognition for a particular scientific contribution, depending on their location in the stratification system. Cole's design allows us to look at groups of papers judged to be roughly equal in quality (as measured by number of citations) at time 2 and to see if there were any differences in these papers at time 1, depending on aspects of the author's location in the stratification system. Results showed that the assessed quality of the paper at time 2 is a more important determinant of its initial reception than any stratification variable. However, while the author's eminence makes no difference in diffusion of high-quality papers, lesser quality papers by high-ranking scientists receive greater attention than papers of equal quality by low-ranking scientists. The Matthew Effect also serves to increase retroactively the early work of scientists who go on to greater fame.

Zuckerman's sample consists of scientists who have achieved the zenith of eminence, the Nobel Prize. Their reported patterns of interaction with their colleagues both antecedent and subsequent to award of the Prize are examined. Nobel laureates publish more and are more apt to collaborate than a matched sample of scientists at every stage of their work cycle, although laureates report strain in, and termination of, collaborative arrangements soon after receipt of the Prize. As laureates report, and as their publications corroborate, they exercise *noblesse oblige* in arranging co-authorship of joint publications. Most importantly, award of the Nobel Prize is followed by decreasing productivity, especially for those who experience the larger increments of prestige through the Prize, and changed work practices because of changed obligations and activities.

The next selection considers how certain characteristics of the organization within which the scientist conducts his research can affect his productivity and, therefore, his level of eminence. Crane finds that at any stage of the career cycle, scientists at universities high in prestige are more likely to be productive and to win recognition than scientists at universities low in prestige, presumably because universities provide different environmental climates for scientific research. As for his chances of attaining recognition, a scientist gains more from an affiliation with a major university than from high productivity or from his academic sponsor's prestige, probably because the major university provides better contacts with eminent scientists in the same discipline. These later may provide important sociometric ties in communication in an "invisible college" of scientists who work a specific area of research.

One of the anticipated rewards that comes from high-quality productivity, and subsequent recognition, is movement into the professional power structure. Positions of leadership in professional societies and organizations are acknowledged to be major centers of decision-making power for academic disciplines. In a study of office holders in the American Sociological Association in the 1950 to 1965 period, Straus and Radel show that eminence is certainly one consideration for choice of officers. However in this sociological society, regional factors, with the desire to maintain an equitable balance between regions in the persons holding office, tended to mute the classic operation of the reward system of science.

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Traits of Eminent American Psychologists

Lauren G. Wispé

Psychologists attending a conference on education for research in psychology (*1*) concluded that "research is learned by doing and taught mainly by contagion," and that in this process "the senior man serves as a teacher and also . . . as a model." Additional studies of psychologists by psychologists have shown that, in the teacher-student relationship, eminence begets eminence—that is, eminent teachers train more students who, themselves, eventually become eminent (*2, 3*). This phenomenon is by no means confined to psychologists. For example, the chemistry department at the University of California, at Berkeley, before the advent of Gilbert N. Lewis in 1912, had only one man starred in *American Men of Science,* but by 1946 it had produced 22 starred chemists as well as several Nobel prize winners. Visher (*4*) has painstakingly examined the relationship, not only in the behavioral sciences but in the physical and life sciences as well, between enthusiastic, eminent young teachers and their students who eventually won recognition. From none of these studies, however, can one learn much about the general traits and behaviors which have comprised this "infusability of the productive spirit." What have been the distinguishing characteristics of eminent scientists? It was in order to gain information about this important facet of graduate training in psychology that an investigation of this subject was undertaken at the National Institute of Mental Health, Bethesda, Maryland. The data reported here were collected as part of a larger study of some of the social and psychological factors associated with eminence in psychology. For this investigation a sample of 95 operationally defined "eminent" psychologists who had received their doctorates in psychology from American universities between 1910 and 1944 and a control group matched for age, date of receiving the Ph.D., and degree-granting university were sent questionnaires on their socioeconomic and familial backgrounds, their undergraduate and graduate training, some aspects of their professional experiences, and certain personality factors. The results are reported elsewhere (*3*).

Answers to the second part of the questionnaire, which was concerned with interpersonal relationships between graduate students and faculty members during the respondents' graduate training, provided the results reported here. This part of the questionnaire began with a brief description of some possible dimensions of the interaction between graduate student and teacher. Then the respondent was asked to evaluate, on a 34-item trait-rating scale, each of the teachers by whom he was

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most stimulated as a graduate student. Space was provided for evaluation of as many as three teachers. For each of the 34 traits, a choice of five alternative answers was given, ranging from a strongly favorable to a very unfavorable response. There was also a space where the respondent could record his opinion that the item was inapplicable, or the feeling that he could not make a meaningful evaluation.

From an extensive review of the literature, three hypotheses were formulated, pertaining to the professional role of the psychologist-scientist as manifested in various kinds of institutions and to certain aspects of the student-teacher interaction. Forms were prepared on which sentences describing traits relating to these hypotheses were listed, and preliminary evaluation tests were made. The preliminary test contained about 33-percent more items than the final version. All of the items were of the same nature as those in the final test. Those items which were unclear or which seemed unrelated to the hypotheses being evaluated were eliminated. The respondents for the preliminary testing were a random sample of professional psychologists. After these preliminary tests, revisions in the sentences (or items) were made where necessary.

Of these items, those in a first group petained to the affective dynamics of the student-teacher interaction—for example, an interest in the success of one's students and a sympathetic understanding of others' problems. A second set of items pertained to professional motivation [see, for example, Boring's reference (*5*) to the will to work longer and harder than other people and the willingness to assume professional obligations]. A third group of items pertained to general research competence and the ability to conceptualize psychological problems. Also, some items were included for the purpose of assessing certain more general personality traits, such as the capacity "to establish warm and friendly relationships," the ability to channel "one's productive energies," and one's "open-mindedness," "ambition," "aggressiveness," "dominance," and "purposiveness."...

However helpful in the selection and training of future scientists information about the personalities and the interaction patterns of eminent scientists may be, the fact of the matter is that such information is peculiarly difficult to obtain. Most biographies and autobiographies of outstanding scientists show a guarded and unsystematic approach. Few people, of course, willingly expose themselves for psychological assessment, and psychologists are no exception! The results of the study in question, therefore, are all the more interesting, for the data represent a unique attempt to obtain the judgments of a sample of trained and experienced psychologists about the scientists who most influenced them during their graduate training. Few men, as I have said, willingly evaluate themselves, but many are ready and willing to evaluate their mentors, and in the latter case one would expect greater objectivity.

Since the purpose of the study was to determine the patterns of traits associated with eminence and noneminence in psychology, the teachers named by the respondents as those who had most influenced them during their graduate training were first classified as "eminent" or "noneminent." To arrive at an operational concept of professional eminence in psychology, a list of 52 psychologically oriented professional associations was submitted to a group of 25 psychologists of varying backgrounds and professional interests, with instructions to leave in the list only those groups in which membership or office was considered to be "an honor" and to cross out the names of groups which were of primarily local significance and to which psychologists had only minor commitments. In this way the investigators obtained a residual list of professional associations (*6*) affiliation with which was considered a mark of eminence in psychology. Other honors considered a mark of eminence were a starred listing in *American Men of Science*, election to the National Academy of Sciences, and award of the Howard Crosby Warren medal. The names of teachers who had been cited who were not psychologists, or who were not teaching at the graduate level, were eliminated from the sample. To preserve as much homogeneity as possible in the sample, the names of women psychologists were also eliminated. Although it is not unlikely that opinions will differ about the validity of one or another of the assumed indicators of professional recognition upon which this study was based, few people would dispute the validity of all of them, and the final sample of 239 teachers classified as "eminent" and 136 teachers classified as "noneminent" probably meets generally acceptable standards of validity.

The eminence-noneminence classification became the criterion variable and was added to the 34 trait ratings. The distributions of the trait ratings were moderately skewed toward the positive ends of the scales, and tetrachoric intercorrelations were used. The 35 × 35 matrix was subjected to factor analysis by means of Wherry's iterative method (*7*) and rotated orthogonally by graphic methods (*8*). . . . Seven factors were extracted, of which the first three were the most important and the most easily interpreted. These three accounted for 44.9 percent of the total variance. The results of the factor analysis are discussed in the next section.*

CHARACTERISTICS OF EMINENT PSYCHOLOGISTS

The item-ratings in factor 1 suggest the "altruistic professorial" type, who apparently concentrates as little upon his own sociopolitical career as upon the scientific aspects of psychology. Factor 1 suggests the selfless person who is sensitive about interpersonal relations. He is characterized as co-operative, tolerant, sympathetic, tactful, and warm in his relations with others. This factorial composition is conspicuous for moderately high negative ratings for ambitiousness, decisiveness, and dominance. As one would guess, the altruist was found to be neither predictably original, self-confident, nor industrious. Factor 1, which accounted for 21.5 percent of the total variance, contained a low negative rating for professional eminence.

Factor 2 suggests the "professional aspirant." This person is also sensitive to interpersonal relations, but in a more assertive way, and is characterized by a willingness to assume professional obligations, by purposiveness, by personal ambition, by persistence, and by a willingness to work longer and harder than others. Factor 2 contains a moderately high rating for the criterion variable, eminence. The professional aspirant of the study, like outstanding biologists (*9*) and artists (*10*), was judged to have high tolerance for frustration and a high capacity to make independent judgments—traits putatively related to ego strength. Although the professional aspirant's interpersonal relationships are unrelated to personal warmth, sympathy, and tact, he stimulates others by his enthusiasm and makes certain that those under him work up to capacity. Factor 2 accounted for 13.6 percent of the variance.

Factor 3 suggests the "idea man," who is original and creative in his thinking and brilliant in his execution of research. The idea man of the study is also erudite in matters psychological, and he recognizes his own abilities and limitations. The idea man is relatively oblivious of other people; he is not interested in his students' success, not patient, not predictably warm and friendly, not willing to assume his professional obligations, and not personally ambitious. He shows the kind of introversion—of preoccupation with ideas rather than affairs—that Cattell (*11*) found in his study of eminent scientists. The idea man of the study under discussion has few sanguine human traits; he is not predictably stable emotionally, nor does he have predictably good health. Factor 3 accounted for 9.8 percent of the variance and contained the highest rating for the criterion variable (*12*).

Factor 4 is conspicuous only for a high loading on the item pertaining to good health and for a relatively high loading on the criterion variable. Factor 5 contains fewer high ratings but suggests the kind of superego control that is associated with careful planning, the maintenance of standards, and dignified conduct. Factors 6 and 7 account for little of the variance and were not interpreted.

The results of the factor analysis suggest that when eminence, altruism, professional commitment, and research ability were considered as four characteristics of the professional psychologist, eminence was related to research ability and professional commitment but not to altruism. One may agree with Adelson (*13*), who maintains in his illuminating discussion of the "Good Teacher" that charisma, competence, and influence do not necessarily go hand in hand. Or, one may consider these results in the light of the sociopolitical and scientific values that underlie the achievement of eminence in

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*EDITORS' NOTE: The items, final orthogonal factor loadings, and communalities can be found in the original article.

professional scientific associations. Conceived as one kind of status achievement, scientific eminence reflects current professional sanction-patterns and values, and these, in turn, are reciprocally related to the way in which the professional association articulates its existence in the greater society of which it is a part. The American Psychological Association made clear in the first article of its first constitution that the Association existed for "the advancement of Psychology as a Science"; by this, as Fernberger pointed out (*14*), it meant research and contributions to knowledge. It is understandable, therefore, that factor 3, which indicates an individual who engages in the kind of activities through which such advances are made, should contain the highest rating for eminence.

However, scientific societies, like other societies, develop secondary sets of norms for regulating internal affairs—norms designed to assure the sociopolitical continuity of the association. Assuming the obligations incurred by the need for professional sociopolitical existence becomes a second kind of approved activity. Eminence therefore is also related to professional commitment (factor 2).

Taussig once noted (*15*) that the "leading and influential economists" tend toward "tough-mindedness," and this may be equally true for the leading and influential psychologists. At any rate, it comes as no surprise, in view of the quasi-impersonal moretic patterns to which science is dedicated, that the "professorial altruist" does not attain eminence. This designation for the individual suggested by factor 1 appears to have been well chosen, for to be noneminent and yet sensitive to others' needs must indeed be altruism.

INTERACTION BETWEEN PERSONALITY FACTORS AND UNIVERSITY VALUES

The prototypical question to which the social psychologist addresses himself concerns the interplay between the variables (such as intelligence, motivation, and perception) which have been traditionally conceptualized as "within the organism" and variables (such as group structure and institutional value patterns) which have been viewed as "outside the organism." In the study under discussion the problem becomes the investigation of the intelligent, professionally motivated, eminent scientist located in an incommensurable academic setting. Empirical evidence is so scanty that one cannot yet say either that gifted scientists will realize their potential regardless of the value patterns of the institutions in which they find themselves or that, given proper environmental support, men of modest attainments may still produce something of sustained social value. In this study it was possible to compare the personality characteristics of the psychologists in contrasting academic settings.

From the "cultural-institutional" viewpoint, the goal of the university is education and research, but the characteristics of the professor may vary according to which of these goals is dominant. In one type of academic orientation, characteristics of the professor that are seen as conforming to this value orientation will be rewarded by the institution with approbation in one form or another, while characteristics seen as anticanonical will be condemned; in extreme instances, teachers with the latter characteristics may be separated from the institution.

In the study under discussion, an attempt was made to categorize universities according to their research orientation, on the basis of the proportion of individuals, among those to whom the university had granted a doctorate in psychology between 1910 and 1944, who eventually achieved professional eminence. The same criteria of "eminence" were used as were used in the earlier part of the study—criteria which heavily favor professional motivation and research. According to this classification, the 11 "better" research universities, in the order of the proportion of eminent recipients of doctorates, were Harvard, Stanford, Columbia, Princeton, California, Johns Hopkins, Chicago, Illinois, Syracuse, Yale, and Clark. To this list Cornell was added because of the large number of eminent psychologists it produced just prior to 1910.

If the sample of judgments is sufficiently large and representative, as we assume it to be, the three major characteristics of the teacher-scientist may be seen as embedded in the social system of the university. Then such academic value orientations as can be inferred from, or as have been related to, the research orientations of the "better" and the "other" universities may be related to these personality characteristics.

In order to make an analysis that would reveal this relationship, it was first necessary to convert the 34 trait ratings into standard scores. These scores were then multiplied by the principal factor loadings, so that all the teachers of the sample could be given three separate factor scores. Analyses of variance were then computed for the three factors, the teachers being classified (*i*) as eminent or noneminent; (ii) chronologically, on the basis of the years during which they had taught; and (iii) according to the classification (as "better" and "other") of the universities with which they had been affiliated.

In the analyses of variance, computed for each factor separately, there were four subgroups: (i) eminent teachers at "better" universities; (ii) noneminent teachers at "better" universities; (iii) eminent teachers at "other" universities; and (iv) noneminent teachers at "other" universities. The *F* ratios for the differences between the eminent and the noneminent teachers, the "better" and the "other" universities, and the interactions, for factors 2 and 3, were all significant. For factor 1 the *F* ratios were not significant, and none of the *F* ratios were significant for the chronological dimension.

It appeared that the largest component of the variance for all factors was the eminence-noneminence classification of the teachers (see Fig. 1). As Fig. 1 shows, there was no significant difference, in the ratings of factor 1 (which suggest the "altruist"), between scores for the eminent teachers and those for the noneminent, but in the ratings of factor 2 (the "professional aspirant") and factor 3 (the "idea man"), the eminent teachers had significantly higher mean scores than the noneminent.

In Fig. 2 is presented graphically the interaction between the "personality" factors and the "better-other" classification of the universities. It may be seen that more of the eminent teachers are affiliated with the "better" universities—a finding similar to Berelson's (*16*). The eminent teachers at the "better" universities were noteworthy for professional aspirations and research ability, while the noneminent teachers at the "other" universities were rated very low on these factors, although they were rated very high on altruistic behavior. Altruistic behavior, although sometimes indicated for the eminent teachers at the "better" schools, was not notable in this group. The two most interesting comparisons came from the "mixed" subgroups. Since social selection is never perfect, not all the members of faculties of the prestigious universities were eminent. There were fewer noneminent teachers in the "better" universities, and these teachers may have been somewhat marginal, from the standpoint of the university. They were rated very high on professional aspirations and moderately high on altruism. This description suggests (i) strong personal ambition and social awareness rather than research ability, and (ii) overconcern with, and overemphasis upon, social relations, suggestive of personal insecurity and

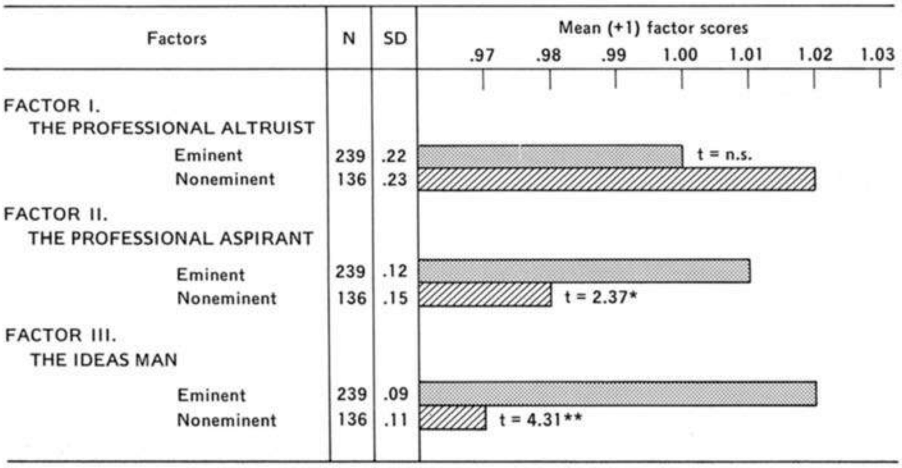

Fig. 1. Means (+1), standard deviations, and t-tests of the factor scores of the eminent and noneminent teachers.

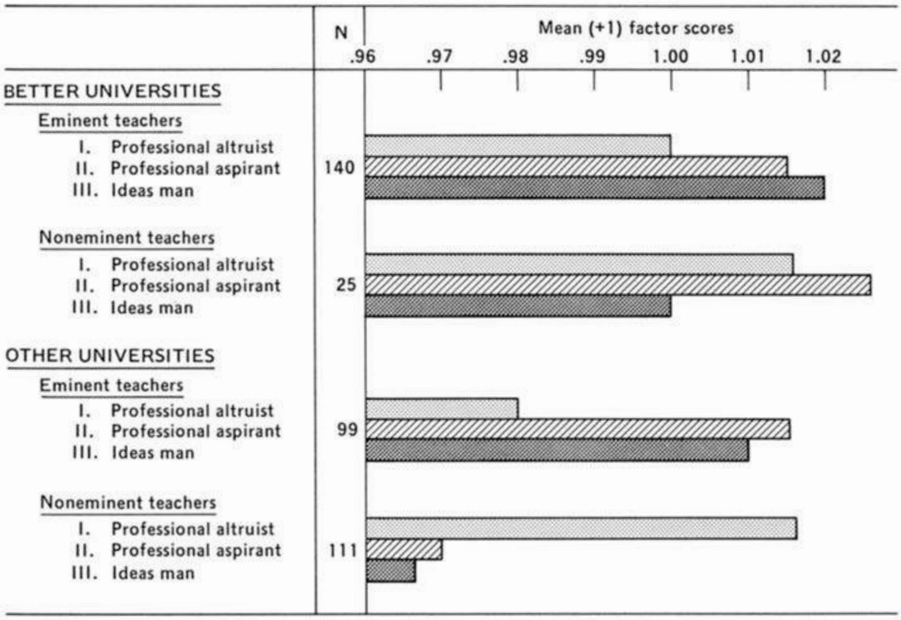

Fig. 2. Factor scores of the eminent and noneminent teachers at the "better" and "other" research universities.

uncertainty about the requirements of the professorial role in a prestigious university. In the second "mixed" subgroup there were eminent teachers at universities not conspicuous for their production of eminent Ph.D.'s. This second "mixed" group was characterized by moderately high ratings on professional aspirations and research ideas and notably low ratings on altruism. This characterization suggests aspirational and occupational disappointments reflected in unsatisfactory interpersonal relationships. This interaction of personality factors and institutional factors is nicely illustrated by the eminent groups in the "better" and in the "other" universities. Members of these two groups were given equally high ratings on professional aspirations and on ideas. On altruism, however, the eminent group in the "other" universities ranked significantly lower than the eminent group in the "better" universities. It is unlikely that the "better" universities selected their professors for their altruism; the more plausible explanation is that eminent men in an unfavorable environment became less altruistic!

SUMMARY

When the trait ratings of 239 operationally defined "eminent" psychologists and 136 "noneminent" psychologists were factor-analyzed, the results suggested that "eminence" in psychology is related to high research and conceptual ability and to strong professional commitments but not to a generalized concern for the welfare of others—labeled "altruism." These findings were interpreted in the light of the sociopolitical and scientific values by which scientific associations justify their existence.

The "idea man" embodies the primary, scientific values of the association, and despite his relative unconcern for people and power, it is hard to deny him professional eminence. The "professional aspirant," on the other hand, fulfills many of the functions that arise from the necessity for continued sociopolitical existence, and his commitment to these aspects of the association's functions, as well as his ambition and drive, eventually lead to professional recognition.

It was also possible to compare the psychologists in the universities that had "better" research orientation with those in "other" universities. As one would expect, there were more "eminent" psychologists in the "better" universities. More illuminating were comparisons of the "noneminent" teachers in the "better" universities, who were ranked high on professional aspiration and ability and moderately high on altruism, with the "eminent" teachers in the "other" universities, who were ranked high on professional aspiration and research ability but markedly low on altruism.

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Professional Standing and the Reception of Scientific Discoveries

Stephen Cole

. . . Progress in science depends upon the rate of discovery and the efficiency with which discoveries are evaluated, diffused, and incorporated into the body of scientific knowledge. For this reason, the sociology of science analyzes the social or "external" conditions which affect the processes of discovery, evaluation, and diffusion.[1] This paper presents data from a series of studies of the diffusion of scientific ideas.

We have started with the null hypothesis that sociological variables have no influence on the processes of science. Indeed, it is this hy-

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[1] Historians of science have divided the influences on the development of science into "internal" and "external" influences. By "external" they mean all those influences which are not intellectual (see Hall 1963).

pothesis which is probably held by many working scientists. The development of science is seen as the result of the internal dynamics of scientific ideas. If science were comprised of nothing more than the dynamics of ideas, it would be the most rational of human institutions; the evaluation and diffusion of discoveries would depend solely upon their intellectual substance. Neither the personal attributes of the discoverer and his location in the social structure of science nor the similar characteristics of his audience would influence the reception of the discovery.

An alternative hypothesis for this paper was presented by Robert K. Merton (1968). In his paper, "The Matthew Effect in Science," Merton suggested that in science as in other areas of human life those who are rich are likely to get richer.[2] The Matthew Effect consists "in the accruing of greater increments of recognition for particular scientific contributions to scientists of considerable repute and the withholding of such recognition from scientists who have not yet made their mark" (p. 58). Merton hypothesized that if two scientists independently make the same discovery, the considerably more eminent one will get the greater or perhaps all the credit. Likewise, if scientists of greatly differing status collaborate, the one who is most eminent will get the lion's share of the credit for the joint effort. Merton performs a functional analysis of the consequences of the Matthew Effect both for individuals and the social system. Since the Matthew Effect involves misallocation of credit, it is dysfunctional for the careers of some individuals; however, it is hypothesized that this same misallocation is distinctly functional for the communication system of science. Since the evaluation and utilization of papers in part depends upon the reputation of the author, discoveries made by eminent men or having eminent men as coauthors are more likely to be quickly incorporated into the body of scientific knowledge. "It leads us to propose the hypothesis that a scientific contribution will have greater visibility in the community of scientists when it is introduced by a scientist of high rank than when it is introduced by one who has not yet made his mark" (p. 59).

It is the purpose of this paper to empirically test the "Matthew Effect" hypothesis. Although Merton implies that the Matthew Effect applies to all scientific work, he limits his analysis to cases of multiple discoveries and collaboration. He did this since a test of the Matthew Effect is valid only if the reception of work of equal quality is considered. In a multiple discovery the work of all participating authors is of equal caliber; and in collaboration, two or more authors are producing the same discovery. When scientific papers are of unequal quality, it cannot be said to what extent differences in reception of these papers result from the position of their authors in the stratification system or from the intrinsic qualities of the papers. For this reason, an ideal test of the Matthew Effect would use multiple discoveries. However, multiple discoveries are difficult to identify, and the concept of the Matthew Effect would have greater utility should it turn out to apply to all kinds of scientific discoveries. We are interested in the influence of location in the stratification system on all discoveries. Is work of a given quality more quickly recognized and more widely diffused when the author occupies a position of eminence? To test the influence of stratification variables we must be able to control for the quality of papers.

Fortunately, the *Science Citation Index* (*SCI*) provides a useful measure of the impact or quality of scientific papers and of their diffusion.[3] By looking at the number of citations to papers, we can roughly gauge their quality. Citation counts must also be used as a measure of the speed and extent of a paper's diffusion. In all the studies reported here we have begun by counting the number of citations received by a sample of papers in the 1966 *SCI*. By control-

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[2] Merton takes the title of his paper from the Gospel according to St. Matthew: "For unto every one that hath shall be given, and he shall have abundance; but from him that hath not shall be taken away even that which he hath."

[3] In recent work most sociologists of science have accepted citations counts as an adequate measure of the quality of scientific work. For a discussion of the validity of this measure, see Cole and Cole (1967). We make no claim that the use of citations to measure quality is free from error. However, the correlation between the number of citations and other indicators of the quality of work is so high that we believe that citations may be used as a measure of quality until a more exacting one is developed.

ling for the number of citations received in 1966 we can get groups of papers that, several years after publication, have equal impact or are, in our terms, of equal quality. We then look at the citations received by the same papers at an earlier point in time, a time closer to their publication. These earlier citations tell us the extent to which papers that were utilized to the same extent at time 2 were utilized at time 1. The hypothesis is that, if assessed quality at time 2 is controlled, the assessed quality of a paper at time 1 will partly depend upon the position of its author(s) in the stratification system. To test this hypothesis, we make use of data drawn from several different studies, all of which are similar in design. All deal with citations to papers at two points in time. One study makes use of a sample of papers published in the *Physical Review;* two others of the research output of a sample of university physicists. Another study makes use of a random sample of papers from several fields of science. All studies will be described in detail as we proceed.

Table 1. Distribution of Citations in 1966 to Papers Published in the Physical Review in 1963

N Citations in 1966	*N Papers*	*Cumulative percentage*
0	342	29
1	216	47
2	173	62
3	128	72
4	89	80
5	62	85
6 or more	177	100

SPEED OF DIFFUSION

The literature of modern science, as Price (1963) has pointed out, grows at an exponential rate. In most scientific journals more than 50 percent of the references are to work published within the preceding five years.[4] In a science inundated by new literature, it is important that new ideas be recognized and utilized quickly. If a discovery is not recognized soon after it is made, there is a high probability that it will not be recognized at all. Given the importance of early diffusion, we begin the analysis with data from a study of the immediate reception of papers published in the *Physical Review* in 1963. We took a two-thirds sample of these papers and looked them up in the 1966 edition of the *SCI*. The distribution of citations in 1966 to the 1,187 papers studied is presented in Table 1. These statistics, generated from an analysis of the papers published in one of the most prestigious scientific journals in the world, suggest that most papers have little or no visible impact on later-published papers.[5] In the third year after their publication, 29 percent of the papers were not cited even once, and 85 percent received five or fewer citations. We decided to study the reception of those papers which have a *relatively* high impact, the 15 percent cited six or more times in 1966. These 177 papers received an average of ten citations in 1966 and an average of six citations in 1964, the year after their publication. The correlation between the number of citations received by the paper in 1966 with the number received in 1964 was high ($r = .72$). This indicates that papers receiving heavy use three years after publication were also likely to receive relatively heavy use in the first year after publication. The correlation, however, is not perfect; some papers that were deemed useful in 1966 went unnoticed in 1964. If the Matthew Effect were in operation, we would expect that, when the number of citations a paper received in 1966 is controlled, the location of the authors in the stratification system of science would affect the initial reception of the papers in 1964. If the response to papers were solely on the basis of their scientific content, there should be no systematic differences in the reception of the work of eminent scientists and noneminent scientists, young and old, scientists in the top departments and those in the less prestigious departments. The data are presented in Table 2.[6]

In A of Table 2 the ranking authors of the 177 papers are classified by the number of citations their other papers received in 1964.

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[4] The *Physical Review* is not only the most prestigious physics journal, it is also the most widely read journal (see Cole and Cole 1968).

[5] For more evidence on this point, see Cole (1968).

[6] The information on the author's characteristics were collected from *AMS*. In the case of academic scientists, the rank of their department was based upon ratings reported in Cartter (1966).

Table 2. Correlation and Partial Correlation Coefficients between Early Recognition of Physics Papers and Several Stratification Variables (Physical Review Sample)

Stratification variable	*Correlation with early recognition**	*Partial correlation with early recognition, controlling for quality†*
A. Scientific repute (*N* citations in 1964 to all other papers)	.23	.18
B. Rank of academic department (1963)‡	.21	.18
C. Membership status in APS (1963)	—.04	.00
D. *N* honorific awards (1963)	.13	—.04
E. Prestige of highest award (1963)	.12	.01
F. Age (1963)	.04	—.02

* Early recognition is measured by the number of citations the paper received in 1964.
† Quality is measured by the number of citations the paper received in 1966.
‡ All authors not working in academic departments were excluded from the analysis.

The number of citations to other papers may be taken as an indicator of repute based upon scientific accomplishment. Those physicists whose other work had received a large number of citations had earned a widespread reputation as a result of their past scientific success. Those whose other work had received few citations had achieved a very limited or no scientific reputation based upon their scientific accomplishment. These data indicate that scientific reputation based upon past performance does have some influence on the reception of new discoveries. The more citations a physicist's other work had received in 1964, the greater was the probability that a new significant discovery would be immediately recognized (i.e., receive a large number of citations in 1964, a year after publication). The zero-order relationship between the number of citations in 1964 to the other work of the physicist and the number of citations in 1964 to the paper under consideration is $r = .23$. In order to eliminate variation due to differences in the quality of these papers, we control for the number of citations they received in 1966 and examine the partial r. Since the partial r in A of Table 2 is .18, we may conclude that a man's reputation based upon past published work did have an independent effect on the reception of a new paper.

Another variable which had an independent effect on speed of diffusion was the scientist's institutional location: B of Table 2 indicates that men at distinguished academic departments are the most likely to have their work immediately cited. Tabular treatment of these data showed that institutional location made little difference among those with more than fifty citations to their other work; however, among those with fewer than fifty citations to other work, the physicists at the distinguished departments still were considerably more likely to have their work quickly recognized. We may conclude that if a physicist had produced work in the past that was currently being heavily utilized, the probability would be very high that a new important discovery by him would be immediately recognized regardless of where he worked. However, if a physicist had not produced important work in the past his chances of having a new discovery immediately recognized would be better if he worked at a high-prestige academic department. These latter physicists are aided by their location at strategic points in the social system of science; they are more likely to be tied into the informal communication system of their discipline.

For C of Table 2 we divided the ranking authors of the 177 papers into those who are Fellows of the American Physical Society (APS) and those who are not. Fellows are elected for having made an original contribution to physics, only 10 percent of the APS's 25,000 members hold this honor. The data indicate that Fellows are no more likely to have their papers immediately recognized than are non-Fellows. If being a Fellow is taken as an indicator of some degree of eminence, we can say that the Matthew Effect does not operate in this situation. This conclusion is supported by the data in D of Table 2. Here we have classified the authors by the number of honorific awards listed after their names in *Ameri-*

can Men of Science (*AMS*). Although the zero-order relationship between number of honorific awards and initial reception of the paper is .13, this correlation is totally due to variation in the quality of the papers. When the number of citations the papers received in 1966 is controlled, the partial r is $-.04$. Similar results were obtained when we used the prestige of the ranking author's highest honorific award as the independent variable (see E of Table 2).[7]

The findings on age are also contrary to the original hypothesis. We at first thought that older scientists who were still publishing might be more visible and therefore have their work more immediately recognized than their younger colleagues. It turns out that papers written by men under forty are no less likely to be immediately recognized than those of scientists over forty (F of Table 2). We may conclude that longevity in the field does not enhance one's chances of having papers immediately recognized.

The data of Table 2 give us a better idea of the extent to which several aspects of the stratification system of science influence the reception of papers. The most important fact is that the quality of the paper being studied, as measured by the number of citations it received in 1966, exerts the primary influence on whether or not a paper receives immediate recognition. Thus, science does closely approach its ideal of rational evaluation of work. Although scientists are clearly universalistic in evaluating new papers, several aspects of location in the stratification system have some influence, the most important of which is the scientist whose repute is based on past work that is being heavily utilized at the time of a new discovery. The Matthew Effect also operates for those people located at prestigious points of the social system of science. Contrary to our hypothesis, however, eminence, at least as measured by the data of C and E of Table 2, had no influence on the speed of diffusion of a scientist's work.

At this point, we can return to an analysis of the functional consequences of the Matthew Effect for individuals and the communication system of science. This study includes only those discoveries which were at time 2 recognized as significant; there is, of course, no way of identifying significant discoveries which are wholly overlooked as a result of the low visibility of their authors. If any such papers exist, there is a possibility that the authors might be "victims" of the Matthew Effect. Since there are no data on overlooked papers, this must remain conjecture.

What about the papers that were not overlooked? The Matthew Effect clearly involves inequities in recognition for work of roughly equal significance. Men, for example, who are not at distinguished departments and whose other work is not widely used, must wait longer for their discoveries to be recognized. Is the Matthew Effect functional for scientific communication? In the cases of multiple discovery and collaboration, the Matthew Effect is functional for science: nothing is lost, and speed of diffusion is achieved. However, when the Matthew Effect is applied to all types of scientific work, it may be seen as dysfunctional for scientific advance. Consider two papers of equal quality but of different subject matter written by authors of different rank. If one is immediately recognized and incorporated into the body of scientific knowl-

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[7] Prestige of awards was determined by a previous survey (see Cole and Cole 1967). In interpreting these data, we should take into consideration that the sample was not large enough to give us great differences in eminence as conferred through formal awards. Tabular treatment of the data showed that those physicists who were not in *AMS* fared less well than those who did appear. These data seem to indicate that only substantial differences in eminence will affect the speed of diffusion. Those scientists who are not known well enough to be listed in the *AMS* and therefore have very low visibility may have to wait longer for their work to be diffused. For evidence on the high correlation between eminence and visibility, see Cole and Cole (1968). In that paper, "visibility" was defined as the extent to which a sample of university physicists said they were familiar with a man's work. At first it may appear odd that we found no correlation between the mean number of citations papers received in 1966 and the eminence (as measured by status in the physical society and number of honorific awards) of their authors. This finding, which goes contrary to those in our previous work, is probably an artifact of the way in which the sample was chosen. Since only high-quality papers were chosen for the sample, it was unlikely that we would find significant differences in the mean number of citations *in 1966* to the papers no matter how we divided the authors. We are confident that had we included *all* papers in the *Physical Review* we would have found a correlation between the number of citations the papers received in 1966 and the eminence of their authors.

edge and the other is ignored, progress will be less rapid than if both significant discoveries were immediately recognized. If we start from the assumption of a model of complete rationality in which all significant discoveries were immediately recognized, then delay in recognition of the work of men of low rank would be dysfunctional. However, if, as we know to be the case, diffusion is partly influenced by the author's characteristics, then we can see the early recognition of the work of high-ranking men as functional. Perhaps most important, however, is the fact that a large majority of significant discoveries are immediately recognized. It is not necessary to explain the immediate recognition of the paper of a high-ranked man. It is more sociologically problematic to explain a delay in the recognition of a paper of equal quality by a low-ranked man.

THE MATTHEW EFFECT IN COLLABORATION

So far the significance of the Matthew Effect in determining the speed of diffusion of scientific discoveries has been examined. Each paper has been treated as if it had only one author. This, of course, is far from the case; the 177 papers had a total of 362 authors. We now address ourselves to the next question: how do the attributes of the coauthors affect the reception of scientific discoveries? The data tell us nothing about the dysfunctions of the Matthew Effect for some of the collaborators, but they do provide an approximate test of the hypothesized communication function of the Matthew Effect in collaboration. If the Matthew Effect is indeed at work, we should find that when a man occupying a low rank in the stratification system publishes a paper with a colleague of substantially higher rank, his work will be more quickly diffused than if his collaborators were of equally low rank. To test this hypothesis we divided the 362 authors into two groups: those who had high scientific repute on the basis of past work and those who had little or no scientific repute on the basis of past work.[8] (Authors having no collaborators were excluded from the analysis.) Then, within each of these groups we computed a correlation coefficient between the scientific repute (number of citations in 1964 to other work) of the most highly cited collaborator and the initial reception of the 1963 *Physical Review* paper.[9] We then had to compute partial *r*'s, controlling for the quality of the paper (number of citations in 1966). The results, presented in Table 3, offer clear support for the alternative hypothesis of this paper. Authors of little or no scientific repute benefit from having high-repute collaborators. Furthermore, this finding is not an artifact of differences in quality of the papers. Although the correlation coefficient is not very high, the Matthew Effect does aid the diffusion of the work of scientists who have not yet acquired a reputation on the basis of past work. When we classified the collaborators by other variables such as the number of honorific awards they have received, we found no evidence of the presence of the Matthew Effect. This was to be expected; for if honorific awards would not aid the diffusion of one's own work they were unlikely to aid the diffusion of the work of one's collaborators.

EXTENT OF DIFFUSION

So far we have limited the analysis to short-term diffusion of papers published in one

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[8] If this were not done, it would be impossible to find any variation due to repute of collaborators. If a paper had a high- and low-repute author, they would cancel each other out; the high-repute author receiving no increment in visibility from his low-repute collaborator. The validity of this procedure is supported by the data. When we lumped together the two groups of authors of Table 3, we got a zero-order correlation of .09 and a partial *r* of .05.

[9] Here again we face the problem of papers having more than one author. We could only include those papers on which the collaborators were either the only author or the first author, as this is how the *SCI* is arranged. However, in previous work we found that the correlation between the number of citations to the work on which a man was the only or first author and the number of citations to all his work was very high, $r = .96$. There was an additional problem in classifying coauthors. In cases where an author had more than one collaborator, we decided to classify the author by his most highly cited coauthor rather than adding the citations to the other work of all collaborators. However, there are some cases in which increases in visibility of work due to the visibility of collaborators might be additive. This would occur when each of the collaborators had different audiences rather than overlapping audiences. A more refined analysis would be necessary to pinpoint the effects on diffusion of various patterns of collaborators.

journal. These data may suggest that science approaches its ideal of rationality more closely than in fact it does. In designing a study that deals with a short period of time and with papers in only one journal, we may be "controlling out" the variables which produce divergences from the ideal. It is possible that some articles published in 1963 that will ultimately be deemed useful were not yet recognized in 1966. We might also find that the stratification system has a greater impact on diffusion of ideas when we consider the whole of a man's work published over a longer period of time in many diverse journals. In this section of the paper we analyze how the stratification system influences the extent of diffusion of discoveries over a longer period of time.

To begin with, the citation patterns of the work of a random sample of 1,196 university physicists were examined.[10] We were specifically interested in the most highly cited paper published by each physicist between 1950 and 1961. Since we want to study the diffusion of discoveries with relatively high impact, any scientist not having a pre-1961 paper with at least ten citations in the 1966 *SCI* was excluded from the analysis. We now wanted to see the extent to which papers judged to be of equal value in 1966 were utilized in 1961, a point in time closer to their publication. If the Matthew Effect is in operation, we should find that when quality of the paper is controlled the higher the rank of an author in the stratification system, the more widely diffused his discoveries would be in 1961.

In analyzing Table 4 we must again begin by noting that the "quality" of papers (i.e., the number of citations in 1966) is a more important determinant of extent of diffusion than any of the stratification variables. The correlation between the number of citations received in 1966 and 1961 was .69. The zero-order correlations between the stratification variables and extent of diffusion in 1961 ranged from .33 for the repute of the scientist's other work to .17 for the rank of the author's academic department. More instructive, however, is the fact that the partial correlations obtained when the number of citations to the paper in 1966 (the control for quality) is held constant are sharply reduced. The only two variables that have even a slight independent effect on reception of the papers are the prestige and number of honorific awards. These data on papers from a wide range of journals and over a longer period suggest that the Matthew Effect, in the sense it is used here, had relatively no independent influence on the diffusion of scientific ideas. Those papers which were heavily used in 1966 were also heavily used in 1961, regardless of the location of the authors in the stratification system.

Although the Matthew Effect did not influence the extent of diffusion of the "best" papers of the university physicists, it might have influenced the extent of diffusion of high-quality work done in other fields of science. We studied all papers (in the various fields of science covered by the *SCI*) which were published between 1950 and 1961 and received at least thirty citations in 1966. These papers, still heavily cited five to sixteen years after publication, represent a sample of the most significant discoveries of the time in a wide range of scientific disciplines. Would the Matthew Effect make any differences in the diffusion patterns of these ideas?

The stratification variable used was the repute of the scientist's other work as measured by the number of citations this work received in 1961. The data indicate that whether or not one's past work was being widely used did not affect the extent of diffusion of the paper. Although the zero-order correlation between citations to other work in 1961 and citations to the "super" paper in 1961 was .20, this correlation was reduced to .02 when the number of citations the paper received in 1966 was held constant. The papers of those scientists whose other work received few citations were just as widely diffused in 1961 as the papers of historically equal value published by more heavily utilized authors. On these top papers, the Matthew Effect would seem to have little influence on diffusion. Quality of work is by far the most significant determinant of early diffusion. The zero-order correlation between the number of citations these "super" papers received in 1966 and 1961 was .80.

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[10] For a description of this sample, see Cole and Cole (1967). It is a random sample of all those members of university departments granting one or more Ph.D.s each year between 1952 and 1963. We have excluded approximately 100 cases on which we could not obtain *AMS* information.

Table 3. Correlation and Partial Correlation Coefficients between Early Recognition of Physics Papers and the Scientific Repute of the Author's Collaborators (Physical Review Sample)

Scientific repute of author	*Correlation between N citations in 1964 to other work of most highly cited coauthor and early recognition**	*Partial correlation between N citations in 1964 to other work of most highly cited coauthor and early recognition, controlling for quality†*
Low (0–19 citations to other work)	.24	.24
High (20 or more citations to other work)	—.02	—.02

* Early recognition is measured by the number of citations the paper received in 1964.
† Quality is measured by the number of citations the paper received in 1966.

Table 4. Correlation and Partial Correlation Coefficients between Extent of Diffusion of Physics Papers and Several Stratification Variables (University Physicist Sample)

Stratification variable	*Correlation with extent of diffusion**	*Partial correlation with extent of diffusion, controlling for quality†*
A. Scientific repute (*N* citations in 1961 to all other papers)	.33	.02
B. Prestige of highest award (1961)	.24	.10
C. *N* honorific awards (1961)	.22	.08
D. Age (1961)	.18	.02
E. Rank of academic department (1961)	.17	.03

* Extent of diffusion is measured by the number of citations the paper received in 1961.
† Quality is measured by the number of citations the paper received in 1966.

Since it is often difficult to specify the one paper in which a discovery is presented, and since discoveries are often communicated in a series of papers, we wanted to look at the effect of stratification on the reception of all of a scientist's work. As in the case throughout the analysis, we were predominantly concerned with the reception of work of relatively high impact. Therefore, we have excluded from analysis those physicists who received fewer than twenty citations in 1966 to all their work published between 1950 and 1961.

The question is how widely diffused was the same work in 1961, a point in time closer to its publication? The data are presented in Table 5 and indicate that the more eminent a physicist was in 1961, the more widely his work was diffused. Prestige of highest award, number of awards, and rank of academic department are all correlated with the total number of citations received in 1961. Furthermore, these correlations hold up when we take into account the variation due to differences in quality of work by controlling for the number of citations received in 1966 by pre-1961 work. These findings contrast sharply with those reported above. While the Matthew Effect had little independent influence on the reception of single papers, it does influence the utilization of the whole body of a physicist's work. Since we know that the Matthew Effect had negligible influence on the reception of the "best" paper of the university physicists (see Table 4), we can conclude that the partial *r*'s of Table 5 are a result of slight but cumulative citations to the physicist's "lesser" papers. We reach the tentative conclusion that the reception of top papers will not be influenced by a scientist's position in the stratification system, but that high-ranking scientists are more likely to accumulate citations to their work of relatively small significance. Good papers do not need the Matthew Effect to attain visibility, but less significant papers benefit from it.

The consequences of the Matthew Effect

Table 5. Correlation and Partial Correlation Coefficients between Extent of Diffusion of a Physicist's Work and Several Stratification Variables (University Physicist Sample)

Stratification variable	*Correlation with extent of diffusion**	*Partial correlation with extent of diffusion, controlling for quality*†
A. *N* honorific awards (1961)	.34	.26
B. Prestige of highest award (1961)	.32	.25
C. Rank of academic department	.24	.17

* Extent of diffusion is measured by the total number of citations in 1961 to all the physicist's work.

† Quality is measured by the total number of citations in 1966 to work published prior to 1961.

in this case depend upon a question we have not yet considered: who are the early and late citers of the work under consideration? The papers generally followed a pattern of increasing diffusion. Who are the men who used a paper in 1966 but did not use it in 1961? If, in fact, the scientists producing the best work in the subject area of the paper knew of the work in 1961 and used it, then the limitation of diffusion due to the author's rank is likely to have little effect on the advance of the field. If, on the other hand, a substantial number of top scientists do not learn of the work and do not make use of it soon after it is published, then the operation of the Matthew Effect in this case may be dysfunctional for scientific advance. These data point to the next step in the analysis of the communication system of science. A study of the structural bases of awareness in science showed that knowledge flowed smoothly throughout the social system of physics; good work was known about equally to physicists in all parts of the system (Cole and Cole 1968). Recent work by Jonathan Cole (1968) showed there were more substantial differences in the utilization of science. Men located at the top levels of the stratification system are more likely to make use of high-quality science than those located at the lower levels. We are currently investigating changes in patterns of utilization over time. Who are the men who will make immediate use of scientific work destined to be heavily utilized? What kind of scientists use a discovery only after it has been recognized by others? In short, what roles are played by different types of scientists in the life history of a discovery?

THE RETROACTIVE EFFECT

So far we have analyzed the importance of the Matthew Effect in the early recognition of discoveries and in their wide diffusion. The Matthew Effect may also serve to focus attention *retroactively* on work of men who go on to become eminent.[11] To examine this aspect of the Matthew Effect we use the study of citations to the work of university physicists. We took all men who were thirty-five or younger in 1961 and counted the number of citations that their pre-1961 work received in 1961 and in 1966. We also counted the number of citations in the 1966 index to work published between 1962 and 1966. We then reversed the procedure of the earlier analysis in which we controlled for citations at time 2 and looked for variations in citation at time 1. Here we controlled for assessed quality at time 1 and looked for variation in citation at time 2. The independent or stratification variable is the number of citations received in 1966 by work published between 1962 and 1966. This procedure basically allows us to control for assessed quality of work at time 1, split up our sample into those who became more successful after time 1 and those who did not, and finally look at the assessment of the quality of *early* work at time 2. The data indicate that work which is originally cited an equal number of times in 1961 is

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[11] Merton (1968) points this out in the situation of retroactive recognition of the role of a junior scientist in collaboration: "Should the younger scientist move ahead to do autonomous and significant work, this work *retroactively* affects the appraisals of his role in earlier collaboration" (p. 58).

not cited equally in 1966. *The men who went on to publish important work after 1961 experience sharp increases in citation to their earlier work.* On the other hand, those scientists who do not go on to publish important work see a decline in the number of citations to their early work.[12] The zero-order correlation between citations in 1966 to pre-1961 work and citations in 1966 to recent work (1962–66) is .47. When we "control" for the number of citations the early work received in 1961 we get a partial correlation of .25.

One possible interpretation of these data is that the scientific community is merely making ceremonial citations to the early work of colleagues of new prominence. This seems unlikely; for ceremonial citations might be better made to the more recent work of these men, the work which has elevated their reputation. It is more likely that the audience is actually going back and reexamining the early work of men who had more recently produced outstanding and recognized contributions. It is quite possible that a physicist's later work has made his earlier work more relevant. Often scientists refer in their papers to earlier work. This earlier work may, from a purely substantive point of view, become more significant as it is developed. If this were true, then the increased rate of citations to early work may not be a result of the current location of the author in the stratification system but be a result of a real increase in the "quality" of the work. This leads to the perhaps obvious conclusion that a judgment of the quality of scientific work is time bound. Work can improve or deteriorate with age.

RESISTANCES OR DELAYED RECOGNITION

We have been examining the impact of stratification on the diffusion of scientific ideas. There is one special case of delayed diffusion that merits our consideration, that is, resistance to scientific discoveries. Historians and sociologists of science have frequently noted that important discoveries are sometimes ignored and at other times actively resisted (Murray 1925; Barber 1962; Merton 1963). Unfortunately, it has been difficult to collect the kinds of data necessary for systematic study of this phenomenon. The data on citation patterns over time can be used as the first step toward such a systematic study. We can operationally define resistance—or, as we prefer to term the phenomenon, delayed recognition—as those cases in which papers published prior to 1961 received a substantial number of citations in 1966 and few or no citations in 1961. This procedure is, of course, dictated not by the requirements of ideal design but rather by the availability of data. Ideally there should be a longer period of time between the two measurements of impact. However, we must also note that five years now probably sees more science come and go than did fifty years in the nineteenth century—the favorite hunting ground for collectors of cases of resistance.

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[12] This conclusion is, of course, not based on the correlation or partial correlation statistics since these tell us nothing about specification or interaction effects. The conclusion is based upon the tabular treatment of the data presented here:

Mean Number of Citations in 1966 to Pre-1961 Work by the Number of Citations the Same Work Received in 1961 and the Number of Citations in 1966 to the Author's Work Published between 1962 and 1966 (University Physicist Sample—35 Years of Age or Less)

	Number of citations in 1966 to work published 1962–66	*Mean number of citations to pre-1965 work in:*			*Number of men*
		1961	*1966*	*Difference*	
Heavy early citations (10 or more)	50 or more	26	46	= +20	20
	49–10	24	24	= 0	38
	9–0	21	8	= −13	29
Light early citations (0–9)	50 or more	3	27	= +24	11
	49–10	2	12	= +10	120
	9–0	2	2	= 0	380

Noting then that the design is less than ideal, let us estimate the frequency of delayed recognition and analyze some of its sociological sources. In order to identify cases of delayed recognition, we must limit ourselves to work which is indeed recognized at time 2. The sample, therefore, consists of 10 percent of the papers from all the fields of science (in the SCI) published prior to 1961 and receiving ten to twenty citations in 1966, and all those papers published prior to 1961 which received thirty or more citations in 1966. This sampling procedure yielded a list of 587 papers. Of these 587 papers, 74 or 13 percent received three or fewer citations in 1961. These papers that received at least ten citations in 1966 and three or fewer in 1961 shall be considered to have received delayed recognition. First, let us note that delayed recognition is relatively rare. On the basis of our sample we estimate that there are only 380 papers in the scientific literature published prior to 1961 that received ten or more citations in 1966 and three or fewer in 1961. Apparently the evaluation and communication system of institutionalized science operates so that only a relatively small number of papers that later turn out to be significant are overlooked at the time of their publication. Despite the rarity of the occurrence, we still wanted to investigate the sociological sources of delayed recognition, as information on deviant cases sometimes also provides knowledge on normal occurrences.

Bernard Barber (1962) suggests several possible conditions making for delayed recognition. He begins his analysis by showing that new concepts and methodologies which are opposed to existing scientific ideas are often resisted. This type of resistance may not be entirely contradictory with science's self-ideal. Skepticism about new ideas until they have been fully developed and adequately demonstrated falls well within the approved value system of scientists. Also, delayed recognition of ideas truly ahead of their time does not imply any sociological influences on science's development. In order to demonstrate the existence of an element of "irrationality" in the evaluation system of science, it is necessary to show that "resistance" is not randomly distributed, that the work of low-ranking men is disproportionately overlooked. This is, in fact, one of Barber's major hypotheses and of direct relevance for our major theme: the significance of professional standing for the reception of a scientist's work.

Barber suggests that "sometimes, when discoveries are made by scientists of lower standing, they are resisted by scientists of higher standing partly because of the authority the higher position provides" (1962, p. 550). He then goes on to give examples of the work by young and little-known scientists that are being ignored. Among these were the mathematician Niels Abel, the mathematician Ohm, and the geneticist Mendel. He also suggests that sometimes "men of higher professional standing sit in judgment on lesser figures *before* publication and prevent a discovery's getting into print" (1962, p. 552).

Barber's paper was a major contribution because it brought to our attention a problem that has itself experienced delayed recognition. However, recent research leads us to question the relevance for modern science of this type of delayed recognition. Indeed, it is possible that the sociology of science may be hampered by an overdependence on historical material. In the study of an area of human activity that has grown and progressed so rapidly as has science and has experienced such rapid institutionalization in the twentieth century, we may misallocate our research effort by assuming that phenomena of past significance are still important. Let us first consider the possibility of a decent piece of science going unpublished. We know from the work of Zuckerman and Merton (1968) that most journals in the hard sciences publish a majority of the papers submitted to them. The *Physical Review* publishes approximately 80 percent of submitted papers. The editors of these journals tell us that rejections are limited predominantly to work that is not "plausible." When we couple these high acceptance rates with the knowledge that the average papers published by the best journals are of relatively low quality and the fact that there are so many scientific journals, we may conclude that the chances of any truly valuable scientific work being refused publication are so slight as to make the problem of limited sociological relevance.

Now let us consider what we might call the "Mendel case," where a significant discovery is published and then ignored, to some extent as a result of the author not having a high status in science.[13] We suggest that this case is almost as unlikely to occur as the case of nonpublication. First, we would be unlikely to find a contemporary "Mendel" working in an obscure monastery; modern day "Mendels" would be in a university science department, or a government or industrial laboratory. Also we know from previous work that the evaluation system of science operates so efficiently that most "Mendels" would be in the top university departments (Cole and Cole 1967). We would suggest that modern science gives such great indications of universalism and rationality that the only cases today of important discoveries going unrecognized for more than a few years would be those cases of delayed recognition for truly intellectual reasons —that is, the discoveries that are truly ahead of their time.

We hypothesized that delayed recognition in modern science was either solely the result of the content of the discovery or partly influenced by differentials in visibility of the author that would lead to short-term delays in the recognition of a minority of important discoveries. The data are presented in Table 6. They suggest that the content of papers is probably more important than the social characteristics of their authors in bringing on delayed recognition. Delayed recognition papers are no more likely to be written by young men than are papers receiving immediate recognition. Likewise, institutional location and number of honorific awards both fail to distinguish the au-

Table 6. Distribution of Authors of Delayed Recognition Papers and Others (Sample of Papers from All Fields Included in SCI Having 10 or More Citations in 1966)

Characteristics of authors	*Delayed recognition papers**	*Other papers*
Age:		
40 or under	14%	15%
41–50	38	45
51–60	26	22
Over 60	22	17
	100%	99%
	(58)	(419)
Institutional location:		
Academic departments (distinguished and strong)	43%	48%
Academic departments (all others)	19	21
Nonacademic	28	28
Retired	10	3
	100%	100%
	(58)	(419)
Number of honorific awards:		
0	36%	41%
1 or 2	24	23
3 or more	40	37
	100%	101%
	(58)	(419)
Scientific repute (*N* citations to work in 1961):		
100 or more	16%	27%
99–20	32	49
19–0	52	24
	100%	100%
	(74)	(513)

* Delayed recognition papers are those which received 10 or more citations in 1966 and 3 or fewer citations in 1961.

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[13] As Barber (1962) points out, Mendel was resisted because of his low professional standing and also because his ideas were considerably ahead of his time.

thors of delayed recognition papers from those not experiencing delayed recognition. The only variable which did make a significant difference was the number of citations to the author's other work at the time of publication. This finding is consistent with those presented above and once again emphasizes our conclusion that the more one's past work is being used, the higher the probability of new work being quickly recognized and diffused.

CONCLUSIONS

Let us briefly summarize the major conclusions of this set of investigations. Perhaps most important, we have shown that modern physical and biological science does approach its ideal of universalism in the reception of scientific discoveries. All the data indicate that the assessed quality of papers at time 2 is a far more important determinant of the paper's initial reception at time 1 than is the author's rank in the stratification system at time 1; significant work tends to be utilized regardless of who has produced it. We would suggest that only a small fraction of significant work is overlooked for more than a few years. Among those papers experiencing delayed recognition, many seem to do so on account of their content and not on account of the author's location in the stratification system.

The Matthew Effect, as we have used the concept, stands for the influence of all aspects of stratification on the reception of scientific ideas. When the quality of work is controlled, the Matthew Effect can be seen to have a greater influence on the extent of diffusion of a scientist's complete work than on any particular paper. Good papers have a high probability of being recognized regardless of who their authors are; but lesser papers written by high-ranking scientists are more likely to be widely diffused early than are lesser papers by low-ranking authors. The Matthew Effect also serves to focus attention on the work of little-known men who collaborate with high-repute scientists, and possibly to retroactively increase the visibility of the early work of scientists who go on to greater fame. The generalization of the concept of the Matthew Effect has led us to raise questions about its functional consequences for scientific advance. When the Matthew Effect is applied to all discoveries of equal quality, we can see that it may result in temporarily ignoring some significant discoveries. This research points to the need of a more complete exploration of not only the conditions under which the Matthew Effect operates but also the conditions under which it is functional, dysfunctional, or nonfunctional for scientific advance.

Finally, we suggest that there is possibly greater *sociological* discontinuity between little science and big science than we have assumed in the past. The sociology of science has developed predominantly as an offshoot of the history of science. The leaders of the specialty were either trained as historians or began their sociological investigation with historical topics. It is therefore natural that latecomers to the field have been influenced by problems and perspectives which emerged from historical analysis. Recent research, however, suggests that the social organization of science has changed so drastically in this century that there may be real discontinuities between what science as a social institution is today and what it has been in the past. It is possible that resistance to scientific discovery is not a significant problem in contemporary science. It is not our purpose to suggest that sociological investigation of the historical development of science is of limited value. On the contrary, we would like to suggest that historical investigation is of great value if put in proper perspective. Certainly there is an overwhelming need for sophisticated studies of the processes of institutionalization. However, we are suggesting that when studying the current social organization of science we may be misled by use of historical examples or the assumption that past problems are also current ones.

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Nobel Laureates in Science: Patterns of Productivity, Collaboration, and Authorship

Harriet Zuckerman

This paper examines certain aspects of the work patterns and publication practices of the topmost elite in contemporary science: recipients of the Nobel prize. Nobel laureates occupy a status of highest prestige and visibility, not only among scientists but also among the better-educated segment of the general population.

In part, their prestige derives from the small absolute and relative number of laureates.[1] There are just 55 in the entire population of American scientists. This prestige is reflected in the open pride populations and organizations take in counting them among their number. Since the prize is granted on the basis of worldwide competition, social identification with the laureate is taken to cast reflected glory on the concentric social circles of which they are a part. Thus, while the president of the University of California was announcing that 36 percent of all American laureates were located in universities of that state,[2] the Rockefeller Foundation measured the success of its fellowship program by calculating that it had supported 100 laureates the world over, "almost always before [they] received their prizes."[3] And the City College of New York has even given Arthur Kornberg, Nobel laureate in medicine, a medal for having been its first alumnus to win "the prize"—probably the first time that an award has been given for having won an award.[4]

The extraordinary esteem of the laureates

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[1] On the sociological significance of absolute and relative numbers, see Georg Simmel, *Sociology*, Kurt H. Wolff (ed.), New York: Free Press, 1950, pp. 97–98. For further analysis of the point see Robert K. Merton, *Social Theory and Social Structure*, New York: Free Press, 1957 (rev. ed.), pp. 3–34, 175, 411.

[2] As reported in the *New York Times*, 26 April 1963, p. 49. This is not the place to examine the further complications of the prestige system in science as when, for example, "nouveau riche institutions" are said to hoard scientific talent. Fred Hechinger, "Stars Plague Academia, Too," *ibid.*, 27 February 1966, p. E9.

[3] Arthur Bernon (ed.), *Toward the Well-Being of Mankind: Fifty Years of the Rockefeller Foundation*, Garden City: Doubleday, 1964, p. 86.

[4] George W. Gray, "Which Scientists Win Nobel Prizes?" in Bernard Barber and Walter Hirsch (eds.), *The Sociology of Science*, New York: Free Press of Glencoe, 1962, p. 565.

is also mirrored in the prestige accorded the prize itself by the scientific community. The Nobel prize is considered the most honorific of all awards in science. All but 1% of the approximately 1,300 physicists queried by the Coles, for example, ranked it first among some hundred awards given for scientific achievement.[5] And they found also that physicist laureates had higher visibility scores—their work was more widely known—among physicists than the physicist members of the National Academy, itself an elite group.[6] As we shall see, the great prestige of the Nobel prize, both within the scientific community and the lay community, has consequences for the role performance of laureates, especially those experiencing great increments of prestige.

As one basis for studying this elite, I interviewed 41 of the 55 laureates at work in the United States in 1963, focussing on their patterns of individual and collaborative research. These qualitative materials are supplemented by data on their publication practices derived from their research bibliographies. To get a sense of how the laureates' publication practices compare with those of less distinguished scientists, I examined the bibliographies of a sample of scientists drawn from *American Men of Science* and matched with the laureates in terms of age, field of specialization, type of organizational affiliation, and initial letter of last name.[7]

Comparisons of publication and work patterns of laureates with those of the matched sample will suggest the extent to which the laureates are like other scientists and the ways in which they differ; they are not, of course, designed to show how these practices vary systematically through the entire range of scientific achievement and prestige.

PRODUCTIVITY OF LAUREATES AND LESS EMINENT SCIENTISTS

Although the laureates and their less distinguished colleagues are alike with respect to age, specialty, and type of organizational affiliation, their publication patterns differ. First, the laureates begin publishing earlier and continue to publish longer. On the average, laureates were not quite 25 years old at the time of their first papers while men in the matched sample were past 28.

To get a sense of the span of publication, we can look at the 9 prize winners and their matches who have passed the age of 70. All 9 laureates but only 3 of the paired scientists continued to publish beyond this age, suggesting that the laureates have more staying power—in part, perhaps, because they have been subjected to consistently greater expectations, from themselves and others, to remain productive and have established routines which facilitate it. The oldest laureate still at work is more than 80[8] and although he feels no *obligation* to publish any more—as he put it, "After

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[5] The Nobel prize received a prestige score of 4.98 out of a possible 5.0; three of the twenty-two physicists who did not rate it first were moved to append an explanation for their seemingly erratic judgment. Stephen Cole and Jonathan R. Cole, "Scientific Output and Recognition: A Study in the Operation of the Reward System in Science," *American Sociological Review*, 32 (June, 1967), pp. 377–379.

[6] Stephen and Jonathan R. Cole, "Visibility and the Structural Bases of Awareness of Scientific Research," American Sociological Review 33 (June, 1968), pp. 397–413.

[7] Bibliographies were received from 55 of the 123 scientists from whom they were requested, permitting comparison of 40 matched pairs of scientists. These pairs were matched in the following respects: (1) Age was ±5 years from the laureate's birthdate. When more than one bibliography was returned for a particular match, the one closest to the laureate in age was chosen; (2) Field of Specialization as listed in *American Men of Science*. Although the numbers of men in each specialty are the same for laureates and the matched sample, the proportion of papers are not, since men were not matched for productivity; (3) Type of Organizational Affiliation. Laureates were classified according to whether they worked in independent research laboratories, industrial laboratories, or universities. The scientists in the sample were matched accordingly. Pelz, among others, suggests that organizational context affects patterns of publication. Donald C. Pelz, Glenn Mellinger, and Robert Davis, "Human Relations in a Research Organization: A Study of the National Institutes of Health," Ann Arbor: Institute for Social Research, University of Michigan, 1953; (4) Initial Letter of Last Name. The reasons for matching the initial letters of last names will become clear later. See also Harriet Zuckerman, "Patterns of Name-Ordering Among Authors of Scientific Papers: A Study in Social Symbolism and Role Ambiguity," paper read at the Laboratory for Socio-Environmental Studies, National Institute of Mental Health, April 1966.

[8] Since this study was completed, the Nobel prize was awarded to F. P. Rous, who is reported as "still hard at work" at the age of 87. *New York Times*, 14 October, 1966, p. 22.

all, enough is enough"—his papers continue to appear in the journals.

The laureates not only start to publish earlier and continue longer; they also publish at a much higher rate, with a median of 3.9 papers each year since they began their careers compared with 1.4 papers per year by the matched scientists.[9] The most prolific laureate has managed to get out 10.4 papers annually—one every five weeks—for more than twenty years.[10] Only one laureate has published less than one paper annually as against twelve men in the sample. Finally, the laureates' productivity peaks somewhat later than that of the matched group of scientists; their productivity is greatest during their forties when they average 4.2 papers a year while their less eminent counterparts are most prolific during their thirties with an annual average of 1.9 papers.

THE LAUREATES AND COLLABORATIVE PUBLICATION

So far we have seen that the publications of laureates begin earlier, taper off later, and continue longer than those of the matched sample so that they have both a higher rate of publication and a considerably larger cumulative number of publications. In this sense, the institutionally evaluated importance of their work correlates with their productivity in terms of numbers of published papers.[11] We now note that the laureates are apt to collaborate more often with other scientists who are themselves distinguished and highly productive.

Laureates far more often collaborate with other laureates or laureates-to-be than do their less eminent counterparts and this is not merely an artifact of there having been eight pairs and two trios who shared the Nobel prize. Although it might be too much to say that laureates are bred by laureates, it is nevertheless the case that 34 of the 55 worked, as younger men, under a total of 46 previous prize winners. In part, this pattern of granting the prize to onetime apprentices of previous laureates may result from the greater visibility and personal ties of these men to laureate nominators. Scientists who as young men worked with a laureate received the award at an average age of 44, nine years earlier than men who had not. These age differences obtain within each of the fields where the award is given. However, high visibility and personal relations with previous laureates may not be enough to account for this. The high degree of consensus among nominators the world over suggests that the larger scientific community has judged the work of the onetime apprentices as worthy of the prize. The laureates report that they were highly selective in choosing a master and that they are now highly selective in choosing their students. Such self-selection and selective recruitment probably bring together distinguished masters and young scientists whose abilities are identifiable at a relatively early age.

As further testimony to the tendency of laureates to associate themselves with other scientists of the first rank, 15 of them collaborated with Nobel prize winners other than those who trained them or with whom they shared the prize, and these collaborations generally took place *before* any of them had been designated as a laureate. When we compare this with the total of two laureates who turned up as co-workers in the bibliographies of their counterparts, the record indicates a pattern of assortative collaboration in terms of potential or demonstrated scientific achievement. This

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[9] Median test $P<.01$, $z = 2.65$. The matched group, drawn from *American Men of Science*, is obviously more productive than the general population of scientists. Derek Price estimates that scientists publish an average of 3.5 papers in a lifetime. Derek J. de S. Price, *Little Science, Big Science*, New York: Columbia University Press, 1963, p. 45. Those men who did not supply their bibliographies may have had fewer publications than the rest but there is no reason to believe that they differed with respect to numbers of authors on papers they have published.

[10] Compared with scientists and mathematicians who are famous for their enormous productivity even the laureates' records pale somewhat. Poincaré wrote nearly 500 papers and 30 books in 34 years. James R. Newman, *The World of Mathematics*, New York: Simon and Schuster, 1956, ii, p. 1375. Cauchy's 789 papers are collected in 24 large quarto volumes and Euler had so many papers that there is not yet an accurate count of them although it is estimated that they will fill 60 to 80 large quarto volumes. Ernest T. Bell, *Men of Mathematics*, New York: Simon and Schuster, 1937, p. 139. Finally, the most prolific of all was Arthur Cayley who published 995 items or a paper every two or three weeks. Price, *op. cit.*, p. 49.

[11] The Coles find that rates of citation, one indication of scientific importance, also correlate with rates of productivity. "Scientific Output and Recognition . . . ," *op. cit.*

Table 1. Percent of Multi-Authored Papers by Date of Publication, for Laureates and a Sample of Contributors to Major Scientific Journals

Date of publication	*Contributors to journals*[a]		*Laureates*	
1900–09	25	(928)	[b]	
1910–19	31	(1,686)	34	(117)
1920–29	49	(2,148)	54	(405)
1930–39	56	(3,964)	63	(1,198)
1940–49	66	(4,918)	68	(1,301)
1950–59	83	(9,995)	62	(1,927)
Total	66	(23,639)	62	(5,512)

[a] The number of authors of articles appearing in the following journals for two of every ten years was counted and classified: biochemistry: *Journal of Biochemistry;* biological sciences: *Biological Bulletin, Genetics, Human Biology,* and *Journal of Morphology;* chemistry: *Journal of the American Chemical Society* and *Analytical Chemistry* (titled *Industrial and Engineering Chemistry* before 1947); physics: *Physical Review* and *Journal of Chemical Physics.* Since the last was not established until 1933, *Abstracts of the American Physical Society* was substituted for 1900–29.

Additional comparisons of publication practices in the behavioral sciences, humanities, mathematics, and statistics may be found in Zuckerman, "Nobel Laureates . . . ," *op. cit.,* pp. 55–104.

[b] The number of papers published by laureates in this decade is too small to justify inclusion.

Note: The base of each percentage appears in parentheses.

pattern of collaboration is much like that found in other systems of social stratification with their marked tendencies for association between those of like status.

This raises the further question of whether they are more likely than other scientists to collaborate. One way of gauging this is to compare rates of collaborative papers[12] among this elite with those of the population of scientists who contribute to journals in the same fields. As can be seen in Table 1, there is a rising proportion of collaborative papers in these journals throughout this century. But the laureates do not have significantly higher proportions of collaborative papers in any decade. This raises an interesting conjecture. The average age of journal contributors has probably decreased as larger and larger cohorts of young scientists enter the population, while the average age of the laureates who published in each decade has increased—from 28 in the period 1910–19 to 50 in the decade of the fifties. Is there, then, some pattern of correlation between age and rates of collaboration? As a first approximation to an answer, Table 2 presents age-specific rates of collaboration for the laureates and their counterparts. Laureates are more apt to collaborate, with 62 percent of their papers being multi-authored compared with 51 percent of the papers by men in the age-matched groups.[13] Moreover, this difference holds at every age. Although our data do not permit a decisive explanation of this pattern, they enable us to speculate about its sources.

Table 2. Percent of Multi-Authored Papers by Age at Publication, for Laureates and a Matched Sample of Scientists

Age at publication	*Laureates*		*Matched sample*	
20–29	58	(523)	40	(288)
30–39	65	(1,382)	55	(756)
40–49	66	(1,641)	53	(590)
50–59	60	(1,198)	51	(622)
60 or more	55	(768)	46	(264)
Total	62	(5,512)	51	(2,520)

Consider first the consistently higher rates of collaboration among laureates at each phase of the life-work-cycle. Throughout we assume only that, on the average, laureates exhibited evidence of greater scientific talent

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[12] This short paper does not examine the relation between the actual patterns of research (individual, with and without assistants; collaboration of varying numbers of peers; small team research and large-scale research) and the number of authors of papers based on these types of research formations. There is evidence that research patterns and size of author-sets are correlated, although not as closely as is sometimes assumed by the many investigators who have made use of authorship data as indicators of research practices. The question is considered in some detail, for the special case of the laureates, in Harriet A. Zuckerman, "Nobel Laureates in the United States: A Sociological Study of Scientific Collaboration," unpublished Ph.D. dissertation, Columbia University, 1965, chapter 5.

[13] Chi square for the distribution of single- and multi-authored papers is significant at the .01 level, $\chi^2 = 82.3$. In a study of 19th-century American scientists, Donald deB. Beaver reports that eminent scientists had a higher proportion of collaborative papers than others. "The American Scientific Community, 1800–60: An Historical and Statistical Study," unpublished Ph.D. dissertation, Yale University, 1966. Laureates also participated more often in large scale research reported in papers with six or more authors. Chi square for the distribution of large scale and other collaboration is significant at the .01 level, $\chi^2 = 7.01$.

than other scientists of their age and specialty, and that this set certain social consequences in motion. In their twenties, as their capacities became recognized, we assume that they were more often selected as apprentices by scientists of assured standing who were therefore more willing to grant co-authorship to apprentices than were "masters" whose standing was less elevated and secure. This would make for the higher rate of published collaboration by laureates-to-be in their youth.

The life chances of scientists whose abilities are identified early are so much better than those of the run of scientists[14] that every laureate, by the time he was in his thirties, had a position in a major university or research laboratory having other scientists in his specialty. By this time, many of the future laureates were making the status transition from junior collaborator to senior collaborator. They acquired resources[15] enabling them to surround themselves with others interested in the same problems.

Moreover, since most of the laureates-to-be had achieved substantial eminence even before the award of the prize, they tended to attract young scientists to their laboratories. As a physicist laureate put it:

> If you're any good, there are always "characters" that hover around wanting to do [things] for you . . . and it's a good thing for them to do it; they learn that way.

The laureates seem to reproduce the same patterns of collaborative work with youngsters which they experienced when they were young. This may be conceived as something like a reenactment of patterns at various stages of the life-work-cycle in which the laureates now occupy the same statuses as their masters did in the past. They are in a position to attract superior young scientists, whose contributions are sufficient to merit authorship, much as the laureates, when they were young, were also included among the authors of papers. They are also in a position, even before receiving the prize, to exercise *noblesse oblige*, the generosity expected of those occupying undisputed high rank, by granting authorship to junior collaborators. A physicist remarked on the small price he paid for being generous to students:

> . . . it clearly did my student . . . no harm at all to have me as a second author of the paper. It called people's attention to the paper who might otherwise not [have] read it at all. . . . Nor as a matter of fact, did it do me any harm, even if I was heavily responsible for it, to have him as co-author.

We cannot demonstrate that laureates are more apt than less distinguished men to acknowledge with authorship the contributions of junior associates since we would need to know how much the younger men had actually contributed. We can, however, compare the degrees and kinds of recognition given to collaborators on jointly authored papers. We can approximate a check on this conception of the laureates' reenactment of collaboration patterns through the life-work-cycle by comparing the name-orders of authors of joint papers published by the laureates and by scientists in the matched sample (whose names, it will be remembered, begin with the same initial letter).

The data are consistent with our model of the life-work-cycle. The laureates-to-be, when they were in their twenties, were first authors on nearly half of all their collaborative papers,[16] at least some of which were done in collaboration with older and more senior investigators; scientists in the matched sample, however, were first authors only a third of the time. And

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[14] For observations on the bias in favor of precocity that is built into our institutions for detecting and rewarding talent, see Alan Gregg, *For Future Doctors*, Chicago: University of Chicago Press, 1957. And for some further sociological implications of this institutionalized bias, see Robert K. Merton, "Recognition and Excellence: Instructive Ambiguities," in Adam Yarmolinsky (ed.), *Recognition of Excellence*, New York: Free Press of Glencoe, 1960, p. 310.

[15] These speculations are consistent with data reported by Walter Hirsch and James F. Singleton in an unpublished paper, "Research Support, Multiple Authorship and Publication in Sociological Journals, 1936–1964," cited in Derek J. deS. Price and Donald deB. Beaver, "Collaboration in an Invisible College," *American Psychologist*, 21 (November, 1966), pp. 1011–1018. Hirsch and Singleton show that financial support and multiple authorship are positively related. It probably has been the case that laureates have more often had access to research support than members of the sample.

[16] Collaborative papers here refer to those having at least three authors. Both the laureates and the matched sample are first authors about half the time on papers with only two authors, which would be expected if no particular preferences were being exercised in ordering names.

progressing rapidly into the role of senior collaborator, the laureates, by the time they are in their forties, take first authorship on only 26 percent of their papers whereas scientists in the matched sample take first position 56 percent of the time. The small cost of this kind of *noblesse oblige* to men who have already made their mark is illustrated by the remarks of a laureate in biochemistry:

> It helps a young man to be senior author, first author, and doesn't detract from the credit that I get if my name is farther down on the list.

Having moved early into the higher reaches of the opportunity structure of science, with its resources and obligations, the laureates are more apt to collaborate at every age than less eminent investigators. This tendency is reinforced by their ability to contribute enough to merit co-authorship and their associations with eminent men when they are young and by their willingness to share authorship with the young and talented when they are mature.

INDIVIDUAL WORKERS AND TEAM MEN

The publications of laureates in the aggregate are more often collaborative than those of other scientists. How does this work out for them as individuals? Do they tend to follow fixed styles of investigation, some being inveterate "team researchers" and others "lonely poachers" (as one biochemist put it)?[17]

The evidence suggests that the laureates are rarely at either extreme. No laureate published as much as 80 percent of his work alone although seven in the matched sample have. And at the other extreme, only two laureates published 90 percent of their papers in collaboration as compared with seven in the matched sample. The laureates are bunched in the middle sector of the distribution (Table 3), giving no comfort to those who hold that outstanding scientists are inevitably loners or to those who see them as thorough-going collaborators.

This evidence accords with the self-reports of the laureates who emphasize the flexibility of their work patterns. They sometimes work alone, sometimes in collaboration, and sometimes do both in the several researches they conduct at the same time. Differential opportunities and differential obligations for both sorts of work may account for differences between the laureates and their counterparts. The less eminent investigators more often find themselves constrained to collaborate with students, leaving them little time to work alone, and conversely, in other settings, they must work alone in the absence of other scientists who could collaborate with them.[18]

Within this context of greater freedom of choice among styles of investigation, the laureates tend to adopt that type which accords with the requirements of the problem. As one physicist observed, good problems do not always call for only one style:

> . . . different research projects require different things . . . in some cases, the major part of the work, the significant new work, can be effectively carried out by one individual and in other cases, it definitely requires participation of a number of people. If . . . an individual only chooses one [type of work] or the

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[17] Previous answers to this question have often been polarized. For a few among the many emphatic expressions of the view that great men of science are inevitably "lone wolves" see John Jewkes, "The Sources of Invention," *Lloyd's Bank Review*, New Series (January, 1958), pp. 17–28, and William H. Whyte, *The Organization Man*, Garden City: Doubleday Anchor Books, 1957, chapter 17. A contrasting position, that only group research can attack many of the basic problems in contemporary science, is put forth in M. B. Luszki, *Interdisciplinary Team Research: Methods and Problems*, New York: New York University Press for the National Training Laboratories, 1958, and G. P. Bush and L. H. Hattery, "Teamwork and Creativity in Research," *Administrative Science Quarterly*, 1 (December, 1956), pp. 361–372.

[18] There is some evidence that eminent scientists tend to have larger numbers of students—Wispé, for example, reports this for a sample of psychologists. Lauren G. Wispé, "Some Social and Psychological Correlates of Eminence in Psychology," *Journal of the History of the Behavioral Sciences*, 1 (January, 1965), p. 93. But it is also reported that the most distinguished scientists generally need not engage in more training than they wish, university administrators not being insensitive to the wishes of their "academic stars." By contrast, some men in the matched sample reported that they have no time to work alone and that all their research has been in collaboration with students. Others in the matched sample, as the only ones in their special fields at small colleges, have no opportunity for collaboration with peers.

Table 3. Distribution of Laureates and a Matched Sample of Scientists According to Percentage of Single-Authored Publications

Percentage of single-authored papers	*Number of scientists*	
	Laureates	*Matched sample*
0–10	2	7
10–20	7	5
21–40	14	7
41–60	13	8
61–80	4	6
81+	0	7
	40	40

Note: $\chi^2 = 9.14$, $P < .10$, 4 df.

> other, then almost certainly, the optimum method for getting results will not be used. . . . It's just improbable that all the problems you might be interested in, happen to fall in [only] one . . . class. It's just improbable.

Although the laureates report choosing the style of inquiry that best suits the problem, this does not mean that they do not prefer one or another style. But these personal preferences are not always translated into practice. When the functional requirements of the problem collide with laureates' preferences for one style of work or another, the demands of the problem seem to take precedence. An exchange with a physicist illustrates this type of reluctant accommodation. The interviewer remarked:

> You mentioned that working in groups is less satisfactory yet, at the same time, it seems that the kind of work you do requires it. . . .

The laureate broke in:

> It does, and it's not a good thing. It takes a lot of the joy out of doing physics.

The overriding constraint to adopt the style of inquiry most suited to solution of the problem, whatever their personal preference, is suggested by the fact that the five laureates who expressed a strong preference for working alone were no more likely to publish individual papers than the others who had no such prefrence or who preferred joint research: 37 percent of their papers had one author as against 38 percent of the rest.

Since different styles of investigation have, for many of the laureates, both attractions and repulsions, they tend not to select problems on the basis of their preference for one or another style. As a result, and as their age-specific publication patterns suggest, the laureates avoid becoming either team man or lone wolf, they oscillate between the two over the course of their careers.

SOME CONSEQUENCES OF THE NOBEL PRIZE

We turn now to a second set of questions about some consequences of having received the topmost award in science. As originally conceived, the Nobel prize was designed both as reward and as incentive: as reward for past major contributions to science and as incentive for future ones. But it appears to have some unintended consequences, at least in the short run.[19] The productivity of laureates, as indicated by the number of papers published, declines sharply in the five years following receipt of the prize. Among the thirty laureates who won the prize before 1960, productivity declined by a third, from an average of 6.2 papers per year in the five years before the award to an average of 4.2 papers a year for the five years after it.[20] However, as we have noted, the productivity of these scientists reaches a peak in the middle years and then tapers off. To assess the impact of the prize independently of this tendency toward declining productivity with aging, we need to compare the laureates with the less eminent scientists: their productivity at the same ages—in the absence of the prize—decreased only 12 percent from an average of 2.0 to 1.76 papers per year. This again sug-

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[19] In establishing the prize, Alfred Nobel intended it primarily to "help men of promise to develop further." H. R. Schück *et al.* (eds.), *Nobel: The Man and His Prizes*, Norman, Oklahoma: University of Oklahoma Press, 1951, pp. 145–146.

[20] This comparison could not be made for later winners since not enough time had elapsed to compare productivity for the full five-year periods. However, for the 11 laureates who won the prize after 1960, if we limit the comparison to the year before and after the prize, we find once again that productivity fell by 22 percent from 4.1 to 3.1 papers. During this same short interval, the productivity of their counterparts showed no change. For a more detailed examination of the social and psychological consequences of being awarded the Nobel prize, see Harriet A. Zuckerman, "Patterns of Collaboration of Nobel Laureates and Some Effects of the Prize," paper read at the annual meeting of the American Association for the Advancement of Science, December, 1964.

gests that the new eminence of Nobel laureates is associated with declining productivity.

This finding should not be taken to mean that honorific awards for scientific accomplishment *generally* lead to declining productivity. The Nobel prize is, after all, almost in a social category by itself. It confers high social visibility upon the recipients, among the general public as well as within the community of scientists, and carries with it a complex of social demands that may account for the immediate reduction in productivity. The prize quite literally assigns a new status to the laureates. The newly-crowned laureate is socially defined not only as a great man of science but also as a celebrity and a sage. The new status expands his role-set and intensifies role obligations other than that of scientific research.

By the laureates' own testimony, the new status is not an unmixed blessing. It releases new and enlarged demands by fellow scientists, university and government officials, journalists, organized groups of laymen, and—as several reminded me—visiting sociologists. All the laureates testify to this system-induced outbreak of requests for advice, speeches, review articles, greater participation in policy decisions, and other public services. . . .

One laureate developed a standardized check-list of responses to cope with the flood of requests, invitations, and demands. It reads as follows:

> Dr. Jones thanks you for your letter but regrets that he is unable to accept your kind invitation to:
>
> send an autograph
> provide a photograph
> cure your disease
> be interviewed
> talk on the radio
> appear on TV
> speak after dinner
> give a testimonial
> help you in your project
> read your manuscript
> deliver a lecture
> attend a conference
> act as chairman
> become an editor
> contribute an article
> write a book
> accept an honorary degree

Following the implications of Durkheim's analysis of the consequences of abrupt upward mobility,[21] we would assume that the impact of the Nobel prize upon productivity would differ among the laureates according to the size of status increment represented by the prize. For many of them, the prize is only the capstone of long and distinguished careers; for others, it represents a sudden and very great increment in prestige. . . .

We want, then, to compare changes in productivity following the prize among the newly eminent and among those who had previously achieved high rank.[22] Since age and productivity are related, we must also take into account the ages at which they received the prize. Table 4 indicates that the greater the increment in rank represented by the Nobel prize, the greater the immediate decline in the production of scientific papers.[23] Among both the younger and older laureates, the previously eminent experience less of a decline than those who have been catapulted into eminence and celebrity. The older laureates have a greater decline in productivity presumably because this represents the joint effect of both the prize and aging. When the laureates are compared with their age-peers among scientists, the effect of the prize seems equally disruptive

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[21] Emile Durkheim, *Suicide,* translated by John A. Spaulding and George Simpson, Glencoe: Free Press, 1951, p. 243 ff.

[22] The twenty men classified as eminent before receiving the prize were all members of the National Academy of Sciences or a foreign academy and had, on the average, three times as many awards as the non-academicians. The previously eminent had collected a total of 46 honorary degrees compared with the non-eminent who, before the prize, had not acquired an honorary degree among them.

[23] Harry Alpert has suggested that those who were not eminent prior to the prize may have received the prize sooner after completing their award-winning work and, as a consequence, that reductions in their productivity reflect a period of planning for new research. Differences between the previously eminent and the non-eminent in time elapsed between publication of prize-winning research and receipt of the prize are small: 8.9 years for the eminent and 7.3 for the non-eminent. Moreover, this difference does not persist when age at prize is held constant. It seems unlikely, then, that reductions in productivity following the prize are attributable to the need for "turn-around time" between projects. These estimates of elapsed time are smaller than the average of 12 years reported by E. Manniche and G. Falk for Nobel laureates between 1901 and 1950. "Age and the Nobel Prize," *Behavioral Science,* 2 (October, 1957), p. 305.

Table 4. Percent Change in Number of Publications in the Five Years Before and the Five Years After the Prize, by Age at Receipt of Prize for Laureates Who Were Eminent at the Time of the Prize and Laureates Who Were Not

Age at receipt of prize	*Laureates* *Eminent before prize*	*Not eminent before prize*	*Total*	*Matched sample*
49 or before	—9 (106 to 96)	—24 (197 to 150)	—19 (303 to 246)	0 (155 to 154)
50 or later	—28 (391 to 282)	—49 (77 to 39)	—31 (468 to 321)	—24 (140 to 106)
Total	—23 (497 to 378)	—31 (274 to 189)	—26 (771 to 567)	—12 (295 to 260)

Note: The numbers upon which each percent change is based are shown in parentheses.

for both the younger and older laureates who had not had a previous record of eminence during which they worked out some accommodation to the demands of high status.[24] . . .

These statistical indications of the differential consequences of the Nobel Prize are confirmed by the personal reports of the laureates. One previously non-eminent laureate contrasts his own problems af adaptation to the new demands with those of an older laureate more practiced in dealing with them:

> After not doing much work since [the prize], one gets to a kind of cross roads and has to make a decision. I want to get rid of a lot of the honors and get back to work. But how do you do it? You have to discharge a certain number of obligations and fight off new ones. That's easier said than done. Professor Brown knows how to do it. . . . He knew how to do it from the beginning. He's a little older than I and smarter anyway. He knew how to do it from the start.

It appears that scientists who became eminent before receiving the Nobel prize had gradual anticipatory socialization to the demands which divert highly placed men from their work. Other laureates had no such preparation for this aspect of their new status.

CHANGES IN PATTERNS OF WORK

If there are substantial short-run changes in the laureates' productivity following the prize, are there also changes in their collaborative relationships? Reports by the laureates suggest that their associations with co-workers change following the award and that these alterations are attributable to the prize, the strains it engenders, and the shift in role definitions occasioned by it.

Several laureates observed that the prize erects barriers between them and their colleagues, both colleagues with whom they shared the award and particularly in groups with only one winner. One physicist, musing on the personal costs of his elevated status, remarked:

> These prizes . . . separate people emotionally. Other [scientists] say, "He has received the Nobel prize. He is not quite my brother any more; he is the first born." The distance between me and them is much greater now.

Scientific collaborators are generally subject to institutionally induced pressures to make public the extent of individual contributions to their joint work, but the award of a Nobel prize seems to deepen the curiosity of the scientific community about the relative contributions of the collaborators. This interest in at-

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[24] A more detailed deviant case or *a fortiori* analysis confirms the hypothesis that the reduction of scientific productivity after the prize results primarily from enlarged social demands upon the laureates. Not included in this table are three laureates who took on major administrative posts shortly after receiving the prize and these, of course, experienced the most thoroughgoing changes in roles. Their productivity fell by 66 percent. It should be noted, however, that these were the most productive of all laureates with an average of 10.6 papers for each of the five years preceding the prize. Despite their great decline in productivity after assuming the new administrative posts, they continued to publish an average of 3.9 papers a year, more than that of the eminent non-administrators.

tributing varying degrees of credit to individual members of groups is a principal source of strain for collaborators. It forces their attention to delicate matters of credit that they prefer to keep undefined.[25] This is apt to be more stressful in research groups which acquire the heightened visibility that comes with the Nobel prize.

The source of strain among joint winners and in groups with only one winner is much the same—the feeling that recognition has not been allocated justly. For collaborators sharing the prize, the issue takes the form: Has equal recognition been given for distinctly unequal contributions? And for the research groups in which only one man has received the award, the question is: Has unequal credit been given for roughly equivalent contributions? One laureate who shared the prize with another described the tension it created among other members of the research team:

> The group couldn't have survived [us] . . . getting the prize. . . . Questions of credit and who did what started coming up.

The Nobel prize is presumably more disruptive to groups in which only one man is so honored, and particularly so when his collaborators do not see him as the principal figure in the research. The laureates tend to see the situation from their own perspective—that of the man who has received this high recognition—but they provide clues to the strain created by only one man being given the prize.[26] A physicist laureate described his own experience:

> It's unfortunate that Jones wasn't recognized somehow in this. While his contribution was not necessarily central theoretically, he was indispensable to the experiment. . . . It would have been more sensible if we had shared the prize, particularly because the two of us were in the habit of working together here and it doesn't help any in our relationship to have this separation drawn.

Assuming that scientists feel more deprived when they receive no formal credit at all than when they share it with others, research groups with only one prize winner should be under greater stress than groups in which several shared the award. A crude indicator of the extent to which the prize puts stress on research groups is the duration of collaboration following the award. For the five pairs of joint winners who were still working together at the time of the prize, the average duration of collaboration after the prize was 5.4 years as compared with an average of 3.6 years among 17 single winners and their prime co-workers. These averages obscure the fact that two of the five pairs of scientists who shared the prize are still working together.

Another category of prize-winning collaborations ends soon after the prize. These terminate not because of strains deriving from differential recognition but because younger co-winners want to establish themselves as capable of working independently without the help of their senior and more distinguished colleagues. Far from feeling that the senior men have received undue credit for the work, some of these younger laureates believe instead that they did not deserve the prize as much as the senior colleagues. . . . Having received so much recognition, these younger laureates feel

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[25] Zuckerman, "Noble Laureates . . . ," *op. cit.*, p. 338 ff. That these strains are not new in kind but only in extent is suggested by the strained relations between Cooke and Wheatstone, English inventors of the telegraph. Wheatstone continuously claimed that he had been exclusively responsible for the invention. "The quarrel became almost childish in the manner in which charge and countercharge were published during a period of some fifteen years." G. R. M. Garratt, "Telegraphy," in Charles Singer, E. J. Holmyard, A. R. Hall, T. I. Williams (eds.), *A History of Technology*, New York and London: Oxford University Press, vol. iv, p. 659. Such public quarrels, despised by scientists, are rare. At the same time, the rights of two laureates to the research for which they won the prize has been openly questioned by their collaborators. See Selman A. Waksman, *My Life with Microbes*, New York: Simon and Schuster, 1954, pp. 279 ff. for his discussion of the controversy over streptomycin and Walter Sullivan, "Who Invented the Laser?", *New York Times*, 16 January, 1966, p. E7 for details on the priority of Charles H. Townes.

[26] These strains are, of course, situationally induced; many laureates try to counteract them by repeatedly emphasizing the importance of their coworkers' contributions. These statements are not only ritualized adherence to the norm of humility, they are intended to reallocate some of the credit for joint work. Frederick Banting, co-discoverer of insulin, went so far as to share his Nobel prize money with his collaborator C. H. Best to symbolize his recognition of Best's contribution. H. Schück *et al.*, *op. cit.*, pp. 221–222.

a need to demonstrate that they can do research that matters. (This may explain why individually-authored papers by younger co-winners rise from 16 percent to 30 percent of their papers in the five years following the prize but decline for other laureates of the same age.) . . . And so they embark on new collaborations or upon individual work to prove to themselves and others that they have not been "riding on the coattails" of others, as one of them described it.

The prize brings about other changes in patterns of research. As we have seen, intensified demands following the prize reduce the time available for research. To cope with this, the laureates delegate relatively routine tasks to others which, before the prize, they would have looked after themselves. This seems to be so even for those who like doing the so-called routine work themselves. A biochemist said, with some sadness:

> I'd enjoy doing more of the technical side myself. That's the thing I've not been able to do in recent years. I just haven't the time. When I can't do [it], I don't get the same kick out of research. I feel I'm missing something when someone else does it for me.

Other laureates have elected to engage in joint research if only to get on with scientific investigation altogether. A physicist echoed his biochemist colleague:

> It's hard to find the time to work alone. Usually I'm pleased to get the problem solved, especially if it's the kind of problem you can get a research associate to do some work on.

These observations suggest that individual work might decrease after the prize and that collaboration might be substituted in its place as a means of coping with diminishing time for work. Using the percent of single-authored papers to gauge the extent of individual work, we find that, for the laureates as a whole, it remains much the same (38 vs. 40 percent). Increases are confined to men who received the prize after they were fifty and are not significantly greater than would be expected simply as a result of aging.

In summary, the Nobel prize—with its psychological and social consequences for the men who receive it—is associated with a variety of changes in work patterns. In the period directly after the prize, the productivity of laureates declines more than the productivity of rank-and-file scientists of the same age, but the social demands of the prize are more disruptive for the laureates who experienced comparatively large increments in standing than for those who were already members of the scientific elite. Changes in collaborative relationships also follow the award; prize-winning collaborations terminate sooner, on the average, when only one co-worker has been given the award than when the award is shared by several men. Laureates find new collaborators—as a group, they are not more apt to turn to individual work after the prize. These changes, neither anticipated nor intended by the Nobel Foundation, testify to the multiplicity of consequences of the most prestigious of all awards in science.

Scientists at Major and Minor Universities: A Study of Productivity and Recognition

Diana Crane

Relations between the social order and cultural phenomena are a traditional concern of sociology, recently expressed in the study of science as an institution. Related studies have attempted to discover the effects of different types of organization on scientific activity,[1] raising the question: Are scientific productivity

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[1] For example, Joseph Ben-David, "Scientific Productivity and Academic Organization in Nineteenth Century Medicine," in Bernard Barber and Walter Hirsch (eds.), *The Sociology of Science*, New York: The Free Press of Glencoe, 1962, pp. 305–328; Donald C. Pelz, "Some Social Factors Related to Performance in a Research Organization," in Barber and Hirsch, *op. cit.*, pp. 356–369.

and recognition entirely the result of achievement, or does the scientist's environment significantly influence his performance and the extent to which he receives credit for it?

Previous studies have shown that scientists located at major universities[2] are more likely to be highly productive and more likely to receive recognition than those located at minor universities.[3] . . .

The relative newness of research activities in many universities, coupled with the fact that scientists at major schools are more likely to be productive and to win recognition, suggests that universities at different levels of the academic stratification system provide rather different environments for scientific research. Various explanations for the success of scientists in certain academic environments are equally plausible, however. The best universities attract the most talented students and hire the most promising graduates. Alternatively, the institution itself, by providing opportunities and encouragement for research, may stimulate a man to greater productivity than he would exhibit in a less favorable setting. The counsel or collaboration of senior men who have been successful in research may have a favorable influence on young men starting their careers. Similarly, minor universities may attract less talented men, or prevent good men from doing their best.

The nature of the rewards expected and obtained for scientific work may also affect the productivity of scientists in major and minor universities. Since the achievement of recognition is considered a principal motive for scientific work,[4] institutionally determined influences on the allocation of recognition may affect productivity. Perhaps a position at a major university places a kind of "halo" over a man's work, so that it looks better to his colleagues than it otherwise might.[5] On the other hand, the propitious effect of the major university may actually be due to contacts it facilitates between junior men and eminent scientists who play important roles in allocating scientific rewards. The question is whether location at less prestigious institutions inhibits scientists' chances for obtaining recognition or whether high productivity is rewarded no matter where the scientist is situated.

This paper, based on my interviews with 150 scientists at three universities of varying prestige will present new information about the distribution of scientific productivity and recognition, in an attempt to assess the importance of talent and achievement relative to that of academic environment.

THE DATA

Scientists were interviewed at three universities selected from the top stratum of American educational institutions. One university was a major institution with a long tradition of graduate education and research activity. The second was a smaller institution with a tradition of research activity in some but not all its departments. The third institution was a state university that had begun to offer graduate degrees shortly after World War II. Only in recent years had its faculty been expected to do scientific research. All three universities were located in the same part of the country.

Three disciplines, representing different types of scientific research, were chosen: biology, political science, and psychology.[6] To ob-

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[2] The major universities are listed in Bernard Berelson, *Graduate Education in the United States*, New York: McGraw-Hill, 1960, p. 280.

[3] The following studies present evidence for the association between productivity and academic affiliation: Berelson, *op. cit.*, p. 127; Paul F. Lazarsfeld and Wagner Thielens, Jr., *The Academic Mind*, Glencoe, Ill.: The Free Press, 1958, p. 30; Jerome C. Manis, "Some Academic Influences Upon Publication Productivity," *Social Forces*, 29 (1951), pp. 267–272. Studies showing the association between recognition and academic affiliation include: Theodore Caplow and Reece McGee, *The Academic Marketplace*, New York: Science Editions, 1961, pp. 128–129; Harold Orlans, *The Effects of Federal Programs on Higher Education*, Washington, D.C.: The Brookings Institution, 1962, p. 158; Logan Wilson, *The Academic Man: Sociology of a Profession*, London: Oxford University Press, 1958, p. 305. Data made available to the author from the study described in Lazarsfeld and Thielens, *op. cit.* showed a similar relationship.

[4] Robert K. Merton, "Priorities in Scientific Discovery: A Chapter in the Sociology of Science," in Barber and Hirsch, *op. cit.*, pp. 454–455.

[5] Wilson has suggested that "papers submitted from major universities look better to the editors because of the institutional prestige and authority behind them." *Op. cit.*, p. 171.

[6] Since funds and time were limited, the physical sciences were excluded. Biology and political science were chosen to represent the biological and social sciences; psychology is a combination of biological and social science. Studies by Derek Price suggest that

tain respondents varying widely in age and experience, as well as a picture of research activity in entire departments, interviews were requested from everyone with at least the rank of assistant professor in each of the departments representing the three disciplines at each of the three universities. The interview schedule was designed to lead the respondent through various stages of his intellectual biography, beginning with his decision to enter his discipline. It concluded with questions about his work habits and background characteristics.

Differences among these institutions with respect to the research activity conducted in them are difficult to assess, because departments within a particular institution are not of uniform quality and often change in this respect from decade to decade. Keniston's lists of the top 15 or 20 departments in 24 disciplines, in 1925 and 1957, provide empirical evidence of such changes.[7] At the major university, two of the departments in which interviews were conducted were among the top five on Keniston's 1957 lists, and the third, which had been improving steadily, would probably have been included in the top group if the Keniston survey had been done more recently. Only one of the departments studied at the other two universities was mentioned by Keniston. A national study of that discipline also rated the same department highly.[8] While considerable changes in the quality of these academic departments could have taken place since 1957, it seems reasonable to assume that comparisons between major and minor universities in this study, in general, involve comparisons between departments of high quality and departments of lower quality.

In constructing indices of productivity and recognition, scientific norms for evaluating publications and honors were taken into consideration. For example, it seemed evident that scientists who evaluate each other's work give greater weight to comprehensive treatment of a subject than to a brief presentation of a single aspect of it. The index of productivity devised for this study reflects the peculiarly cumulative character of scientific activity. . . .

The index of productivity[9] distinguished between major and minor publications. A book was considered a major publication. A series of four journal articles, each one of which explored some aspect of a single problem or closely related problems, was considered approximately equivalent to a book in terms of contribution and effort and was treated as a major publication. On the assumption that a scientist's work is evaluated chiefly in terms of his major publications, those who had achieved a major publication were given no further credit for minor publications. Productivity was assessed in relation to "professional age" (the number of years since obtaining the Ph.D.). The number of major publications achieved by approximately the top third of each "professional age" group was considered high productivity.

Thus, the index reflects a particular style of scientific work. The productivity of individuals who make single contributions to a number of different areas is underestimated by this measure, but this pattern is probably more characteristic of the "soft" sciences, such as political science, than it is of the hard sciences. In the soft sciences, however, a single contribution is more likely to be a book and hence would qualify as a major publication.

The criterion used to assess the recognition received by respondents was the number of honors which they had won.[10] To take into

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the processes to be described here are more exaggerated in the physical sciences, where membership in the "invisible college" of colleagues (which is related to university affiliation) is almost essential for scientific productivity and recognition. See Derek Price, *Little Science, Big Science,* New York: Columbia University Press, 1963, pp. 62–91.

[7] Hayward Keniston, *Graduate Education and Research in the Arts and Sciences at the University of Pennsylvania,* Philadelphia: University of Pennsylvania Press, 1959, pp. 115–150.

[8] Kenneth Clark, *America's Psychologists,* Washington, D.C.: American Psychological Association, 1957.

[9] Other indices of productivity are described in Lazarsfeld and Thielens, *op. cit.,* p. 403; Bernard Meltzer, "The Productivity of Social Scientists," *American Journal of Sociology,* 55 (1949), p. 26; Leo Meltzer, "Scientific Productivity in Organizational Settings," *Journal of Social Issues,* 12 (1950), pp. 32–40.

[10] Other methods of measuring recognition are reported in Clark, *op. cit.;* Dennis, *op. cit.,* p. 180; Lazarsfeld and Thielens, *op. cit.,* p. 403; Bernard Meltzer, *op. cit.,* p. 26; Donald C. Pelz, "Motivation of the Engineering and Research Specialist," General Management Series, No. 186, New York: American Management Association, 1957, p. 30; Elbridge Sib-

account the fact that certain scientific honors are more sought after than others, the index contained two categories of recognition. The higher category included the presidency of a national professional association or special purpose association, membership in certain honorary societies,[11] and of course the Nobel Prize. An honorary degree from a university that was neither the respondent's *alma mater* nor his current appointment was also included in this higher category.

Somewhat less prestige is conferred by honorary post-doctoral fellowships such as the Guggenheim, Rockefeller, National Research Council, National Science Foundation fellowships; a year's leave at the Institute for Advanced Study in the Behavioral Sciences; National Institutes of Health career development awards; any prize (other than the Nobel Prize) given for outstanding work in a scientific field; service on a government advisory board, study section or committee, or service on an editorial board of a journal.

Again, the amount of recognition obtained by approximately the top third of each "professional age" group was considered a high degree of recognition. One could argue that the index represents a particular kind of recognition and one more likely to be obtained by an "operator" than by a "scholar." In other words, it may measure "success" more than "merit." To measure the latter, however, it would be necessary to obtain ratings of respondents' work by a sample of men in each field.

PRODUCTIVITY AND ACADEMIC STRATIFICATION

The setting in which a scientist receives his training has more effect on his later productivity than the setting in which he works afterwards. A scientist trained at a major university was more likely to be productive than one who had been trained at a minor institution (see Table 1). When the prestige of the scientist's current academic affiliation was related to scientific productivity, the proportion of highly productive scientists declined from one prestige level to the next, but scientists trained at major universities were likely to be highly productive regardless of where they were presently located (Table 2). Scientists trained at minor universities were unlikely to be highly productive unless they were located at the major university.

Table 1. Productivity by Prestige of Graduate School*

	Prestige of graduate school†	
Productivity	*Top 12 universities*	*Other universities*
High	45%	18%
Low	55	82
Total	100	100
(N)	(86)	(56)

$\chi^2 = 11.40$, 1 df, $p < .01$

* This table does not include eight respondents who were trained outside the U.S. Prestige of undergraduate school is not related to productivity.

† Based on Berelson's ratings (*op. cit.*, pp. 280–281).

This pattern of scientific productivity might be explained by the superior talent of the Ph.D. graduates of major universities. Berelson shows that the major universities admit only half their graduate applicants while other universities admit almost three-quarters of theirs.[12] On the other hand, previous studies indicate that graduate students choose their schools on the basis of incomplete information about the quality of academic departments and that non-intellectual reasons play a significant role in their choices.[13] This suggests that superior students do not necessarily enroll in the

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ley, *Support For Independent Scholarship and Research*, New York: Social Science Research Council, 1951, pp. 35–36, 95; S. S. Visher, *Scientists Starred 1903–1943 in "American Men of Science,"* Baltimore: The Johns Hopkins Press, 1947, p. 18; Logan Wilson, "Prestige Patterns in Scholarship and Science," *Southwestern Social Science Quarterly*, 23 (1943), p. 317.

[11] E.g., The National Academy of Sciences, the American Academy of Arts and Sciences and the American Philosophical Society. For a discussion of the ways in which members are elected to these societies, see "Appendix A: The Techniques of Major National Institutions Which Now Recognize Mature Excellence," in *Recognition of Excellence*, Working Papers of a Project of the Edgar Stern Family Fund, Glencoe, Ill.: The Free Press, 1960, pp. 174–204.

[12] Berelson, *op. cit.*, p. 111. He also found that winners of national fellowships were most likely to attend top graduate schools.

[13] Caplow and McGee, *op. cit.*, p. 225: Berelson, *op. cit.*, p. 143.

Table 2. Productivity by Prestige of Graduate School and Prestige of Current Academic Affiliation

	Prestige of graduate school			
	Top 12 universities		*Other universities*	
	Current academic affiliation		*Current academic affiliation*	
Productivity	*Major university*	*Minor university*[a]	*Major university*	*Minor university*[a]
High	48%	41%	36%	12%
Low	52	59	64	88
Total	100	100	100	100
(N)	(52)	(34)	(14)	(42)

$x^2 = 14.41$, 3 df, $p < .01$

[a] Includes both the small private university and the state university.

major universities.[14] Nevertheless, a student's choice of graduate school has far-reaching effects on his academic career, since major universities tend to hire their own graduates and those of other major schools.[15]

In assessing the relative importance of ability and training as determinants of productivity, an important area to examine is the student's relations with his teachers. A productive scientist is no doubt more likely to transmit the appropriate skills and values to his students. The only information about the research careers of respondents' sponsors readily available was their prestige. Since prestige is related to productivity,[16] a prestigious sponsor can reasonably be assumed to be a productive one. Those who had had prestigious sponsors were indeed more likely to be productive. This relationship was not affected when those who had had close relations with prestigious sponsors were separated from those who had not.

Since eminent sponsors were more likely to be located at major graduate schools, the school rather than the teacher might have been the important variable. Prestige of graduate school does affect productivity, but an unprestigious sponsor at a major school was unlikely to produce a productive scientist (see Table 3). This suggests that the student's ability may be a more crucial factor than the prestige of his sponsor. Presumably, eminent sponsors attract, or select, the best students in their departments, while less talented students, though admitted by the major universities, are not capable of high productivity. Similarly, eminent sponsors at minor universities also trained productive scientists but they were less likely to do so than were eminent scientists at major universities.

Thus, the training of a scientist may be regarded as an increasingly selective process in which most of the best students are channelled into the best graduate schools and, in turn, the best of these are selected for training by the top scientists. This highly select group becomes the next generation's most productive scientists, most frequently chosen for positions

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[14] Orlans, using two categories of universities which, though not identical with Berelson's ratings, are probably comparable, found no difference in the mean intelligence scores of their Ph.D. graduates. He did find substantial differences in the scores of entering students but apparently the poorer students drop out, so that the level of graduates is the same. However, since the I.Q. test does not measure many of the productive scientist's most significant qualities, such as his motivation, persistence, and originality, these findings are not very meaningful. (Orlans, *op. cit.*, pp. 4, 157).

[15] Berelson, *op. cit.*, p. 109.

[16] Wayne Dennis, "Bibliographies of Eminent Scientists," *Scientific Monthly*, 79 (1954), pp. 180–183, reports that most eminent scientists in the 19th century had published a considerable amount. Kenneth Clark found that eminent psychologists had a substantially larger number of publications than other psychologists in a group initially selected as "high producers" in psychology. In other words, among the most productive members of the entire discipline, those who were eminent were more productive than the rest (*op. cit.*, pp. 31–32, 45–46). Lazarsfeld and Thielens report that the most productive members of their sample were also the most eminent (*op. cit.*, p. 9), and the same relationship was found in the present study.

Table 3. Productivity by Prestige of Sponsor and Prestige of Graduate School

	Prestige of sponsor			
	High prestige of graduate school		*Low prestige of graduate school*	
Productivity	*Top 12 universities*	*Other universities*	*Top 12 universities*	*Other universities*
High	56%	32%	21%	11%
Low	44	68	79	89
Total	100	100	100	100
(N)	(57)	(19)	(28)	(35)

$\chi^2 = 22.43$, 3 df, $p < .01$

Table 4. Current Academic Affiliation by Prestige of Sponsor and Prestige of Graduate School

	Prestige of sponsor			
	High prestige of graduate school		*Low prestige of graduate school*	
Current academic affiliation	*Top 12 universities*	*Other universities*	*Top 12 universities*	*Other universities*
Major university	63%	32%	54%	20%
Minor university	37	68	46	80
Total	100	100	100	100
(N)	(57)	(19)	(28)	(35)

$\chi^2 = 18.61$, 3 df, $p < .01$

in major universities (Table 4). Graduates of major universities were likely to be located at the major university, regardless of their sponsors' prestige, while students of eminent sponsors at minor universities were not so favored. This suggests that ascriptive elements in the academic stratification system do affect the careers of talented scientists who do not attend top graduate schools.

THE UNIVERSITY AS A RESEARCH SETTING

Whatever influence a minor university has on the productivity of its faculty appears to affect graduates of minor universities more than those of major universities. The latter are as productive in the two minor universities as they are in the major one. Studies done in other settings have made it increasingly clear that more than talent and training go into the making of a scientist.[17] Two important factors are motivation and good judgment regarding the selection of research topics. Relatively little has been written about the factors influencing the selection of research topics by scientists.[18] Textbooks on methodology discuss the intellectual components of such choices but not the effects of the nature of scientific activity, on the one hand, or those of the scientist's immediate institutional environment, on the other. When asked about their research decisions, scientists in this study were likely to respond in terms of any or all of these factors.

The highly cumulative nature of scientific activity affects scientists' attitudes toward the selection of research topics. Many of the respondents expressed the point of view exemplified in the following comment by a psychologist in the sample:

> There are no substantial contributions of the hit-and-run type. Substantial contributions come from ten to twenty years of working on

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[17] See Morris I. Stein and Shirley J. Heinze, *Creativity and the Individual: Summaries of Selected Literature in Psychology and Psychiatry*, Glencoe, Ill.: The Free Press, 1960; Calvin W. Taylor and Frank Barron (eds.), *Scientific Creativity: Its Recognition and Development*, New York: John Wiley, 1963.

[18] Two well-known references are W. I. B. Beveridge, *The Art of Scientific Investigation*, New York: Random House, 1957; Robert K. Merton, "Problem-Finding in Sociology," in Robert K. Merton, Leonard Broom, and Leonard S. Cottrell, Jr. (eds.), *Sociology Today*, New York: Basic Books, 1959, pp. ix–xxxiv.

basically one problem area and really getting to understand it.[19]

. . . Some scientists could see continuity in their research, stemming from their training in graduate school, while others said that their work developed continuity after they had obtained their degrees. Discontinuity in research occurred when the scientist shifted from one area of research to another, either frequently or perhaps at one or two points in his career. This group should be distinguished from those whose research covers several areas, for they leave an area entirely instead of temporarily. Success in scientific work is clearly related to continuity in research. A scientist who is unsuccessful with a particular type of research never develops continuity in it; a scientist who wants to develop a new research interest is careful to keep up an old one so as to cut his losses if the new venture fails. One man who had switched to a new research area complained, "You can be more productive if you stay in the same area. You can just keep grinding out the stuff."

Since this aspect of scientific activity affects productivity, the productivity index used in this study depends partly on continuity in the selection of research problems. The scientist's own estimate of the amount of continuity in his work was correlated with high productivity in biology and in psychology, but not in political science. Since political science is still largely a non-quantitative science, one would expect research contributions to be less cumulative, though many political scientists did find considerable continuity in their work.

Scientists at the state university were less likely to report continuity in their research than scientists at the other two universities; presumably this circumstance affected their productivity. The interviews revealed two principal sources of discontinuity in their research interests: (a) During their graduate training, they had not become involved in a productive research area. (b) They were unable to pursue research interests developed in graduate school because facilities were lacking in their new institutional settings. . . .

Scientists at the state university, many of whom had been trained at minor universities, were less likely to have studied with highly productive scientists who could either involve their students in their own work or provide them with an adequate model of the optimal way to proceed. This suggests why graduates of minor universities were less likely to be productive than graduates of major universities.

Scientists interviewed at the state university indicated that a young scientist who does not have a research interest, or is unable to do the kind of research he would like to do, is highly dependent on the opportunities that his institution provides. The most typical pattern for the development of a new research interest after the Ph.D. was through collaboration, either formal or informal, with an older and already productive scientist. . . .

Such opportunities were more readily available at both the major university and the small private university. Evidence from the interviews suggested that a young scientist must develop a research program within the first few years after receiving his doctorate. Otherwise he is likely to become so demoralized, as far as research is concerned, that he functions ineffectively, even after he does succeed in getting some research started.[20] He is likely to lose confidence in his ability to do successful research. . . .

A position at either of the minor universities seemed to delay, rather than inhibit altogether, a scientist's productivity. At both the minor universities, a man's first major publication occurred later in his career than at the major university, as is shown in Table 5. Evidently, unproductive scientists either lack a commitment to a particular research area or are prevented by a variety of environmental conditions from pursuing one.

A second important factor in the development of a productive scientist is his motivation for scientific work. As Pelz has shown,[21] the motivation for scientific work can come

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[19] An eminent biologist has expressed a similar opinion: "It seems probable that co-ordinate progress in research, progress characterized by a natural development from one group of ideas to another, instead of a flitting from interest to interest in a quite inconsequential manner, is conducive to persistent effectiveness in productive scholarship." Walter B. Cannon, *The Way of an Investigator*, New York: W. W. Norton, 1945, p. 218.

[20] As Price has shown, a large proportion of scientists produce no more than three or four papers. *Op. cit.*, Ch. 2.

[21] Pelz, *op. cit.*, p. 28.

Table 5. Publication of First Major Work by Current Academic Affiliation*

Publication of first major work (number of years after Ph.D.)	*Current academic affiliation*		
	Major university	*High minor university*	*Low minor university*
0–5 years	72%	56%	43%
6–10 years	22	35	19
Over 10 years	6	9	33
No information	0	0	5
Total	100	100	100
(N)	(54)	(23)	(21)
$x^2 = 14.16$, 4 df, $p < .01$			

* Includes only those with major publications.

from a desire for recognition by other members of a scientific discipline or from a desire for advancement in an organization. There was some evidence in the interviews that these types of motivation are differently distributed in the academic stratification system, though this type of information was not obtained from all members of the sample. At the state university and, to a lesser extent, at the small private university, motivation was more closely related to the attainment of rewards from the university administration, such as promotion, tenure, and salary increases. Although the administration's attitude toward research was almost never mentioned spontaneously by respondents at the major university, it came up fairly frequently without prompting by the interviewer at the other two universities. Respondents at the state university said that the administration was pressuring them to do research without fully appreciating the difficulties they encountered in setting up research programs.

Since recognition from the discipline is related to the prestige of the scientist's academic affiliation, rewards for the achievements of a scientist at a minor university are likely to come primarily from the institution itself. As in other stratification systems, those at the lower levels, both students and faculty, are less likely to develop the type of motivation—in this case, the desire for scientific recognition—necessary for achieving the goals set by the discipline, and less likely to be rewarded for their accomplishments if they do.[22]

In addition, motivation for scientific work seemed less intense at the state university than at the other two universities. Scientists there were less likely than those at the other two universities to devote more than 25 per cent of their time to research. . . .

Since the distribution of recognition is related both to academic affiliation and to productivity, the interaction between these three variables has important implications for the motivation of scientists at major and minor universities, and I shall turn to this three-way relationship now.

RECOGNITION, PRODUCTIVITY, AND ACADEMIC AFFILIATION

Previous studies have found that later eminence is associated with the prestige of the institution from which the scientist received his doctorate,[23] but in this sample, recognition was related only to the prestige of scientists' current academic affiliations. . . . Although respondents who had attended major graduate schools were most likely to be located at the major university, current academic affiliation was a more important factor than prestige of graduate school (see Table 6). Having attended a major graduate school did not improve a scientist's chances of receiving recognition, if he was currently located at a minor university, though chances of recognition were very good for the few graduates of minor universities now at the major university.

These figures suggest that the major universities send their less talented graduates to

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[22] Melvin Tumin, "Some Principles of Stratification: A Critical Analysis," in Lewis A. Coser and Bernard Rosenberg (eds.), *Sociological Theory*, New York: Macmillan, 1957, p. 430.

[23] Berelson, *op. cit.*, p. 109; Caplow and McGee, *op. cit.*, p. 225.

Table 6. Recognition by Current Academic Affiliation and Prestige of Graduate School

	Current academic affiliation			
	Major university prestige of graduate school		*Minor university prestige of graduate school*	
Recognition	*Top 12 schools*	*Other schools*	*Top 12 schools*	*Other schools*
High	56%	75%	30%	24%
Low	44	25	70	76
Total	100	100	100	100
(N)	(41)	(8)	(30)	(34)

$\chi^2 = 13.70$, 3 df, $p < .01$

minor schools, and that a major university selects the more talented individuals from the ranks of those who attended minor universities, as the high likelihood of winning recognition in this group indicates. Other explanations must also be considered. Since scientific rewards are allocated by scientists rather than by institutions, an important factor in determining whether or not a scientist receives recognition is his visibility to his colleagues outside his university. Location at a major school may increase a scientist's visibility to others in his discipline, through his contacts with eminent senior scientists in the major-university setting,[24] and the mere fact that he has a position at a major university may enhance his work in the eyes of his colleagues. Presumably, too, high productivity attracts the attention of fellow scientists.

Although the very productive were most likely to have won the highest honors, highly productive scientists at the major university were more likely to have won recognition than highly productive scientists at a lesser school (Table 7). The latter were, in fact, no more likely to win recognition than unproductive men at the major school. Evidently, productivity did not make the scientist as visible to his colleagues in his discipline as did a position at a major university.

Some scientists, especially those from the best schools, were the students of eminent professors and were probably brought into contact, while they were students, with important members of their specialties at scientific meetings and conferences and during informal visits. Men who had been students of eminent sponsors did not report frequent contacts with other members of their specialties more often than did students of other men, but students of eminent teachers were more likely than others to have received recognition. Perhaps their sponsors brought their work to the attention

Table 7. Recognition by Productivity and Current Academic Affiliation

	Current academic affiliation			
	Major university productivity		*Minor university productivity*	
Recognition	*High*	*Low*	*High*	*Low*
High	67%	40%	44%	25%
Low	33	60	56	75
Total	100	100	100	100
(N)	(27)	(25)	(18)	(51)

$\chi^2 = 12.57$, 3 df, $p < .01$

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[24] More than 15 years ago, Visher expressed doubt as to the objectivity involved in selecting members of the National Academy of Sciences. He listed as an important requirement for election: "the possession of at least two influential friends in the section of the Academy which must first approve of his election, friends who are actively working for his election. Without such support, a man has almost no chance of election." *Op. cit.*, p. 4.

of important colleagues even though as students they did not have much personal contact with these eminent figures.

No matter where the eminent sponsor was located, his students were more likely to have won recognition (Table 8). Eminent sponsors also helped former students now located at a minor university (see Table 9), but recognition was more likely to come to those at the major university, regardless of the sponsor's prestige. Relatively unproductive scientists were also more likely to win recognition if they had had eminent sponsors, but high productivity won recognition without an eminent sponsor (see Table 10).

The three variables—productivity, academic affiliation, and prestige of sponsor—can now be assessed in terms of their relative effects on a scientist's chances of obtaining recognition. A scientist is more likely to gain recognition from high productivity than from his sponsor's prestige, and from affiliation with a major university more than from productivity.

Location at a major university may lead to

Table 8. Recognition by Prestige of Graduate School and Prestige of Sponsor

	Prestige of graduate school			
	Top 12 universities Prestige of sponsor		*Other universities Prestige of sponsor*	
Recognition	*High*	*Low*	*High*	*Low*
High	50%	36%	50%	17%
Low	50	64	50	83
Total	100	100	100	100
(N)	(48)	(22)	(16)	(24)

$x^2 = 8.20$, 3 df, $p < .05$

Table 9. Recognition by Current Academic Affiliation and Prestige of Sponsor

	Current academic affiliation			
	Major university Prestige of sponsor		*Minor university Prestige of sponsor*	
Recognition	*High*	*Low*	*High*	*Low*
High	58%	53%	37%	18%
Low	42	47	63	82
Total	100	100	100	100
(N)	(36)	(15)	(30)	(34)

$x^2 = 19.75$, 3 df, $p < .01$

Table 10. Recognition by Productivity and Prestige of Respondent's Sponsor

	Prestige of sponsor			
	High productivity		*Low productivity*	
Recognition	*High*	*Low*	*High*	*Low*
High	58%	38%	70%	20%
Low	42	62	30	80
Total	100	100	100	100
(N)	(31)	(34)	(10)	(40)

$x^2 = 14.64$, 3 df, $p < .01$

Table 11. Recognition by Continuity of Research and Current Academic Affiliation

	Current academic affiliation			
	Major university Type of continuity		*Minor university Type of continuity*	
Recognition	*Pre-Ph.D. continuity*	*Post-Ph.D. continuity or discontinuity*	*Pre-Ph.D. continuity*	*Post-Ph.D. continuity or discontinuity*
High	54%	60%	44%	17%
Low	46	40	56	83
Total	100	100	100	100
(N)	(35)	(20)	(25)	(41)

$\chi^2 = 15.61$, 3 df, $p < .01$

recognition either because it casts a "halo" over a man's work or because it provides contacts with eminent scientists. The latter hypothesis is favored by evidence of an alternate route to recognition. Some informants stressed the importance of working for a substantial period in one area as a means of enhancing one's reputation. . . .

Indeed, respondents who reported continuity in their research interests proved more likely to have won recognition. Of course, this correlation could be interpreted in two different ways. A man who is successful with a particular type of research is most likely to continue it; on the other hand, continuity in research, contact with colleagues outside one's own university and recognition were associated. Perhaps scientists whose work is continuous are more likely to come into contact with colleagues who are in a position to confer scientific rewards.

Recognition was associated with continuity in research, regardless of the prestige of the scientist's sponsor, but this relationship was affected by institutional affiliation. Continuity was related to recognition at the minor universities but not at the major university (see Table 11). This suggests that continuity may be a means whereby scientists at minor universities make contact with eminent scientists and thus improve their chances of obtaining recognition. Scientists at major universities have more contact with eminent scientists and can therefore shift from one line of research to another without jeopardizing their opportunities for recognition. . . .

Eminence, Productivity, and Power of Sociologists in Various Regions

Murray A. Straus and David J. Radel

The recent revision of the constitution of the American Sociological Association (ASA) was stimulated in part by the objections of sociologists in certain regions to what was believed to be a regional bias in the control of the association. During the several-year period of the gestation and birth of this new constitution, it is remarkable that the controversy was carried on without benefit of empirical data on this aspect of the need for constitutional revision. Although it is obviously past the time (if it ever existed) when empirical data might influence the process of constitutional revision, the issue of a possible regional power bias remains important to sociology as a profession. This issue is also of more general importance for the understanding it can provide concerning the social organization of science, including, besides regional variations, such matters as the functioning of scientific societies and the relationship between productivity, eminence, and office-holding of individual sociologists.

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SAMPLE AND METHOD

The investigation took as its starting point and hypothesis the views of certain of the authors' colleagues who asserted that control of the ASA was disproportionately in the hands of sociologists in the Northeastern states. In this study, the notion of control and power is limited to the exercise of the influence within the ASA that is presumed to be associated with holding one of the constitutionally specified offices. For this purpose, an officeholding score was computed for each officer listed in the 1950 through 1965 volumes of the *American Sociological Review*, using the following weights: President, 8; President-Elect, 7; Vice-President, Second Vice-President, 6; Vice-President-Elect, 5; Other Executive Committee member, 4; Secretary, 3; Council member except those listed below, 2; Council member elected from affiliated society or regional society, 1. Regional power was estimated by summing the officeholding scores of all officeholders from each region.

The notion of a regional imbalance of power implies that the power exercised by a region is disproportionately high or low relative to some standard legitimizing the exercise of power. Three bases of legitimate power were therefore investigated: membership, productivity, and eminence.

1. The membership data used are the number of voting members in each state according to a mimeographed list distributed by the ASA executive office in the spring of 1967.

2. Productivity was indexed by the number of articles in the two leading American sociological journals, the *American Journal of Sociology* (AJS) and the *American Sociological Review* (ASR), and by books reviewed in the ASR. A productivity index was computed for each ASA member who was listed in the 1959 directory as having a Ph.D. by assigning 1 point for each article (whether sole or joint author), 2 points for an edited book, 4 points for a jointly authored book, and 6 points for a solely authored book. An individual's productivity score is the sum of these weights for publications listed under his name in the cumulative index for volumes 1 to 70 of the AJS and volumes 1 to 30 of the ASR. It should be obvious that this measure, although similar to the productivity indexes used by other investigators,[1] is limited in scope because it excludes all but the two leading journals and also excludes non-sociological books. Although a more comprehensive index would have been preferable (and even essential if our purpose were to describe the productivity of American sociologists), we believe that such an index is satisfactory for purposes of comparing regions of the country in relation to regional differences in officeholding in the ASA.

3. Eminence was indexed by the frequency of reference to an author's work. For this purpose, we used the number of different pages on which an author was mentioned in two widely used textbooks with rather different viewpoints (Broom and Selznick, 1963; Rose, 1965) and two recent comprehensive reference works (Faris, 1964; Berelson and Steiner, 1964). As in the case of the productivity index, our measure of eminence is viewed as indirectly indexing the concept rather than describing it.[2]

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[1] For example, Meltzer (1949) gave the same weight to all publications except nonedited books. Equating a book chapter with an article, he assigned to a sole-authorship book a weight equivalent to the average number of chapters in current social science books, which, according to his calculations, was 18. The weight for a co-authored book was determined by dividing 18 by the number of authors. Axelson (1959) used two measures of productivity—one based on journal articles (he surveyed the contents of *Social Forces* in addition to the AJS and ASR) and the other, on all books except those that were edited or simply reissued. His articles index gave half weight for dual authorship and did not count articles with three or more authors. Crane (1965) used a measure that involved more interpretation on the part of the scorer. She distinguished between books and other major publications and minor publications, treating, for instance, a series of at least four articles, "each one of which explored some aspect of a single problem or closely related problems," as roughly equivalent to a book.

[2] Other studies have used a variety of approaches to measuring eminence. For example, respondents have been asked to nominate the living Americans who have made the greatest contributions to their discipline (Meltzer, 1949; Wilson, 1943). The "Honors Index" used by Lazarsfeld and Thielens (1958: 403) consisted of four items: two related to productivity, one to having held office in a professional society, and one to having worked as a consultant. Crane's measure of recognition (1965) was based on the number of honors that an individual had won, ranging from a post-doctoral fellowship to a journal editorship to election to the presidency of one's national professional association.

Using the four variables just discussed, we can now operationally define the concept of "regional bias" in control of the ASA. We will compare the officeholding scores of sociologists in a region with the membership, productivity, and eminence of sociologists in that region. To the extent that a region's officeholding exceeds its proportion of the membership, controlling for differences in productivity and eminence, a regional bias will be said to exist.

REGIONAL DIFFERENCES IN OFFICEHOLDING

For purposes of regional comparison, the officeholders and members were assigned to the following four regions on the basis of their institutional affiliation listed in the 1963 directory of members (the most recent directory available at the time of this study): Northeast (NE), Midwest (MW), South (S), and Mountain and Pacific (M & P).[3] By comparing officeholding of each region with the membership in that region (Table 1), we can take a first step toward determining if a regional bias exists.

Row 3 of Table 1 shows the regional distribution of the 110 sociologists holding office during the fifteen-year period studied. Comparing this row with either the total voting membership in 1967 (row 1) or the Ph.D.-holding membership (row 2), it is apparent that regional distribution of officeholders is virtually identical with the regional distribution of members.

Such a conclusion may be misleading, however, because the data on officeholding on which it is based represent only a crude measurement of power. A somewhat more accurate measurement is provided by the officeholder score. The regional distribution of these scores is presented in row 4 of Table 1. When this score is compared to the regional distribution of members (rows 1 or 2), some regional disproportion is apparent. Specifically, the Northeast and the Mountain and Pacific states are

Table 1. Percentage of ASA Officers, ASA Members, and Ph.D. Sociologists in Different Regions of U.S.

	Region				
	(*N*)	*NE*	*MW*	*S*	*M & P*
1. 1967 voting members	(3,954)	32	31	20	16
2. Ph.D. sociologists[a]	(1,689)	31	29	23	17
3. 1950–1965 officeholders	(110)	32	29	21	18
4. 1950–1965 officeholding scores	(1,230)	41	21	13	24
5. Mean officeholder score		14.5	8.1	7.1	14.9

[a] All ASA members in 1959 with a Ph.D. in sociology or a Ph.D. in another field but substantial commitment to sociology in 1959 and/or 1963.

For the present study none of these tacks could be followed. To use Crane's approach, for instance, would be impossibly costly for a large sample unless one were to go directly to each individual for the information, even though it is, of course, all a matter of public record. In addition, the reputational method produces eminence scores for only a very limited number of persons, whereas for the present study it was desired to use a measure that would not assign a score of 0 to most of the sample. Finally, for some of the analyses that were planned, an index of eminence was needed that was independent of holding or having held an office in the national professional association for sociologists, i.e., the ASA.

As to the validity of using only total number of citations as the measure of eminence, Westbrook's (1960) analysis of four types of citations index indicates that the crudest of these, gross number of citations, gives results about comparable to the others. Using the *Science Citation Index*, the Coles (1967) come to a similar conclusion, although they find it slightly preferable to assign differential weights based on the age of the article being cited.

[3] The states assigned to each region are as follows: NE = New England, New Jersey, New York, Pennsylvania; MW = Iowa, Illinois, Indiana, Kansas, Michigan, Minnesota, Missouri, Nebraska, North Dakota, Ohio, South Dakota, Wisconsin; S = Alabama, Arkansas, Delaware, District of Columbia, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, West Virginia; M & P = Alaska, Arizona, California, Colorado, Hawaii, Idaho, Montana, Nevada, New Mexico, Oregon, Washington, Wyoming.

"overrepresented" and the Midwest and South are "underrepresented." The greatest overrepresentation is found for the Mountain and Pacific region and the greatest underrepresentation for the South. If we combine the Pacific and the Northeast regions, it can be seen that these two regions, although having less than half of the members (48 per cent), have nearly two-thirds (65 per cent) of the officeholder scores. The regional disproportion in power is also indicated by row 5 of Table 1, which shows that of sociologists holding office in the ASA, those from the Northeast and Pacific coasts had an average score about twice the size of those from the Midwest and South.

Even though the more powerful positions are disproportionately in the hands of the officeholders from the East and West Coasts, one cannot construe this as grounds for an indictment of unfair policies or "political manipulation." For one thing, officeholding in a professional association is not expected to be strictly representative of number of members. Professional eminence is surely a key consideration in the selection of officers in such an organization and it is quite clear that centers of professional excellence are not equally distributed by region.

An adequate analysis of the regional differences in power should, then, take into account not only the regional variation in membership but also the amount and quality of performance, i.e., productivity and eminence, of that membership; and it is to this question that we turn in the next section.

REGIONAL DIFFERENCES IN PRODUCTIVITY AND EMINENCE

From the productivity index in Table 2, it can be seen that on the average the Northeast is most productive, followed fairly closely by the West (M & P) and then the Midwest. By far the least productive sociologists are found in the South. If one examines the components of the productivity index, a pattern of regional strengths is seen to exist. The Western sociologist writes more articles than others, while the Easterner is more likely to write books. The South is uniformly at the bottom on all the components of productivity, whereas the sociologists in the Midwest are about as likely to write journal articles as those from the West and considerably more likely to do so than those from the Northeast.

Turning now to the question of regional differences in eminence as given in Table 2, one can readily see that the general pattern seen earlier continues. Again the two coasts are highest, the Midwest third, and the South last. In only one respect is there an important difference between regional differences in productivity and in eminence: the Mountain and Pacific area has a clear-cut edge over the Northeast on the mean citations score. The general correspondence between regional ranks

Table 2. Productivity and Eminence of Ph.D. Sociologists, by Region

	Region			
	NE	*MW*	*S*	*M & P*
Productivity:				
Means:[a]				
Productivity index	6.88	5.99	4.34	6.34
AJS article score	.49	.73	.39	.70
ASR article score	1.03	1.15	.66	1.25
Book score	5.36	4.11	3.29	4.39
Per cent of authorship scores for:				
AJS (N = 972)	26	38	16	21
ASR (N = 1,721)	31	33	15	21
Eminence:				
Per cent of citations (N = 2,667)	37	29	11	23
Mean citations score	1.92	1.52	.74	2.09

[a] Based on 1,689 ASA members holding the Ph.D. in 1959 and distributed by region as shown in Table 1, row 2.

in output and citations is probably a reflection of the frequently reported correlation between individual productivity and eminence (Clark, 1957; Manis, 1951; Meltzer, 1949; Platz and Blakelock, 1960).

A CONTROLLED ANALYSIS

Since the data in Table 2 clearly indicate that the regions differ in both average productivity and eminence, we can return to the question of imbalance in distribution of power with the data necessary to control for productivity and eminence. We can, in short, provide at least a partial empirical answer to the question of the extent to which the regional imbalances reflect the acknowledged reward system of science (Cole and Cole, 1967; Storer, 1966).

The mean officeholder scores shown in Table 3 have been standardized by weighting the scores according to the ratio of the region's productivity and eminence to the national average productivity and eminence.[4] If the differences in power between regions are due merely to the differences in the productivity and/or eminence of the core group of Ph.D. sociologists found in each region, then regional differences should disappear in Table 3.

The standardized officeholder scores shown in Table 3 can be understood more easily if they are expressed as percentages of the standardized mean power score for all regions combined. The mean of the four scores shown in the first row of Table 3 is .35. The mean power score of the Midwest (.22) is only 63 per cent of the mean for all regions, indicating that sociologists from the Midwest held only 63 per cent of the offices to which their eminence and productivity "entitle" them. At the other extreme, the standardized power score for Southern sociologists is .58, which is 166 per cent of the over-all mean of .35, indicating that sociologists from this region held 66 per cent more offices than would be the case if offices were distributed strictly according to eminence and productivity. In between these two extremes lie the figures for the Northeast and the Mountain and Pacific states. Very similar results are obtained if only sociologists holding the Ph.D. are considered.

CONCLUSIONS

The results of this study lead to no simple conclusion. If one judges bias in power to be indicated by officeholding disproportionate to the number of members in a region, these data suggest that there is, in fact, some degree of regional bias. The Midwest had representatives in positions of power during the period 1950 to 1965 somewhat below its membership contribution, and the South was even more underrepresented. The Northeast, with 32 per cent of the members, had 41 per cent of the scores in the index of officeholding, and the Mountain and Pacific states, with only 16 per cent of the members, had 24 per cent of the officeholding scores.

Table 3. Officeholder Scores, Standardized for Productivity and Eminence, and Percentage of Combined Mean Power Score, by Region

	Region				*Mean for all regions*
	NE	*MW*	*S*	*M & P*	
Standardized officeholder score:					
Voting members	.29	.22	.58	.32	.35
Ph.D. members	.71	.54	1.26	.73	.81
Percentage of combined mean power score:					
Voting members	.83	.63	1.66	.91	
Ph.D. members	.88	.67	1.56	.90	

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[4] The specific formulas for computing these standardized office-holding scores are: *Productivity standardized score* = (regional mean officeholding score × mean U.S. productivity index) ÷ regional mean productivity index; *Eminence standardized score* = (regional mean officeholding score × mean U.S. citation index) ÷ regional mean citation index; *Productivity and eminence standardized score* = (productivity standardized officeholding score) (mean U.S. citation index) ÷ regional mean citation index.

If the determination of bias is based not on deviation from number of members but on deviation from the relative publication contribution and eminence of sociologists in each region, the situation changes. Relative to its publication contribution and number of eminent scholars, the Midwest has been severely underrepresented in ASA offices held. Thus, there seems to be some empirical basis for the complaints that have been voiced. Although the data do not provide clues to the reasons for the underrepresentation of the Midwest, neither do they give support to the notion of a regionally based power clique. In fact, simultaneously with the underrepresentation of the Midwest, the data show no overrepresentation for either the Northeast or the Mountain and Pacific states. Rather, it is the South, the region with the lowest productivity and eminence scores, that is the most overrepresented in officeholding. The ASA offices that, by virtue of productivity and eminence, should have been held by Midwesterners seem, instead, to have been held by Southerners. It may be that the "bias" evidenced by the overrepresentation of the South reflects the operation of a desire to maintain an equitable balance between regions in the men holding office. If this is the case, then rather than a power-clique theory, the data presented in this paper suggest the operation of an *equity* principle that serves to mute the operation of the classic bases of the reward system of science.[5] But why any such equity principle should have been achieved at the expense of the Midwest rather than other regions remains unclear.

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[5] These results can also be viewed as an instance of Goode's theory of "the protection of the inept" (Goode, 1967).

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E. Alternate Career Patterns

Introduction

Some minority groups of scientists are limited in their career development simply because they belong to a particular subgroup. While some constraints occur because the entire group is discriminated against as a visible minority, other constraints may develop because individual members of a group cannot adopt the expected patterns and obligations of the professional role or adjust to the given social environment.

The majority of articles dealing with career-limiting factors involve women. The selection by Wispé, Ash, Awkard, Hicks, Hoffman, and Porter is one of the few that considers other minorities, in this case Negro psychologists. The black psychologists differ in origin and background from the majority of psychologists. They are less likely to obtain their doctorate, but if they do, the doctoral institution is more likely to be a low-prestige Midwestern school. Over half of the black respondents stated that race had limited their professional opportunities, and this was true especially for the high-income older psychologists with Ph.D.s. The authors conclude that to be a black psychologist in America is a severe handicap, for Negro psychologists in terms of doctoral institutions, jobs, and prestige still are excluded from the mainstream of American psychology.

The balance of the articles in this section deal with the most prevalent minority group in science—women. Bachtold and Werner, and Helson consider the personality characteristics of successful women scientists. Using personality self-inventories, they question whether successful women psychologists (Bachtold and Werner) and creative women mathematicians (Helson) are similar to successful or creative men in the same fields. Helson finds that while the creative woman may be somewhat more socially aloof and self sufficient and less dominant and socially assured than her male counterpart, in general, creative women appear similar in personality and cognitive characteristics to creative men. These traits seem to be characteristic of creativity regardless of sex and include independence, introversion, strong symbolic interest, and strong ego-involvement in work. Successful academic women psychologists and creative mathematicians appear to differ from adult women in general or women college students in many of the same personality characteristics in which they resemble successful or creative academic men. As a group they appear more intelligent, socially aloof, dominant, serious, adventuresome, sensitive, flexible, unconventional, and imaginative than do women in general. Despite these similarities, it should be noted that creative women mathematicians published less and, if they had a job at all, occupied less prestigeful positions, thus indicating that women are excluded in some way from positions commensurate with their skills.

The next selections deal with problems surrounding the integration of women into the occupational structure of science and engineering. The "selective" patterning of careers by sex of engineer and scientist is evidenced to some extent when graduates first enter the labor market and becomes more pronounced during the course of the career (Perucci). Women receive lower salaries than men for positions of similar responsibilities and advance at a slower rate than do men.

Perucci shows the possible adaptations that can be made by women to mitigate the effects of "deviant" sex status. Specifically, for two age groups of women, pursuit of a full-time career is accompanied by a temporal ordering among the events of college graduation, employment, marriage, and childbearing. In comparison to non-career women of the same age group, career women are more likely to be unmarried, to complete college work prior to marriage, and to be childless or have a smaller family. In fact, young career women may tend toward complete avoidance of the marriage and maternal roles and, in any case, are delaying these roles.

White asserts that the main barrier to the woman's achievement of excellence in science is the expectation that women's career patterns and motivation will be the same as men's. The limited opportunities for acceptance and informal interaction with colleagues may hamper the scientifically trained woman. Women, especially those who have experienced interrupted or discontinuous careers because of multiple roles and obligations, find opportunities for professional socialization and informal acceptance and communication difficult to obtain. Because most men are hesitant about encouraging a woman as a protégé, the woman scientist may be excluded from important sponsorship, and thus recognition. White suggests that the scientific community can foster female scientific commitment by permitting more flexible opportunities for professional participation and by being aware of practices which exclude women on the basis of gender, rather than ability.

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The Negro Psychologist in America

Lauren Wispé, Philip Ash, Joseph Awkard, Leslie H. Hicks, Marvin Hoffman, and Janice Porter

Little is known about the origins, education, and training of Negro psychologists. Even less is known about the discrimination they have faced in the course of their professional careers. To obtain this kind of information, which would be valuable in itself and could also serve as a basis for recommendation to the Board of Directors of the APA, the Committee on Equality of Opportunity in Psychology undertook to survey psychologists in America who are Negro.[1] The findings reported below are the results of this survey. Because of the difficulty in obtaining information about race and discrimination, these findings must be taken as the best possible tentative answers under the present conditions to very complex problems.

PROCEDURE

To obtain the names and addresses of Negro psychologists and graduate students currently working and studying in the United States, requests for this information were sent to 94 Negro colleges and universities, to the chairmen of 216 psychology departments with graduate training programs listed in the *American Psychologist* (Ross & Harmon, 1966), to the deans and chairmen of the 78 largest Colleges of Education listed in the *Educational Directory* (Office of Education, United States Department of Health, Education and Welfare, 1965), to the 50 State Psychological Associations, and to about 15 of the larger government departments and centers where psychologists were employed. A 6-page questionnaire containing items about family background, undergraduate and graduate education, occupational history, and present earnings was mailed to the 492 Negro psychologists whose names were obtained in this way. The questionnaire also contained items about ethnic factors which were professionally advantageous or disadvantageous to Negroes, and about ways the APA could help to counter professional discrimination.

The questionnaires, with a covering letter, were mailed July 1966. An unusually energetic telephone campaign, three follow-up letters, and additional airmail-special questionnaires raised the response rate from 61% in January 1967 to 81% by July 1967.

A one-third sample of the 398 usable returns showed that 27% were members of APA. This appeared to compare unfavorably with the percentage of non-Negro psychologists

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[1] The Committee on Equality of Opportunity in Psychology was established by the Board of Directors in 1963. The Board had received a proposal from Division 9 relative to the training and employment of Negroes in psychology. The Committee was charged with exploring the possible problems encountered in training and employment in psychology as consequence of race. (Proceedings of the Seventy-First Annual Business Meeting, August 30 and September 3, 1963. Reported in the *American Psychologist,* 1963, *18,* 769.) The Committee is presently a standing committee under the Education and Training Board. The conclusions drawn in the paper are the sole responsibility of the authors, as are the analysis and interpretation of the data.

who belong to the APA. Of the 94 nonrespondents, only 15% were listed in the 1967 *APA Directory*, so that nonrespondent bias was almost impossible to check. Their refusal to respond to the questionnaire, however, despite repeated personal and telephone contacts, suggested that they may have differed in critical ways from respondents.

RESULTS

Origins and Background

Population Comparisons. As Table 1 clearly indicates, the distribution of Negro psychologists closely parallels the distribution of Negroes in the United States. Over half (52.8%) of the American Negroes live in the Southeast and over half of the respondent Negro psychologists (51%) live in the Southeast. The distribution of APA members, however, differs significantly from the distribution of the total United States population; psychologists are overrepresented in the Northeast (36.1% to 24.5%) and underrepresented in the Southeast (15.4% to 23.7%). Even if allowance is made for the difference in dates of the various distributions, Table 1 shows clearly that Negro psychologists are distributed in a significantly different way from APA members.

Age. The median age for male respondents was 38.8 years, and for female respondents, 38.1 years. Regional differences in age were slight.

Place of Birth and Residence. Almost 60% of the Negro psychologists were born in the Southeast, and two thirds of that group still live there. Table 2 shows, however, that there has been a significant net migration out of the Southeast. Of all respondents born in the Southeast, one third moved out; while of all those now living there, slightly under one fourth are in-migrants. The major movement, similar to the national trend, has been to the Western States, with an in-migration that resulted in a net increase of 71%.

Occupational status of parents. Table 3 indicates that both the fathers and mothers of Negro respondent psychologists were, on the average, of higher occupational status than the general 1965 Negro population. Only 34.2% of the fathers and 9.4% of the mothers of respondents were manual laborers, compared with population estimates of 74.5% and 34.7%, respectively. More of the fathers of respondents were in the managerial and professional category, and more of the mothers kept house than Negroes in the general population.

Education

Regional Differences. Table 4 shows the percentage of bachelor's, master's, and doctor's degrees in psychology earned by Negro psychologists from schools in different regions of the United States. In the last column of Table 4 is the distribution of doctoral degrees in psy-

Table 1. Percentage of Negro–White Population and Responding Negro and APA Psychologists Compared by United States Regions

Region	*Total United States population*[a]	*Total Negro United States population*[b]	*Total APA psychologists*[c]	*Responding Negro psychologists*[d]
West	22.3	12.9	24.0	13.3
Midwest	28.3	18.3	24.4	17.0
Southeast	23.7	52.8	15.4	51.0
Northeast	24.5	16.0	36.1	14.8
Outside USA	1.3	—	0.1	1.7
National Register	—	—	—	2.0

[a] $N = 185{,}890$. United States Bureau of the Census. *Statistical abstract of the United States, 1967.* (88th edition) Washington, D.C., 1967. Table No. 10, p. 12. (In thousands.)

[b] $N = 18{,}872$. United States Bureau of the Census. *Statistical abstract of the United States, 1967.* (88th edition) Washington, D.C., 1967. Table No. 27, p. 29. (In thousands.)

[c] $N = 24{,}604$. In 1966 Count, United States members only (includes Puerto Rico).

[d] $N = 398$.

chology for all graduates during the years 1960–1966 (National Science Foundation, 1967b). Most Negro psychologists received their bachelor's degree in the region of their birth. This explains the large proportion (62%) of bachelor's degrees in the Southeast. Master's degrees were more equally distributed among geographic regions, reflecting the geographic differences in availability of the bachelor's and master's degrees. The geographic distribution

Table 2. Migration Patterns of Responding Negro Psychologists in Percentages

	Residence						
Birthplace	*West* (*N* = *53*)	*Midwest* (*N* = *68*)	*Southeast* (*N* = *203*)	*Northeast* (*N* = *59*)	*Outside United States* (*N* = *7*)	*National Register* (*N* = *8*)	*Total %*
West	4.0	1.0	1.5	1.0	—	.3	7.8
Midwest	3.0	7.0	5.5	2.0	.3	.3	18.1
Southeast	4.8	8.0	39.2	4.8	—	1.5	58.3
Northeast	1.5	1.0	4.0	6.3	—	—	12.8
Outside USA	—	—	.5	.8	1.5	—	2.8
National Register	—	—	.3	—	—	—	.3
Total %	13.3	17.0	51.0	14.9	1.8	2.1	
Net change	+22	—4	—29	+8	—4	—	
% change[a]	+71.0	—5.6	—12.4	+15.7	—36.4	—	

[a] Birthplace/migrant = % change.

Table 3. Distribution of Parents' Occupations Compared with 1965 Distribution of United States Negro Population in Percentages

Occupation	*Respondent's father*	*Negro men*	*Respondent's mother*	*Negro women*
Housewife	—	—	57.6	39.7
Manual labor	34.2	74.5	9.4	34.7
Skilled labor	25.6	10.7	4.9	0.3
Clerical and technical	6.5	6.7	6.6	6.0
Managerial and professional[a]	33.0	8.2	21.4	5.0
Other[b]	—	—	—	14.3
Total %	99.3	100.1	99.9	100.0

[a] Includes self-employed.

[b] In school, etc.

Table 4. Geographic Distribution by Educational Degrees of Respondent Negro Psychologists and All 1960–65 Doctorates in Psychology

Region of degree granting school	*BA* (*N* = *59*)	*MA* (*N* = *173*)	*PhD* (*N* = *166*)	*All 1960–66 PhDs* (*N* = *6,443*)
Southeast	62%	32%	11%	13%
Northeast	12%	28%	33%	31%
Midwest	19%	31%	42%	30%
West	7%	9%	14%	26%
Total *N*	59	173	166	—
Total %	14.8%	43.5%	41.7%	100%

Note—National Science Foundation (1967b).

of doctoral degrees for Negroes showed the closest similarity to that of the total group of psychologists who received PhD degrees between 1960–1966. Nevertheless, proportionately more of the Negro psychologists received doctorates in the Midwest and proportionately fewer in the West. Although the percentage of Negro doctorates from the Southeast is not different from the percentages of all 1960–1966 doctorates in psychology (11% versus 13%), because of the large percentage (62%) of Negro bachelor's degrees from the Southeast, one would have expected a much larger proportion of Negro PhDs from this region. This low figure for the Southeast is again a product of a history of segregated colleges and the absence of a single Negro university which offered the PhD in psychology.

Table 5. Respondents' Ages and Bachelor's Degrees Classified by Type of Institution

Age	*% Negro colleges/ universities*	*% non-Negro colleges/ universities*	*Total %*
60 and older	71.4	28.6	100.0
50–59	66.7	33.3	100.0
40–49	71.1	28.9	100.0
30–39	49.6	50.4	100.0
20–29	47.4	52.6	100.0
National Register	14.3	85.7	100.0
Total	57.1	42.9	100.0

[a] $N = 218$.
[b] $N = 163$.
[c] $N = 382$.

Region, Race, and Negro Colleges. Since most of the Negro colleges are located in the South, the question arises whether the reason so few Negroes pursue the PhD in psychology is regional or educational. To try to answer this question a group of 28 non-Negro colleges were matched as well as possible with 28 Negro colleges on public-private control, geographic region, number of faculty and students, operating income, and value of buildings. According to available data from the 28 Negro colleges (National Science Foundation, 1967b), 76 graduates received their doctorates in psychology during the period 1920–1966. The NSF data show that during the same period the matched non-Negro colleges produced 167 bachelors who later obtained the PhD degree in psychology. Thus when one controls such important factors as geographic region, student-faculty ratio, etc., it may be seen that students from white institutions pursue the PhD in psychology more frequently than do students from Negro colleges. Although the issue is a complex one, this analysis suggested that factors inherent in Negro colleges militated against the Negro students' pursuit of the PhD in psychology.

Table 5 presents the number and percentage of responding Negro psychologists who received their bachelor's degrees from Negro and non-Negro colleges and universities. This table shows that a majority (57.1%) of the respondents received their undergraduate training at Negro colleges and universities, and among respondents over 40 years old this proportion was much higher. If, for whatever reason, graduates of Negro colleges have not pursued the doctorate in psychology, the fact that Negroes are now increasingly attending non-Negro schools could be viewed as favorable for psychology.

Doctoral Institutions. Table 6 presents the 25 institutions granting the largest number of doctorates in psychology, 1920–1966 (National Science Foundation, 1967b), and the number of Negro doctorates granted by these schools to the respondents in the present study. It can be seen that several Midwestern schools ranking quite low in the total number of doctorates granted have produced disproportionately more Negro doctorates. Moreover, as Table 7 shows, if the rated quality of psychology departments (Cartter, 1966) is related to total doctorates produced and to Negro doctorate production, it is clear that few Negro PhDs in psychology have come from the 10 presently best-rated psychology departments. The top 10 departments produced 24% of the total doctorates in psychology between 1920 and 1966, but only .5% of the total Negro doctorates in psychology. Closer inspection of Table 7 shows, moreover, that seven of the eight Negro doctorates from these 10 leading departments were produced by three Midwestern schools. Those Ivy League schools among the 10 leading departments produced none of the Negro doctorates in the present study. It is doubtful that these trends would be much affected even if all Negro psychologists in the United States were included.

Table 6. Negro and Non-Negro Psychology Doctorates Granted by the 25 Universities Producing the Largest Number of Doctorates in Psychology, 1920–1966

Universities	*Total no. psychology PhDs 1920–1966*	*No. Negro PhDs among respondents*
New York	804	13
Columbia	770	10
Chicago	680	9
Michigan	651	3
Ohio State	647	4
Minnesota	615	3
Iowa	583	0
Purdue	533	1
Berkeley	405	1
Harvard	383	0
UCLA	307	0
Illinois	302	0
Yale	293	0
Stanford	292	0
Texas	285	3
Cornell	261	0
Penn State	257	4
Northwestern	246	6
Southern California	245	2
Michigan State	240	6
Pittsburgh	234	1
Western Reserve	229	4
Indiana	223	14
Boston	217	4
Pennsylvania	212	5
Total	9,914	93

Note—National Science Foundation (1967a).

Table 7. Negro Doctorates in Psychology from Leading Psychology Departments

Rank[a]	*Universities*	*Total psychology PhDs 1920–1966*[b]	*No. Negro PhDs among respondents*
1	Harvard	383	0
2	Stanford	292	0
3	Michigan	651	3
4	Berkeley	405	1
5	Yale	293	0
6	Illinois	302	0
7	Minnesota	615	3
8	Wisconsin	206	1
9	Brown	87	0
10	Iowa	533	0
Total		3,767	8

[a] Cartter (1966).

[b] National Science Foundation (1967a).

Salary

Median annual salaries provided one of the few measures comparing Negro and non-Negro psychologists on the same dimension. The median annual salaries of responding Negro psychologists, Negro psychologists who were included in the National Register, and all National Register respondents were used for this purpose. The National Register data were obtained in March 1966 by the APA, in cooperation with NSF's continuing study of the nation's scientific manpower. The results were based on 19,027 individuals who responded to the National Register form and were classified as psychologists. Of the National Register respondents, 64% were APA members, as contrasted with an estimated 27% of the present sample; 66% had the doctorate, as contrasted with 41% in the present study; 78% were male, as compared with 72%; and the median age was 41 years, as compared with 38 years for the present group. The criteria for inclusion in the National Register study, as established by the APA, would probably not include respondents with bachelor's degrees comparable to the respondents in the present study. The studies, therefore, are comparable for the PhDs but not the BAs, although information for both degrees is presented in Table 8. Moreover, not all of the respondents in either study provided salary information, and it is impossible to know the effects of these selective omissions. In order to answer questions arising out of possible differences in samples and procedures, therefore, a subsample of Negro respondents to the National Register was drawn. This subsample included 87 psychologists who responded to both studies, and 6 psychologists who responded to the National Register but not to the present study. The National Register data provide about as representative a basis for salary comparison as is available, but interpretations must be made with caution because, in one case, the comparisons are being made between two different studies and, in the other case, the subsample size is very small. Nevertheless, certain differences emerged with sufficient consistency to warrant serious consideration.

Table 8 gives the characteristics on which the three samples were compared. For the Equal Opportunities questionnaire, 92% of the respondents provided salary information. The median salary for these Negro psychologists was $7,775.00. This is considerably less than the median salary for all psychologists responding to the National Register, but this difference must not be taken literally because of the larger number of younger respondents with only bachelor degrees in the present study. The median salary for Negro psychologists in the National Register sample was $11,500.00, which is the same as the median salary for all respondents to the National Register. In general, for Negro as well as for non-Negro psychologists, men earn more than women, older psychologists earn more than younger psychologists, psychologists with the PhD earn more than those with less than the doctorate, and psychologists employed with the Federal Government earn more than those employed by universities.

A closer perusal of Table 8 reveals a tendency for the disparity in median annual salaries between Negro and non-Negro psychologists to decrease with age, and this is true regardless of the type of employer. Although the younger age cohorts are not comparable between studies, for the ages over 40 there is a steady, if slight, improvement in Negro salaries so that by age 55 equity has nearly been achieved. Table 8 shows that Negro doctorates in psychology earned as much as non-Negro doctorates, although Negro MA psychologists in the present study earned less than the National Register MA respondents, and this was true regardless of type of employer.

The comparison of the salaries of psychologists in the present study and National Register psychologists working in universities presented an interesting apparent contradiction to the idea that the disparity between Negro and

Table 8. Median Annual Salary of Responding Negro Psychologists and Negro and Non-Negro Psychologists in the 1966 National Register (NR)

Characteristics	*Negro psychologists*[a]	*Negro psychologists in NR*[b]	*All psychologists in NR*[c]	*% differences Negro and NR psychologists*
Total	7,775 (365)	11,500 (89)	11,500 (19,027)	—32
Sex				
Male	8,425 (264)		12,000	—19
Female	6,200 (101)		10,000	—26
Age				
60–64	13,125 (15)	11,800 (7)	12,700	+3
55–59	12,475 (25)	13,300 (10)	12,600	—1
50–54	11,250 (30)	12,500 (17)	13,000	—14
45–49	12,150 (52)	11,200 (16)	13,000	—7
40–44	10,475 (65)	10,200 (17)	12,400	—16
35–39	10,500 (68)	10,400 (15)	11,400	—8
30–34	8,495 (69)	10,000 (8)	9,900	—14
25–29	6,075 (49)		8,300	—27
Highest degree				
PhD	12,625 (166)	12,100 (60)	12,100	+4
MA	8,275 (173)	9,700 (93)	9,800	—16
Type of employer				
College or university (calendar year)	10,325 (233)	12,000 (39)	12,000	—14
Federal Government	12,850 (52)	12,700 (12)	13,400	—4
State and local government	8,600 (29)	11,800 (11)	10,400	—17
Industry and business	8,750 (10)	—	15,100	—42
Academic rank				
Professor	12,225 (56)	13,400 (20)	15,600	—22
Associate professor	10,000 (18)	11,100 (2)	12,500	—20
Assistant professor	9,750 (31)	10,200 (10)	10,600	—8
Instructor	8,225 (21)	10,900 (4)	8,900	—8

Note—Numbers in parentheses are *N*s for that median.

[a] $N = 398$.

[b] $N = 93$.

[c] $N = 19{,}027$.

non-Negro salaries decreased with age. Table 8 shows that professors and associate professors earned about 20% less annually, while assistant professors and instructors earned about 8% less. Academic rank is, of course, related to age. Further analysis revealed, however, that a large proportion (71%) of these professors and associate professors were teaching in Negro colleges in the Southeast, 77% were over 40 years old, and 18% had only the master's degree. By contrast, most (58%) of the assistant professors and instructors were younger, already had their PhDs, and were teaching in colleges and universities in the West, Midwest, and Northeast. It should be remembered from the analysis of educational background that more of these younger respondents received their baccalaureate degrees from non-Negro colleges. These statistics serve not only to explicate this apparent contradiction, but also to illuminate the greatly disadvantaged teaching positions of many of the older Negro psychologists, and the sharp disparity between the median annual salaries of Negroes and non-Negroes at the higher academic ranks. On the other hand, it is worth noting that this situation may be changing for the assistant professors and instructors.

Discrimination

Nearly half of the respondents (48.2%) stated race had limited their professional opportunities. When regional differences were examined this pattern was the same whether the question was asked of those presently living in a region, or of those who had been born and presumably grew up within the area. Table 9 shows that more respondents in the West (58.5%) than in any other region, including the Southeast (53.2%), reported that race had limited their opportunities.

One hypothesis to account for these regional differences in reported racial discrimination is that this discrepancy reflected not an actual difference in experience as much as a

Table 9. Opinions about Limitations of Professional Opportunities and Respondents' Present Residence

	"Do you feel your professional opportunities have in any way been circumscribed . . . because of your race?"			
Present residence	*% yes (N = 192)*	*% no (N = 177)*	*% no response (N = 29)*	*Total %*
West (*N* = 53)	58.5	37.7	3.8	100
Midwest (*N* = 68)	44.1	47.1	8.8	100
Southeast (*N* = 203)	53.2	39.9	6.9	100
Northeast (*N* = 59)	33.9	54.2	11.9	100
Outside USA (*N* = 7)	.0	100.0	.0	100
Region unknown (*N* = 8)	37.5	63.5	.0	100
% total (*N* = 398)	48.2	44.5	7.3	—

Table 10. Kinds of Professional Restrictions Reported and Respondents' Region of Birth

	Kinds of professional restrictions					
Region of birth	*% employment*	*% training*	*% negative self-image*	*% professional discrimination*	*% other*	*Total*
West (*N* = 18)	27.8	27.8	27.8	16.7	0	100.1
Midwest (*N* = 30)	60.0	10.0	10.0	3.3	16.7	100.0
Southeast (*N* = 111)	53.2	12.6	14.4	13.5	6.3	100.0
Northeast (*N* = 16)	50.0	0	6.3	12.5	31.3	100.1
% total (*N* = 175)	51.1	12.5	14.2	11.9	10.2	

difference in expectation, and a greater candor about the disadvantages of being black. Unimpeded by those employment barriers which traditionally exist in the South, western Negroes felt free to compete in a wider job market where race supposedly would less often bar them from obtaining a job. Given these expectations, Negroes living in the West paradoxically encountered more discrimination than they were prepared for, and proved more sensitive to the handicap of being Negro than those respondents residing in the South.

Table 11 shows that fewer young Negroes reported racial barriers than did older ones. Whether this finding is to be interpreted as a relaxing of racial barriers or as a function of the youthfulness of the group, many of whom have not yet been exposed to those employment and professional situations where they might find race a handicap, is a moot question. That so large a percentage within this age group gave no response to this item (three times as many as in the preceding age groups did not answer this item) may reflect the group's awareness of its lack of experience.

One interesting sex difference ought to be noted. Although the number of respondents in each age cohort was not large, Table 11 shows that fewer younger women respondents reported restrictions on their professional opportunities than did men in their own age group, older men, or older women. For example, 22 of the 31 women aged 21–30 (71%) said their opportunities had not been circumscribed by race, while only 28 of the 64 men (43.8%) in the same age cohort would agree. Moreover, there was an interesting parallel between age and percentage of women respondents reporting circumscribed job opportunities; the younger women consistently reported fewer professional restrictions. This was not true for the male respondents. These findings may suggest the greater acceptability of Negro females to the culture. If this interpretation is valid, then it would appear that it is the younger Negro woman psychologist more than the Negro male psychologist who has benefited from the lowering of racial barriers.

Further analysis of these data showed that more older psychologists, with PhDs and higher incomes, reported that race had been a limiting factor in their careers. This finding suggested that reports of circumscribed professional opportunities were not the rationalizations of the less successful among the respondents. These data are harder to interpret without comparable reports from the non-Negro psychologists. It may be that there is less "room at the top" for anybody, Negro or white; or it may mean that the higher one moves in the professional hierarchy, the more race becomes a limiting factor. Whatever the interpretation, these data were doubly interesting because the comparison of median annual salaries suggested that these were the psychologists who, despite their perception of racial barriers, were doing as well financially as their non-Negro peers.

DISCUSSION

The Committee was not unaware that of all possible approaches the questionnaire was among the most likely to underestimate the depth of feelings of many, if not most, of the respondents. The unusual effort the Committee expended to raise the response rate to an acceptable 81% indirectly reflected the amount of resistance many of the respondents felt, and some expressed. Many of the nonrespondents, for example, wrote long letters, but refused to return a completed questionnaire! Many felt it was unutterably naive to ask if race had limited their opportunities. Of course it had. Others, after long-distance calls, agreed reluctantly to complete the questionnaire, but expressed doubts about its efficaciousness. The intensity of the frustrations and resentments of both the respondents and the nonrespondents cannot be quantified, but these facts must be kept in mind as we turn now to what we believe are the main implications of this study.

Negro Psychologists Are Excluded from the Mainstream of American Psychology

For the most part, the Negro has received his early training in Negro schools. He moved outside the Negro institution to get an MA and PhD, since no Negro schools granted the higher degree in psychology. Having obtained a PhD, he then usually returned to the Negro college to work, if pursuing an academic career. Of all respondents employed by colleges or universities, for example, 154 worked in Negro institutions, 46 in white.

A sense of inadequacy and fear has worked hand in glove with actual discrimination to in-

Table 11. Opinions about Limitations of Professional Opportunities and Respondents' Age and Sex

Respondent age	*"Do you feel your professional opportunities have in any way been circumscribed . . . because of your race?"*														
	N (*N* = *398*)			*% yes* (*N* = *192*)			*% no* (*N* = *177*)			*% no answer* (*N* = *29*)			*Total %*		
	M	*F*	*T*	*M*	*F*	*T*	*M*	*F*	*T*	*M*	*F*	*T*	*M*	*F*	*T*
51–74	45	18	63	64.4	61.1	63.5	28.9	33.3	30.2	6.7	5.6	6.3	100	100	100
41–50	76	26	102	48.7	61.5	52.0	44.7	38.5	43.1	6.6	—	4.9	100	100	100
31–40	101	37	138	47.5	56.8	50.0	48.5	40.5	46.4	4.0	2.7	3.6	100	100	100
21–30	64	31	95	37.5	19.4	31.6	43.8	71.0	52.6	18.8	9.7	15.8	100.1[a]	100.1[a]	100

Note—Abbreviations: M = male; F = female; T = total.

[a] Net equal to M% and F% because of rounding.

sure the isolation of most black psychologists. As one respondent put it, "Recognizing the limitations of a segregated society, I protected my ego by enrolling in predominantly Negro institutions and by seeking employment only in Negro institutions." We are not here taking a stand on the Negro college or its future. Our point is that Negro schools are by and large divorced from the mainstream of psychology. As many respondents stated, facilities, research monies, and professional contacts are almost nonexistent in the Negro institution.

The isolation of Negro psychologists is further revealed by the finding that although 85% of the sample had obtained either the doctorate or master's degree only an estimated 27% of them were members of APA. A clue to the reason for this low percentage is provided by the respondent who wrote, "I have never been active in the APA, since I have always felt it was part of the white academic club. Besides, the ghetto colleges don't know what it [APA] means, and usually don't even know what psychology means." It is not so much to actual instances of discrimination that we point, depressing as these are to read, but rather to the fact that most black psychologists feel themselves, and until recently were, alienated from American psychology because of the totality of what it means to be black.

To Be Black in America Is a Terrible Handicap, and To Be a Black Psychologist Is Not Much Better

It was at least discomforting to learn that nearly half the respondents felt race had limited their professional opportunities in psychology. It was even more devastating to recognize that this percentage is probably an underestimation of the true figure. Even respondents who said their own experiences had been good, often added, as in the quotation above, that they had deliberately restricted their lives to avoid the possibility of rejection. Social as well as professional factors were expressed. Many respondents feared segregated housing were they to teach in a predominantly white college; many wondered whether their families would find friends, and how hard they would have to work to prove themselves equal. The inescapable conclusion to be drawn from this study, therefore, is that being a Negro psychologist may reduce the handicap of being black, but it does not remove it.

The Responsibility of APA for Affirmative Action

Many respondents suggested that the APA first put its own house in order. Respondents noted the absence of Negroes holding APA office and presenting papers at conventions. One respondent said, "Negroes play a relatively minor role in its [APA] organization and functions to the point that only a few . . . apply for offices. . . ."

The question, "How can the APA correct professional discrimination?" was not answered by 58% of the respondents. Moreover, there were no significant age or sex relationships to this failure to respond. In light of the many comments like the one above, this high percentage of no answers could be interpreted as a lack of faith in the APA's willingness or ability to take corrective action. Of those respondents who indicated ways in which the APA might help, over half suggested that the APA enforce nondiscrimination in employment and training, refuse to list such openings in APA employment literature, refuse to grant advertising space to those who would not comply with a nondiscriminatory policy, etc. Such policies could also be adopted for internship programs in clinical psychology.

This report cannot conclude without addressing itself most strongly to the issue of making more flexible the opportunities for Negro graduates to enter psychology. As one respondent wrote, "The real problem is, as I see it, making opportunities for quality education available. We are still guilty of a serious failure to develop and nurture the talents of a sizable group of promising but disadvantaged youngsters who are Negro." This could be the most exciting and rewarding undertaking of all.

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Personality Profiles of Gifted Women: Psychologists

Louise M. Bachtold and Emmy E. Werner

In two recent studies of personality factors of talented boys and girls in middle childhood and adolescence by the authors (Werner, 1966; Werner and Bachtold, 1969), the personality profile of the gifted boys showed a striking resemblance to that of recognized creative persons (artists, writers, and scientists) in the adult population (Cattell and Drevdahl, 1955; Drevdahl and Cattell, 1958) and of college students nominated for creative potential (Drevdahl, 1956). The findings, for the gifted boys, were generally applicable, regardless of age group, method of selection, type of educational program, and special area of interest.

This trend did not hold for the gifted girls. Their personality profiles, both in middle childhood and adolescence, were less characteristic of the distinguishing traits found for creative adults, and fewer personality factors differentiated the gifted girls from the norm group of their age and sex. The question arose whether these findings reflected the special selection procedure used for the education of gifted students, or whether there were consistent sex differences in personality factors among the gifted in childhood, college, and adulthood.

The reference groups for Cattell and Drevdahl's studies of creative artists and writers and eminent researchers, teachers, and administrators in physics, biology, and psychology, and the other major studies of distinguished scientists (reviewed by Taylor and Barron, 1963) did not differentiate between men and women.

A search of the literature revealed that studies of the personality characteristics of creative women have been exceedingly rare. The exception is a series of reports by Helson (1961, 1966, 1967a, 1967b), who studied the personality traits, cognitive style, and developmental history of college women nominated as high in creative potential in the arts, sciences, and humanities in their senior year and five years after graduation, and who compared women mathematicians considered creative by their peers with less creative women mathematicians and creative men. Personality characteristics in these studies were assessed with the California Personality Inventory (CPI), the MMPI, and by Q-sort and staff ratings. Senior women with creative potential scored significantly higher than their classmates of similar scholastic aptitude in complexity of outlook, flexibility, originality, independence of judgment, psychological-mindedness, and level of nonconformity. Five years later the creatives showed more increase in these characteristics than the noncreatives, with the "active" creatives, both married and single, registering the greatest growth. Creative women mathematicians had higher scores than comparison women on flexibility and lower scores on commonality and achievement by conformity. In contrast to the creative men, however, creative women mathematicians were less self-confident and less sociable, and had a narrower range of in-

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terest. A similar finding has been reported by Kurtzman (1967) in a study of creative ninth-grade boys and girls.

There are, as yet, no published studies of the personality characteristics of successful academic women in the fields first explored with men via projective tests by Roe (1953) and with the Sixteen Personality Factor (16 PF) Questionnaire by Cattell and Drevdahl (1955; Drevdahl and Cattell, 1958): scientists in psychology, physics, and biology, and artists and writers.

One study of the vocational interests of women psychologists (Campbell and Soliman, 1968) suggested that psychology as a career, now as a generation ago, attracts women who have more intellectual, scientific, and verbal linguistic interests than the average women, and fewer interests in the traditional feminine role. A study of femininity and creativity among high school students in physics (Walberg, 1969) raised the question whether the very factors that correspond to the successful feminine and student role may penalize the girl's chances for later eminence in science.

The present study is the first in a series conducted with the 16 PF Questionnaire to fill the gap in our knowledge about the personality characteristics of successful and productive adult women in the social and natural sciences, arts, and humanities.

It was undertaken with the hope that the identification of personality factors that enable women to succeed in the sciences and in academia, in spite of powerful sex-role expectations to the contrary, will contribute to the recognition and development of potential creativity among gifted girls.

A need for further knowledge in this area is indicated as we witness the steady decline of the proportion of women awarded higher degrees, in the sciences (Rossi, 1965b) and in academic positions (Barnard, 1964) in the very decade that has seen such a surge in research on cognitive functions, creativity, and sex-role expectations.

PROCEDURE

It was the purpose of this study to obtain a personality profile of academic women in psychology in order to compare them (*a*) with the general female population, (*b*) with college women, (*c*) with the "successful academic men" described by Cattell and Drevdahl (1955), (*d*) with the psychologists among the academic men, and (*e*) to determine differences in women's profiles in relation to their area of vocational interest, productivity, and major activity.

The directory of the American Psychological Association provided a selected group of professional women who could be compared with the "successful academic men." Criteria of success were attainment of a doctoral degree and affiliation with a college or university. Choice of age levels was influenced by Barnard's (1964) report on academic women, wherein she noted that trends in the proportion of academic personnel who are women appeared closely related to trends in the proportion of higher degree winners who are women. She found these trends to be up from 1910 to about 1930, and down thereafter. By setting the latest year of birth at 1910, it was anticipated that the group of women in this study would represent a generation that (*a*) had sufficient vocational maturity to demonstrate maximum attainment and (*b*) were minimally affected by social factors related to the drop in proportion of academic personnel who are women.

In order to allow for direct comparison with the personality profile of the male group, personality characteristics were measured by the test used by Cattell and Drevdahl, the 16 PF Questionnaire. The handbook for the 16 PF Questionnaire (Cattell and Eber, 1957) provides descriptive information on each factor, which is presented as a bipolar measure, and is summarized with a high score corresponding to the description on the left, and a low score to the behavior on the right, as follows:

A	cyclothymia; sociable	vs. schizothymia; aloof
B	intelligence; bright	vs. mental defect; dull
C	ego strength; mature	vs. dissatisfied emotionality; immature
E	dominance; aggressive	vs. submission; mild
F	surgency; enthusiastic	vs. desurgency; serious
G	superego strength; conscientious	vs. lack of rigid internal standards; casual
H	parmia; adventurous	vs. threctia; shy
I	premsia; sensitive	vs. harria; tough
L	protension; suspecting	vs. relaxed security; accepting

M	autia; bohemian introverted	vs. praxernia; practical
N	shrewdness; sophisticated	vs. naiveté; unpretentious
O	guilt proneness; insecure	vs. confident adequacy; self-secure
Q_1	radicalism	vs. conservatism of temperament
Q_2	self-sufficiency; resourceful	vs. group dependency; sociably group dependent
Q_3	high self-sentiment formation; controlled	vs. poor self-sentiment formation; uncontrolled
Q_4	high ergic tension; excitable	vs. low ergic tension; composed

To provide bases for comparisons among the female group, information was requested on areas of vocational interest, major activities in the field of interest, and number of publications in professional journals. Areas presented were counseling and guidance, clinical, developmental, educational, experimental, industrial, learning, statistics, social, and school psychology to be ranked by number if there were more than one. Activities to be checked according to percentage of time spent were teaching, research, clinical and consulting, and administration. Respondents were also asked to indicate the number of publications for which they were senior author or coauthor. It was suggested that all material be submitted anonymously.

The survey materials were mailed to all women members of the APA in 1965 whose biographical information met criteria, that is, the doctoral degree, affiliation with a college or university, and birth in 1910 or earlier. Names of 296 women were thereby obtained. Even though addresses were checked in the 1967 APA directory supplement, 63 surveys were returned because of unknown address. An additional 6 were returned because of death or disability. Data were supplied by 124 respondents.[1]

Results were analyzed by a computer program that provided means and standard deviations of raw scores for the total group and for subgroups; since no directionality of differences was hypothesized, all *t* tests were two-tailed tests. Within-group differences of the women were reported in terms of significant differences between raw score means. Because a revised form of the 16 PF Questionnaire (Cattell and Eber, 1962; Form A) was used, comparisons between male and female groups were based on standard scores. Standard scores are expressed in stens (Cattell and Eber, 1957). Sten scores range from 1 to 10, with a mean of 5.5 and a standard deviation of 2. To compare the similarity of sten profiles between men and women, a pattern similarity coefficient (r_p) was obtained.

RESULTS AND DISCUSSION

The psychologists who provided the personality profile of the "successful academic women" represent a generation with an increasing proportion of women winning doctorates and entering the academic field. Despite their increasing numbers, access to areas considered the province of men was not generally approved nor readily gained. It would therefore be anticipated that these women would differ from women in general in a number of personality characteristics.

Since the youngest was 58 at the time of the study, and 65.7 was the mean age for the group, it must be also assumed that for most their peak in professional attainment and productivity had been achieved. Responses on the distribution of time spent in activities revealed that for 49% the major portion of their time involved teaching; for 34%, counseling and consulting; for 14%, administration; and for 8%, research. The rank order of activities compares closely with the "main phases of work" reported by 129 women psychologists in colleges and universities two decades ago (Fjeld and Ames, 1950). Teaching was given considerably greater emphasis than guidance-related work such as counseling and testing, while considerably less emphasis was noted for research and administration. In 1950, 24% reported research as a major emphasis of work, while in the present study the proportion of women in research was considerably smaller. The apparent drop in research activity over a 20-year period for this age-selected group raises a question of whether research tends to be a function assigned to women early in their career, or whether this activity has become increasingly the domain of men.

Major interests were distributed as fol-

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[1] The percentage of respondents to the questionnaire correspond to those reported for the successful academic men by Cattell and Drevdahl (1955), to a survey of America's psychologists by Clark (1957), and to a questionnaire study of women psychologists (Fjeld & Ames, 1950).

lows: 24%, clinical; 20%, counseling and guidance; 22%, developmental; 10%, educational; 2%, experimental; 1%, industrial; 3%, learning; 1%, statistics; 3%, social; 5%, school psychology; and 9% in diverse areas of specialization, such as exceptional children.

A wide range in number of publications was reported. It is understandable that full-time teaching, counseling, or administration, or combinations thereof would limit if not preclude opportunities for writing. Thus, productivity in terms of senior authorship was zero for 22 respondents, yet soared to a prodigious 275 for one psychologist.

Comparisons with the General Female Population

Contrasted with the norm group of women (Cattell and Eber, 1962), the psychologists differed significantly on 14 of the 16 personality factors. Means and standard deviations are shown in Table 1 for the psychologists, with significance of differences between psychologists and (*a*) women in general and (*b*) college women.

Because age trends in relation to the personality factors have not been demonstrated, no age corrections on scores were made. However, an assumption of stability in personality characteristics after maturity was supported by Roe's (1953, p. 226) study in which she found a "practically negligible" effect of age on the personality of scientists. More recently, Helson (1967a), in her study of creative women, demonstrated that personality patterns become consolidated with maturity.

The following comparisons are expressed in the descriptive terms provided by Cattell and Eber (1957) for persons scoring above or below the norm on each factor. No significant differences appeared on ego strength (C) and self-control (Q_3). In these respects the academic women scored in the normal feminine range of emotional maturity and will power.

It is not surprising to find these college-affiliated women more intelligent (B+), and liking words, working alone, and intellectual companionship (A−). Such characteristics seem essential for the years of concentrated study required in their professional preparation. That they also are more silent and introspective, more serious and concerned (F−) than the average woman is understandable for the same reason. Involved with the manipulation of ideas rather than action-oriented processes, social scientists tend to be more introverted than extroverted, a characteristic that is further described by a high M score. The M+ person is characterized as having intense subjectivity and inner mental life.

These personality characteristics could

Table 1. Means and Standard Deviations for Women Psychologists and Significance of Differences between the Psychologists and Average Women, and College Women

Factor	*M*	*SD*	*Differences from average women*	*t*	*Differences from college women*	*t*
A	9.37	3.57	—2.51	8.20*	—2.39	7.05*
B	9.85	1.56	+3.93	20.79*	+2.13	12.59*
C	15.91	4.11	— .12	.31	+1.14	3.03*
E	12.49	3.93	+2.95	6.77*	+1.80	4.85*
F	11.45	3.77	—1.90	4.75*	—3.91	9.41*
G	12.48	3.29	—1.16	3.83*	— .40	1.24
H	14.22	4.84	+1.91	4.09*	+1.91	3.94*
I	13.23	2.90	+1.39	5.15*	+1.47	5.17*
L	6.67	2.51	—1.74	6.17*	—1.92	6.70*
M	13.71	2.99	+ .97	3.19*	+1.25	3.85*
N	11.34	2.66	+ .70	2.94*	+ .89	3.47*
O	7.67	3.28	—3.03	8.56*	—2.94	8.31*
Q_1	12.12	2.47	+3.35	13.14*	+3.48	12.97*
Q_2	12.54	3.31	+2.52	7.94*	+2.85	8.65*
Q_3	11.72	2.80	+ .20	.72	+1.09	3.90*
Q_4	10.05	4.47	—2.87	6.26*	—2.75	5.95*

* $p < .01$.

lead to a premature conclusion that here is a recluse. On the contrary, academic women psychologists as a group are imaginative in inner life and conversation, with a liking for new experience (I+). Accepting and adaptable, open, and ready to take a chance (L−), they are also more inclined to experiment with problem solutions, more radical, and less inclined to moralize (Q_1+). The implied tolerance for ambiguity may explain a G score that is below the mean for the general population. It is probable that a "loosening" of rigid internal standards (G−) is a requisite for a sensitivity to problems and the freedom to experiment with problem solutions.

The "successful academic women" scored high on assertiveness and self-assurance (E+), supporting an inference by Maccoby (1963) that dominance appears to be a prerequisite in analytic thinking.

Independence is indicated by the high Q_2 score, indicating that the women psychologists as a group are self-sufficient, and accustomed to making their own decisions. Decision making is enhanced by having an exact, calculating mind, a flexible viewpoint, and insight regarding self and others (N+). Dominance and self-sufficiency are qualities that the culture apparently does not seem to foster in the growing female. How, then, did the academic women overcome apparent cultural influences toward dependency?

A partial answer may be given by the high H score; the H+ person is described as adventurous, impulsive, showing little inhibition by environmental threat. Additional strength for resisting environmental pressure is provided by the strong self-confidence (O−) and low anxiety level (Q_4-) of the study group. Of all the factors, it is probable that these three characteristics were most sustaining to these academic women of a generation that took a predominantly negative view of females in higher education.

In summary, the composite profile describes the academic woman psychologist as an introspective person, with an imaginative inner mental life. Although she enjoys working alone and is independent and self-sufficient, she is ready to take a chance and welcomes new experiences. Self-assertive, she is also insightful regarding herself and others, and not inclined to moralize. Without rigidity in standards, she is flexible in viewpoint and adaptable. Emotionally stable and with adequate self-control, she also seems fairly free from general anxiety.

Similarities are observed with earlier studies of young bright adults. Helson (1961) described her group of women mathematicians as serious, introverted, highly intelligent, and independent-minded. In their study of very bright adults, Southern and Plant (1968) found their women, compared to the norm, to be significantly more theoretically, aesthetically, and independently oriented, and also relatively impulsive and uninhibited.

Comparisons with Women College Students

The academic women differed from the college women as they did from the general population norm on all factors, except C, Q_3, and G. Differences were significant for every factor except G, superego strength, as shown in Table 1.

Although the academic women did not differ from women in general in ego strength (C) and will power (Q_3), they were significantly more emotionally mature and controlled than the women college students. It seems probable that age difference might account for the greater emotional stability and self-control of the older women. Perhaps the planned study of a younger group of "successful academic women" will provide more information on the function of age on Factors C and Q_3.

It is rather provocative to note that the students and academic women did not differ in superego strength (G). The trend for these college-educated women was the same as for the men; contrasted with the college students, eminent academic men (Cattell and Drevdahl, 1955) likewise were not found to differ on Factor G. Both male and female groups scored lower than average. Could it be that the successful intellectual person tends to be less judgmental both of himself and of others?

Comparisons with Successful Academic Men

Cattell and Drevdahl's (1955) groups of leading scientists differed markedly from the norm for the general population. In Figure 1 mean sten scores for the "successful academic men" are presented with mean sten scores for

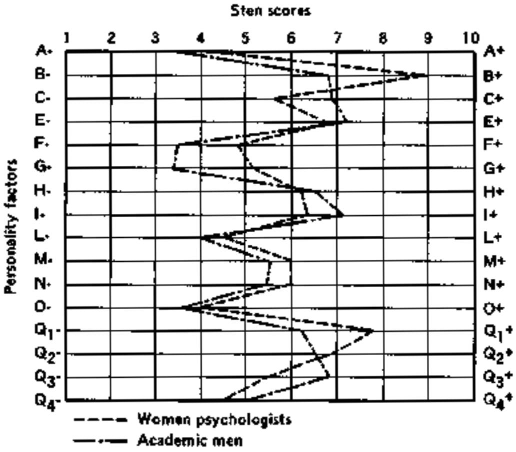

Fig. 1. Personality profiles of academic men, and women psychologists.

the "successful academic women." The profiles are strikingly similar, moving on most factors in the same directions from the average, represented by the area between stens 5 and 6.

Both groups differ from the general population in their aloofness (A−), intelligence (B+), assertiveness (E+), seriousness (F−), lack of rigidity (G−), adventuresomeness (H+), sensitivity (I+), adaptability (L−), self-confidence (O−), radicalness (Q_1+), and self-sufficiency (Q_2+). Computation of the pattern similarity coefficient (Cattell & Eber, 1957) showed an r_p of .70, indicating a high correlation of profiles.

However, observation of the somewhat greater distances between the successful academic male and female on stens B, C, F, G, Q_1, and Q_3 suggests possible differences. Relative to intelligence Barnard (1964) observed, "women who receive the doctor's degree are, on the usual types of measure, intellectually superior on the average to men who do [p. 78]." Although the profile of eminent men manifests well above average intelligence (B+), the higher sten for the academic women suggests that they may need even greater intellectual power than the men to compete in scientific and academic attainment. On the other hand, while above average ego strength (C+) and will power (Q_3+) were not indicated as components in feminine success, the academic men seem to have particular emotional maturity and stability and more exacting will power.

The academic women show a tendency to be less serious (F) and somewhat more rigid in internal standards (G). Although the academic men are less conservative (Q_1) than average, the women seem considerably more radical, a characteristic no doubt essential to their departure from the typical female image.

Comparisons with Psychologists among the Successful Academic Men

It is of particular interest to note the profile of the women psychologists as compared with Cattell and Drevdahl's (1955) group of 107 eminent psychologists in higher education presented in Figure 2.

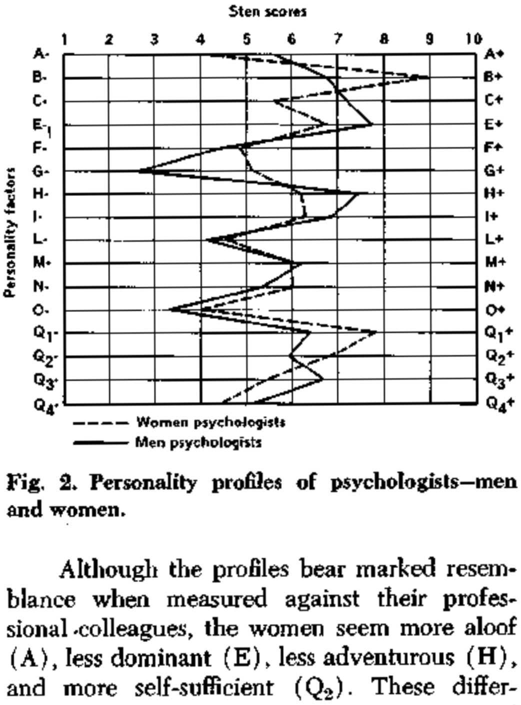

Fig. 2. Personality profiles of psychologists—men and women.

Although the profiles bear marked resemblance when measured against their professional colleagues, the women seem more aloof (A), less dominant (E), less adventurous (H), and more self-sufficient (Q_2). These differences may be related to the composition of male and female groups. Of the men psychologists, 49% were engaged in research, 26% in administration, and 25% in teaching; whereas only 8% of the women spent the major portion of their time in research, 14% in administration, and 49% in teaching. Furthermore, 34% of the women were involved in clinical and consulting activities that were not represented in the male group. Personality differences may, therefore, be more closely related to variations in vocational activity than to differences between male and female psychologists in general.

Of perhaps greatest importance is the strong similarity between the men and women psychologists, indicated by the pattern coefficient of similarity which was the same as for Figure 1, r_p .70.

Differences in Personality Factors among Women Psychologists in Different Interest Groups

The analysis of differences in personality factors between interest groups includes counseling and guidance, clinical, and developmental psychologists. There were too few in each of the other areas of specialization among the women of this generation to justify statistical comparisons. There were many more similarities than differences in personality factors among the women who specialized either in clinical, counseling, or developmental psychology. Means, standard deviations, and *t* tests for each subgroup are shown in Table 2.

Differences in the greatest number of factors (A, H, O, Q_2, and Q_4) were found between the women who identified themselves as counseling and guidance psychologists and those who considered themselves developmental psychologists. These two groups seem to represent two ends of a continuum ranging from a more adventuresome and confident, warm and sociable, relaxed, but also more group-dependent personality (the counseling psychologists) to the more socially aloof, intellectually independent, tense, and self-sufficient developmental psychologists. There were only two significant differences between the counseling and the clinical psychologists (on Factors H and O), with the counseling psychologists scoring higher on adventuresomeness and self-confidence than the clinical psychologists. In turn, the clinical psychologists differed from the developmental psychologists on only *one* factor: they were, as a group, more sociable than the developmental psychologists.

With the exception of a study of interests and activities among psychologists by Clark (1957) and a study by Merenda and Clark (1963) that shows a greater discrepancy between the perception of real, ideal, and job self for clinical than for guidance psychologists, there are no comparable studies of personality factors among men psychologists which have demonstrated differences in personality characteristics among different areas of specialization.

The differences found in the present study among women psychologists point to a more "action and social relations" oriented personality among the counseling and educational psychologists, a less "secure" personality among the clinical psychologists, and a more "intellectually independent, reflective, and self-sufficient" personality among the developmental psychologists. The comparative differences among the subgroups of women psychologists should be regarded with caution. As pointed out by Clark (1957), unlike medicine, psychology does not have a series of clearly demarked specialty areas. Furthermore, the specialty groupings represent the first choice of major interest areas, and many of the women indicated a secondary interest.

Differences in Personality Characteristics between Women Psychologists with Few and Many Scientific Publications

For purposes of comparison we chose 30 women with no publication or only one and compared them with 30 women psychologists with the largest number of publications (26–275)—senior authors only. They represented the bottom and top quartile of the publication distribution among the respondents.

A comparison of the 16 PF Questionnaire profiles of the "high" and "low" publishers among these women psychologists reveals significant differences on only *one* personality factor: A (see Table 2). The more productive women psychologists as a group preferred contemplation over social relationships, liked working alone, intellectual companionship, and rigor of thought. In this they resemble strongly the "significant contributors" to American psychology studied by Clark (1957), 95% of whom were men. As a group the highly productive women psychologists also scored higher on Factor B (intelligence) and lower on Factor Q_4 (less unresolved tension and anxiety), but the differences on these two factors between the high and low publishers did not reach the .05 level of confidence.

The largest proportion of women psychologists with many publications (about 40%) came from the area of developmental psychology; the lowest proportion came from the area

Table 2. Means and Standard Deviations for Interest Groups and High and Low Publishers and Significance of Differences within Groups

	CG		*C*		*D*		*H*		*L*		*Differences*			
											CG/C	*CG/D*	*C/D*	*H/L*
Factor	*M*	*SD*	*M*	*SD*	*M*	*SD*	*M*	*SD*	*M*	*SD*	*t*	*t*	*t*	*t*
A	10.52	3.14	10.10	3.16	8.07	3.69	8.27	3.69	10.43	3.27	.50	2.57*	2.25*	2.41*
B	9.68	1.25	9.90	1.54	9.85	1.59	10.27	1.55	9.80	1.32	.59	.43	.13	1.25
C	17.04	3.42	15.29	3.35	14.81	4.84	15.53	4.65	15.43	3.71	1.93	1.90	.44	.09
E	12.32	4.11	12.94	3.96	11.81	4.12	12.77	4.21	12.03	3.20	.57	.44	1.05	.76
F	12.12	3.42	12.35	3.34	10.44	4.30	11.03	3.86	11.83	3.54	.26	1.55	1.90	.84
G	12.48	3.42	13.16	3.19	12.37	3.01	12.70	3.28	12.43	3.36	.77	.12	.97	.31
H	16.44	5.03	13.55	4.68	13.41	5.20	14.47	5.14	14.27	4.90	2.22*	2.13*	.11	.15
I	13.92	2.43	13.52	3.05	13.59	2.36	12.93	2.41	13.07	3.50	.54	.49	.11	.17
L	5.96	2.56	6.16	2.37	5.67	2.20	5.57	2.64	6.10	2.32	.31	.44	.82	.83
M	13.04	2.82	14.26	3.01	13.81	3.36	13.97	3.82	13.13	2.54	1.55	.90	.53	.99
N	11.80	2.24	11.61	3.25	10.59	2.14	11.63	2.76	11.43	2.22	.24	1.99	1.39	.31
O	6.68	2.93	8.77	3.53	8.48	3.27	7.53	3.30	7.77	3.40	2.38*	2.09*	.33	.27
Q_1	11.68	2.67	11.84	2.22	12.56	2.93	12.40	2.16	12.30	2.77	.24	1.12	1.06	.16
Q_2	11.24	3.72	12.39	3.35	13.15	2.91	12.67	2.80	12.30	3.59	1.21	2.07*	.92	.44
Q_3	12.32	2.34	11.29	2.47	11.37	2.31	11.83	2.94	11.23	2.34	1.59	1.47	.13	.87
Q_4	9.32	3.60	11.35	4.18	12.00	4.81	9.70	4.93	10.83	4.27	1.93	2.26*	.55	.95

Note—CG = Counseling and Guidance ($N = 25$); C = Clinical ($N = 31$); D = Developmental ($N = 27$); H = High publishers ($N = 30$); L = Low publishers ($N = 30$).

* $p < .05$ (two-tailed).

of clinical and counseling psychology (15% each). Nearly two-thirds of the women psychologists with no or only one publication in their professional lifetime came from the clinical and counseling fields, demonstrating a concern expressed in a study of counselors by Rubin (1957), who believed they should be doing more research. The majority of the very productive women psychologists were engaged in teaching, with the remainder about evenly divided between research, clinical work, and administration.

However, only about 20% of the women with a significant number of publications worked full-time in one activity; nearly half of the productive women psychologists spent their time divided between teaching and other activities. In contrast, nearly 90% of the women with no or only one publication during their professional lifetime were full- or at least three-quarters time engaged in the pursuit of one job activity, and found or took little time for writing.

The more varied the activities were that the women psychologists engaged in, the greater the likelihood for their productivity. In this they resembled the significant contributors to psychology among the men (Clark, 1957).

Both Clark's (1957) assessment of American psychologists and Drevdahl's (1964) studies of personality characteristics of productive and nonproductive psychologists raised the question whether the lack of productivity of the practicing clinician (as measured by contributions to the scientific literature) might be due to the kind of person he is, his orientation to life and people, and his preference for social action over contemplation, rather than to any educational experience. Clark (1957) suggested:

> The psychologists who do not achieve eminence have the same sort of motivation for service to society but express this to a much greater extent through face-to-face contact with individuals and helping persons achieve solutions to personal problems. Any great expenditure of time in this kind of enterprise would prevent a person from engaging in activities which would call him to the attention of other psychologists around the country. Thus it very well may be that what we observe are differences in manner of achieving a solution to human problems rather than differences in attitude about the need for such solutions [p. 95].

This difference in orientation is also apparent among the women psychologists who represent different fields of *interest*. The great similarity, however, in the personality profiles of the "high" and "low" publishers among the women psychologists makes one wonder whether opportunities and restrictions of the major fields of *activity* play a greater role in scientific productivity than differences in personality factors among this group of successful academic women. Barnard (1964) concluded that academic position is "inextricably related to productivity [p. 154]." Rossi (1965a) has pointed out that self-selection works much more strongly among women, so that at each higher level of education, women probably have a greater potential for significant achievement than men, but greater claims are also made on their "instant availability" in social interaction and service for others. It is a much rarer social phenomenon to find women than to find men with intense channeling of energy in professional work and tolerance of and preference for social isolation.

Differences in Personality Factors among Women Psychologists Engaged in Different Types of Activities

Again, there were many more similarities than differences among the personality profiles of women psychologists who were engaged in different types of professional activities, as shown in Table 3.

With a single exception, there were *no* significant differences among the women psychologists who were predominantly engaged in administrative, consulting, or research work. On one factor, N, the women in administration indicated greater social alertness than those in a consulting or clinical function. Women whose major activity was college teaching, however, differed from the other groups on a number of personality factors. When compared with women psychologists in administration, they appeared less dominant (E), less self-assured (O), and more anxious (Q_4). When compared with women psychologists who were engaged in counseling, they were more serious and exacting (F; Q_3) and less dominant (E).

Table 3. Means and Standard Deviations for Activity Groups and Significance of Differences within the Groups

	T		*R*		*CC*		*A*		*Differences*					
									T/R	*T/CC*	*T/A*	*R/CC*	*R/A*	*CC/A*
Factor	*M*	*SD*	*M*	*SD*	*M*	*SD*	*M*	*SD*	*t*	*t*	*t*	*t*	*t*	*t*
A	9.02	3.66	9.00	3.57	10.11	3.42	9.43	3.76	.02	1.40	.37	.86	.27	.62
B	9.82	1.67	9.78	1.79	9.78	1.51	10.21	1.72	.06	.09	.78	.01	.59	.87
C	15.24	4.04	15.44	5.13	16.54	4.31	17.07	2.76	.13	1.43	1.58	.66	.99	.43
E	11.65	3.81	11.78	2.64	13.54	4.26	14.00	3.92	.09	2.16*	2.02*	1.18	1.49	.35
F	10.47	3.55	11.89	2.76	12.92	3.74	11.29	4.45	1.13	3.10†	.72	.77	.36	1.32
G	12.22	3.43	12.67	3.61	12.19	3.54	13.14	2.32	.35	.05	.94	.36	.39	.93
H	13.22	4.73	13.22	4.12	15.11	5.29	15.64	4.70	.00	1.74	1.69	1.00	1.26	.33
I	12.98	3.17	12.78	1.30	13.84	2.37	12.50	2.56	.19	1.38	.52	1.29	.30	1.76
L	5.73	2.46	4.89	3.48	5.70	2.38	5.29	2.89	.89	.06	.58	.84	.30	.53
M	13.69	3.24	13.00	2.40	13.81	2.89	13.21	2.19	.61	.17	.52	.78	.22	.70
N	11.67	2.81	10.78	2.05	10.78	2.76	12.50	2.18	.91	1.47	1.02	.01	1.89	2.09*
O	8.45	3.15	7.33	4.56	7.59	3.45	6.29	2.70	.91	1.19	2.33*	.19	.70	1.28
Q_1	12.31	2.56	11.11	2.20	11.95	2.27	11.93	2.13	1.31	.68	.50	.99	.89	.02
Q_2	12.80	2.97	13.33	1.66	11.97	3.21	12.14	4.96	.53	1.23	.62	1.22	.69	.14
Q_3	12.29	2.60	10.11	4.86	11.05	2.36	11.00	2.22	1.98*	2.26*	1.68	.85	.60	.07
Q_4	11.00	4.62	9.11	4.76	10.14	4.08	7.79	4.41	1.12	.90	2.32*	.65	.68	1.80

Note—T = Teaching ($N = 49$); R = Research ($N = 9$); CC = Clinical and Consulting ($N = 37$); A = Administration ($N = 14$).

* $p < .05$.

† $p < .01$.

When compared with women psychologists engaged full-time in research, they appeared more controlled and exacting. There was a greater similarity in personality profiles between the women psychologists whose major activity was college teaching and those whose major activity was research than between the college teachers and the other two groups (administration, counseling). A study of college teachers in the sciences (Eckert and Stecklein, 1961) has shown that women teachers seem more psychologically dependent on comfortable relations with other people, both peers and the young. This, together with their decided minority status in the academic field (less than 10% of all academic positions, only 1% of the professorships in the biological and social sciences are held by women; Mattfeld and Van Aken, 1965), may explain why, in spite of greater will power, women psychologists in college teaching may appear less confident and secure than women choosing other activities within the academic setting. The most dominant and self-assured would be most likely, over a professional lifetime, to achieve administrative positions.

Cattell and Drevdahl's (1955) 16 PF Questionnaire study of teachers, administrators, and researchers among men in several scientific fields likewise found no significant differences between teachers and researchers among the academic men in psychology. The differences between teachers and administrators among the men were in the direction of greater adventuresomeness (H), unconventionality (M), will control (Q_3), and anxiety (Q_4). These differences also appear in our comparisons between academic women who were college teachers and those who were administrators.

CONCLUSIONS

The women in this study were selected to represent a sample of academic women who had reached their peak attainment in a scientific profession. Generalizations from the findings, therefore, are intended for this particular reference group. Successful academic women in psychology differ from adult women in general and from women college students in many of the same personality characteristics in which they resemble successful academic men. As a group, they tend to be more intelligent, socially aloof, dominant, serious, adventuresome, sensitive, flexible, imaginative, insightful, unconventional, secure, and self-sufficient than adult women in the general population and women in college, and less anxiety prone. They score within the normal adult feminine range in emotional maturity and will power, but appear significantly more emotionally mature and controlled than women college students.

The great number of significant differences between the personality profiles of the college women and the successful academic women and the lack of similarity between the personality profiles of gifted girls in special education programs and recognized creative people, both men and women, seem to indicate that a selection strictly on the basis of intelligence and achievement tests does not lead to the discovery and encouragement of potentially creative girls in special classes from the elementary to the college level.

In spite of a striking similarity between the 16 PF Questionnaire profiles of successful academic men and women and the men psychologists, the women psychologists score, as a group, higher than the successful academic men on intelligence, superego strength, and unconventionality (radicalism), and lower than the academic men on self-sentiment.

Of the three major interest groups represented among the respondents, the developmental psychologists among the women of this generation contained the greatest proportion of significant contributors to the scientific literature. The counseling and guidance psychologists were more "social action and people oriented"; the clinical psychologists had more conflicting and less secure personality profiles. Both groups contributed a much smaller proportion of scientific articles.

Differences between high and low publishers centered on Factor A; the significant contributors among the women psychologists were more socially aloof and exacting. More productive women were college teachers, and more shared their professional time between teaching and other activities. Women in clinical and administrative work contributed relatively fewer publications. Demand made on the women's time or type of assignments appeared more important than differences in personality characteristics among these women whose self-selection would yield a greater percentage with potential for scientific contributions.

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Women Mathematicians and the Creative Personality

Ravenna Helson

Women mathematicians are rare. It has been suggested by both mathematicians and psychologists, informally, that a *creative* woman mathematician would have a brain different from that of other women. A normal woman, others say, could not so reject the life of feeling and concreteness without stifling her originality in the process.

Yet creative women mathematicians do exist. It seemed possible that these women, if they were not "mutants," might show conspicuously the essential traits of the creative personality, without which they would not have overcome whatever barriers make their numbers so small. A study of these Ss, then, might contribute to our understanding of creativity, regardless of sex, and certainly to the appraisal of creativity in women, and women's potential for scientific accomplishment (Mattfeld and Van Aken, 1965).

This paper describes the personality, research style, and background of some 45 women mathematicians, a sample from an estimated 300 in the United States at the time of the study (Albert, 1957). Of the 45, 18 included virtually all creative women mathematicians in the United States.

The study is one of several which have been conducted by the Institute of Personality Assessment and Research in the area of the creative personality. Particular reference will be made to a companion study of creativity in male mathematicians (Helson, 1967; Helson and Crutchfield, 1970).

METHOD

Selection of Sample

Names of women, who had attended graduate school and obtained the PhD in mathematics between 1950 and 1960, were furnished by the following institutions: Bryn Mawr College, Cornell University, Stanford University, Yale University, University of California (Berkeley and Los Angeles), University of Oregon, University of Texas, and University of Washington. Mathematicians at these and other institutions also provided additional names, particularly of women they considered creative. Columbia University, Massachusetts Institute of Technology, New York University, Radcliffe, University of Chicago, University of Illinois, and University of Pennsylvania each produced at least two of these Ss.

The Ss were invited to participate by means of a letter which explained the long-term interest of the Institute in studying soundness, achievement, creativity, and other forms of high-level functioning, and its present interest in conducting studies of professional women. A small honorarium was offered. Of 53 invitations extended, 44 (83%) were accepted. Three of these Ss were tested later than the others, and their data are not included in all analyses. Three additional mathematicians, being wives of faculty members at the University of California (Berkeley), were asked to provide data only about their research style. The number of Ss thus varies between 41 and 47.

The creativity of each S was rated by mathematicians in her field of specialization. A 7-point scale was used, a rating of 4.0 signifying that S was about as creative as the author of an average research paper in a mathematical journal. An average of three ratings

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was obtained for each S. Fewer than three were obtained for several older women who had not published beyond their dissertation. Ratings were highly reliable. More than half of the Ss received ratings with a range of less than two, and only two Ss received ratings with a range of more than three. The distribution of ratings was as follows: 8 Ss received average ratings of 3.0 or below, 8 were rated between 3.0 and 4.0, 12 between 4.0 and 5.0, 8 between 5.0 and 6.0, and 8 above 6.0. The Ss rated above 5.0 were classified as "creative." The creative group thus consisted of women rated as clearly more creative than the author of an average research contribution in mathematics. Subsequent comments from mathematicians lead us to believe that there were no important omissions from the creative group.

In age, Ss ranged from 24 to 64, the average age being 41. Two-thirds were married. One-third had Jewish parents, and most of the rest were from Protestant backgrounds. Creative and comparison women did not differ in these respects, nor in quality of graduate school. As in the sample of male mathematicians (Helson and Crutchfield, 1970), foreign cultural influence was strong. Half of the creative Ss were born in Europe or Canada, and almost half of the Ss born in the United States had at least one parent born in Europe. The difference between creative and comparison Ss in foreign birth is significant at the .10 level. However, foreign-born and native-born creatives differ not at all in any of the characteristics that we shall report as significantly differentiating the creative from comparison Ss. The personality of the creative mathematician, among women, seems to cut across national boundaries.

Procedure

Two weekend assessments were held at the Institute of Personality Assessment and Research, and two others in the East, at Bryn Mawr and Swarthmore Colleges. Staff observers and interviewers did not know which Ss were creative, and indeed the criterion judgments had not yet been obtained. The assessment included a great variety of tests and measures, and the following have been selected for range of coverage and to demonstrate the consistency with which some of the salient findings recur:

1. Intelligence: Concept Mastery Test (Terman, 1956) and Mechanical Comprehension Test (Bennett, 1951); the Wechsler Intelligence Scale was administered after the assessments as part of a larger study by MacKinnon and Hall (1968) of the relation between intelligence and creativity.
2. Overall personality characteristics: California Psychological Inventory (Gough, 1964); Minnesota Multiphasic Personality Inventory (Hathaway and McKinley, 1951); Type Indicator (Myers, 1962); staff observations recorded by means of the 100-item Clinical Q Sort (Block, 1961); Adjective Check List (Gough and Heilbrun, 1965).
3. Interests: Strong Vocational Interest Blank (Male Form) (Strong, 1959); Activities Check List (Gough and Hall, 1957).
4. Cognitive and aesthetic tests: Gottschaldt Figures, Street Gestalt, Insight Puzzles, and Masked Word Tests as adapted by Crutchfield (MacKinnon, Crutchfield, Barron, Block, Gough, and Harris, 1958); Unusual Uses and Match Problems Tests (Guilford, Wilson, Christensen, and Lewis, 1951); Art Scale (Barron and Welsh, 1952); Mosaic Construction Test (Hall, 1958).
5. Mathematical style: Mathematicians Q Sort (Helson, 1967).
6. Personal history: Personal history questionnaire and interview.
7. Professional history: Professional history questionnaire.

RESULTS

Validation of Criterion

Several findings support the criterion in showing that the women classified as creative were indeed performing at a level superior to that of the comparison Ss and had done so in the past. The creatives received the PhD at an average age of 26.0, the comparison Ss at an average age of 28.5 ($p < .05$). The creatives submitted their first paper for publication at an earlier age (before the PhD rather than after), had published more papers, and had received more grants and fellowships since graduate school ($p < .01$). Some of these findings are illustrated in Table 1.

Intelligence

According to data obtained for a partial sample of the women mathematicians by MacKinnon and Hall (1968), the group had an average IQ on the Wechsler Adult Intelligence Scale of 131. Creative and comparison Ss did not differ significantly, but only seven creatives were tested. On the Concept Mastery Test, developed to measure the utilization and enrichment of high intelligence in adult cultural experience, the creatives had a mean score of 144, and the comparison women a mean of 126. By t test, the difference is significant at the .10 level (Table 2), and the correlation with criterion ratings of creativity (.31) reaches the .05 level. Since the average score for the Stanford gifted *Ss* was 137, for industrial research scientists, 118, and for military officers, 60, one may judge that the mathematicians' scores are high. The creative male mathematicians had a mean score of 148, which is very similar to that of the creative women.

The Bennett Mechanical Comprehension Test has repeatedly shown large sex differences in the ability to understand physical and mechanical relationships (Bennett & Cruikshank, 1942), and it has been supposed that sex differences in this ability contribute to the

Table 1. Professional Achievement of Creative and Comparison Women Mathematicians

Achievement	*Creative Ss*	*Comparison Ss*	χ^2[a]
Age first published paper submitted			
Under 25	10	2	13.06[*]
25 or older	6	26	
Number of research papers			
5 or more	12	3	15.98[*]
Fewer than 5	4	25	

[a] Yates' correction embodied.
[*] $p < .001$.

Table 2. Differences in Intelligence and Personality between Creative and Comparison Subjects

Measure	*Creative Ss*		*Comparison Ss*			
	M	*SD*	*M*	*SD*	*t*	*df*
Intelligence						
Wechsler Adult Intelligence Scale	128.3	7.5	132.6	9.2	—	25
Concept mastery	143.6	30.1	125.6	33.5	1.75	42
Mechanical comprehension	31.0	10.0	33.7	9.2	—	39
Personality						
California Psychological Inventory						
Flexibility	15.3	3.5	11.0	4.6	3.09†	39
Achievement via conformance	26.2	3.4	29.7	3.4	−3.16†	39
Communality	24.1	1.7	25.4	1.9	−2.28[*]	39
Minnesota Multiphasic Personality Inventory						
Mean level	56.6	6.3	53.1	5.0	1.96[*]	41
Validity (*F*)	59.0	6.1	54.0	6.0	2.52[*]	41
Hypochondriasis	56.1	9.8	49.4	6.6	2.58[*]	41
Depression	60.0	9.9	51.1	9.7	2.76†	41
Masculinity–femininity	48.5	5.1	45.0	10.0	—[a]	41
Psychasthenia	58.4	9.3	51.2	6.8	2.84†	41
Schizophrenia	60.6	9.0	54.9	6.8	2.26[*]	41
Hypomania	48.8	11.6	54.9	7.5	−2.02[*]	41
Social introversion	63.9	10.8	56.6	9.9	2.20[*]	41
Repression (Welsh)	22.5	3.7	18.2	3.5	3.70‡	41
Aesthetic measures						
Mosaic construction: artistic merit	54.0	12.5	39.9	17.1	2.06[*]	27
Art scale	28.1	12.5	26.9	15.4	—	41

[a] $F = 3.76$; $p < .01$.
[*] $p < .05$ (two-tailed).
† $p < .01$.
‡ $p < .001$.

disinclination of women for the abstract sciences. The women mathematicians did well ($M = 32.7$) as compared with most women, although less well than a sample of about 90 men, most in middle management but some in research supervision ($M = 41.5$). Military officers, whose low Concept Mastery scores were previously reported, had a mean on this test of 35.0. The creative and comparison women mathematicians did not differ (Table 2).

Personality Characteristics

Inventories. On the California Psychological Inventory, the women mathematicians score slightly below the mean on measures of social poise and assurance, and have peak standard scores of about 65 on Psychological Mindedness and Achievement by Independence. The creative Ss show these characteristics somewhat more markedly than the comparison Ss.

As shown in Table 2, the creatives have higher scores than comparison women on the Flexibility scale ($p < .01$) and lower scores on Achievement via Conformance ($p < .01$) and Communality ($p < .05$). On all three of these scales, their mean standard scores—69, 45, and 41, respectively—are the most extreme of any creative or comparison group studied at the Institute of Personality Assessment and Research. Though strongly motivated to create their own form and to express and validate their own ideas, then, the creative Ss do not like to perform routine duties, nor can they work well within a highly structured framework. They are not bound by, do not recognize, and perhaps somewhat stubbornly resist, conventional patterns.

On the Minnesota Multiphasic Personality Inventory, the profiles for creative and comparison Ss show little similarity. Although the comparison women have higher scores on the Hypomania scale ($p < .05$), the creatives have a higher "mean level" on the eight clinical scales (Table 2). On individual scales, they score higher on Validity (F), Hypochondriasis, Depression, Psychasthenia, Schizophrenia, and Social Introversion. Barron (1968) and MacKinnon (1962) have reported a similar elevated mean level for creative writers and architects. Since the women mathematicians, like the writers and architects, do not score low on measures of ego strength, the interpretations offered by Barron and MacKinnon seem to apply in this sample as well. To some extent, the elevated scores may be taken to reflect complexity of personality and lack of defensiveness. However, as both of these authors go on to say, the creative Ss seem really to have more psychological difficulties than the comparison Ss, though they differ from most neurotic or psychotic patients in also having excellent resources for dealing with their troubles. It may be relevant to the apparent maladjustment of the creative women mathematicians and male writers and architects that all these Ss are in "cross-sex" fields (Roe & Siegelman, 1964). Creative male mathematicians had higher scores than comparison Ss only on the Hypochondriasis scale.

Creative men usually score high on the Masculinity-Femininity scale of the Minnesota Multiphasic Personality Inventory. The creative women are significantly less variable on this scale than the comparison Ss, and they are (insignificantly) more feminine (Table 2). The creatives have higher scores on the Repression factor (Welsh, 1956). The combination of high Repression and high Flexibility, as manifested on the California Psychological Inventory, is unusual. In this context, the high Repression would seem to reflect a rather rigid rejection of outside influences and attractions.

The Myers-Briggs Type Indicator shows the women mathematicians to be strongly introverted and intuitive, in the Jungian sense of these terms. The preference for thinking over feeling is very slight, particularly for the creatives. No differences between creative and comparison Ss on the scales of the Type Indicator reached the .05 level.

Staff Observations. After the assessment weekend was over, at least five psychologist-observers performed a clinical Q sort for each S. Reliability was satisfactory. Of 100 items, the following had the highest average placements for the entire sample: Genuinely values intellectual and cognitive matters; Appears to have a high degree of intellectual capacity; Values own independence and autonomy; Is a genuinely dependable and responsible person; and Prides self on being objective and rational. Items correlated with the creativity criterion (Table 3) show the creatives to be more original, intellectual, narcissistic, and unconventional.

Table 3. Clinical Q-Sort Items Correlated with Creativity Ratings

Q-sort item	*Creativity**
Thinks and associates to ideas in unusual ways; has unconventional thought processes	.64
Judges self and others in conventional terms like "popularity," "the correct thing to do," social pressures, etc.	—.62
Is an interesting, arresting person	.55
Tends to be rebellious and nonconforming	.51
Genuinely values intellectual and cognitive matters	.49
Appears to have a high degree of intellectual capacity	.46
Is a genuinely dependable and responsible person	—.45
Behaves in a sympathetic or considerate manner	—.43
Is self-dramatizing; histrionic	.42
Has fluctuating moods	.40
Favors conservative values in a variety of areas	—.40
Is moralistic	—.40

* $p < .01$.

Ten psychologists described each S on the Adjective Check List. The creatives were described more frequently as individualistic, original, preoccupied, artistic, complicated, courageous, emotional, imaginative, and self-centered. Comparison Ss were described more frequently as cheerful, active, appreciative, considerate, conventional, cooperative, helpful, organized, realistic, reliable, and sympathetic ($p < .05$).

Interests

On the Strong Vocational Interest Blank, the women mathematicians as a group had seven average scores in the A range, all in Groups I, II, VI, and X. Scales significantly correlated with the criterion ratings of creativity at the .01 level were Artist, Psychologist, Physicist, Author-Journalist and (negatively) Mortician. The entire group was thus characterized by strong symbolic interests, and the creatives especially so.

On the Activities Check List, creatives and comparison Ss expressed *strong* interest in about the same number of leisure activities, but the comparison Ss indicated a *moderate* interest in many more activities than the creatives ($p < .01$). Creatives were more homogeneous than comparison Ss in their strong interests, which were mostly intellectual in nature. More than half of the creatives checked Attending concerts, Listening to classical records, Going to plays, Reading the classics, Other reading, and Hiking. The only activity in which half of the comparison group expressed strong interest was Going to plays.

Other measures of interests on the professional history questionnaire seem to show that the creatives had simplified their lives to a few things about which they cared very much. They spent most of their time in research and homemaking; they spent less time than comparison Ss in teaching, administration, community activities, and politics ($p < .05$).

Cognitive and Aesthetic Measures

Although the creatives showed a slight superiority on most of the cognitive tests (see Procedure section), few differences reached even the .10 level of significance. However, mosaic designs made by the creatives were judged as having more artistic merit and as more pleasing than those by comparison women (Table 2). Judges were faculty members from the University of California departments of art and of design. The creative and comparison Ss did not differ significantly on the Art Scale. Both groups scored about the same as creative men mathematicians, higher than the comparison men, and lower than creative writers and architects.

Mathematical Style

In performing the Mathematicians' Q Sort, S placed 56 items about professional interests and attitudes according to a forced normal distribution, judging how characteristic each statement was of her own orientation. Items placed highest by 18 creatives and 28 comparison Ss show little overlap. The creatives seem to show a greater involvement in research, more participation of the unconscious in the research process, less interest in interpersonal communication, and less orderliness (Table 4).

Personal History

The personal history findings have been evaluated by χ^2. Results significant at the .10 level of confidence are included, since most of them deal with important variables for which precise and differentiated measures are not available. Answers to open-ended questions were rendered anonymous and evaluated independently by two raters.

Two-thirds of the fathers of the creative women were professional men, whereas most of the fathers of comparison women were businessmen or skilled workers ($p < .01$). However, the creatives frequently described their family's socioeconomic position as having been poor or insecure. The lawyer could not collect his fees, the engineer was unemployed, the professor died, etc. (Table 5).

The fathers of the creatives tended to have received more education than the fathers of comparison Ss ($p < .10$), and a disparity in education between father and mother was more frequent in the creative group ($p < .05$). In the sample of male mathematicians, both fathers and mothers of creative Ss were more highly educated than the parents of comparison Ss.

Whereas more creative than comparison men mathematicians had been eldest children, what tended to distinguish the creative women in sibling relationships was that very few of them had a brother. In families of fewer than five children, even the exceptions are interesting. One creative had an elder brother but no father, one had a brother 10 years younger, and one had a brother 2 years younger.

The personal history interviewers used checklists to describe the salient features of the home background of the Ss. Although the home backgrounds had in common an almost universal emphasis on intellectual and cultural values, the pattern was a little different for the two groups. Items checked for more than one-third of the creatives were, in order of frequency, as follows: father had strong intellectual and cultural values; father was dominant in the family; and mother had strong intellectual and cultural values. The items checked for more than one-third of the comparison Ss were these: mother had strong intellectual and cultural values; father had strong intellectual and cultural values; father was a warm person; and mother was dominant in the family. The interviewers judged more creative than comparison women to have identified primarily with the father ($p < .05$).

Asked to evaluate factors in the origin of the Ss' interest in mathematics, the interview-

Table 4. Items from Mathematicians' Q Sort Differentiating Creative and Comparison Women

Q-sort items	*t*
Items placed higher by creative *Ss* ($N = 18$)	
Subordinates other things to research goals; puts these values above all others	3.24†
Is thorough and patient in approach to research issues; does not get upset if progress is slow	2.86†
Research interests lie within a rather narrow range	2.70†
Must exert effort to express a mathematical train of thought in words	2.67†
Work is characterized by inventiveness and ingenuity	2.55*
Solution to a problem often comes from an unexpected direction	2.11*
Items placed higher by comparison *Ss* ($N = 28$)	
Has a need to teach; enjoys instructing and working with students	3.34†
Grasps other people's ideas quickly	3.05†
Desire for a salary increase is an important motivating factor	2.98†
Interested in philosophical problems which arise in mathematics	2.71†
Has an active, efficient, well-organized mind	2.58*
Has interests and talents appropriate for writing on mathematics for intelligent laymen	2.38*
Is neat and orderly in habits and manner of work	2.18*

* $p < .05$ (two-tailed).
† $p < .01$.

ers more frequently checked for creatives "sublimation of curiosity about the body and its functions" or "satisfaction of need for autonomy in fantasy," whereas for comparison *Ss*, they more frequently checked "reaction formation to primitive expressiveness" or "withdrawal" (Table 5).

Most of the *Ss* did well in school, and many of them had been regarded as precocious children. During adolescence and in college, the creatives had a stronger intellectual orientation than the comparison *Ss*, more of whom were concerned with physical appearance or social relationships (Table 5). However, the years in graduate school were described as a period of social expansion by more creative than comparison *Ss*.

Professional Achievement of Creative Men and Women

In age at PhD and age of submitting the first published paper (Table 1), the creative women did not differ from the 34 creative men studied by Helson and Crutchfield (1970b). However, at the time of the study, the creative men had published more papers ($p < .01$). They also held important positions at prestigeful universities. Only two or three of the creative women taught graduate students, and one-third, including some of the highest rated, had no regular position at all. Several had young children. Most of those married were married to mathematicians, so that nepotism was a frequent problem.

Table 5. Personal History: Creative and Comparison Women

Personal history	*Creative Ss*	*Comparison Ss*	χ^{2} [a]
Place of birth			
United States	8	23	3.63
Other	8	5	
Occupation of father			
Professional	12	7	8.44†
Other	4	21	
Economic position of family			
Poor or insecure	8	5	3.63
Other	8	23	
$\phi = .79$			
Education of parents			
Father more educated than mother	11	7	5.84*
Other	5	20	
Sex of siblings (small families)			
Brother	3	14	2.92
No sibling or sister(s) only	10	10	
Father was warm person			
Checked	3	14	2.98
Not checked	13	14	
Identification with parents			
Primarily with father	10	7	4.80*
Other	5	19	
Origin of interest in mathematics			
Sublimation or satisfaction of need for autonomy in fantasy	10	8	4.14*
Other	4	17	
Most important aspect of college			
Intellectual growth or discovery	12	7	8.44†
Other	4	21	
$\phi = .84$			
Development in graduate school			
Expansion of social interests stressed	11	8	5.16*
Not stressed	5	20	
$\phi = .65$			

[a] Yates' correction embodied.

* $p < .05$.

† $p < .01$.

Marriage would thus seem to have been a handicap to the careers of the creative women. However, 6 of 11 creatives (and 4 of 19 comparison Ss) named their husband as the greatest asset in their work lives. He provided intellectual or mathematical companionship, they said, or a circle of mathematical friends.

On the California Psychological Inventory, the creative men and women have similar profiles, except that the men are higher on measures of social poise and assurance—on Dominance, Sociability, Social Presence ($p < .05$), and Self-acceptance ($p < .01$). The men also have higher scores on Intellectual Efficiency ($p < .05$) and lower scores, of course, on Femininity ($p < .001$). On the Strong Vocational Interest Blank, the creative men have higher scores on the scales for YMCA Physical Director, YMCA Secretary, Office Man, Mortician, and Sales Manager. The women have higher scores on scales for Artist, Architect, Mathematician, Physicist, and Chemist ($p < .01$).

On the Mathematicians' *Q* Sort, creative men placed the following items higher ($p <$ .01): Has interests and talents for writing on mathematics for intelligent laymen; Desire for a salary increase is an important motivating factor; Has an earnest desire to "make a mark" in mathematics; and Is flexible and adaptable in his thinking, able to shift and restructure easily. Creative women placed higher ($p <$ .01) the following items: Is somewhat deficient in command of basic sources and technical literature; Research interests lie within a rather narrow range; Does not enjoy collaboration; Must exert effort to express a mathematical train of thought in words; Is more interested in discrete problems than in continuous ones; and Lacks confidence, is afraid to strike out in new directions.

These findings reflect the differences in professional status between the two groups, but other factors may be involved also. Support has been demonstrated elsewhere for the hypothesis that the creative men and women mathematicians have distinctive research styles which differ in level of ego control and degree of unconscious participation in the research process (Helson, 1967).

DISCUSSION

First, the findings offer no support for the idea that creative women mathematicians are "mutants" with cognitive abilities different from those of other women PhDs in mathematics. Neither do the findings show the creative women to be more masculine, if one means by this that they might have been expected to score higher on measures of masculinity–femininity, or dominance, assertiveness, or analytical ability. We cannot evaluate the hypothesis that the creative Ss may have had some greater specific talent for higher mathematics (Revész, 1940) which was only slightly reflected in intelligence measures. However, the many large differences between the creative and comparison Ss in background and personality would seem to indicate that personality characteristics are powerful determinants of creativity in women mathematicians.

The traits most characteristic of the creative women would seem to be these: (*a*) rebellious independence, narcissism, introversion, and a rejection of outside influence; (*b*) strong symbolic interests and a marked ability to find self-expression and self-gratification in directed research activity; (*c*) flexibility, or lack of constriction, both in general attitudes and in mathematical work.

These traits have all been ascribed to the creative person, regardless of sex, but they appear more clearly in creative women mathematicians than they do in creative men mathematicians (Helson & Crutchfield, 1970a, 1970b). Among the creative men, some were original, flexible, ambitious, but essentially conventional individuals. One may suppose that a conventional woman would never develop the concentration, the "purity of motive" (Ghiselin, 1952), which seems to be necessary for a new symbolic structure to emerge. A rejection of outside influence and a cathexis of symbolic activity would seem to support, or constitute, purity of motive. Although one would expect to find this complex of traits in creative persons of either sex, it shows more clearly—being more necessary—in the creative women. The third characteristic, flexibility, may be interpreted as a lack of conflict in the person's basic goals. There is cooperation between the ego and the life of impulse; the individual has his own will, and his conditions of life harmonize with his work. That the creative women sought and to a considerable degree attained an integration and simplification of life, despite obstacles, would appear to be one of the important findings of the study. The fact that the creative person can attain a high degree of integration while also manifesting a high level of pathology may perhaps be related to the extreme concentration on the world of symbols. There is a rapprochement between conscious and unconscious, but the separation from people or from society is not overcome, and indeed it may maintain the creative motivation.

It could be argued that the creative women mathematicians manifest the essential characteristics of the creative person. Do their life histories also show us the essential conditions which mold the creative personality? Let us keep this question in mind as we review the main findings about background factors and personal history.

Almost all of the women mathematicians grew up in homes with strong respect for learning and cultural values. Most of them, as little girls, must have been rewarded for intellectual successes. That many *Ss* grew up outside the United States, or had at least one parent who was European, suggests that they were able to avoid some anti-intellectual influences of the mainstream of American culture.

The comparison *Ss* grew up in homes they considered secure. Their fathers, described as warm in about half the cases, were usually businessmen or skilled workers. The mothers were as well educated as the fathers, and the Ss identified primarily with their mothers. In some cases, a shy, intelligent girl found that mathematics was a subject in which she could excel, and the standards of her family—sometimes the rather narrow standards of the immigrant trying to make good—encouraged her to pursue scholastic excellence in a conventional way. In other cases, the child semed concerned to defend herself against impulse, and to use mathematics for this purpose. There were, of course, other patterns which attained less statistical prominence.

The background of the creative Ss was different in a number of respects. Financial insecurity was common. The father was a professional man. He was seldom a warm person, and there was a differential in intellectual status between the father and mother. Except in large families, there was usually no brother. The interviewer judged that *S* identified more with the father than with the mother, and that in coping with her problems she used sublimation and a search for autonomy in fantasy rather than repressive techniques.

One forms the picture of a very intelligent child who was attracted by her father's intellectual status, felt alienated from her mother, adopted her father's attitudes toward work and achievement but received relatively little attention or affection from him. Isolated from both parents, she developed the strategy of making herself autonomous by nurturing, gratifying, and "growing" herself in symbolic activity. Though such a scheme describes a few creatives well, in many cases it seems to omit important special factors—that this girl had bouts of deafness, that this father was psychotic, that this motherless child resented the fact that she and her sister had to do all the housework while her brothers were a privileged elite.

Nevertheless, the personality and background characteristics reported in this study are similar to those obtained for a very different sample of creative women—college seniors, most of whom were interested in the arts and social sciences (Helson, 1966, 1968). In these studies, data were available from parents and siblings as well as from the creative and comparison women themselves. Ambivalence toward the mother, the need for autonomy, and the development of strong symbolic interests, a father who seems to have modeled the use of intellectual activity for self-expression and for purpose in life—this constellation recurs. Several parts of this pattern of findings have also been reported by Anastasi and Schaefer (1969) in a study of creative adolescent women. The importance of the father, as revealed in autobiographies of several outstanding women mathematicians of the past, has been emphasized by Plank and Plank (1954).

Most boys, of course, undergo an estrangement from the mother as a part of acquiring a masculine identity. This estrangement, termed independence, is eased by considerable social support, and the main obstacle to the development of a creative personality in men seems to be that what the mother represents will be devalued and repressed too much, so that pleasure in imaginative play or attention to feeling is rejected as feminine. Thus one finds among the men mathematicians that the mother is described with more respect and warmth by the creative men than by the comparison Ss. In a sense, it would seem that respect for the mother encourages a cathexis of symbolic activity in the boy, whereas a lack of respect may engender it in the girl. This statement contains some suggestive implications, but it is an oversimplification; a responsible comparison of the development of the creative personality in boys and girls would require a more extended discussion than the scope of the present paper allows. It shall be left as a hypothesis (which owes much to Rank, 1945) that the creative boy or girl experiences an estrangement in the primary milieu, a disadvantageous position from which he (or she) makes an adjustment away from people, seeking to have his own will and provide his own security and emotional satisfaction in intellectual activity, and retaining the hope of bringing about a reconciliation in the symbolic medium.

In the introduction, the question was raised as to how a woman could so suppress her feminine nature to be a mathematician without suppressing her originality also. Part of the answer seems to be that the women mathematicians are introverts, whose "natures" are not the modal American type. Beyond this, the creative women differed from the comparison Ss in their ability to express themselves rather fully and freely in creative activity, with emotional involvement, rather than emotional restriction, and with considerable participation from the unconscious.

The present study does not clarify why so few women in this country go into higher mathematics. The reasons may be deep seated and perhaps innate. However, the extent of foreign birth and parentage in the whole sample, and the degree to which the creative women were found to be rejecting of outside influence—these findings suggest that countervailing social pressures are strong.

Pribram (1963) seems to conceive the question of how women could be creative as that of how women could be made into men "in the best sense of the word." He fears the process would be agonizing if not impossible. However, this difficult approach does not seem necessary. Creative men and women show many similarities in basic personality. It is true that each group has characteristics of its own. Creative men mathematicians are more outgoing, self-accepting, and masterful. It seems very likely that these traits interact with strong symbolic interests, independence, etc., to bring about forceful direct assaults on difficult problems, critical breadth, a high level of productivity, etc. Even under optimal circumstances, creative women might be expected to make a contribution different in type from that of creative men. Of course, it should be noted that among creative men mathematicians, the most creative were not the most outgoing, self-accepting, and efficient. In any event, confidence and effectiveness would seem to be enhanced by success and cultural support. The striking differences between creative men and women in professional status and in productivity after graduate school seem to reflect social roles and institutional arrangements more than fundamental creative traits.

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Psychological and Social Barriers to Women in Science

Martha S. White

Talented and educated women with family responsibilities often face special problems of identity and self-esteem when they attempt to continue their professional activity. Although many do so successfully and encounter few problems, others find it more difficult. I first became aware of some special aspects of these problems when I interviewed women scholars at the Radcliffe Institute—women with outstanding intellectual and creative ability who had been awarded fellowships so that they might continue their professional interests on a part-time basis (*1*).

The Institute members were particularly questioned about their feelings of identity as a professional. Did they feel any more professional as a result of their fellowship at the Institute? What made a person feel professional? Had their commitment to their work changed as a result of their Institute experience? At the time, I hypothesized that one outcome of the fellowship, of the opportunity to work deeply and seriously on a project, would be a greater sense of competence. The greater the sense of inner competence, my reasoning went, the greater would be the sense of commitment to future productive work. This proved to be only partially true.

Many indicated that the fellowship program had resulted in a genuine change in their conceptions of themselves as professionals, but their responses suggested that this change was rarely due solely to the opportunity to work on their projects. Although this was important, equally significant was the access to stimulating colleagues, both within and outside the Institute, which the special status conferred by the fellowship made possible. Appraisals of their work by others, coupled with acceptance and recognition by people whose professional opinions were relevant and appropriate, made a significant difference in determining whether a woman felt like a professional, and whether she in turn had a strong sense of commitment to future work.

Challenging interaction with other professionals is frequently as necessary to creative work as is the opportunity for solitude and thought (2). Yet comments from many women indicate that it is particularly difficult for them to attain, especially for the woman who seeks reentry. As one woman astutely noted: "Those of us who have interrupted our careers because of children or moving with our husbands across the country have special difficulties. Our departments maintain no ties with us. Often no one knows us, and the articles and books on which we are working may not be published for another three to five years. Meanwhile, if we are to be productive, we need to be professionally involved again."

Although women offer unique qualities to intellectual and creative endeavors, one of the main barriers to women's achievement of excellence and commitment is the expectation that women's career patterns and motivation will be the same as men's. When they are not, there are many phrases ("lost to marriage," "didn't pan out," "dropped out") which indicate the disappointing nature of their acts, the hopelessness of their making choices which are uniquely theirs as women. Many, possessing energy and talent, will choose the same career paths and find great personal satisfaction in meeting the same demands as many men. But others who live life differently and who may choose differently from the traditional career pattern, also have much to offer, and our gain is greater if we can include their talents among those

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which society and science utilize. Attracted to scholarship and scientific research, they continue on to graduate school or professional school after college because of their deep interest in a field. Many have clear and well-defined plans for a career, while others wish to combine "worthwhile work" with homemaking. Because of their serious intellectual interest and involvement, such women usually do well academically and are excellent students. Yet they find that their interest in learning and in excellence does not receive the same recognition after college or graduate school as it did before unless they determinedly indicate that they plan a full-time uninterrupted career. But clearly the dominant (and in many cases preferred) life pattern for many a highly trained woman still includes multiple roles, dual commitments, and occasional interruptions. If she wishes to continue her professional activity on a flexible or modified schedule, or faces temporal or geographic discontinuities, she is frequently excluded from important aspects of what the sociologists call "socialization into a profession."

PROFESSIONAL SOCIALIZATION

In the normal course of men's and women's professional careers, only a part of their professional training takes place in college, in graduate or professional schools, or in a training program. Many professions and occupations have periods analogous to that of the medical intern or resident (though these stages are frequently informal and rarely explicitly recognized) during which the individual learns to behave in ways which other people in the field regard as "professional." Such "socialization" usually occurs during graduate school as well as during the first decade of employment after one is launched on a career, and consists of learning the roles, the informal values and attitudes, and the expectations which are an important part of real professional life. During this stage of a career, the person not only learns occupational roles and skills, but gains a firmer image of himself as competent and adequate. Appraisals of his work by others permit self-criticism to grow and standards of judgment to develop. Such a sense of competence may come quickly and early for some, but develop slowly and gradually for others. Once a person has this sense of competence and regards himself as a professional, it is probable that he has less need to learn from colleagues and indeed may have greater freedom to diverge from the accepted way of doing things, seeking his own pathway instead. It is this firm sense of professional identity and capability which women, regardless of their ability, may find difficult to achieve, or achieve only at a high personal price.

Many people are unaware of this period of role learning in scholarly, scientific, or academic professions, and fail to realize how important such a stage is, and how lengthy it has become because of the increased complexity of professional life. Everett Hughes, the sociologist, has noted that many still think of professions as they were in the 19th century, although they have vastly changed since then (*3*). Many professions are practiced in complicated organizations, with consequent nuances of status and levels of organization to contend with. There are elaborate social systems in all parts of academic and business life, and purely technical training is rarely enough. The aspiring young scientist must be knowledgeable about many aspects of institutions, journals, professional meetings, methods of obtaining source materials, and funding grant applications. Knowing how to command these technical and institutional facilities requires numerous skills, many unanticipated by the young student. But once gained, such skills often seem very simple in retrospect and even thoughtful professionals forget that they were once not second nature. This is the kind of learning we speak of as "caught," not "taught," and it is a valued by-product of acceptance and challenging association with other professionals.

SPONSORSHIP

Studies of professions and professional identity have also stressed the importance of sponsorship as a device for influencing commitment and affecting the self-image. Referred to by some writers (*4*) as the "protégé system," sponsorship is common to the upper echelons of almost all professions, including the scientific fields. One must be "in" both to learn crucial trade secrets and to advance within the field. Unfortunately a man may be hesitant about encouraging a woman as a protégé. He may be delighted to have her as an assistant, but he may not see her as his successor, or as one who will carry on his ideas, or as a colleague. He may believe that she is less likely to be a good gamble, a risk for him to exert himself for, or that she is financially less dependent upon a job. Because of subtle pressures from his wife, he may temper his publicly expressed enthusiasm or interest. He may fail to introduce her to colleagues or sponsor her for jobs. And as one of Anne Roe's studies of eminent scientists indicated (5), the advancement and success of protégés are important to his own feelings of satisfaction in his professional efforts; nonachieving protégés reflect on the sponsor's public and private image.

In addition, sponsorship affects the recognition an individual receives. One might assume (or hope) that excellence and productivity in scientific work is all that is needed for recognition, but in reality ideas are more likely to be accepted if they are promoted or mentioned by eminent sponsors, or if they are the product of joint authorship with a well-known professional, or derive from a well-known laboratory or university. Whether a woman is "sponsored" in these ways will partially determine who reads her work, listens to her reports, or even offers friendly comments on the draft of a paper. Such informal signs of recognition increase motivation, and affect one's subjective feelings of commitment to a field, as well as feelings of professional identity.

ARE WOMEN IN THE CLUB?

A recent study of women Ph.D.'s (6) showed that the full-time employed woman Ph.D. published as much as her male colleagues, and was more likely than the average male scientist to be in research. She was involved in the activities of her professional organization, was sought out as a consultant, and was more likely to be awarded fellowships and be accepted in honorary societies. Despite all this evidence of productivity and commitment, the authors of the study noted that the women often felt left out, and suggested that "the problem which bothers the woman Ph.D., who is a full-time contributor to her profession, is that she is denied many of the informal signs of belonging and recognition. These women report that even on such daily activities as finding someone to have lunch or take a coffee break with, or finding someone with whom she can chew over an idea, or on larger issues such as finding a partner with whom she can share a research interest, the woman Ph.D. has a special and lower status." This exclusion from the informal channels of communication is of particular importance in fast-moving science, where as Sir Alfred Egerton has noted, "of the total information extant, only part is in the literature. Much of it is stored in the many brains of scientists and technologists. We are as dependent on biological storage as on mechanical or library storage." Jessie Bernard astutely comments: "It is this access—the brains of fellow scientists, that may be more limited for women than for men" (7).

The need for stimulating colleagues was also attested to in a study by Perrucci of women engineers and scientists (8). She found that women were more apt than men to endorse as important the opportunity "to work with colleagues who are interested in the latest developments in their field," and "to associate with other engineers and scientists of recognized ability." Interestingly enough, no differences appeared between men and women of comparable education as to whether they desired challenging work or work involving "people versus things."

The evidence also seems to indicate, however, that in many cases women are reluctant to put themselves forward or to protest their being left out. It is a vicious circle: men indifferent or unaware of excluding women; women insecure and hesitant of intruding. The remedy is not necessarily more individual boldness, but must include new institutional arrangements and programs which do not depend on individual initiative (9). However, as the Radcliffe data indicated, such arrangements and programs are not too difficult to achieve.

There have been lone individuals who have flourished on society's neglect and produced great ideas or masterpieces, but this is not characteristic of those in the professions or the majority of people. For most people, acceptance by others and interaction with challenging groups or organizations are a source of deep personal significance and of creative energy as well. Yet it is this acceptance and this interaction which is often denied, both purposefully and inadvertently, to women, whether they participate full time or on a flexible schedule, whether they remain continuously in the field or seek reentry.

A NEW CAREER CONCEPT

Because of their life patterns, many women with scientific training have nonprofessional roles and identifications to which they are deeply committed. They seek an occupational or professional identify which recognizes and takes into account this dual commitment. For women with these values, a new concept of professional "career" may be necessary.

Numerous women, either because of their own inclinations or their personal situations, enjoy and competently manage full-time work and a full-time career. Others, however, seem to be seeking to invent for themselves a new and more varied conception of career, one which has not existed before and for which there are few models or patterns available. They have a full-time commitment, but do not always plan to work on a full-time basis; their lives and where they work are governed to a greater degree by nonoccupational factors. As a result of the smaller size of families, and the shorter span of child-rearing, few of these women see their maternal role as bringing their professional life to an end. They think of themselves as a permanent part of the working force, and regard flexible schedules and part-time work as a necessary part of the solution. Some seem to be seeking an alternative career model which is neither upward moving nor "success-oriented," but which recognizes their commitment to family responsibilities as an important part of their choice. To accommodate this lateral career, or "career of limited ambition," (*10*) they seek to improvise a new professional role which is more differentiated and diversified than the accepted pattern. (I should note parenthetically that this interest in new career patterns is by no means limited to women.) Such an alternative model might be represented schematically by an ascending spiral movement, indicating career choices which are upward in direction, but slowly paced with long horizontal stopovers. Deeper knowledge or more varied experience would be the goals of such a career: not greater status, but greater esteem; not primarily extrinsic rewards, but intrinsic satisfactions (*11*).

Such new models are long overdue. Almost a century of experimentation has been spent in attempting to fit women's career patterns into those followed by most men, and the result has not been phenomenally successful. If such alternative career patterns can gain general recognition, the result may be more productive, creative work. As Epstein (*4*) has so succinctly noted, the barriers to women's advancement and achievement are not merely a function of prejudice or incapacity. The structures of professions, narrow and inflexible as they often are, may create limits which are largely unintended. But groups and colleagues are powerful forces in shaping attitudes and behavior; the institutional settings and social mechanisms which inhibit commitment and identity can also be used to promote change and to encourage different consequences.

SUGGESTIONS

What can women do to cope with these barriers and discriminatory practices which intensify the effects of discontinuity in their lives? How can they fully utilize their talents, yet make choices that are suitable to their goals and life-styles? What constructive action can be taken to remedy the inadequate socialization which the current structure of the professions make inevitable for many women?

In overcoming the barriers, the importance of sponsorship and the maintenance of communication with stimulating colleagues should not be underestimated. When a woman has to interrupt her training, graduate study, or employment, she should talk over future alternatives or avenues of return with an adviser, and ask for letters of recommendation which may be used when a return is contemplated. She can seek ways to keep in touch with her department or work group. As one successful woman observed, "She should leave no gap unfilled."

Women with similar fields of interest can often profit by forming or joining other women in associations which provide professional stimulation and motivation, as well as information and access to new opportunities. Such groups can be particularly effective in assisting women who have temporarily retired to return to or keep up with their field. Several studies (*12*) suggest the possibility that women who are more ambitious in the traditional male-career sense may be more stimulated to achieve by the presence of men who are achieving, while women who regard intellectual achievement as part of the feminine role may react more favorably to the presence of capable women colleagues. This at least suggests that many talented younger women might be more encouraged by knowing and observing in the professional role other women who value the feminine family role. Such models are still too often a rarity.

Part-time work has only begun to be utilized effectively. Many men have long known that they are most productive when they engage in a variety of functions, carrying out activities which complement but may have very little immediate relation to each other. Although the initial stages of learning how to accomplish this are not easy, many women are discovering that such juggling can work even in complex and demanding scientific and engineering fields. Pilot projects using women scientists in the federal government have found both a shorter week and shorter day successful (*9*). Enthusiastic women report that they get almost as much done, while employers note that they get more than their money's worth, since there is little wasted time, and important thinking time often comes free. In fields with hard-to-find skills such as data processing, part-time job opportunities may make it easier to recruit employees. An innovation which has been used with great success in education, social work, library work, and medical residencies is to have two women share one job. Thoughtfully and carefully planned, the partnership job has proved not only eminently suitable to the needs of women, but of benefit to employers as well (*13*). Partnership teaching has proved so useful in education that one wonders why it was not thought of before.

Sometimes women create their own part-time opportunities. In the San Francisco area, a group of women biologists found they lacked opportunities for part-time work and for keeping up with their fields while their children were small. They organized a talent pool, incorporating as an educational group, Natural Science Education Resources. They have since offered a unique series of classes on plants and animals to mothers and their preschool children, served as consultants to teachers and schools, developed new ecology programs, presented adult education courses, and obtained a pilot National Science Foundation grant. Several women have now moved on to other jobs, leaving a vacant place eagerly taken by someone else seeking such a part-time opportunity.

Although all of these part-time approaches serve to prevent technical obsolescence, retraining programs and reentry techniques are also needed. Although some writers counsel noninterruption as the only answer, it seems more realistic to assume that discontinuity will continue to be a fact of life for many women. A woman's interests change between the time she is in college and the point at which she decides to involve herself more deeply again. Fortunately mid-career retraining is becoming mandatory for many scientific fields; if reentry opportunities for women can only be included by companies, universities, and professional societies along with the continuing education programs for full-time professionals, these transitions can be more easily accomplished.

Some women have planned their own transitional reentry programs. One woman chemist talked to a local college professor, and offered to assist him in his laboratory courses during the year in order to bring her knowledge up to date. She proved so capable that he admitted her to advanced seminars, supervised her in tutorial reading, and is now working to retain her in a permanent capacity in the department.

Master's degree programs aimed at updating skills are particularly promising. Rutgers University has had one for mathematicians, and Wellesley College has had a 2-year program for chemists.

Some may raise the question: Aren't women now insisting on the same opportunities as men? Do women want the same opportunities or do they want special opportunities? The answer is simply that they need both. Career commitment takes a variety of forms for women and may increasingly do so for men. Longevity, population pressures, and the explosion of knowledge have created new needs and life stages for us all. If we become obsessed with simply giving women the same opportunities as men (important though this may be), we not only obstruct effective recognition of the differences in women's lives, but may fail to note what is already a trend—more complex educational and occupational patterns for both men and women. Many of the programs and innovations developed to suit women's needs are needed for men as well. They too are feeling the impact of new knowledge, the expectation of intellectual retooling every decade, and the need for part-time refresher courses to up-date proficiency. They too have discovered that interests change after 20 years in a field, that challenge can outweigh security, and that mid-life may bring a desire to shift the focus of a career. And surely we are all learning the lesson that education is most useful when one is most ready for it. Many young students are no longer so eager to cram all their education and professional training into the beginning of their adult years.

While the patterns of women's lives may be more varied, the interruptions more pronounced and profound, and possibly the needs for guidance greater, our attempts to foster a social climate which meets the complex needs of women today may well be pointing the way toward meeting the diverse needs of both the men and women of tomorrow. . . .

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Minority Status and the Pursuit of Professional Careers: Women in Science and Engineering

Carolyn Cummings Perrucci

Minority status, whether based on sex, race, ethnicity, or religion, is an important influence upon occupational placement, even in high-status professional occupations (Hall, 1946, 1948; Carlin, 1962, 1966; Ladinsky, 1963; Smigel, 1964; Bock, 1967). With respect to sex, it is found that many professions are sex-typed (Epstein, 1967), resulting by and large in the continuation of either male or female predominance among the practitioners therein (Gross, 1968). Consistent with the mi-

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nority status of women, female-dominated professions tend to be of lower status than those dominated by men (Epstein, 1967).

Investigation of the relative integration of minorities into the world of work reveals variations by minority group and by specific profession and work place (Wilensky and Ladinsky, 1967). In general such integration appears to be occurring most slowly, if at all, for those whose minority status is most visible and at the same time irrevocable, including race and sex distinctions (Price, 1964; Parrish and Block, 1968). When it occurs, sexual integration of traditionally female-dominated professions appears to involve a form of intraprofessional stratification whereby men assume the supervisory or otherwise more prestigious forms of the occupation (Gross, 1968) or, in effect, take control of the occupation (Wilensky, 1968).

Similarly, there is evidence that women who work in traditional male professions are overrepresented in the lower-prestige specialties within the profession and are more likely than men to serve only minority-status clients (Hughes, 1958; Epstein, 1967). They are also more likely than not to have work functions which reflect an extension of their sex role in the society in question. It is suggested (Epstein, 1967) that possession of a "deviant" sex status by women professionals may pose distinctive problems coming from the confusion of sex roles with occupational roles or from attitudes about the priorities of sex roles over occupational roles. With respect to the latter concern, however, studies which focus on the dynamic aspects of timing of marital and fertility behavior (seen here as the most relevant aspects of women's sex roles) in relation to stages in women's professional training and career are rare indeed. Also lacking are attempts to assess career-family patterns of cohorts or generations of women professionals with the noteworthy exception of a longitudinal study of a sample of college graduates—class of 1961 (Rossi, 1965)—for which preliminary analysis of data indicates a relationship between career goals at graduation and marital and work behavior three years later.

Variation in minority group integration into specific occupations is considered in Blalock's (1962;1967) developing theory of minority-group relations. It is postulated that, given constancy of job prestige level and general labor market conditions, integration of visible minorities into professions is most likely for work having several specified characteristics, including that in which it is difficult to prevent the minority from acquiring the necessary skills; individual performance is positively related to the productivity of the work group (which is preferably a team of various specialists), all of whom share in the productivity rewards; individual performance level is relatively independent of skill in interpersonal relations; high individual performance does not lead to power over other members of the work group; individual performance is readily evaluated; total productivity is not markedly limited by consumer demand; and there is extensive competition among employers for outstanding personnel. Although the above characteristics are acknowledged to be typical of few areas of work in the American occupational structure, save professional baseball and some other entertainment fields, it is suggested that science-based professions fit the model fairly closely. This is largely an untested assumption for, to date, little detailed information concerning sex (or race) differences in scientific work experiences has been published. A beginning has been made by Rossi (1965) who shows sex differences in income, type of employer and work hours for scientists and engineers employed at the time of the 1960 *Census of Population*.

The present study attempts to broaden understanding of the integration of minorities into the world of work by focusing on sexual integration of professions, specifically, the place of women in science and engineering. This objective is approached in two ways: (1) by presenting for the first time, detailed description of some salient features of women scientists' labor force participation in comparison to work experiences of men in the same fields; and (2) by analyzing the education–work–family temporal patterns for two age groups of women science and engineering graduates. Comparative analysis of employment experiences of men and women graduates permits documentation of the existence or absence, as well as the nature of sex differences in utilization. An examination of work profiles for initial and current employment determines trends in sex-based patterns over time. Finally, analysis of work–family temporal patterns will indicate whether the successful pursuit of a career by minority-status holders requires adaptations aimed at offsetting the "deviant" status. In the case of sex as the minority status, it is expected that the negatively valued aspect of female status consists primarily of the possible intrusion of marital and maternal roles into the occupational role, to the presumed detriment of the latter (Williams, 1950; Davis and Olesen, 1963). The general hypothesis here is that "careerist" women will adjust their marital and maternal roles in order to minimize the difficulties of their minority sex status.

METHOD AND PROCEDURE

Data analyzed in this paper were collected as part of a larger study of postwar engineering and science graduates of a large midwestern university (Perrucci and LeBold, 1967). Seven populations were selected for study: (1) recipients of the Bachelor of Science degree in engineering of the classes of 1947 through 1964; (2) recipients of the Master of Science degree in engineering, 1950 through 1964; (3) recipients of the Doctor of Philosophy degree in engineering, 1950 through 1964; (4) all other living women engineering graduates; (5) recipients of the Bachelor of Science degree in science of the classes of 1947 through 1964; (6) recipients of the Master of Science degree in science, 1950 through 1964; and (7) recipients of the Doctor of Philosophy degree in science, 1950 through 1964.

For the bachelor's degree engineering and science samples, equal numbers were selected by major field by year of graduation. The fields within engineering which were included were aeronautical, agricultural, air transportation, industrial, chemical, civil, electrical, engineering mechanics, engineering science, mechanical, and metallurgical. Four science fields were included: biological science, chemistry, physics, and mathematical sciences. For the four advanced degree samples, equal numbers were selected by year of graduation only. A questionnaire containing items about pre-college backgrounds, college and postcollege experiences was mailed to all who were sampled. The percent returns ranged from a low of 64.2 percent for the doctorate scientists to a high of 78.5 percent for the doctorate engineers, with an overall percent return of 69.5, representing 3,589 respondents, 3,289 of whom are male and 300 of whom are female graduates.

With respect to the objective of examining selected work experiences by sex of graduates, comparability of educational level among respondents is necessary. Owing to the small number of women having graduate degrees, career aspects are documented for bachelor's degree recipients only; 75 percent of the women ($N = 225$) and 45 percent of the men ($N = 1,480$) are classified thusly. Initial employment characteristics are contrasted for *noncareerist* and *careerist* women in order to ascertain possible clues which anticipate the eventual dropping out of the *noncareerist* women. Regarding current employment characteristics of women graduates, data obtained from the *young* and *old careerists* only are utilized for they alone have sufficient experience in the labor market to be contrasted with men graduates' current employment as well as women *careerists'* initial employment (from the point of view of determining long-range career patterns). Ninety-eight of the 225 women bachelor's degree recipients (43 percent) are *careerists.*

For analysis of education–work–family temporal patterns of women engineers and scientists, women graduates are classified into four groups on the basis of length of time, in years, since highest degree and proportion of that time actually spent in full-time occupational employment. With median years since highest degree for all women as the cutting point, those with ten or fewer years from graduation to time of data collection (1965) are labeled "young" while women with eleven or more years since graduation are labeled "old." The *young* group of women is actually younger in age as well; median age is twenty-nine, with three-fourths being twenty-seven or older. Median age is forty for the *old* women, with three-fourths being thirty-six years of age or older. Each of these two groups is dichotomized into those who have worked less than a majority of the years since graduation (*noncareerists*) versus those who have worked a majority (or all) of the years since college graduation (*careerists*). Of the total group of female engineering and science graduates, 18.3 percent (55) are *young noncareerists;* 32.7 percent (98) are *young careerists;* 33 percent (99) are *old noncareerists;* and 16 percent (48) are *old careerists.*

RESULTS

Male and Female Work Profiles

Almost all of the graduates who do not immediately pursue advanced degree studies embark on their initial job directly after college graduation. Selected aspects of the initial employment of graduates, including place of employment, level of technical and supervisory responsibility, principal function and average monthly salary are considered and compared with identical aspects of their current (1965) employment for *noncareer* and *career* women and men.

Beginning with type of work place, there are pronounced differences by sex of employee. . . . Although industry and business . . . employ about three-quarters of all engineering and science graduates, they account for a larger proportion of men than women as sites for initial and current employment. Men, for example, are twice as likely as women to work in durable manufactured product industry. Regarding nonindustrial employers, educational institutions employ a larger percent of women than men at both times. Moreover, a comparison of the distribution of *career* women graduates across the various work places for initial and current jobs indicates a larger percent of *careerists* now working in educational institutions compared with the percent of all women graduates working initially therein (*28* percent versus *19* percent), although this shift is not statistically significant.

Principal function at work varies significantly according to career pattern of women as well as sex of graduates. . . . *Noncareerists* are more likely than *careerists* to work initially in a preprofessional capacity; in development; testing, construction, and sales; and are less likely to teach. In comparison with men, *career* women are more likely to perform initially in a research or teaching capacity and are less likely to be in testing, construction sales and service. Comparison of current and initial functions of employees indicates that *career* women are less likely to be involved now than previously in work of a preprofessional nature and are more likely to be in teaching and management. Men, also, are less likely to work currently than previously in a preprofessional capacity, and more likely to function in an administrative capacity, but the shifts in these functions over time are more pronounced for men than for women employees.

Involvement in supervisory positions is an especially important aspect of women's careers in the high-prestige professions since such positions are likely to give women authority over male colleagues. . . . Only *30* percent of all graduates say that they have any supervisory responsibility in their initial job after college. The minority who supervise, moreover, do so for nonprofessional personnel, usually nontechnical employees. Over the years, however, there appears to be a significant difference in the career line of *career* women and men. In their current work positions, *72* percent of men compared with only *49* percent of women assume some supervisory duties. This sex differential in supervisory responsibility concerns primarily the supervision of relatively higher levels of personnel in the work place, that is, professionals and management.

Engineers and scientists also engage in work which varies considerably in terms of the level of technical expertise required. . . . The distributions of graduates according to technical responsibility in initial jobs are similar; that is, tasks tend to be relatively simple and standardized for at least *80* percent of both sexes regardless of career pattern. Career advancement of women tends to fall short of that experienced by men, however, when initial and current positions are compared; specifically, *57* percent of the women and *70* of the men perform complex and pioneering tasks in their current work.

A final characteristic of graduates' work to be considered is remuneration. Despite comparable levels of technical and supervisory responsibility in initial jobs after college,[1] women engineers and scientists earn lower salaries

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[1] Rossi (1965:67) presents previously unpublished data from a postcensal survey of professional and technical occupations which are being analyzed by Seymour Warkov, National Opinion Research Center, University of Chicago. At each level of education, median salary of women is considerably lower than that for men employed in engineering, physics, biology, and mathematics-statistics. Differences between men and women besides educational level which are suggested to explain income differentials are type of employer, work activity, and length of work week, although salary data are not presented by sex while controlling for these additional variables.

than men (*$341.* versus *$432.* respectively). It is probable that two factors mentioned earlier account at least in part for this initial income differential by sex of employee; that is, variation in place of employment and principal function. It is known, for example, that salaries in educational institutions tend to be lower than those in industry; and women are more likely than men to work in the former. A third factor which may be contributing to lower salaries for women is the smaller proportion of women (*32* percent) than men (*75* percent) who initially work in an engineering rather than a science field. It is shown elsewhere (Perrucci, 1967a; Perrucci and LeBold, 1967) that at each college-degree level, the median salaries of engineers exceed those of scientists in the study sample.

In order to make better male-female comparisons, included are only those graduates who are comparable in terms of work place and principal function and field. . . . First is the exclusion from analysis of the data from graduates whose initial place of employment is *other than* industry and government and whose initial function involves teaching and any unspecified activity. With these groups excluded, a discrepancy in salary between the sexes remains but it is diminished in magnitude (*$407.* for women versus *$436.* for men) and pertains only to *noncareer* women versus men. A second effort to improve sex comparisons is made by controlling for field of initial employment. The data then show that it is among engineers only that women earn a lower salary for initial job than men. Although the number of cases becomes quite small, a comparison of initial salary according to career pattern within field shows that among scientists, *noncareerists* only earn less than men. Among engineers, however, both *noncareerists* and *careerists* earn less, on the average, than men, with *careerists* earning the least of all three engineering groups.

The salary differential by sex is markedly larger for current job than for initial job after college graduation. . . . It is no doubt related to different trends in employment on the part of women and men, particularly the increased proportion of *career* women who work in educational institutions (most of whom teach), contrasted with the increased proportion of men who move into management. In addition, field of current employment is more likely to be engineering for men than for women graduates (*70* percent versus *30* percent respectively). When the base of cells is confined to provide comparability in terms of work place, function and field, it is found that current, as well as initial, salary differential by sex of employee is greater among engineers than among scientists. On the average, men's annual salaries exceed those of *career* women by *$1,400.* in engineering and by *$600.* in science.[2]

Differential utilization of women and men in science-based fields and the correlated income differential by sex may negatively influence retention of women employees; data concerning work values of our sample of graduates, discussed in detail elsewhere (Perrucci, 1968) support this interpretation. Briefly, men and women are compared for a total of thirty-four work values dealing with the nature of work, career advancement, colleague relationships and professional activities. Sex differences are found for only *6* of the *34* values. It is concluded that, in general, values are relatively similar for working women and men of comparable education, yet women are less likely to hold positions which enable them to realize their goals.

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[2] There are two variables mentioned in the literature as possible causes of such large current salary differences between men and women which are not examined above but for which some limited data are available. One factor is the alleged immobility of female professionals, especially married ones. The latter are often viewed as more exploitable since their ability to seek better positions is presumably limited by husbands' career needs (cf. Hull, 1963). A comparison of number of employers during the career for both men and women in this study provides no support for the immobility—exploitation hypothesis. Since receiving the bachelor's degree, *80* percent of the women and *86* percent of the men report having three or fewer employers. This may suggest that career women are no less mobile than men in science and engineering. It is not known, however, what motivated members of either sex to change employers.

The second factor to be considered is length of work week, especially overtime work, which could result directly in greater earnings. Census data for 1960 show that men work overtime to a much greater extent than women (cf. Rossi, 1965: 69 ff). Women engineering and science graduates in this study, however, are no less likely than their colleagues to "spend time at home on work-related matters." Work at home is generally not extensive for these graduates, for *90* percent of the women and *83* percent of the men report that they work six or fewer hours per week at home. Moreover, marital status of men and women makes no significant difference in their work hours.

Temporal Patterns in Marriage and Childbearing

The second objective of this paper is to examine family and career sequences among women scientists and engineers in order to determine possible adaptations to mitigate the effects of "deviant" sex status in the professional context.* Marital status is generally assumed to be a key variable in the work lives of women but not especially so for men (for focus on marital-career patterns among male engineers see Perrucci, 1967b; Salvo, 1969). Indeed, work and marriage are considered to have once been mutually exclusive alternatives for women professionals (cf. Klein, 1965). An examination of census data for 1960 indicates that marriage is still more problematic for employed women than for men; four out of five men are married and living with their wives, but an average of only two out of five women in engineering and science are married and living with their husbands.

Of our sample of women scientists and engineers, *82* percent are currently married or have been married at some time in their lives. Marital status varies with career pattern of women in that *careerists*, in general, are less likely to ever marry than are *noncareerists*. This pattern persists, moreover, when level of education is controlled (B.S. degree versus advanced degree recipients, data not shown). More specifically, among the *old* group of women, one-quarter of the *careerists* are single; whereas, none of the *noncareerists* is single. Among the *young* women, *32* percent of the *careerists* compared with *18* percent of the *noncareerists* are unmarried. The greater proportion of single women among the *young* than *old* groups, irrespective of career pattern, partially reflects differences in age; it will be remembered that median age is twenty-nine for the former and forty years for the latter group.

For the married women only, when timing of marriage in relation to college graduation and initial employment is examined, it can be seen that three-fifths of all women engineers and scientists marry after or very near the close of their college work rather than prior to completion of this basic professional training. The college graduation—marriage time interval varies directly by career duration for women; for instance, *36* percent of the *old careerists* compared with *9* percent of the *old noncareerists* marry as late as four or more years after leaving college. There also appears to be a secular trend of decreasing age at marriage, especially when *young* and *old noncareerists* are compared; that is, *56* percent of the *young noncareerists* marry prior to or during year of graduation, whereas, only *36* percent of the *old noncareerists* marry this soon. Despite such a trend, among the most recent graduates (i.e., *young* groups) *careerists* are more likely than *noncareerists* to complete their basic college studies before marrying.

In examining timing of marriage in relation to initial employment, one sees that *55* percent of all the married women embark upon a career prior to marriage. Marked variation in length of time interval between first job and marriage (if not the basic work–marriage sequence itself) according to career pattern occurs for the *old* women especially. Thirty-five percent of the *old careerists* work four or more years prior to marriage, compared with *9* percent of the *old noncareerists* who do so. Again, there is a time trend evident in that the *young* groups women engineers and scientists (*careerists* and *noncareerists*) are less likely than *old* groups to become employed prior to marriage.

A common aspect of marriage which may inhibit women's work in science-based fields is childbearing and rearing. *Career* women in our sample are more likely than *noncareerists* to be childless. Among the *old* female engineers and scientists, for example, *17* percent of the *careerists* are childless, whereas none of the *noncareerists* are without children. In addition, another *14* percent of the *old careerists* have only one child while *2* percent of the *old noncareerists* have so small a family. A majority of the *young careerists* (*51* percent) have no children; but only *7* percent of the *young noncareerists* are childless at this stage in their lives.

For respondents who are mothers, data regarding time interval between marriage and initial childbirth show that over half of these women become parents prior to the completion

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*EDITORS' NOTE: Although the measure of careerism used in this paper is behavioral, it is assumed to reflect a careerist orientation and is used as an independent variable in the following data analysis. It is believed that some justification of this procedure lies in the fact that work experience precedes in time sequence both marriage and childbearing for a majority of the engineers and scientists in this study.

of three years of married life. The data suggest that a somewhat greater length of time elapses between onset of marriage and initiation of a family among career women; that is, within each age grouping of women, a larger percent of *careerists* than *noncareerists* bear their first child four or more years subsequent to marriage. The *young* appear less likely than the *old* scientists and engineers to remain childless for so long a time.

One factor which may influence the extent to which childbearing inhibits women professionals' work outside the home is the stage in their career at which this event occurs. For married women who are mothers, . . . families are seldom begun prior to initial employment of graduates. The data suggest patterns in child spacing after college for *career* versus *noncareer* women; the former tend to begin their families at a later point in the career. For example, almost twice the percent of *old careerists* as *old noncareerists* work six or more years prior to bearing their first child (*44* percent and 22 percent respectively). Also . . . *young careerists* tend to have less work experience before starting their families than do *old careerists*.

SUMMARY AND IMPLICATIONS

. . . Deviation of science, and especially engineering, from the ideal-typical model of rational professions (with regard to integration of minorities) is indicated in the finding of significant "selective patterning" of careers by sex of engineer and scientist, which is evident to some extent when graduates first enter the labor market and which becomes more pronounced during the course of their careers. . . . Differential sex concentration by current field reflects sex differences in college major field which are more pronounced in engineering than in science (Rossi, 1965; Epstein, 1967). Sex segregation of work function pertains primarily to *noncareerist* women versus men with respect to preprofessional-level work. That *careerists* are more likely than men to be teaching (in educational institutions) is consistent with the hypothesis (Epstein, 1967) that work functions often reflect an extension of the sex role for women. Most importantly, perhaps, as an indication of future trends, is the relatively large proportion of women compared to men in research. Women's opportunities may increase with the possibility of increased government research contracts (Mello, 1965) and, presumably, nondiscriminatory hiring and promotion practices in government and industry (Price, 1964). It should be recognized, of course, that the latter puts all minorities, not only women, into competition for the new positions.

A comparison of current job of *career* women with that of men indicates some advancement over the years for all, but an even greater disparity in career paths between the sexes for current than initial job. This disparity is consistent with the hypothesis of intraprofessional stratification (Bock, 1967; Epstein, 1967) in that men are more likely than women to have higher levels of responsibility and to earn a higher salary.[3] Even when amount of work experience, place of employment and principal job function are comparable, *career* women currently earn less than men; this salary differential by sex is twice as great among engineers as among scientists.

The significance of increasing disparity in career paths and career success between the sexes for women's participation in engineering and science cannot be ascertained easily with the data at hand. It is sometimes posited in the literature, for instance, that even for women in the high-prestige masculine fields, other factors such as lack of financial "need" characterize women compared to men and serve to offset possible negative influences of differential success. That is to say that single women presumably have no need for as high an income as men who, by and large, have family responsibilities, so that motivation for comfortable living is more readily satisfied at a lower salary for unmarried women scientists than their married men colleagues. It is further suggested that a high proportion of the married women who work in science-based fields have college-educated, employed husbands, so they do not need the income and responsibility that goes with it that men scientists do. For our sample of graduates, however, work values are relatively similar for working women and men of comparable

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[3] In a study of recent male and female doctorate recipients, including natural scientists, Simon *et al.* (1967) also find salary discrepancies by sex, especially at the associate professor level. There is a steady decrease in size of salary discrepancy, however, from 1959 through 1963.

education, yet women are less likely to hold positions which enable them to realize their goals. It is suggested, therefore, that discrepancies between what women employees themselves value and their perceptions of their actual work situation may indicate potential problems in their employment.

The second major finding of this paper concerns possible adaptations to mitigate the effects of "deviant" sex status in the professional context; specifically, for two age groups of women pursuit of a fulltime career in science and engineering is related to a temporal ordering of the events of college graduation, employment, marriage, and childbearing. . . . *Young careerists* in engineering and science, who are less involved in marriage and childbearing, may be tending toward complete avoidance of marital and especially maternal roles rather than making a temporal adaptation to career demands (i.e., postponing marriage and childbearing). It is known that *young careerists* are no less likely than *old careerists* to pursue advanced degree work (Perrucci, 1968: Table 7); some indication of commitment to their field. Over half of the *young* married *careerists* wait until after the completion of basic college training (B.S. degree) to marry.[4] Also important is the fact that most *careerists* work prior to marriage, which is correlated with work after marriage (Weil, 1961) and work several years prior to starting families. Given these indicators of the greater importance of the career role over marriage and the maternal roles during early adulthood, it is quite possible, but not yet indisputable, that the marital and maternal status of *career* women are consequences of values they hold and the most acceptable balance they can strike between work and family under current circumstances.

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[4] Given a continuation of the recent secular trend of rising age at first marriage for women (Parke and Glick, 1967), apart from career planning, a follow-up of the *young* groups of women might reveal that a high percent do marry and bear children, only at a somewhat later age.

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PART FIVE / ROLE CONFLICTS AND ALTERNATIVE ROLES

A. Conflicts in Role Activity

Introduction

The now classic Kubie articles (1953; 1954) on irrational factors at work in the scientist has apparently served as a stimulus for work on personal, often emotionally laden behaviors, which were at one time considered antithetical to the rational processes of science. Once seen in appropriate context, however, this area has led to some lively and revealing writing, much of which has served to strip away the idealized stereotypes of science and to show that scientists share universal behaviors, motivations, and attitudes. Kubie's contribution shows the many points at which scientific activity is invaded and affected by unconscious irrational pressures and conflicts, such as choice of research problem, communication with colleagues, and productivity.

The articles in this section move from this focus on unresolved neurotic residues which effect career to the kinds of emotional problems generated during course of work. Some of the motivations for scientific work have been suggested in the studies of self-images which scientists share as a group, and the perceptions of science with which adolescents identify as they opt for a scientific career (see Section III C). Once in science, however, the operation of these same motivations can lead to other behaviors which are not so consonant with the idealized perception of how a scientist should behave. Reif discusses how the quest for prestige can cause conflicts between the goals of science and the goals of the scientist. He traces the genesis of the feelings of failure, threat, and competition which, in turn, can actually distort normally accepted scientific practices. Hagstrom, in further analysis, shows that there are differences in the extent and intensity of competitive behaviors that develop in various disciplines within natural science. He relates these differences to a number of factors in the rules and regulations which accompany scientific practice, the social controls of science. These formal and informal regulations are not completely unambiguous, and the rewards of science not completely unresponsive to manipulation. The differences in competitiveness found in different disciplines is also related to the degree to which a definite and testable information base exists.

Glaser discusses the issue of whether professional recognition by colleagues and superiors has any specifiable relationship to stability of career. Professional recognition is regarded as one of the main rewards in science (Cole and Cole, 1967)—a gift exchanged for production of scientific papers, in Hagstrom's words. Drawing upon the role of self-images, and particularly the identification with idols of the great men of science which has lured individuals into research, Glaser points to the inevitable unfavorable self-concept that an individual develops, unless such rewards for scientific effort are earned. Still another facet of the pressures under which scientists work is presented by Reif and Strauss, when they discuss the sequelae of making a major discovery early in the professional career. There are some real problems associated with "staying alive" in science, and regenerating creativity in the many years that follow a "breakthrough."

These selections highlight only a few of the conflictful attitudes and experiences with which the mature scientist is confronted. The whole question of protecting one's work from invasion by others; the establishment of priorities, which Merton has discussed, and which Watson's (1970) more public presentation has enlivened; the friendships and enmities which have a professional base; the narcissistic over-investment among persons sharing research interests elucidate some of the seemingly inevitable concomitants of the drive for scientific success. By and large the work in this area has taken one point in time in the career of a man for study. Longitudinal studies of scientists, their attitudes and behaviors, as they proceed within the scientific career show promise of elaborating the more dynamic parts of these behaviors, especially as they change or develop over time—much in the spirit of research activity itself (Roe, in Section IV C; Eiduson, 1970).

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Social Control in Science

W. O. Hagstrom

The study of social control in science involves the search for characteristic types of behavior that produce conformity to or deviance from scientific norms and values. Many scientists would assert that the study of social control is of little importance because there is no problem of deviation in science—no significant tendencies by scientists to deviate or to induce conformity in others. Let us consider how such a position may be developed before attempting to discover the sources of social control in science; it is unprofitable to study inherently trivial forms of behavior.

THE LIMITS OF SOCIALIZATION

The socialization of scientists tends to produce persons who are so strongly committed to the central values of science that they unthinkingly accept them. Research as an activity comes to be "natural" for them: they find it self-evident that persons should be excited by discoveries, intensely interested in the detailed working of nature, and committed to the elaboration of theories that are of no use whatever in daily life. They develop a vocabulary of motives that makes curiosity about nature and an interest in understanding it an intrinsically important component of the human personality.[1]

These commitments are the outcome of a prolonged training process, lasting well into adult life, in which the student is effectively isolated from competing vocational and intellectual interests and in which he is extremely dependent on his teachers. Not only does the teacher control the fate of his student, determining whether he will be permitted to enter a scientific profession and, if so, at what kind of institution, but also the self-conception of the student is dependent on the response of the teacher: the teacher's evaluation tends to be taken by the student as an indication of what he "is." The peer group of students (at the graduate level) also reinforces commitment to scientific values.[2] Although science students are not isolated from other students in any formal way during the course of their training, the pressure of work and study and simple propinquity tend to produce informal groups composed of those in the same fields. In this respect, of course, there are large differences among scientific fields; laboratory sciences are perhaps more likely to generate social isolation. In any case, isolation and the acquisition of visible status symbols are not as important to the scientist as they are to the physician, for example. Medical education, with its emphasis on school-class solidarity, fraternal living, and distinctive garb and titles, needs to create a visible identity because the student physician confronts nonphysicians as an essential part of his task and must be able to have his expertise accepted at face value. The basic scientist does not face these challenges to his self-identity.[3] Even so, his evaluation of his own competence and of the importance of his work depends on the social validation of his teachers and his peers.

The content of the texts, lectures, and laboratory work presented in the course of higher education in the sciences integrates the general norms and values of science with a specific set of beliefs and techniques.[4]. . . Deviation from vague norms is more likely than deviation from norms specified for a concrete set of practices. It follows that physical scientists are less likely to deviate from the norms of science and scholarship than are social scientists or humanists.

As the scientist begins his professional life, his tasks typically reinforce the beliefs he has acquired as a student. In normal scientific research[5] "the characteristic problems are almost always repetitions, with minor modifications, of problems that have been undertaken and partially resolved before."[6] What the scientist has learned usually "works," and his technical success, regardless of any social confirmation of it, reinforces his commitments.

The effects of scientific socialization are reinforced by a highly selective system of recruitment. Of the fraction of the population

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who enter college, only fractions of those interested are permitted to graduate in the exact sciences and enroll in graduate school. Attrition in graduate school tends to be high, and only the more competent and highly motivated students obtain the doctorate.[7] Among those who do obtain doctorates in science, only a fraction are permitted to enter careers in basic research; the rest become teachers, administrators, and applied scientists. Basic scientists, then, are a highly selected and highly socialized elite group.

The entire socialization and selection process tends to produce scientists who are "self-starting" and "self-controlling." A common view of the organization of science, held implicitly or explicitly by most scientists, is that these individual characteristics are sufficient to account for conformity to scientific values and norms. It is often asserted that the scientist does what he wishes to do, attempts to solve problems that are intrinsically interesting and important, and is guided by aesthetic considerations. His social relations with others either interfere with this or are happy, but secondary, consequences of it.

This highly individualistic view is incomplete. It leads to no propositions about the actual scientific community as we know it, except perhaps that the socialization of recruits plays an unusually important part in the community's activities, and the importance of socialization is also consistent with other theoretical approaches. Some facts . . . are inconsistent with the view of the scientist assumed by an individualistic theory. For instance, scientists seek to publish their accomplishments and are greatly disturbed if proper recognition for them is not forthcoming. Moreover, scientists who experience prolonged isolation from their colleagues cease being productive. A more obvious objection to this individualistic approach is that scientists seldom consciously set to work on problems that they know others have solved. If scientists received their major gratifications from problem-solving alone, the mere fact that others have solved the problem should not deter them from solving it themselves.[8]. . .

Not only is the extremely individualistic view directly controverted by obvious facts about the scientific community, but there is every theoretical reason to expect this to be so. First, the autonomy of the scientific community cannot be taken for granted; it must be maintained by internal social controls, among other things. Without them, scientists would tend to respond more readily to the goals and standards of nonscientists. Second, communities of autonomous specialists tend to be rigid; they incorporate new goals and standards only with difficulty, for the socialization that produces commitment to norms and values at the highest levels also produces commitment to more specific norms. The scientific training that produces committed scientists also tends to commit them to techniques and particular theories. Since change is intrinsic in any community incorporating scientific values, if science is to thrive, scientists must respond to discoveries by continually changing their goals, techniques, and theories. Third, commitments to norms tend to erode in the absence of reinforcement. Many of the procedures known collectively as the "scientific method" are important only because they make possible communication among scientists. In the absence of sanctions, deviance from such norms would be common.

We may conclude that the socialization of scientists must be supplemented by a dynamic system of social control, if the values and effectiveness of science are to be maintained. Negative arguments are unsatisfying; the best reason for studying social control in science is that it leads one to discover the characteristic tensions within the scientific community, and this endeavor makes meaningful many varieties of scientific behavior that are otherwise unseen or dismissed as idiosyncratic and the consequence of aberrant personalities.

THE SOCIAL RECOGNITION OF DISCOVERY

Manuscripts submitted to scientific periodicals are often called "contributions," and they are, in fact, gifts. Authors do not usually receive royalties or other payments, and their institutions may even be required to aid in the financial support of the periodical.[9] On the other hand, manuscripts for which the scientific authors do receive financial payments, such as textbooks and popularizations, are, if not despised, certainly held in much lower esteem than articles containing original research results.

Gift-giving by scientists is thus similar to one of the most common modes of allocating resources to science, for this often takes the form of gifts from wealthy individuals or organizations. This has been true from the time of Cosimo de Medici to today, the time of the Rockefeller and Ford foundations. The gift status of moneys spent by industrial firms and governments on research is ambiguous; usually money seems to be spent with specific goals in mind, but the vast sums spent on space programs, particle accelerators, radiotelescopes, and so forth often seem like a potlatch by the community of nations. Neil Smelser has suggested that the gift mode of exchange is typical not only of science but of all institutions concerned with the maintenance and transmission of common values, such as the family, religion, and communities.[10]

In general, the acceptance of a gift by an individual or a community implies a recognition of the status of the donor and the existence of certain kinds of reciprocal rights.[11] These reciprocal rights may be to a return gift of the same kind and value, as in many primitive economic systems, or to certain appropriate sentiments of gratitude and deference. In science, the acceptance by scientific journals of contributed manuscripts establishes the donor's status as a scientist—indeed, status as a scientist can be achieved *only* by such gift-giving—and it assures him of prestige within the scientific community. . . .

The organization of science consists of an exchange of social recognition for information. But, as in all gift-giving, the expectation of return gifts (of recognition) cannot be publicly acknowledged as the motive for making the gift. A gift is supposed to be given, not in the expectation of a return, but as an expression of the sentiment of the donor toward the recipient. . . .

Gift-giving is capable of cynical manipulation; if this is publicly expressed, however, the exchange of gifts ceases, perhaps to be succeeded by contractual exchange. Consequently, scientists usually deny that they are strongly motivated by a desire for recognition, or that this desire influences their research decisions. . . .

Some of my informants allowed themselves to be pressed into admitting that recognition was a source of gratification. . . .

It is only the exceptional scientist, however, who sees the desire for recognition as a prime motivating force for himself and his colleagues.[12]. . .

Nevertheless, the public disavowal of the expectation of recognition in return for scientific contributions should not be taken to mean that the expectation is absent. . . . In science, the failure to recognize discovery may give rise, if not to warfare, at least to strong antagonisms and, at times, to intense controversy. A historical summary and analysis of priority controversies has been given by Robert K. Merton,[13] who pointed out that the failure to recognize previous work threatens the system of incentives in science. The pattern is not infrequent today. Of my seventy-nine informants, at least nine admitted to having been involved in questions of disputed priority either as the culprit or the victim. (The question was not asked in all seventy-nine interviews.) . . .

The desire to obtain recognition induces scientists to publish their results. "Writing up results" is considered to be one of the less pleasant aspects of research—it is not intrinsically gratifying in the way that other stages of a research project are. Some respondents were asked about the source of the greatest gratifications in research work. Generally the response was that most gratification came when the problem was essentially solved—when one became confident that an experiment would be successful, or when the outlines of a mathematical proof became clear—although details might remain to be cleaned up. For example, a theoretical physicist said: "[I get most pleasure] when the problem has been solved in principle but when some hard work remains to be done—when you have enough security to know you're not wasting your time but while there is some challenge left in the problem."

An experimental physicist said one receives most gratification: ". . . when you find the effect you're looking for—everything else is anticlimax. Also in seeing some new and unexpected effect—in seeing new phenomena before others do."

Research is in many ways a kind of game, a puzzle-solving operation in which the solution of the puzzle is its own reward.[14] "Everything else"—including the communication of results—"is anti-climax." Writing up results, "cleaning up loose ends," may be an irksome chore. . . .

The desire to obtain social recognition induces the scientist to conform to scientific norms by contributing his discoveries to the larger community. Thomas Sprat, writing near the dawn of modern science, perceived the importance of this: "If neither *Chance*, nor *Friendship*, nor *Treachery* of Servants, have brought such Things out; yet we see *Ostentation* alone to be every day powerful enough to do it. This Desire of Glory, and to be counted Authors, prevails on all. . . ."[15]

Not only does the desire for recognition induce the scientist to communicate his results; it also influences his selection of problems and methods. He will tend to select problems the solution of which will result in greater recognition, and he will tend to select methods that will make his work acceptable to his colleagues.

The range of acceptable methods varies. In mathematics, for example, the standards of rigor have changed steadily. E. T. Bell has pointed this out in noting that eighteenth-century mathematicians were lucky, by later standards, not to have made more mistkes than they did.[16]. . .

In this field, and most others, the change of standards is one of progress; the later standards can be shown to be technically superior. However, the definition of appropriate standards is not a technical matter only. For example, one informant was relatively famous for a method he had devised for making a certain kind of biochemical analysis. This method depended on distinctive nutritional requirements of certain bacteria. He noted that his method, while clearly superior to its alternatives for some purposes, and while very widely used, tended to be neglected by chemists:

> [The method] fell into some disrepute because we're using organisms, and the chemists said "Huh. Nobody can do the quantitative work with an organism." . . . There are other methods which chemists adopt. Chemists are a peculiar breed. They feel it is slightly debasing to use organisms. They just couldn't do that. . . . Most chemists use [other methods] for psychological reasons if nothing else. But I would never agree that other methods are generally better, except for specific purposes.

That is, social recognition in biochemical work done by chemists induces them to use techniques defined as distinctively chemical.[17]

Similarly, in mathematics the style of a proof, its "elegance," is often considered as important to its merit as the truth of the theorem proved. While there are technical reasons for this, there are distinctively social ones as well. . . . Conformity with methodological standards is necessary if social recognition is to be given for contributions.

Similarly, the goals of science as they are specified in particular disciplines at particular times cover a restricted range, and the process of the reward of social recognition tends to produce individual conformity to the differentiated goals. As John R. Baker has put it:

> The scientist is able to construct a sort of scale of scientific values and to decide that one thing or theory is relatively trivial and another relatively important, quite apart from any question of practical applications. There is, as Poincaré has well said, "une hierarchie des faits." Most scientists will agree that certain discoveries or propositions are more important because more widely significant than others, though around any particular level on the scale of values there may be disagreement.[18]

Sanctions to enforce conformity in this respect, as well as with regard to appropriate techniques, are of two general kinds. First, works that deviate too far from the norm will be refused publication in scientific journals. . . . Such an exercise of sanctions makes it impossible for the great mass of scientists to evaluate for themselves the importance and validity of the information presented. Delegating considerable power to a few authorities obviously infringes on the norms of independence in science. For this reason, editors and referees tend to be tolerant, basing their decisions on estimates that others will find manuscripts interesting even if they themselves do not. This and the fact that there are many journals, some of them unrefereed,[19] means that the sanction is of little importance to most scientists most of the time. A more important sanction is the social recognition published work receives; this sanction is exercised by the community at large and applies to all published research.

Another type of sanction is not primarily important in science, although it is often alleged to be. This consists of extrinsic rewards, primarily position and money.[20] It is alleged that scientists publish, select problems, and select methods in order to maximize these rewards. University policies that base advancement and salary on quantity of publication sometimes seem to imply that this is true, that scientists' research contributions are not freely given gifts at all but are, instead, services in return for salary. While it is important for extrinsic rewards to be more or less consistent with recognition, the ideal seems to be that they should follow recognition, and this seems to be the general practice. In any case, an explanation of scientific behavior in terms of extrinsic rewards is weakened by the fact that many scientists in elite positions, whose extrinsic rewards will be unaffected by their behavior, continue to be highly productive and to conform to scientific goals and norms. Furthermore, scientists usually feel that it is degrading and improper to submit manuscripts for publication primarily to gain position without really caring if the work is read by others.

But why should gift-giving be important in science when it is essentially obsolete as a form of exchange in most other areas of modern life, especially the most distinctly "civilized" areas? Gift-giving, because it tends to create particularistic obligations, usually reduces the rationality of economic action. Rationality is maximized when "costs" of alternative courses of action can be assessed, and such costs are usually established in free-market exchanges or in the plans of central directing agencies. When participants are paid a money wage or salary for their efforts, and when this effectively controls their behavior, the system is more flexible than when controls derive from traditional or gift obligations.[21] Why, then, does this frequently inefficient and irrational form of control persist in science? To be sure, it also tends to persist in other professions. Professionals are expected to be motivated by a desire to serve others.[22] For example, physicians do receive a fee for service, yet they are expected to have a "sliding scale" and serve the indigent at reduced fees or for no fees at all. The larger community recognizes two types of public dependence on professions: professional services are regarded as essential, concerned with values that should be realized regardless of a client's ability to pay; and nonprofessionals are unable to evalute professional services, which makes them vulnerable to exploitation by unqualified persons. The rationale for the norm of service is usually the former type of dependence. In science, for example, the fact that a community has no one willing and able to pay for an important item of useless knowledge is not supposed to interfere with its ability to acquire the knowledge. But the idea of the gift and the norm of service is also related to the dependence of the public that follows from its inability to evaluate services.

The rationality of professional services is not the same as the rationality of the market. In contractual exchanges, when services are rewarded on a direct financial or barter basis, the client abdicates, to a considerable degree, his *moral control* over the producer. In return, the client is freed from personal ties with the producer, and he is able to choose rationally between alternative sources of supply. In the professions, and especially in science, the abdication of moral control would disrupt the system. The producer of professional services must be strongly committed to higher values. He must be responsible for his products, and it is fitting that he not be alienated from them. The scientist, for example, must be concerned with maintaining and correcting existing theories in his field, and his work should be oriented to this end. The exchange of gifts for recognition tends to maintain such orientations. On the one hand, the recipient of the gifts finds it difficult to refuse them (they are "free"), and, on the other, the donor is held responsible for adhering to central norms and values. The maxim, *caveat emptor,* is inapplicable.[23] Furthermore, the donor is not alienated from his gift, but retains a lasting interest in it. It is, in a sense, his property.[24] One indication of this is the frequent practice of eponymy, the affixing of the name of the scientist to all or part of what he has found.[25]

Emphasis on gifts and services occurs frequently in social life, and we can get at the root of this generality by focusing on certain paradoxical elements implicit in the argument presented thus far. It has been argued that scientists are oriented to receiving recognition from colleagues and that this orientation influences their research decisions. Yet evidence that scientists themselves deny this has also been presented. There is a normative component to this denial, one that appears more clearly in analyzing scientific fashions. It is felt that, if a scientist's decisions are influenced by the probability of being recognized, he will tend to deviate from certain central scientific norms—he will fail to be original and critical. Thus, while it is true that scientists are motivated by a desire to obtain social recognition, and while it is true that only work on certain types of problems and with certain techniques will receive recognition at any particular time, it is also true that, if a scientist were to admit being influenced in his choices of problems and techniques by the probability of being recognized, he would be considered deviant. That is, if scientists conform to norms about problems and techniques as a result of this specific form of social control, they are thereby deviants.

This apparent paradox, that people deviate in the very act of conforming, is common whenever people are expected to be strongly committed to values. In general, *whenever strong commitments to values are expected, the rational calculation of punishments and rewards is regarded as an improper basis for making decisions.* Citizens who refrain from treason merely because it is against the law are, by that fact, of questionable loyalty; parents who refrain from incest merely because of fear of community reaction are, by that fact, unfit for parenthood; and scientists who select problems merely because they feel that in dealing with them they will receive greater recognition from colleagues are, by that fact, not "good" scientists. In all such cases the sanctions are of no obvious value: they evidently do not work for the deviants, and none of those who conform admit to being influenced by them. But this does not mean that the sanctions are of no importance; it does mean that more than overt conformity to norms is demanded, that inner conformity is regarded as equally, or more, important.

Thus, the gift exchange (or the norm of service), as opposed to barter or contractual exchange, is particularly well suited to social systems in which great reliance is placed on the ability of well-socialized persons to operate independently of formal controls. The prolonged and intensive socialization scientists experience is reinforced and complemented by their practice of the exchange of information for recognition. The socialization experience produces scientists who are strongly committed to the values of science and who need the esteem and approval of their peers. The reward of recognition for information reinforces this commitment but also makes it flexible. Recognition is given for kinds of contributions the scientific community finds valuable, and different kinds of contributions will be found valuable at different times.

The scientist's denial of recognition as an important incentive has other consequences related to those already mentioned. When peers exchange gifts, the denial of the expectation of reciprocity in kind implies an expectation of gratitude, a highly diffuse response.[26] It will be shown that this kind of gift exchange occurs among scientists, although the more important form of scientific contribution is directed to the larger scientific community. In this case the denial of the pursuit of recognition serves to emphasize the universality of scientific standards: it is not a particular group of colleagues at a particular time that should be addressed, but all possible colleagues at all possible periods. . . .

While this orientation is consistent with the scientist's need for autonomy—being dependent on the favors of particular others is terrifying—it also contains a strong element of the tragic. Scientists learn to *expect* injustice, the inequitable allocation of rewards. Occasionally one of them makes this explicit. Max Weber addressed students on "Science as a Vocation" in the following way:

> I know of hardly any career on earth where chance plays such a role. . . . If the young scholar asks for my advice with regard to habilitation, the responsibility of encouraging him can hardly be borne . . . one must ask every . . . man: Do you in all conscience believe that you can stand seeing mediocrity after mediocrity, year after year, climb beyond you, without becoming embittered and without coming to grief? Naturally, one always receives the answer: "Of course, I live only for my calling." Yet, I have found that only a few men could endure this situation without coming to grief.[27]

More common than such an explicit statement is the myth of the hero who is recognized only after his death. This myth is important in science, as in art, because it strengthens universal standards against tendencies to become dependent on particular communities. . . .

The distinctive functions of the system by which gifts of information are exchanged for recognition can be seen within science. Textbook-writing and the preparation of popularizations are expected to be neither original in the scientific sense nor critical of existing theories. Since texts draw on what is already known, teachers who adopt them are usually competent to judge the validity of the material presented. Consequently, writing of this sort is regarded as a technical skill and not a highly responsible task, and it has little effect on a scientist's reputation. . . .

Because authors of texts and popularizations need not be highly committed to the values of the scientific community, their activities have little bearing on their standing within it. As a result, incentives for such tasks must be of a more general nature. Royalties are such a generalized reward; unlike recognition, cash can be used outside the community of pure science. The exchange of information for recognition, on the other hand, binds donors and recipients in a community of values.

Thus, gift-giving in exchange for recognition is an appropriate method of social control in science, and it is apparently relatively effective.[28]. . .

NOTES

1. Since many readers will be scientists, they will share these values and may find it difficult to conceive how the idea of "disinterested curiosity" strikes others. The writings of Friedrich Nietzsche and other antirationalist philosophers are helpful in this regard; Nietzsche viewed disinterested curiosity as a form of psychopathology. See, for example, the sections, "Immaculate Perception" and "Scholars," in *Thus Spake Zarathustra;* the sections, "Prejudices of Philosophers" and "We Scholars," in *Beyond Good and Evil;* and sections 23 through 25 of his essay, "Ascetic Ideals."
2. Berelson asked recent recipients of the Ph.D. whether they had learned "a great deal from one another" as graduate students. "About three-fourths said they had—a little more in the sciences, a little more in the top places (where the better students are). As a matter of fact, when I went on to ask, 'When you get right down to it, and taking everything into account, did you learn more from your fellow students or from your professors?,' only about three-fourths said their professors. Most of the others said the score was about even." Bernard Berelson, *Graduate Education in the United States* (New York: McGraw-Hill, 1960), p. 105.
3. Cf. the following articles by Howard S. Becker and James Carper: "The Development of Identification with an Occupation," *American Journal of Sociology,* 61 (1956), 289–298; and "The Elements of Identification with an Occupation," *American Sociological Review,* 21 (1956), 341–348. They point out that engineers are neither as likely as scientists to develop a distinctive sense of identity nor as isolated in their educational careers as scientists are. This is related to the fact that engineering, unlike medicine or science, is more likely to be perceived as a step to a different occupation than as a permanent one.
4. For a description of this kind of education, see Thomas S. Kuhn, "The Essential Tension: Tradition and Innovation in Scientific Research," in Calvin W. Taylor and Frank Barron, eds., *Scientific Creativity: Its Recognition and Development* (New York: Wiley, 1963), pp. 341–354. On pp. 344f. Kuhn goes on to suggest that, incompatible as it is with the values of liberal education, this kind of education seems to work—the sciences that have it develop more rapidly than those that do not.
5. Research during crisis and revolutionary situations is something else again and will be discussed in Chapter VI.
6. Kuhn, *op. cit.,* p. 348; see also Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962), chs. III and IV.
7. See Berelson, *op. cit.,* pp. 167–171, and James A. Davis, *Stipends and Spouses* (Chicago: University of Chicago Press, 1962), pp. 109–115, 264.
8. This sometimes seems to hold true in other professional contexts. An engineering executive in an electronics firm complained that the engineers under him did not pay adequate attention to the literature. He described them as suffering from the "N.I.H. factor": the belief that if something was "not invented here," in this firm, in this department, or even in this very workroom, it did not exist. As a result, much

time was wasted inventing things that had already been invented in the firm or in other firms. Unlike scientists, engineers are not as closely bound into a larger professional community.

9. When this is so, editorial decisions to publish are kept independent of the possibility of payment. Thus, in 1962, only 78 per cent of the pages published in the *Journal of Mathematical Physics* were paid for by the authors' institutions. See Henry A. Barton, "The Publication Charge Plan in Physics Journals," *Physics Today*, 16, 6 (June 1963), 45–57.
10. Neil J. Smelser, "A Comparative View of Exchange Systems," *Economic Development and Cultural Change*, 7 (1959), 173–182.
11. Cf. Alvin W. Gouldner, "The Norm of Reciprocity," *American Sociological Review*, 25 (1960), 161–178; and Marcel Mauss, *The Gift: Forms and Functions of Exchange in Primitive Societies* (Glencoe, Ill.: Free Press, 1954), pp. 40f., 73, *et passim*.
12. In a series of interviews with twenty eminent American biologists, Anne Roe was given the same impression about the suppression of the wish for recognition: ". . . the concentration is on the work primarily as an end in itself, not for economic or social ends, or even for professional advancement and recognition, although they are not indifferent to these." Roe, "A Psychological Study of Eminent Biologists," *Psychological Monographs*, 65 (1951), p. 65. Bernice T. Eiduson makes a similar report based on her study of forty scientists in her *Scientists: Their Psychological World* (New York: Basic Books, 1962), pp. 162 and 178f. See also Charles Darwin, *The Autobiography of Charles Darwin, 1809–1882*, Nora Barlow, ed. (London: Collins, 1958), p. 141, for another scientist's disavowal of desire for recognition.
13. Merton, "Priorities in Scientific Discovery," *American Sociological Review*, 22 (1957), 635–659.
14. See Kuhn, *The Structure of Scientific Revolutions*, *op. cit.*, pp. 35–42. The layman can get some ideas of the gratification involved by reading such novels as C. P. Snow's *The Search* or Sinclair Lewis' *Arrowsmith*.
15. Sprat, *The History of the Royal Society of London* (London, 1673), pp. 74f. See also Karl Mannheim on the importance of the desire for recognition in science and other cultural pursuits: *Essays on the Sociology of Knowledge* (London: Routledge and Kegan Paul, 1952), ch. VI, especially pp. 239, 242–243, 272.
16. Eric T. Bell, *The Development of Mathematics* (2nd ed.; New York: McGraw-Hill, 1945), p. 153.
17. Compare the reception by chemists of chromatographic techniques, discovered by Tswett, a Russian botanist, in 1906: ". . . the chromatographic method got off to a bad start . . . for a lowly botanist to assault thus the whole chemical profession was unthinkable! . . . the chromatographic method fell largely into disrepute, and . . . Tswett spent the later part of his life in misery and poverty." The importance of the technique was only recognized in the late nineteen twenties. James E. Meinhard, "Chromatography: a Perspective," *Science*, 110 (1949), 387–392.
18. *Science and the Planned State* (London: George Allen and Unwin, 1945), p. 33.
19. In a survey by the International Council of Scientific Unions of one hundred fifty-six editors of well-known primary journals, 16 per cent reported that manuscripts were not sent to referees and 8 per cent gave equivocal answers to a question on the topic. Reported by J. R. Porter, "Challenges to Editors of Scientific Journals," *Science*, 141 (1963), 1014.
20. Simon Marcson shows how organizations "structure recognition by means of appropriate symbols, including titles, size of office, accessibility, financial rewards, and so on." *The Scientist in Industry* (New York: Harper, 1960), p. 73. However, the difference between recognition and other rewards should be kept clear, for otherwise it is difficult to analyze social control and to specify the source of control. In this work "recognition" means only the written and verbal behavior and the "expressive gestures" of scientists that indicate their approval and esteem of a colleague because of his research accomplishments.
21. Put another way, flexibility and rationality are maximized when workers are alienated from their products. Cf. Talcott Parsons, "*Voting* and the Equilibrium of the American Political System," in Eugene Burdick and Arthur J. Brodbeck, eds., *American Voting Behavior* (Glencoe, Ill.: Free Press, 1959), p. 89. See also Max Weber's stress on "formally free" and actually alienated labor as a defining characteristic of capitalism: *General Economic History* (Glencoe, Ill.: Free Press, 1927), p. 277.

22. Talcott Parsons, *Essays in Sociological Theory* (Glencoe, Ill.: Free Press, 1954), ch. II.

23. This does not mean that scientists are not supposed to be skeptical; that they should be skeptical about their own work as well as that of their colleagues is one of the more important institutionalized norms of science. Cf. Robert K. Merton, *Social Theory and Social Structure* (Glencoe, Ill.: Free Press, 1949), pp. 315f. It does mean that unlike the consumer in the free market, the "consumer" of scientific products can hold the producer morally responsible for "defective products."

24. Cf. Merton, "Priorities in Scientific Discovery," *op. cit.*, pp. 640f., and *Social Theory and Social Structure, op. cit.*, pp. 312f.

25. Cf. Merton, "Priorities in Scientific Discovery," *op. cit.*, pp. 642–644. See also Edwin G. Boring, "Eponym as Placebo," in his *History, Psychology, and Science: Selected Papers* (New York: Wiley, 1963), pp. 5–28, where it is suggested that eponymy has psychological benefits even for scientists who cannot hope to be remembered this way themselves. Compare Edward A. Gall's questionnaire study of medical scientists, in which he attempted to determine the extent to which they favored or opposed the use of eponymous terms: "The Medical Eponym," *American Scientist*, 48 (1960), 51–57. His results were inconclusive, which is not surprising since incentives of this type are seldom adopted on rational grounds or evaluated according to technical standards.

26. As Georg Simmel says, gratitude "establishes the bond of interaction, of the reciprocity of service and return service, even where they are not guaranteed by external coercion." *The Sociology of Georg Simmel*, Kurt Wolff, ed. and trans. (Glencoe, Ill.: Free Press, 1950), p. 387. He goes on to note that persons make great efforts to avoid receiving gifts in order not to make such commitments to others. Something like this may occur in science.

27. Hans H. Gerth and C. Wright Mills, trans. and eds. *From Max Weber: Essays in Sociology* (New York: Oxford University Press, 1946), pp. 132, 134. Weber was partly concerned with the particular aspects of science in German universities, but the entire essay shows that he was also concerned with the more universal aspects of science as a profession.

28. Recognition by colleagues is probably a more effective means of social control among scientists, whose products are necessarily public knowledge, than it is among some other professionals. The limitations of colleague control in medicine are pointed out in Eliot Freidson and Buford Rhea, "Processes of Control in a Company of Equals," *Social Problems*, 11 (1963), 119–131.

Behavior Patterns of Scientists

Robert K. Merton

The history of science indelibly records 1953 as the year in which the structure of the DNA molecule was discovered. But it is 1968 that will probably emerge as the year of the double helix in the history that treats the behavior of scientists, for James Watson's deeply personal account of that discovery, now in its ninth printing, has evidently seized the public imagination. . . .

To judge from the popular reviews, the essential message of the book was taken to be that scientists are human, after all. This phrasing, it turns out, does not mean that scientists can be assigned at long last to the species Homo sapiens. Many Americans and some Englishmen were apparently prepared to entertain that serviceable hypothesis even before the appearance of *The Double Helix*. Evidently, what is meant by the Watson-induced thought that scientists too are human is that scientists are all too human; that, in the succinct jaundiced words of the St. Louis *Post-Dispatch*, "they can be boastful, jealous, garrulous, violent, [and even] stupid. . . ."

What, then, are the stories Watson tells about the social and intellectual interactions that entered into the discovery, stories eliciting the popular response that scientists are all too human? Above all else, he tells of the race for priority; a close awareness of the champion rival who must be defeated in this contest of minds; a driving insistence on getting needed

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data from sometimes reluctant, sometimes inadvertent collaborators; a competition for specific discoveries over the years between the Cavendish and Caltech; an allegedly English sense of private domains for scientific investigation that bear no-poaching signs; an express ambition for that ultimate symbol of accomplishment, the Nobel. He tells, too, about alternating periods of intense thought and almost calculated idleness (while the gestation of ideas pursues its course); about false starts and errors of inference; about quickly getting up needed scientific knowledge despite an impressive inventory of initial ignorance; about the complementarity of talents, skills and character-structure of the symbiotic collaborators; about an unfailing sense for the key problem, and an intuitive and stubbornly maintained imagery of the nature of its solution, together with the implications as these were expressed in that masterstroke of calculated understatement wrought by Francis Crick: "It has not escaped our notice that the pairing we have postulated immediately suggests a possible copying mechanism for the genetic material."

The stories detailed in *The Double Helix* have evidently gone far to dispel a popular mythology about the complex behavior of scientists. That this response should have occurred among the public at large is not surprising. Embodying as they do some of the prime values of world civilization, scientists have long been placed on pedestals where they may have no wish to be perched—not, at least, the more thoughtful among them. This is not the result of a conspiracy, not even a conspiracy of goodwill. It is only that men and women of science have long been pictured, through collective acts of piety, as though they were more than human, being like gods in their creativity, and also as less than human, being deprived in their work of the passions, attitudes and social ties given to ordinary men. As a result, scientists have been dehumanized in the public mind by being idealized and, on occasion, idolized. Contributing greatly to this centuries-long process of distortion are the pious biographers who, in sapless prose, convert indubitably great men of science into what Augustus de Morgan once described as "monsters of perfection."

In part, too, the imagery of scientists moving coolly, methodically and unerringly to the results they report may stem from the etiquette that governs the writing of scientific papers. This etiquette, as we know, requires them to be works of vast expurgation, stripping the complex events and behaviors that culminated in the report of everything except their cognitive substance. Compare only the lean, taut, almost laconic, nine-hundred-word article that appeared in *Nature* that momentous April in 1953 with the tangled web of events reported in Watson's forty-thousand-word account of the same discovery.

The sense of popular revelation upon learning that scientists are actually human testifies, then, to the prevalence of an earlier belief to the contrary. Ironically enough, that older mythology now threatens to be displaced by a somewhat new variant, expressed in responses to the Watson memoir by scientists and humanists alike. (I use the term mythology in its decidedly untechnical sense to denote a set of ill-founded beliefs held uncritically by an interested group.) The new variant has several interrelated components. The patterns of motives and behavior set out in Watson's irreverent, naturalistic narrative are held to be distinctive of the newest era of science, staffed by "a new kind of scientist and one that could hardly have been thought of before science became a mass occupation." Only in our highly competitive age, allegedly, are appreciable numbers of scientists concerned to "scoop" others at work in the field and so to gain recognition for their accomplishments. . . .

There is a certain plausibility to this view that the mores of science and the behavior of scientists must surely have changed in the recent past. For plainly, all the basic demographic, social, economic, political and organizational parameters of science have acquired dramatically new values. The size of the population of working scientists has increased exponentially from the scattered hundreds three centuries ago to the hundred or more myriads today. The time of the amateur is long since past; scientists are now professionals all, their work providing them with a livelihood and, for some, a not altogether impoverished one. The social organization of scientific inquiry has greatly changed, with collaboration and research teams the order of the day . . . The monumental budgets assigned to science—although never large enough, as all of us know—are orders of magnitude greater than the straitened budgets of only a few generations ago, to say nothing of the immense contrast with those of the more remote past. The vast increase in numbers of scientists and in funds for science practically dictates the exponential increase in the quantity of published research. As science has become more institutionalized, it has also become more intimately interrelated with the other institutions of society. Science-based technologies and the partial diffusion of a scientific outlook have become great social forces that move our history and greatly affect the relations obtaining between the nations of the world. Scientists do not, of course, make the major political decisions, but they now affect them significantly. The Szilard-Einstein letter to the President, for example, would be described by some as one of the most consequential communications in recorded history.

. . . With all these profound changes, as any sociologist is apt to tell you if you give him half a chance, there must also be a new ethos of science abroad, a new set of values and institutionally patterned motives. And, as I have noted, practicing scientists in biology and physics and chemistry have indeed suggested that we now have a new breed of scientists, actuated by new motives, oriented to the main chance, and gravely agitated by failures to achieve. Like other men, scientists become disturbed by the panhuman problem of evil, in which, to assume the language of Gilbert Murray, "the fortunes of men seem to bear practically no relation to their merits and efforts."

. . . I must interrupt these introductory observations with a personal confession. It was just thirty years ago that I suggested in a footnote tucked away in a monograph on science in seventeenth-century England that the race for priority might constitute a strategic subject for study and might provide clues to ways in which the institution of science shapes the motives, passions and social relations of scientists. So far as I can tell, the youthful author of that footnote proved to be its only reader: At any rate, no one, not even he, harkened to the muted clarion call. Some ten years ago, when addressing a captive audience of a thousand sociologists, I tried to make amends for this lapse of two decades and examined the import of priority races for an understanding of both the institution of science and the behavior of scientists. In more recent years, my colleagues at Columbia and I have examined these implications in a series of investigations. In what follows, I shall draw mercilessly upon these inquiries.*

Now to return to the belief system that regards the rough-and-tumble of contest and competition in science as peculiar to our own deteriorating times, that treats such contest as inevitably self-aggrandizing and takes the drive to be first in reaching a discovery as necessarily displacing that "relish of knowledge" (of which John Locke spoke), as doing away with intrinsic joy in discovery or pleasure in the beauty of a powerful simplifying idea.

As with most mythologies, this one is not altogether out of touch with the world of everyday experience. Although it may have surprised the outsider, Watson's unabashed report on the race for priority scarcely came as news to his fellow-scientists. They know from hard-won experience that multiple independent discoveries at about the same time constitute one of their occupational hazards. They not only know it, but often act on that premise. That the consequent rush to achieve priority is common in our time hardly needs documentation. The evidence is there on every side. A few years before Watson reached his much wider audience, Arthur Schawlow casually noted in the public prints that Charles Townes and he had been "in a hurry, of course. We feared that it might be only a matter of time before others would come up with the same idea. So we decided to publish before building a working model. . . . Subsequently, Theodore Maiman won the frantic race between many experimenters to build the first laser. Our theory was verified." Townes had ample biographical rea-

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* R. K. Merton, "Priorities in scientific discovery," *American Sociological Review*, December, 1957, Vol. 22, 635–659; "Singletons and multiples in scientific discovery," *Proceedings* of the American Philosophical Society, October, 1961, Vol. 105, 470–486; "The ambivalence of scientists," *Bulletin of the Johns Hopkins Hospital*, 1963, Vol. 112, 77–97; "Resistance to the systematic study of multiple discoveries in science," *European Journal of Sociology*, 1963, iv, 237–282; *On the Shoulders of Giants* (New York: The Free Press, 1965: Harcourt, Brace and World, 1967); "The Matthew effect in science," *Science*, January 5, 1968, Vol. 159, 56–63. This work has been supported, in part, by the National Science Foundation and by the Columbia University Institute for the Study of Science in Human Affairs.

son to be in a hurry. After all, in the early 1950's, he had been involved in that fivefold independent discovery of the maser, along with Willis Lamb, Joseph Weber, Nikolai Basov and Aleksandr Prokhorov. . . .

On every side, then, there is evidence that some unknown proportions of contemporary scientists are actively engaged in trying to get there first. The fact is a commonplace. But does the fact warrant the inference, drawn in the emerging mythology, that intense competition for discovery is in a significant sense distinctive of the new era of science, with its enlarged population of scientists, its grants, prizes and professional rewards? I think not. This component of the mythology is the result of parochial perception. It emerges from the simple expedient of not looking at what there is to see throughout the centuries of modern science. It is a mythology achieved by emasculating the history of that science.

For the plain fact is, of course, that the race for priority has been frequent throughout the entire era of modern science. Moving back only a generation or so, we observe the good-natured race between Hahn and Boltwood, for example, to discover the "parent of radium" which Boltwood was able to find first, just as, when Hahn discovered mesothorium, Boltwood acknowledged his having been outdistanced, saying only, "I was almost there myself . . ." There is Ramsay telegraphing Berthelot in Paris "at once" about his isolation of helium; writing Rayleigh to the same effect and sending a note to the Royal Society to establish priority, just as he and Travers were to announce having nosed out Dewar in the discovery of neon. There is the forthright account by Norbert Wiener of the race between Bouligand and himself in potential theory, making Wiener "aware that he must hurry," but having it end in a "dead heat" since Bouligand had submitted his "results to the [French] Academy in a sealed envelope," just a day before Wiener had gotten off a short note for publication in the *Comptes Rendus*. . . .

In this respect the behavior of scientists does not much vary, transcending differences of time and national culture. Peter Kapitza puts it all in the of-course mood as he describes the behavior of Lomonosov, the father of Russian science, saying: "No less importance was attached to priority in scientific work at that time than now." Of this, Lomonosov and his colleagues provide dramatic evidence. When the physicist Richman was killed by lightning in 1763, the Russian Academy of Sciences canceled its general meeting, only to have Lomonosov ask that he nevertheless be given the opportunity to present his paper on electricity, "lest," in his words, "it lose novelty." The president of the Academy saw the point and arranged for a special meeting in order, as he explained, "that gospodin Lomonosov should not be late with his own new productions among scientific people in Europa, and his paper thereby be lost in electrical experiments made meanwhile."

The fact is that almost all of those firmly placed in the pantheon of science—Newton, Descartes, Leibniz, Pascal or Huyghens, Lister, Faraday, Laplace or Davy—were caught up in passionate efforts to achieve priority and to have it publicly registered. Consider only a highly condensed account of how things stood with Newton. Now, I do not undertake to compare Newton and Watson in terms of their nature-given talents or their society-nurtured accomplishments. Such comparison would be not merely odious but downright foolish. But when we are told that the aggressive, prize-seeking, competitive and pathbreaking behavior of Watson is something new unleashed in the mid-twentieth-century world of science, there is some point in examining the apposite behavior of the seventeenth-century giant of science. One incidental similarity of bare chronology is trivial enough to require no more than passing mention. They were both in their golden years, decidedly young men. Just as Jim Watson took up the problem he made his own in his twenty-third year, so we will remember, from Newton's own account, the *annus mirabilis* when at twenty-three or twenty-four he discovered the binomial theorem, started work toward invention of the calculus, took his first steps toward establishing the law of universal gravitation, and began his experiments on optics.

Long after he had made these incomparable contributions to mathematics and physical science, Newton was still busily engaged in ensuring the luster and fame owing him. He was not merely concerned with establishing his priority but was periodically obsessed by it. He developed a corps of young mathematicians and astronomers, such as Roger Cotes, David Gregory, William Whiston, John Keill and, above all, Edmond Halley, "for the energetic building of his fame" (as the historian Frank Manuel has put it in his recent *Portrait of Isaac Newton*). Newton's voluminous manuscripts contain at least twelve versions of a defense of his priority, as against Leibniz, in the invention of the calculus. Toward the end, Newton, then president of the Royal Society, appointed a committee to adjudicate the rival claims of Leibniz and himself, packed the committee with his adherents, directed its every activity, anonymously wrote the preface for the second published report on the controversy—the draft is in his handwriting—and included in that preface a disarming reference to the legal adage that "no one is a proper witness for himself and [that] he would be an iniquitous Judge, and would crush underfoot the laws of all the people, who would admit anyone as a witness in his own cause." We can gauge the pressures for establishing his unique priority that must have operated for Newton to adopt such means for defense of his claims. As I shall presently suggest, this was not so much because Newton was weak as because the newly institutionalized value set upon originality in science was so great that he found himself driven to these lengths.

By comparison, Watson's passing account of a priority-skirmish within the Cavendish itself can only be described as tame and even-handed, almost magnanimous. That conflict largely testified to the ambiguous origins of ideas generated in the course of interaction between colleagues, touched, perhaps, with a bit of cryptomnesia.

For those who believe that the Watson memoir expresses a new and extreme drive for getting there first in science, the antidote will be found in reading the *Philosophical Transactions* of the Royal Society for January and February 1715, devoted almost entirely to the angry quest for priority of Newton over Leibniz. And those who consider that Watson's account converts science into an arena for spectator sport, new or peculiar to our time, have something to learn from the observation by Frank Manuel that

> Two of the greatest geniuses of the European world, not only of their own time, but of its whole long history, had been privately belaboring each other with injurious epithets and encouraging their partisans to publish scurrilous innuendoes in learned journals. In the age of reason they behaved like gladiators in a Roman circus. Here were two old bachelors, Leibniz not far from death, Newton with a decade more of life, each fighting for exclusive possession of his brainchild, the right to call the invention of the calculus his own and no one else's.

. . . Here is one pattern that repeats itself through the centuries of modern science. Two or more scientists quietly announce a discovery. Since it is often the case that these are truly independent contributions, with each scientist having exhibited originality of mind, the process is sometimes stabilized at that point. But as the behavior of Newton, Leibniz, Hooke and an indefinitely large number of other scientists testifies, this peaceful acceptance of the fact of independent discovery does not always occur. Since the situation is often ambiguous, with the role of each scientist not easy to identify, and since each one *knows* that he had himself arrived at the discovery, and since the institutionalized stakes of reputation are high and the joy of acknowledged discovery immense, this militates against mutual acknowledgment of a parallel contribution. One or another of the discoverers—or, often, his colleagues or fellow nationals—suggests that he rather than the rival was really first, and that the independence of his rival is at least unproved. Then begins the familiar deterioration of standards governing conflictful interaction. The other side, grouping their forces, counter with the opinion that plagiary had indeed occurred, that let him whom the shoe fits wear it and, furthermore, to make matters quite clear, the shoe is on the other's foot. Reinforced by group loyalties and sometimes by ethnocentrism, the controversy gains force, mutual charges of plagiary abound and there develops an atmosphere of thoroughgoing hostility and mutual distrust.

It is symbolically fitting that the man who arranged for the recognition in perpetuity of major scientific accomplishments, which are often cases of acknowledged multiple discovery or of barely established priority, should himself have been engaged in a struggle over priority of technological invention. For it happens that the Maecenas who established these prizes—Alfred Nobel himself—was deeply involved in a battle with Frederick Abel and James Dewar over the invention of smokeless, nitroglycerin gunpowder. The documentation of this particular scrap "fills several yards of shelves in the Nobel Foundation's archives." And it was small comfort to the agitated Nobel to have the Lord Justice, compelled on technical grounds not to find for Nobel, borrowing and adapting the aphorism made famous by Newton when he declared that "it is obvious that a dwarf who has been allowed to climb up on the back of a giant [the giant being Nobel, of course] can see farther than the giant himself." Nobel's frustrations and resentment over having been deprived of his intellectual property were only slightly siphoned off in his play, *The Patent Bacillus*, which lampooned the British court system.

This sampling of historical evidence is perhaps enough to put into question the belief that science today is competitive to a degree unknown before. If there has been a change in this aspect of the ethos of science, it seems to be of quite another kind. Scientists have apparently become more fully aware that, with growing numbers at work in each special field, any discovery is apt to be made by others as well as themselves, and so are less often apt than before to assume that parallel discoveries must be borrowed ones. Among the multitude of multiple discoveries in the history of science, Elinor Barber and I have examined a sample of two hundred and sixty-four in detail and have found, among other things, that there is a secular decline in the frequency with which multiples are an occasion for intense priority-conflicts. Of the thirty-six multiples before 1700 that we have examined, ninety-two percent were strenuously contested; the figure drops to seventy-two percent in the eighteenth century; remains at about the same level in the first half of the nineteenth century and declines to fifty-nine percent in the second half, reaching the lowest level of thirty-three percent in the first half of this century. Perhaps the culture of science today is not so pathogenic as it once was.

The absence of historical perspective marks another component of the new mythology of science. This one holds that quick, if not premature, publication to ensure priority is peculiar to our new breed of scientists, as witness the manuscript that went off to the editors of *Nature* on that fateful April 2nd of 1953. Again, it will do no harm to examine this opinion from a sociological and historical perspective. Today as yesterday, scientists are caught up in one of the many ambivalent precepts contained in the institution of science. This one requires that the scientist must be ready to make his newfound knowledge available to his peers as soon as possible *but* he must avoid an undue tendency to rush into print. (Compare Faraday's motto: "Work, Finish, Publish" with Ehrlich's "*Viel arbeiten, wenig publizieren!*") To see this in fitting historical context, we must remember that the first scientific journals confronted not an excess but a deficiency of manuscripts meriting publication. The problem did not arise merely from the small number of men at work in science. There was the further restraint that the value set upon the open disclosure of one's scientific work was far from universally accepted. Intent upon safeguarding their intellectual property, many men of science in the seventeenth century set a premium upon secrecy (as is evident from their correspondence with close associates).

To convert this motivated secrecy into motivated free disclosure, Henry Oldenburg, the first editor of the *Philosophical Transactions*, introduced an expedient for maintaining property rights through prompt publication. In this way, the contributor would be assured his priority. We see the effectiveness of this socially patterned motivation beautifully exemplified in the case of Robert Boyle who, like others of his time, was chronically and acutely anxious about the danger of what he described as "philosophical robbery"—what would be less picturesquely described today as pilfering from circulated but unpublished manuscripts. Boyle felt that he had often been so victimized. But now, the editor Oldenburg could assure Boyle and others that their priority rights would be guarded as never before. Exceedingly prompt publication in the *Transactions* would take care of that. As Oldenburg wrote Boyle about his perennially "lost papers": "They are now very safe, and will be within this week in print, as [the printer] Mr. Crook assureth, who will also take care of keeping ym unexposed to ye eye of a Philosophical Robber." Thus, from its very beginning, the journal of science introduced the institutional device of quick publication to motivate men of science to replace the value set upon secrecy with the value placed upon the open disclosure of the knowledge they had created (a value that, in our own time, has often acquired, through the displacement of goals, a spurious emphasis on publication for its own sake, almost irrespective of the merit of what is published). The concern with getting into print fast is scarcely confined to contemporary science. . . .

Nevertheless, ours are changing times in the ethos of science. But Watson's brash memoir does not testify to a breakdown of once prevailing norms that call for discreet and softspoken comment on scientific contemporaries. A memoir such as his would have been regarded as a benign model of disciplined restraint by the turbulent scientific community of the seventeenth century. That it should have created the stir it did testifies that, with the institutionalization of science, the austere mores governing the public demeanor of scientists and the public evaluation of contemporaries have become more exacting rather than less. As a result, Watson's little book, so restrained in substance and so mild in tone by comparison with the caustic and sometimes venomous language of, say, Galileo or Newton, violates the sentiments of the many oriented to these more exacting mores.

Within such a context, the behavior of scientists involved in races for priority or in the increasingly rare disputes over priority tends to be condemned, rather than analyzed. It is morally judged, not systematically investigated. The disputes are described as "unfortunate" with the moral judgment being substituted for the effort to understand what they imply for the psychology of scientists and the sociology of science as an institution. At least since Goethe, we note references to "all those foolish quarrels about earlier and later discovery, plagiary, and quasi-purloinings." We are free, of course, to find this behavior unfortunate or foolish or comic or sad. But these affective responses to the behavior of our ancestors-in-science or our brothers-in-science have usurped the place that might be given to analysis of this behavior and its implications for the ways in which science develops. It is as though the physician were to respond only evaluatively to illness, describe it as unfortunate or painful and consider his job done, or as though the psychiatrist were to describe the behavior of schizophrenics as absurd and to substitute this sentiment for the effort to discover what brings that behavior about. The undisciplined tendency to respond in terms of sentiments has generated resistance to recognizing the central role of competition throughout the modern era of science.

This resistance is expressed in various ways: by seeking to trivialize the fact, by regarding the concern with priority as rare or aberrant (when it is in truth frequent and typical), by motivated misperceptions of the facts or by a hiatus in recall and reporting. Such resistance often leads to those wish-fulfilling beliefs, false memories and mythologies that we describe as illusions. And of such expressions of resistance the annals of science are uncommonly full. So much so, that I have arrived at a rule of thumb that seems to work fairly well. The rule is this: whenever the biography or autobiography of a scientist announces that he had little or no concern with priority, there is a reasonably good chance that, not many pages later in the book, we shall find him deeply embroiled in one or another episode where priority is at issue. . . .

Not only the historians and biographers of science but scientists themselves often manifest ambivalence about the facts of priority-oriented behavior. Even while he was assembling documents to prove his priority, for example, Darwin registers his mixed feelings, writing Lyell: "My good friend, forgive me. This is a trumpery letter, influenced by trumpery feelings." In a postscript, he assures Lyell that "I will never trouble you or Hooker on the subject again." The next day, he writes Lyell: "It seems hard on me that I should lose my priority of many years' standing." Then, a few days later, he writes again to say: "Do not waste much time [on this matter]. It is miserable in me to care at all about priority." Moreover, we need not have waited for the Watson memoir to be reminded that different styles of scientific investigation are variously bound up with different roles in achieving priority. Fifty years after the joint Darwin-Wallace paper was presented to the Linnean Society, Wallace was still insisting upon the contrast between his own hurried work, written within a week after the great idea came to him, and Darwin's work, based on twenty years of collecting evidence. "*I* was then (as often since) the 'young man in a hurry,'" said the reminiscing Wallace, "*he*, the painstaking and patient student seeking ever the full demonstration of the truth he had discovered, rather than to achieve immediate personal fame."

Freud recognizes his own ambivalence when he writes of his work on the Moses of Michelangelo that, having come upon a little book published in 1863 by an Englishman, Watkiss Lloyd, he read it

> with mixed feelings. I once more had occasion to experience in myself what unworthy and puerile motives enter into our thoughts and acts even in a serious cause. [Take note of the language: "unworthy and puerile motives," for we shall be returning to its implications before long.] My first feeling was of regret that the author should have anticipated so much of my thought, which seemed precious to me because it was the result of my own efforts; and it was only in the second instance that I was able to get pleasure from its unexpected confirmation of my opinion. Our views, however, diverge on one very important point.

This degree of self-awareness is a far cry from the ambivalence of a Descartes who manages to write that "he does not boast of being the first discoverer" and then proceeds to insist on his priority over Pascal or to beg his friend Mersenne "to tell him [Hobbes] as little as possible about . . . my unpublished opinions, for if I'm not greatly mistaken, he is a man who is seeking to acquire a reputation at my expense and through shady practices."

All of this brings us finally to the question touched off by the responses of many scientists and laymen to the Watson memoir. We are perhaps ready to see now that those responses relate to the long-standing denial that through the centuries, scientists, and often the greatest among them, have been concerned with achieving and safeguarding their priority. The question is, of course: what leads to this uneasiness about acknowledging the drive for priority in science? Why the curious notion that a thirst for significant originality and for having that originality accredited by competent colleagues is depraved—somewhat like a thirst for, say, bourbon and 7-Up? Or, in Freud's self-deprecatory words, that it is an "unworthy and puerile" motive for doing science?

In one aspect, the embarrassed attitude of a Darwin or Freud toward his own interest in priority is based upon the implicit assumption that behavior is actuated by a single motive, which can then be appraised as good or bad, as noble or ignoble. It is assumed that the truly dedicated scientist must be moved only by the concern with advancing knowledge. As a result, deep interest in having his priority recognized is seen as marring his nobility of purpose as a man of science (although it might be remembered that "noble" once meant the widely-known). The assumption of a single motive is of course unsound, as no one knew better than Freud himself. Scientific inquiry, like human action generally, stems from a variety and amalgam of motives in which the passion for creating new knowledge is supported by the passion for recognition by peers and the derivative competition for place.

There is, nevertheless, a germ of psychological truth in the suspicion enveloping the drive for recognition in science. Any extrinsic reward—fame, money, position—is morally ambiguous and potentially subversive of culturally esteemed values. For as rewards are meted out, they can displace the original motive: concern with recognition can displace concern with advancing knowledge. An excess of incentives can produce distracting conflict. But when the institution of science works effectively (and, like other social institutions, it does not always do so), recognition and esteem accrue to those scientists who have best fulfilled their roles, to those who have made the fundamental contributions to the common stock of knowledge. Then are found those happy circumstances in which moral obligation and self-interest coincide and fuse. The ambivalence of scientists toward their own interest in having their priority recognized—an ambivalence we have seen registered even by that most astute of psychologists, Freud—shows them to assume that such an ancillary motive somehow tarnishes the purity of their interest in scientific inquiry. Yet it need not be that scientists seek only to win the applause of their peers but, rather, that they are comforted, reassured and gratified by it, when it does ring out. In the rare instance, they may even catch a glimpse of their own immortality.

In another aspect, the ambivalence toward priority means that scientists reflect in themselves the ambivalence built into the social institution of science itself. On one side, the institutional norms of science exert pressure upon scientists to assert their claims, and this goes far toward explaining the seeming paradox that even those meek and unaggressive men, such as Henry Cavendish and James Watt in the Water Controversy, ordinarily slow to press their claims in other spheres, will often do so in their scientific life. The ways in which the norms of science help produce this result seem clear enough. On every side, the scientist is reminded that it is his role to advance knowledge and his happiest fulfillment of that role, to advance knowledge greatly. This is only to say, of course, that in the institution of science originality is at a premium. For it is through original contributions, in greater or smaller increments, that knowledge advances. Having acquired this sentiment from the institution of science, scientists find it difficult to give up a claim to a new idea or a new finding that in effect testifies to others and to themselves that they have lived up to their commitment. Yet the same institution of science emphasizes selfless dedication to the advancement of knowledge. Concern with achieving priority and ambivalence toward that concern register in the individual scientist what is generated by the complex value system of science.

In still another aspect, ambivalence toward concern with priority derives from the mistaken belief that it must express naked self-interest, that it is altogether self-serving. On the surface, the hunger for recognition appears as mere personal vanity, generated from within and craving satisfaction from without. But when we reach deeper into the institutional complex that gives added edge to that hunger, it turns out to be anything but personal, repeated as it is with slight variation by one scientist after another. Vanity, so-called, is then seen as the outer face of the inner need for assurance that one's work really matters, that one has measured up to the hard standards maintained by at least some members of the community of scientists. Sometimes, of course, the desire for recognition is stepped up until it gets out of hand. It becomes a driving lust for acclaim; megalomania replaces the comfort of reassurance. But the extreme case need not be taken for the modal one. In providing apt recognition for accomplishment, the institution of science serves several functions, both for men of science and for maintenance of the institution itself.

The community of science thus provides for the social validation of scientific work. In this respect, it amplifies that famous opening line of Aristotle's *Metaphysics:* "All men by nature desire to know." Perhaps, but men of science by culture desire to know that what they know is really so. The organization of science operates as a system of institutionalized vigilance, involving competitive cooperation. It affords both commitment and reward for finding where others have erred or have stopped before tracking down the implications of their results or have passed over in their work what is there to be seen by the fresh eye of another. In such a system, scientists are at the ready to pick apart and appraise each new claim to knowledge. This unending exchange of critical judgment, of praise and punishment, is developed in science to a degree that makes the monitoring of children's behavior by their parents seem little more than child's play. Only after the originality and consequence of his work have been attested by significant others can the scientist feel reasonably confident about it. Deeply felt praise for work well done, moreover, exalts donor and recipient alike; it joins them both in symbolizing the common enterprise. That, in part, expresses the character of competitive cooperation in science.

The function of reassurance by recognition has a dependable basis in the social aspects of knowledge. Few scientists have great certainty about the worth of their work. . . . But authentic reassurance can be provided only by the scientists whose judgment one in turn respects. As we sociologists like to put it, we each have our reference groups and individuals, whose opinions of our performance matter. Our peers and superiors in the hierarchy of accomplishment become the significant judges for us. Darwin writing Huxley about the *Origin of Species* "with awful misgivings" thought that "perhaps I had deluded myself like so many have done, and I then fixed in my mind three judges, on whose decision I determined mentally to abide. The judges were Lyell, Hooker, and yourself." In this, Darwin was replicating the behavior of many another scientist, both before and after him. The astronomer John Flamsteed, before his vendetta with Newton, wrote that "I study not for present applause. Mr. Newton's approbation is more to me than the cry of all the ignorant in the world." In almost the same language, Schrödinger writes Einstein that "your approval and Planck's mean more to me than that of half the world." And a Leo Szilard or a Max Delbrück, widely known as exceedingly tough-minded and demanding judges who, all uncompromising, will not relax their standards of judgment even to provide momentary comfort to their associates are reference figures whose plaudits for work accomplished have a multiplier effect, influencing in turn the judgments of many another scientist.

Other strategic facts show the inadequacy of treating an interest in recognition of scientific work as merely an expression of egotism. Very often, the discoverers themselves take no part in arguing their claims to the priority or significance of their contributions. Instead, their friends or other more detached scientists see the assignment of priority as a moral issue not to be scanted. For them, the assigning of all credit due is a functional requirement for the institution of science itself. After all, to protect the priority of another is only to act in accord with the norm, which has been gathering force since the time of Francis Bacon, that requires scientists to acknowledge their indebtedness to the antecedent work of others. As Kapitza says of his master, "If anybody in publishing his work forgot to mention that the given idea was not his own, Rutherford immediately objected. He saw to it in every possible way that . . . true priority be maintained." Or, to take perhaps the most momentous instance in our day, there is Niels Bohr, agitated by the thought that Meitner and Frisch, and for that matter, Hahn and Strassmann too, might have their priority in the splitting of the atom lost to view in the avalanche of publicity given the Columbia University experiments, going to immense pains to set the record straight.

Now these bystanders stand to gain little or nothing from advancing the claims of their candidates, except in the Pickwickian sense of having identified themselves with them. Their behavior can scarcely be explained by egotism. Their own status is not being threatened. Instead, their disinterested moral indignation is a signpost announcing the violation of a moral norm in the institution of science. In this sense, the concern with priority, with all the passionate feelings it often evokes, is not merely an expression of self-interest or hot tempers, although these may raise the temperature of the controversy. Rather, they constitute responses to the institutional norms of intellectual property, norms that transcend the personality-needs of this or that scientist.

From still another perspective we can see the fallacy of the new mythology that construes the thirst for priority as altogether self-serving. Often the drive for recognized originality is only the other side of the coin of the elation that comes from having arrived at a new and true scientific idea or result. The deeper the commitment to the discovery, the greater, presumably, the reaction to the threat of having its originality denied. Concern with priority is often the counterpart to elation in discovery—the eureka syndrome. We have only to remember what is perhaps the most ecstatic expression of joy in discovery found in the annals of science: here, in abbreviation, is Kepler on his discovery of the third planetary law:

> When I prophesied 22 years ago as soon as I found the heavenly orbits were of the same number as the five (regular) solids, what I fully believed long before I had seen Ptolemy's Harmonics, what I promised my friends in the name of this book, which I christened before I was 16 years old, what I urged as an end to be sought, that for which I joined Tycho Brahe, for which I settled in Prague, for which I spent most of my life at astronomical calculations—at last I have brought to light and seen to be true beyond my fondest hopes. It is not 18 months since I saw the first ray of light, three months since the unclouded sun-glorious sight burst upon me! . . . The book is written, the die is cast. Let it be read now or by posterity, I care not which. It may well wait a century for a reader, as God has waited 6000 years for an observer.

We can only surmise how deep would have been Kepler's anguish had another claimed that he had long before come upon the third law, just as we know how the young Bolyai, despairing to learn that Gauss had anticipated him in part of his non-Euclidean geometry and with the further blow, years later, of coming upon Lobachevsky's parallel work, suffered a great fall from the peak of exhilaration to the slough of despond and never again published any work in mathematics. The joy in discovery expressed by the young Jim Watson does not outstrip that of a Gay-Lussac, seizing upon the person nearest him for a victory waltz so that he could "express his ectasy on the occasion of a new discovery by the poetry of motion." Or, to come closer home, William James, "all aflame" with his idea of pragmatism, confessing that he is unable to contain his exhilaration over it. Or, in more restrained exuberance, Joseph Henry, once he had hit upon a new way of constructing electromagnets, reporting that "when this conception came into my brain, I was so pleased with it that I could not help rising to my feet and giving it my hearty approbation." There is ample precedent for the soaring spirits, the "pitch of excitement," the "delight and amazement" experienced by Watson and Crick in their successful quest for "the secret of life."

In short, when a scientist has made a discovery that matters, he is as happy as a scientist can be. But the height of exultation may only deepen the plunge into despair should the discovery be taken from him. If the loss is occasioned by finding that it was, in truth, not a first but a later independent discovery, that he had lost the race, the blow may be severe enough, although mitigated by the sad consolation that at least the discovery had been confirmed by another. But this is nothing, of course, when compared with the traumatizing experience of having it suggested that not only was the discovery later than another of like kind but that it was really borrowed. The drive for priority is in part an effort to reassure oneself of a capacity for original thought. Thus, rather than being mutually exclusive, as the new mythology of science would have it, joy in discovery and the quest for recognition by scientific peers are stamped out of the same psychological coin. In their conjoint ways, they both express a basic commitment to the value of advancing knowledge.

The Competitive World of the Pure Scientist

Fred Reif

The "pure scientist" is likely to be pictured as a person who devotes himself to the study of natural phenomena without regard to their possible practical or technological applications. Motivated by intellectual curiosity and immersed in his abstract work, he tends to be oblivious of the more mundane concerns of ordinary men. Although a few older scientists have become active in public affairs in recent years, the large majority who remain at work in their university laboratories lead peaceful lives, aloof from the competitive business practices or political manipulations of the outside world.

STEREOTYPE VERSUS REALITY

There is some truth in this stereotyped portrait. But if a young student took its apparent serenity too seriously, he would be forced to revise his perspective very early in his scientific career. The work situation of the scientist is not just a quiet haven for scholarly activity, ideally suited to those of introverted temperament. The pure scientist, like the businessman or lawyer, works in a social setting, and like them, he is subject to appreciable social and competitive pressures. The institutional framework within which he functions is distinctive; it is basically the university system. Furthermore, his competition does not resolve primarily around money; there is no very direct relationship between the quality of the scientist's professional performance and the economic rewards he receives. But competition need not be confined to the acquisition of wealth or political power. It is, therefore, of particular interest to discover how intense competition can become in an area as remote as pure science. In recent years rapid expansion has occurred in many branches of science. More scientists are active in many fields, more laboratories (including some in industry and government) engage in pure research activities, and more

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dollars are spent on such research. While this expansion has given the scientist a more prominent social role, it has also intensified the competitive pressures under which he works.

A few examples will illustrate how such competition can manifest itself. I shall take these illustrations from the field of physics, because physics is a well-developed pure science and because this is the field with which I am most familiar. In this country research work in physics has traditionally been published in a bimonthly journal called the *Physical Review*. In addition to full-length research reports, this journal used to publish "Letters to the editor," short notes whereby scientists could briefly communicate important new developments. The time elapsed between submission of a manuscript and its appearance in print was approximately 5 months for a regular paper and 2 or 3 months for a "letter." But in a period of rapid growth and development the pressure to publish fast and to establish priority claims became sufficiently great to make the *Physical Review* appear an inordinately slow medium of communication. Three years ago, therefore, its editors decided to eliminate the "Letters" section and to found a separate bimonthly journal, the *Physical Review Letters*, devoted entirely to the fastest possible publication of short notes on important discoveries. The time between submission of a manuscript and its appearance in print has been reduced to as little as 4 weeks! Not only is the existence of such a journal a significant phenomenon in itself; it has also necessitated the formulation of new editorial policies. As a result, although editorials in scientific periodicals are ordinarily very rare, some illuminating examples have found their way into issues of the *Physical Review Letters*.

In one of these (*1*) the editor comments that a large number of manuscripts are submitted whose importance and meagre content are not adequate to justify publication in the *Letters*. He goes on to say: "When a 'hot' subject breaks there is a deluge of follow-up contributions. . . . With the rapid exploitation of new ideas, priority questions become serious problems. Possibly important technical applications often lurk in the background. . . ." After explaining that he feels compelled to reject as unworthy of publication more than 40 percent of the manuscripts received, he concludes: "We do not take kindly to attempts to pressure us into accepting letters by misrepresentation, gamesmanship, and jungle tactics, which we have experienced to some (fortunately small) extent."

From the foregoing comments it is apparent that scientists seem most eager to see their work appear in print as soon as practicable. But to achieve that purpose, even the *Letters* can appear unduly slow. Certainly, the daily press is even faster; and though it may be less suitable for erudite publication, it is more effective for publicity and no less effective for establishing priority. Consequently, there have been several instances in recent years when important discoveries in physics were first announced in the New York *Times*. This procedure is not, by traditional values of the scientific community, considered to be very ethical. Nor is it, as the *Letters* editor points out in another editorial, an activity to be confused with the well-developed public information and publicity activities carried out by his own office and by such agencies as the American Institute of Physics. The editor expresses himself quite forcefully (2): "As a matter of courtesy to fellow physicists, it is customary for authors to see to it that releases to the public do not occur before the article appears in the scientific journal. Scientific discoveries are not the proper subject for newspaper scoops, and all media of mass communication should have equal opportunity for simultaneous access to the information. In the future, we may reject papers whose main content has been published previously in the daily press."

In the passages quoted, the editor of the official journal of American physicists makes some revealing comments about the behavior of his fellow scientists. What are some of the factors responsible for such behavior? Why should there be this exorbitant desire to publish and to do so ahead of others? The following discussion will focus attention on some of these questions in an attempt to clarify the conditions of modern science which contribute to this behavior. We shall first examine the great importance of prestige to the scientist. It will become apparent that the scientist carries out his work in a setting where he is extraordinarily dependent on the good opinion of others, and where his reputation becomes translated into many concrete consequences for him. Personal recognition thus assumes even more importance for the scientist than for most other people, and he competes persistently to achieve maximum prestige. I shall illustrate how this competition takes place and how it affects the manner in which scientific research is carried on. Finally, we shall ask how the existence of such competition serves to advance or impede scientific activity. This question will reveal the existence of some conflicts between these competitive pressures and scientific work proper. Throughout this discussion it should be borne in mind that the situation is not static and that the rapid expansion of science has made many of these problems more conspicuous than they were a few years ago.

PRESTIGE AND SUCCESS

The scientist is not different from others in his desire to be successful, but his definition of "success" has some distinctive features. The work of the pure scientist is abstract; it consists essentially only in gathering new data and formulating new concepts. To constitute scientific knowledge, these must be verifiable by other scientists and usable by them as the basis for further exploration. Thus, the very nature of scientific activity implies the need for recognition of the value of one's work by others in the field. Furthermore, success in such activities is not readily measurable in quantitative terms recognized by all. It does not revolve around tangible things such as amount of money earned or number of factories owned. Only other scientists in his field can understand the scientist's work and judge its merits. Indeed, throughout his life the scientist is dependent on the good opinion of significant other scientists for practically everything he does or hopes to attain. A review of the scientist's professional career will illustrate the truth of this statement.

While still in high school, the scientist-to-be becomes aware that competition and prestige will affect his future success. He must strive for good grades in order to be admitted to college and later to graduate school. He realizes the importance of attending a college of high reputation, not only because it will provide him with a better education but also because it will facilitate his later admission to a good graduate school. Finally, he must earn the good opinion of his teachers to secure the letters of recommendation which will help him enter college and gain scholarship grants or prizes.

After the student obtains his Ph.D. degree, his dependence on the good opinion of others is by no means ended. His first task is to find a suitable position. Characteristically, jobs in the better universities or in top industrial research laboratories are practically never advertised but are handled by personal communication between well-established scientists, who inquire informally whether their colleagues happen to know of some candidates for a given position or have an opening in their organization for a particular candidate. The job-seeking scientist is clearly in a more advantageous situation if he comes from a well-known institution and has been associated with a scientist of reputation. Invariably it is essential to him that there should be prominent scientists in the world who are willing to comment favorably upon the quality of his work. In most cases, before an appointment is decided upon, the hiring institution formally requests letters of recommendation concerning the candidate from several such prominent scientists. It is thus very important for the scientist to create, either through personal contact or through published work, a favorable impression among as many key scientists as possible.

Professional mobility of the scientist depends, therefore, in an essential way on the reputation he has acquired among prominent people in his field. This is true when he is securing his first job and true in his subsequent moves from one position to another. (In this connection it may be remarked that to move from an institution of high prestige to one of lower prestige is significantly easier than to move in the reverse direction.) Promotion to higher academic rank is subject to similar criteria. Again the university requests letters of recommendation from outside scientists and in some cases may appoint reviewing committees before deciding to promote someone to a tenure position. Even when the scientist has obtained a full professorship he has not reached the end of possible advancement based on his reputation. Within the academic hierarchy there are still some "name" professorships, or ultimately some administrative posts such as dean or university president. In these days of increasing importance of science in world affairs there are also potential opportunities in government—for example, advisory positions to the President or appointments to some such agency as the Atomic Energy Commission. Industrial organizations, as well, may offer key positions, such as the directorship of a research laboratory. Needless to say, the academic promotions which the scientist achieves carry with them increased financial rewards and, at the higher ranks, the security of a permanent position.

To carry on his work, the scientist needs money and adequate research facilities. Since World War II the financial expenditures required to perform the increasingly complex research of modern science have become so great that universities can provide only a very small fraction of the necessary funds. The remainder must come from outside sources—some of them private foundations but by far the greatest number government agencies such as the National Science Foundation, the Atomic Energy Commission, or the Office of Naval Research. On what basis do all these groups award their available funds to individual investigators? The usual procedure is to send the research proposal of the investigator to some prominent scientists for review. These scientists then make appropriate recommendations based on their evaluation of the specific proposal and their opinion of the merits of the scientist submitting it. The scientist today is thus increasingly dependent upon the reputation he has established among his colleagues to obtain the very means necessary for carrying out his work: funds for buying equipment and supplies and for paying the salaries of the personnel in his research group. In addition, the scientist's prestige helps him attract good and numerous students and postdoctoral fellows who can be of significant assistance in furthering his research program.

At times the scientist may be interested in obtaining a fellowship or grant—for example, a Guggenheim or National Science Foundation senior postdoctoral fellowship. Grants of this nature permit him to travel abroad for a year; or spend some time at a different university, where he can learn new techniques; or gain temporary relief from teaching duties to devote himself full time to his research. In applying for such a fellowship, the scientist will again be judged by some select prominent scientists, and once more his reputation among these scientists determines whether the award will be made to him.

The prestige acquired by the scientist very directly influences the likelihood of his nomination by fellow scientists for special honors or distinctions. Examples are the award of a Nobel prize or selection to membership in the National Academy of Sciences. Selection to serve as an officer of the national scientific organization is another recognition of distinction. The scientist's prestige may also lead to special invitations to attend scientific conferences as guest speaker or to join another university as visiting professor; finally, it may result in offers of remunerative consultantships in industry.

I think it is worth while, before leaving this discussion of the prestige system, to remark on a few of its peculiarities. One of these is the "positive feedback" involved—the fact that the possession of prestige tends to facilitate the acquisition of further prestige. For example, a person of prestige is likely to be affiliated with one of the better-known institutions, likely to obtain more funds to do effective research, and likely to attract better students—all of which circumstances, of course, tend to enhance his prestige even further. There is a similar relation between the prestige of individuals and the prestige of institutions. Institutions of good reputation can attract individuals of distinction whose presence, in turn, lends increased prestige to the institution.

Another feature of interest concerns the people who set the standards against which the individual scientist appraises himself and whose opinion determines his general reputation in the field. It is mainly the well-established scientists in the major universities of the world who set these standards. Since the institution with which the individual scientist is affiliated tends to evaluate him chiefly on the basis of his reputation, it becomes of greater concern to the individual to seek the good opinion of people on the national or international scene than to strive for accomplishments which attract only local attention. The scientist thus tends to have stronger loyalty to his field than to the specific institution of which he is a member. This is particularly true in the present days of expansion, when there is great mobility between different positions. The trend, in the major universities of this country, to minimize the importance attached to the teaching functions of the faculty reflects the situation. Teaching undergraduates is a local activity which may be appreciated by the students but does not serve to enhance the scientist's international prestige, on the basis of which the university will decide whether he is worthy of promotion. "Research and the training of graduate students are valued highly by the faculty; teaching, by contrast, is second-class. . . . It is a more usual, and probably a more realistic, view that time taken for teaching is time stolen from research, and that the road to academic heaven is paved with publications" (3).

The growing importance of science has also led to a proliferation of industrial research laboratories. The oldest and most distinguished of these are active in pure research and are staffed by some very competent persons who might readily have joined a university had opportunities in industry not been available. These people are eager not to be considered inferior by the rest of the scientific community, despite their industrial affiliation. Hence, they adopt for themselves standards very similar to those prevalent in the universities and compete within the same prestige system. This also preserves their mobility and leaves open the road back into some university position. Since the pure scientist's reputation, irrespective of the particular institution to which he belongs, is determined by the same reference group of prominent scientists, there exists a common prestige system which cuts across purely organizational lines. Thus, more prestige may be attached to a good position at a major university than to one in an industrial laboratory, but a position in a top industrial or government laboratory carries more prestige than one in a smaller university.

PUBLISHING "FUSTEST AND MOSTEST"

Because the social context within which the scientist receives his training and does his research is one where the possession of prestige is highly rewarded, competition among scientists is largely directed toward the acquisition of prestige. The particular forms assumed by this competition are determined by the nature of the scientific discipline and the character of the institution where the scientist carries out his work. A scientist strives to do research which he considers important. But intrinsic satisfaction and interest are not his only reasons. This becomes apparent when one observes what happens if the scientist discovers that someone else has just published a conclusion which he was about to reach as a result of his own research. Almost invariably he feels upset by this occurrence, although the intrinsic interest of his work has certainly not been affected. The scientist wants his work to be not only interesting to himself but also important to others. He wants it to attract the maximum attention from other people, and in this quest priority is a crucial factor. An important discovery becomes intimately associated with the name of the scientist responsible for it. If somebody else makes this same discovery at about the same time, several names become attached to it and the contribution to his own prestige is correspondingly diluted. The chances of receiving a Nobel prize or a promotion are similarly decreased. Finally, if someone else succeeds in making this discovery a few months or weeks before he does, almost all of the scientist's efforts on the problem have come to naught. He may not even be able to publish his own results, since they may then represent only uninteresting duplication of work already in the scientific literature. Under the circumstances, it is not surprising if the scientist sometimes works at feverish speed under constant fear that he may be "scooped." Even a couple of weeks' delay can sometimes make a difference!

Being the first to make an important scientific contribution is, of course, only one way of obtaining recognition. For a scientist to be on the verge of making some discovery of far-reaching implications is relatively rare. Most of the time he is engaged in the less spectacular task of doing useful work leading gradually to increased knowledge. In this situation the most effective way to attract the continuing attention of other scientists is to publish as many papers as possible, to attend numerous scientific meetings, and to give many talks on one's research. The great emphasis on publishing copiously is exemplified by a motto familiar to all young faculty members—"publish or perish" —a phrase that well illustrates how the young scientist feels about the competitive pressures to which he is subject. Under the "up-or-out" rule, common in large universities, instructors and assistant professors are allowed only a fixed maximum number of years within their academic rank. If they are not promoted before the end of this time, their dismissal from the university is automatic. Whether or not an individual is promoted depends, of course, on the reputation he has achieved as a result of his publications.

Some of these competitive pressures have been familiar features of academic life for a long time. The expansion of scientific activity since World War II, has, however, significantly changed the conditions under which the scientist does his work. One consequence has been the emergence of new and intensified patterns of competition as the number of scientists at work in many areas has multiplied. Not only are more universities engaged in active research; more industry and government laboratories are also carrying out pure research of a type nearly indistinguishable from its academic counterpart. Many people in different institutions are thus likely to be working along fairly similar lines. Furthermore, the time lag between advances in basic science and the associated technological developments has become increasingly small. Sometimes new ideas or techniques arising in the work of the pure scientist may be such as to warrant patenting without further exploration. Even when potential technological applications are not immediately apparent, there are well-equipped industrial laboratories constantly poised to exploit all possible consequences of a basic advance. In addition, research has become an activity which involves the expenditure of large sums of money and which has come to attract attention even from the general public. Under these circumstances it is easy to understand why the scientist finds increasing difficulty in carrying out his work immune from outside pressures.

Rapid publication of results and questions of priority assume, therefore, great importance; nor is the need for a journal such as *Physical Review Letters* too surprising. No longer does a scientist study a topic at some length before publishing his findings in a paper or monograph. Instead, he tries to publish a note on a subject as soon as he obtains any result worth mentioning—and occasionally even before. The threat of someone else's getting there first is too great. At times a scientist may publish just a proposal for an experiment, merely pointing out that such an experiment might be interesting and feasible. To obtain preliminary experimental results before publishing anything may take too much time—time during which the scientist might "get scooped" by someone else. For similar reasons scientists may be led to engage in various practices which the editor of *Physical Review Letters* finds reason to discuss. In his words (*4*), there is the "author who uses the *Letters* merely to announce a later paper and whose Letter is incomprehensible by itself"; the "author who submits many Letters hoping that statistics rather than quality will cause one to be accepted"; or the "author who tries to sneak a Letter in to 'scoop' a competitor who has already submitted an Article."

The emergence of rapidly changing "fashionable areas" of scientific activity is still another consequence of the expansion of science. In a highly developed discipline such as physics, genuinely new ideas or unexpected breakthroughs are not really very common. When such a discovery does occur, many people are eager to drop more routine work in order to explore the potentially important consequences of the new development. Present conditions are also such as to permit a substantial number of scientists to shift their field of research quite rapidly. One reason is that the major university and industrial laboratories provide the flexibility of a large variety of experimental facilities and adequate manpower resources. Moreover, since work is often proceeding along similar lines in a number of different laboratories, scientists active in areas related to the discovery are in a particularly good position to turn their attention to an investigation of its consequences. Every new discovery, therefore, results in a burst of intense and very competitive activity. In physics there ensues a profusion of "Letters," until the editor decides that the subject has become sufficiently old to be routine. Since so many people concentrate their efforts in one area, the road from the novel to the routine is often traveled in a few months.

The preceding discussion illustrates the increasingly important role played in modern science by large-scale research organizations. This is true not only in industrial and government laboratories but also in the universities, where specialized research institutes have become quite common. Here the scientist is usually a member of some group organized around a particular project or a special research facility, such as a high-energy accelerator, and work is often done jointly by several people. An experiment was recently reported in a "Letter" by no less than 24 coauthors! Working under these conditions is appreciably different from the individualistic endeavors prevalent 10 or 20 years ago, and the scientist must compete in some novel ways. He must establish an individual reputation even though he works as a member of a larger group. He also has to compete in a setting which tends to be organized along hierarchical lines, where scientists in the top positions determine policy and the direction of research. Finally, many members of research institutes constitute a "secondary faculty" of research associates. They do not teach or belong to a department, nor do they have permanent positions. If they hope to gain the security of a tenure position they must strive for sufficient eminence to be appointed to regular academic rank.

CONFLICTING VALUES

After this description of the existing conditions in pure science, let us consider some of the consequences of competition in this area. This competition certainly affects the functioning of scientific research in several beneficial ways. The prestige system helps to maintain high standards of accomplishment which reflect the collective judgment of important scientists and are therefore fairly uniform throughout the world. Prestige accrues predominantly to those whose discoveries prove fruitful as a basis for further work by other scientists. Specific areas of activity in science thus become fashionable not just because they are novel and different but because they are likely to lead to scientific contributions of permanent value. Even when current fashion leads to duplication of work by different investigators, the resulting critical checking of results may occasionally help in avoiding mistakes and oversights. Competition under these conditions encourages continuing active exploration as well as rapid and thorough exploitation of all new discoveries. Research institutions have become well adapted to carry out these functions. Not only are they well equipped and staffed but they are capable of using their resources with considerable flexibility.

On the other hand, the competitive atmosphere has results which are less desirable. It subjects the individual scientist to appreciable strains, thus increasing further the demands made upon him by an already rigorous scientific discipline. But apart from such psychological effects, there are possible deleterious consequences affecting his research activity itself. These are usually the result of conflicts between the requirements of the scientific work proper and the pressures of competition. To the individual scientists they may appear as conflicts between the values inherent in science and more selfish personal values.

One such conflict is that of reflection versus production. The scientist may desire to take some time to think and speculate; he may want to get a fresh point of view by reading about developments outside his special field and to discover suggestive analogies worth pursuing; or he may be tempted to undertake an experiment sufficiently novel in character for him to be uncertain about its ultimate feasibility. Activities of this kind are potentially fruitful precisely because they focus attention upon lines of investigation off the beaten track. But, by the same token, they are also risky, since in many cases they may lead to no results at all. In order to make his reputation with a steady stream of publications, it is safer for the scientist to work along more conventional and familiar lines, where he has greater assurance of obtaining results. Young scientists are in a particularly vulnerable situation. Since they must establish their reputation in a relatively short period of time to achieve a permanent academic position, undertaking risky projects during this period is dangerous. Interesting in this connection are instances where a fundamental discovery is made by someone in a small laboratory in an out-of-the-way place. As soon as the result is published, many big laboratories employ their superior facilities to exploit the consequences of the discovery so effectively that the scientist originally responsible for it finds it difficult to compete with them. People in the big laboratories had available, of course, all the resources necessary to make the original discovery themselves, but they used them less imaginatively. Organizations well adapted to the exploitation of a field in which the direction of approach has become clear are not necessarily the best for stimulating exploration of the genuinely unknown.

A further conflict, which may lead to slipshod work when competitive pressures are pronounced, is that of careful versus fast work. Another *Letters* editorial describes the dilemma succinctly (*5*). "One of our most ticklish problems concerns the large number of contributions that pour into our office when a 'hot' subject breaks and many groups initiate related work. . . . Because of the rapid development, and the intense competition, we have found it necessary to relax our standards and accept some papers that present new ideas without full analysis, relatively crude experiments that indicate how one can obtain valuable results by more careful and complete work, etc.—in short, papers which under less hot conditions would be returned to authors with the recommendation that further work be done before publication. . . . Such incomplete papers have been accepted reluctantly since we realize that thereby we penalize some physicists who, working along the same lines, want to do a more complete job before publishing."

Another conflict is that of communication versus secrecy. It is intrinsic in scientific activity that knowledge and ideas are common property, to be shared and used by all scientists. But if scientist *A* has an interesting idea and describes it to scientist *B*, the latter may exploit it before scientist *A* himself can do so. It may then be better for *A* not to disclose his ideas before they are published and before his claim to priority is safely established. Closely related to this conflict is that of cooperation versus rivalry. Should scientist *A* tell scientist *B* about some new technique he has developed if *B* may use it in his own work to compete more effectively against *A*? Lack of full communication can, of course, slow down scientific progress. A significant amount of energy is diverted from struggling with the subject matter of science to fighting other people in the field.

There exist other conflicts, such as that between research and teaching. But instead of elaborating further, I might better give a specific example illustrating how the pursuit of a purely scientific problem can give rise to the competitive pressures described. A few years ago Mössbauer, a young German physicist, discovered that the radiation emitted by certain atomic nuclei in solids is characterized by an exceedingly well defined frequency. This observation suggested to several people, in particular to two scientists, *X* and *Y* (*6*), that such nuclei might be used as extremely accurate clocks well suited for checking a consequence of Einstein's general theory of relativity. This theory predicts that the rates of two identical clocks should be minutely different if they are located at different heights in a gravitational field. Both *X* and *Y* undertook to check this prediction experimentally. Scientist *X*, however, first published a "Letter" outlining his proposal for the experiment, long before he was ready to obtain actual data. A few weeks later, again before either *X* or *Y* had published any preliminary results in the scientific literature, the front page of the New York *Times* carried a picture of scientist *X*, together with an article describing the experiment he was undertaking. When *X* discussed his experiment at a scientific meeting 6 weeks later he reported reluctantly that, despite hard work at great speed, he had not yet been able to reach any conclusions. At the same meeting *Y* announced that he had successfully carried out the experiment and obtained results in agreement with the theory; shortly thereafter *Y* published his findings. It was not until some 2 months later that *X*, in a "Letter," was able to report his own experiment, which also confirmed the theoretical expectation. He pointed out, however, the necessity of controlling the temperature of the experiment quite carefully to avoid introducing large extraneous effects; indeed, since *Y* had not taken such precautions, his findings lacked significance. In this instance an important experiment was performed in a short time and ultimately in a reliable way. But the example shows vividly the actual circumstances under which the experiment was carried out—the announcement of an experiment before it was undertaken, the newspaper publicity, the hurried activity of two scientists working under pressure to be the first to publish—and the lack of sufficiently careful work which may result from these conditions.

While much more could be said about the differing patterns of competition in various sciences and about the rapid changes taking place in many of these disciplines, my aim has not been to treat the topic exhaustively. It is sufficient if the perspectives of the outside observer have been broadened, to make him aware that the scientist is not just somebody concerned with new ideas and techniques, but that he carries out his work in a human, and sometimes all too human, context.

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Comparative Failure in Science

Barney G. Glaser

A perennial problem for some scientists is their *feeling* of *comparative failure* as scientists. This problem becomes clearer if we consider two major sources of this feeling that are inherent in the very nature of scientific work. (*i*) In science, strong emphasis is placed on the achievement of recognition (*1*); (*ii*) the typical basic scientist works in a community filled with "great men" who have made important and decisive discoveries in their respective fields; they are the acknowledged guiding lights. These esteemed scientists, who have attained honors beyond the reach of most of their colleagues, tend to become models for those who have been trained by them or who have worked under them. As Eiduson has put it in her recent psychological study of basic research scientists (2, p. 167): "Scientists are idols-oriented."

To take these honored men as models is important for training as well as for a life in research. During training, one learns to think creatively. Emulation of these models results in the internalization of values, beliefs, and norms of the highest standard. This emulation of the great continues and guides the scientist in his research work, however individual in style his work may be.

But it is precisely here that a feeling of comparative failure may arise. In emulating a great man the scientist tends to compare himself with the model. He estimates how closely he has equaled his model in ability to adhere to high standards of research, to think of relevant problems, to create "elegant" research designs, to devise new methods, to write clearly, to analyze data. In addition, because of the strong emphasis on attaining recognition for research contributions, the scientist perhaps will compare his own degree of success with his model's to gauge how he himself is doing. In using the great man's achievements and the recognition accorded him as criteria, the scientist may be motivated to strive continually and unremittingly toward greater heights (*3*). On the other hand, he may see himself, over time, as a comparative failure for not having attained a comparable amount of recognition (*4*).

Eiduson brings out the dynamics of this problem for scientists (2, p. 189): "The model, then, is the ego ideal figure, who represents the ultimate position, and in fact, defines what a scientist should do, how he should think, how he should act.. *By comparison, everything else is inevitably of lesser worth* [italics mine]. We have seen the way the scientists in this group rebuke themselves as they become old, distracted, sit on committees or government advisory boards, or become administrators—and thus move away from the ideal. From this picture it is obvious that the scientist is hard on himself. He has a built-in, clearly marked scalar system, along which attitudes and kinds of performances are measured. When he moves away and deviates from the pattern,

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he becomes a maverick, or a person who has tossed aside the flaming torch."

AVERAGE SUCCESS

With this problem in mind, I recently made a study of the organizational careers of basic research scientists, one purpose of which was to ascertain the consequences, for the scientist's career, of receiving or not receiving an average amount of recognition (*5*). At the time of the study, these scientists were employed in a government medical research organization devoted to basic research. This was a high-prestige organization from the standpoint of scientists and was run much as though it were a series of university departments. The study is relevant to this discussion in showing something of the career history of basic research scientists, who are today in increasing proportions leaving the university setting to become affiliated with high-prestige organizations devoted to basic research. In these contexts organizational scientific careers are still primarily dependent on professional (not organizational) recognition (*6*).

By "average amount of professional recognition" I mean supervisor's favorable evaluation of the quality of the scientist's current research, and proper credit, through publication and through acknowledgment in the publications of others, for his contribution to the cumulative knowledge in his field. This definition gives the three major sources of recognition within reach of the typical scientist: references from superordinate colleagues, publication, and publication acknowledgments in the work of others. This "average" degree of professional recognition is attained by most of the country's scientists at any one time and by practically all scientists at one time or another. This degree of recognition is in marked contrast to the highly regarded, and restricted, high-prestige honors (in the form of awards, prizes, grants, lectureships, professorships, and so on) that are part of the professional recognition accorded those scientists who make great and decisive discoveries—the "great men."

Three general aspects of scientists' careers were studied: performance; security in, and advancement of, position; compatibility with others, and satisfaction with one's location in science. With respect to performance, an average degree of recognition was found basic to high performance. That is, recognition maintained high motivation to advance knowledge, and high motivation resulted in the scientist's devoting more of his own time to research; this, in turn, resulted in high-quality scientific performance, as judged by the researcher's closest professional colleagues.

Since, of course, such performance on the part of many individuals is the basis of organizational prestige, it was not surprising to find the organization providing, in return, a stable scientific career for a scientist who received average professional recognition. The scientists accorded this degree of recognition, in contrast to those accorded less, felt more satisfaction in their jobs and salaries. They tended to be more optimistic about their chances of promotion, and their rate of promotion was higher. With respect to the conditions for research—a most important consideration for basic-research scientists—they fared considerably better than scientists not accorded average recognition. They had more freedom to work on their own ideas, had more chance for originality, had more chance to use their current abilities and knowledge as well as to gain new abilities and knowledge, and had generally better research facilities and supplies. In sum, the "average" recognition accorded them was sufficient to give them security and advancement in their scientific careers.

Lastly, with average recognition, the high-quality performance and steady advancement could be achieved in a setting that provided personal satisfactions. The scientists accorded average recognition, again in comparison to those accorded less, were more content with their research and non-research colleagues. More of them felt intense interest in working with close professional associates. They were more satisfied with their assistants and with the other scientists, the organization leaders, their own supervisors and the directors of their particular institutes. They felt strengthened through belonging to work groups, such as sections and laboratories. They depended more on personal contacts for scientific information, both inside and outside the organization. They participated more in seminars, meetings, and the activities of professional clubs and other small groups.

Closely linked with this compatibility with their associates was a satisfaction with their location in the community of organizations of science. The scientists accorded average recognition, in comparison to those accorded less, felt strongly attached to their respective institutes and organizations. Indeed, they were more satisfied with the organization's reputation in the scientific world, and more of them felt that a sense of belonging to an organization which had prestige in both the scientific and the general community was of utmost importance. In comparing their own organization (from the standpoint of what job factors they deemed most important) with the "best" of universities, hospitals, industrial research organizations, and government research organizations, more of them consistently reported that their organization was generally better. In sum, the context of their careers in science was highly favorable.

Together these findings suggest that an average amount of recognition has a generally stabilizing effect for the careers of the scientists within the high-prestige organization of the study. (Even for individuals who received little or no recognition, the pressure on careers was not so great as to cause an exodus from the organization or from science itself. The great majority of these men thought the lack of recognition was only temporary and planned to continue in the organization, trying to advance knowledge.)

These findings suggest that career stability based on average professional recognition is probably found in other organizations similar in nature to the basic-research organization of this study, and that in organizations of lesser standing even less recognition may assure career stability.

In the light of these findings it appears that the feeling of comparative failure that may result when the average scientist judges his lesser success by the considerable success of his "great man" model tends to occur in many instances within the context of a stable, promising career. Further, most scientists can gain, if they do not have it currently, the degree of recognition necessary for a stable career. Comparative failure, then, is an evaluation resulting from a social comparison. It is not to be taken as absolute failure (loss of position as a scientist). A comparative failure can still be successful; an absolute failure is through.

THE SCIENTIFIC CAREER: A CARNIVOROUS GOD?

Comparisons with great men are, however, taken not as comparative but as absolute failure by Kubie in his famous article "Some unsolved problems of the scientific career" (7). Kubie warns future scientists of the perils ahead when devoting themselves to that "carnivorous god, the scientific career." His criteria in warning of potential failure, are absolute (not comparative) judgments, based on the careers of the more notable great men of science. For example, he talks of the "ultimate gamble which the scientist takes when he stakes his all on professional achievement and *recognition* [italics mine], sacrificing to his scientific career recreation, family, and sometimes even instinctual needs, as well as the practical security of money." Implying again that the scientist whose success falls short of the great man's is an absolute failure, he characterizes the young scientist as having "a self deceiving fantasy: that a life of science well may be tough for everyone else, but that it will not be for him," and as having "ambitious dreams; unspoken hopes of making great scientific discoveries; dreams of solving the great riddles of the universe."

Kubie states that the young scientist "dreams unattainable dreams." More directly relating his judgments to great men, he cautions against choosing science as a career, because of the "many failures it took to make one Pasteur." He states that most young scientists, in using great men as models, unwittingly set themselves up to become failures: ". . . most young men view their prospect solely by identifying with their most successful chiefs, never stopping to consider how many must fail for each one who reaches this goal." Without making the distinction between absolute and comparative failure, this last statement clearly implies the former.

Admittedly, from this standpoint many must fail and few will attain the stature of their models, but this is hardly a reason for dissuading young men from becoming scientists. The chance is slight that they will equal or surpass their models, but they should be informed that most can gain the fundamental degree of recognition indicated in my study as necessary for a promising career in science. Surely the career to which they commit themselves need not be, as Kubie says, "devoid of security of any kind, whether financial or scientific."

Furthermore, these young men should be encouraged to enter science and take great men as their models, for most will be the artisans who do the commendable, but not earth-shaking, research which accumulates to form the foundation for future decisive advances. Kubie himself has recently, although somewhat ambivalently, recognized this, in comparing the typical scientist with the internationally famous scientist (*8*): "These little known and unrewarded men are the expendables of science. They are no less essential than are the few who reach their goals. Therefore, until many years had passed it would be hard to weigh which of these two men had had the more profound impact on scientific knowledge."

Perhaps my discussion draws the kind of "implication" from "statistics" that Kubie is looking for in future research when he says in his article on the scientific career: "It is the . . . duty of scientists and educators to gather such vital statistics on the life struggles of a few generations of scientists and would-be scientists and to make sure that every graduate student of the sciences will be exposed repeatedly to the implications such data may have for his own future." Career decisions are perhaps among the most important determinants of a man's fate, and anything which contributes to an understanding of the career in science may help people make these decisions more wisely.

REFERENCES AND NOTES

1. Merton accounts for this in the following manner: ". . . originality can be said to be a major institutional goal of modern science, at times, the paramount one, and recognition for originality a derived but often as heavily emphasized goal" [R. K. Merton, *Am. Sociol. Rev. 22*, 640 (1957)].
2. B. T. Eiduson, *Scientists: Their Psychological World* (Basic Books, New York, 1962).
3. See O. Klapp, *Heroes, Villains and Fools* (Prentice-Hall, Englewood, N.J., 1962), pp. 18–24 for some functions of role models. I have reference to the function of "providing the individual with self-images and corresponding motivation."
4. In their comprehensive statement on careers, Becker and Strauss note the relative nature of failure: "Of course, failure is a matter of perspective. Many positions represent failure to some but not to others" [H. S. Becker and A. Strauss, *Am. J. Sociol. 15*, 257 (1956)]. The relative nature of failure can be seen in marked contrast to its absolute nature when a person simply has failed to keep a position. On absolute failure, see E. Goffman, *Psychiatry* 62, 451 (1952).
5. B. G. Glaser, *Organizational Scientists: Their Professional Careers* (Bobbs-Merrill, Indianapolis, 1964).
6. C. V. Kidd, *Personnel Admin. 15*, No. 1, 16 (1952); W. Kornhauser, *Scientists in Industry* (Univ. of California Press, Berkeley, 1962), pp. 131–133.
7. L. S. Kubie, *Am. Scientist 41*, 596 (1953); *ibid. 42*, 104 (1954) [reprinted in M. R. Stein, A. J. Vidich, D. M. White, *Identity and Anxiety* (Free Press, New York, 1960) and in B. Barber and W. Hirsch, *The Sociology of Science* (Free Press, New York, 1962)]. The remarks by Kubie are based on 30 years' observation. He sees these observations as "random," but their consistently negative character suggests that, by and large, they are observations of his analysands and are random only in that context. My references are to but one short section of an excellent article.
8. ———, *Daedalus 91*, 304 (1962).

The Impact of Rapid Discovery Upon the Scientist's Career

Fred Reif and Anselm Strauss

One of the most striking features of present-day science is the rapidity of scientific discovery, verification, and technological exploitation. This rapid rate of change is intimately connected with other outstanding characteristics of modern science, its bigness, visibility, and increasing professionalization. The number of men engaged in research is so large and the organizational framework so efficient that new fields get explored and developed with great speed. . . .

This very organizational framework, while unquestionably furthering discovery, can however mitigate against the continuing creativeness of individual scientists. Men in many fields have complained about the restrictive aspects of working within the context of contemporary large-scale research organizations, or have murmured against the heavy administrative and consultative tasks into which creative scientists are frequently drawn. The most creative years of research seem increasingly to belong to the young men . . . so much so that a recent editorial in *Science* called for "revitalizing the mature scientists." Thus it appears that the scientist who wishes to keep contributing to the impressive rate of important discovery is operating against appreciable odds quite apart from his own biological slowdown.

Among the greatest of these odds, we shall suggest in this paper, are those stemming from the rapid rate of discovery itself—particularly in fields like physics, chemistry, and modern biology where recent activity and growth have been most pronounced. Rapid change may create situations of considerable strain for individual scientists. We shall discuss the areas where such strains are particularly manifest and consider their impact upon the scientists' career. Our discussion will deal mainly with the elite of science—those who make major fundamental discoveries—and will focus attention particularly upon those critical periods in the scientific career which are characterized by greatest personal strain and greatest hazard for scientific work itself. We shall then examine how scientists wishing to remain productive in research cope with the pressures to which they are subject, and how they adapt to their changing situations. Finally, we shall make some comments about the influence of rapid discovery and change upon the institution of modern science itself and about possible implications for its future.

THE EARLY RAPID PERIOD OF TRIAL

The admission to a graduate school is the first critical hurdle encountered by the prospective scientist. He begins early to enter upon a period of intense trial during which his future as a creative scientist, if he aspires to become one, is at stake. This period comes to an end only when, a few years after obtaining his Ph.D., it has become apparent to himself and others what his potentialities are likely to be and in what kind of working environment he is likely to spend his most productive years.

While a graduate student, he recognizes quite clearly that he has already embarked upon a period of trial during which he must prove that he has what it takes to become a good scientist, and to become accepted as such by people who, as yet, seem remote and formidable by virtue of their knowledge, experience, and reputation. . . .

Yet this process is not just one of growth and of the individual's "finding himself," for the pressures of time are severe and are felt to be such. The opportunities for upward mobility are quite high today, as organizations compete for the relatively small number of competent scientists. Thus bright students can, only a few years after obtaining their Ph.D., become sufficiently well known by their work to obtain faculty positions at major universities or to figure prominently in scientific conferences. . . .

. . . The ambitious young scientist is likely to perceive that achievement in science tends to occur at an early age and is concerned that he himself be productive before it is too late. . . .

Not only is the young scientist aware of the illustrious early achievers of the past; he

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encounters them very directly in the present. For example, it is only natural that his attention should be attracted by recent Nobel prize winners. During the past half dozen years, their mean ages at the time of the award have been between 45 and 50 years in physics, chemistry, and medicine. But the young scientist is likely to note that some men were in their late twenties when they did the outstanding work leading to subsequent awards of the prizes; indeed, their work was done at an average age of less than 35 years.

Thus the young scientist feels that he must prove himself rather quickly. He has also learned to appreciate the importance of prestige in scientific work and he knows that he must gain recognition early to have a successful scientific career. The prestige he can command will bring him a host of extrinsic rewards. It will determine the positions he will be offered; the monetary income he can expect; the funds he will obtain to support his research; the students he can attract; the fellowships or awards he might receive; and the power he is likely to wield in his field, university, or larger community. At the same time, his prestige in the field will also have great intrinsic importance to him because it will help to determine his own feelings toward himself, his sense of self-esteem and self-perception as a success or failure.

The task thus faced by the aspiring scientist is not an easy one, and the pressures felt by him are likely to be severe. His self-expectations are in large measure based on the role models which he has encountered. Since these models derive from outstanding scientists, his self-expectations can be quite demanding indeed. The young scientist also knows that achievement in pure science is characterized by the property that the best is very much better and more important than the second best. The best is also much more attention attracting; i.e., it contributes much more to one's prestige. Thus it is not really sufficient for the scientist to be merely competent. . . .

Quite naturally, the ambitious young investigator aspires to enter the group of elite scientists. But, even if he should set his sights lower, there exist powerful outside pressures which insist that he be creative . . . and quickly. In earlier times creative originality was appreciated as a quality possessed by a few gifted individuals and it was recognized and rewarded as such. But in the post-war era the growth of science and technology have led to a situation where the demand for scientific creativity has also become institutionalized in ideological form. Creativity is commonly *expected* by employing institutions, whether industries or universities, because they are concerned with their own technological productivity or because they themselves gain institutional reputations through the productivity and prestige of their employees. Hence the employees are very aware that their overt creativity gets regularly assessed and rated on the basis of published papers or other measurable output. . . .

Quite apart from ideological considerations, there are very concrete conditions, inherent in the very structure of organized science, which necessitate the quick demonstration of talent. The expansion of science has given rise to a genuine scarcity of high caliber people to fill jobs opening up in industrial establishments and universities. There exists, therefore, considerable competition between the purchasers of scientific talent. To satisfy their needs, these institutions must be prepared to risk hiring young men whose promise is barely established. Yet they must select wisely; hence they look to the first rate university departments as the most likely source of low-risk recruits. In consequence, the young scientist's future career possibilities are already appreciably determined on the basis of the evaluation of his *pre*-doctoral work. . . .

The young scientist runs thus a real risk of getting started on the wrong foot or of getting out of step with career demands. He may do his graduate work in a second rate department and thereby diminish his chances for top positions. He may, even in an excellent department, choose his field of research unwisely, such as one which is falling out of fashion. He may imitate role models whose examples can only harm his own trial performances; for example, he may imitate men who are very careful in their work and may thus be led to publish very slowly by virtue of being overly cautious. He may be "scooped" in his early work by the competing research of more established men. . . .

Those who do not emerge from the trial period with flying colors become the "journeymen of science." No denigration is implied by that term. A great deal of very competent and necessary work gets done by men who have failed to gain outstanding or brilliant reputations. In this respect, too, the graduate schools help to perform an important function: they help to channel these men into appropriate institutional settings. There some of them also become, at relatively early ages, scientific administrators who supervise the work of other highly competent journeymen and of less skilled scientific technicians.

THE YEARS OF MATURITY

Assuming that the scientist has succeeded in becoming recognized as having genuine promise, we now wish to examine the effects of the current rapid rate of discovery upon his continued productivity. The scientist entering this stage of career is likely to conceive that he has only a limited number of years to do his best work, since the most original research seems associated with youthful minds. His anticipation tends to be progressively aggravated by the need to keep up with the field; the task of staying in the forefront is correspondingly more difficult. The scientific literature being published has become overwhelming. New advances are made at a rate such that five years may see significantly new methods and techniques introduced into a field. Neighboring disciplines also tend increasingly to intrude themselves, particularly in the most active areas of research. For instance, experimental physicists in solid state physics have discovered that they must become familiar with some of the abstruse mathematical formalism of quantum field theory to understand the recent literature pertinent to their field. Thus the demand made on the individual scientist's adaptability is both great and continuous. . . .

Since the complexity and volume of scientific activity is increasing, there exists, of course, a natural tendency to become expert in some smaller subfield. But specialization, although effective for keeping up with developments in a particular area in which one can be productive, involves a significant danger. In an age when science is expanding rapidly and is pursued by an increasing number of investigators, the continual emergence of new discoveries leads to fashionable lines of research which attract much attention before they are replaced by still newer developments. Thus the specialist who works in a certain field for some years may discover one day that he is no longer in the mainstream of scientific activity. There may be no dearth of unanswered questions remaining in his field, but these have become of minor importance as the center of gravity of the larger science has moved elsewhere.

There arises then the question of whether the scientist will continue to explore his chosen area or shift his research into newer directions so as to participate in more glamorous current developments. This question raises several issues. The scientist may be unable to shift his activities readily because of his existing commitments; or because his knowledge and training, although ample in his own area of specialization, are inadequate to make readily possible the transition to new lines of investigation. Furthermore, new developments take place constantly and are generated by new groups of people. How often and to what extent can the same scientist shift his own field of activity accordingly? Or at what stage does he resign himself to pursue his own specialty—even if it has become more remote from the mainstream of his science—and become content to watch younger people pursue the new areas?

The rapid developments result in a situation where the scientist's relation to his work changes not only by virtue of his own individual growth and aging, but also because the field in which he is active changes rapidly under his very feet. It has become increasingly difficult for an individual investigator to remain identified with a particular line of work for a long time in a satisfying way. . . .

Let us briefly summarize the main points of the preceding discussion. Rapid changes in his science tend to make an individual's knowledge obsolete and his field of specialization outdated; these trends aggravate his personal aging and natural loss of intellectual vigor. The whole character of a particular field of science is also likely to change appreciably during a time which is appreciably shorter than an individual's normal career span; the field may thus become less congenial to his particular temperament and set of skills. Nevertheless, the demands for recognition-producing work on the part of the individual remain high. These demands are generated from within, as a result of acquired norms and internalized role models; and from without, as a result of the institutionalized expectation of creativity existing in present-day science. At the same time, the rapid expansion of scientific activity generates a market for people to fill new roles, either within the field proper, as supervisors or administrators, or in business or government. The cumulative effect of all these pressures tends ultimately to bring the scientist's productive research career to an end.

REMAINING A RESEARCH SCIENTIST

. . . It becomes of interest to examine the conditions under which he may continue to be productive, and the various ways in which he may cope with the pressures impinging upon him. In this context it will be helpful to differentiate between the situations facing technicians, journeymen, and elite.

Most persons trained in scientific fields are absorbed into the many positions created by the expansion of science and technology. They fill the demands of industrial and government laboratories for highly skilled technical personnel, and contribute importantly to technological advancement. For many, scientific research is a "job" not significantly different from other high level occupations, a way of earning a living which offers money, prestige, and satisfactory working conditions. Some entered their chosen field predominantly for such reasons. Others, having come to realize their own limitations of ability and motivation, have only later resigned themselves to regard their work as a job rather than a calling. They have thus relinquished the self-concept of the scientist as an original discoverer. . . .

For most technicians and technologists there is neither much opportunity nor disposition to abandon the kind of research in which they are active; nor are they particularly sensitive to the types of pressures depicted earlier. . . .

An average level of competence is the norm in modern scientific research. Although the brilliant do get rewarded, most institutions of modern science are geared to the merely competent . . . and *average* recognition is relatively easily attainable. There *is* a large demand for research activity and for persons capable of training additional scientists; and financial support is given even to routine research. Work which is competent and sufficiently voluminous *does* attract the attention of scientists of similar competence, even though it might be ignored by the top elite. . . .

On this more restricted scale, the journeyman can thus attain a fair amount of prestige and all of its associated rewards.

This kind of career is quite stable. Once the scientist has acquired a certain amount of recognition and a reasonable position, there are many factors which reinforce further pursuits along similar lines. Suppose, for example, that after a few years the scientist finds himself dissatisfied with his own accomplishments; or feels his field of specialization is becoming overworked and sterile; or would like to work at the forefronts, but finds his store of knowledge inadequate. Under these circumstances, his most likely course of action is to adapt as best he can by remaining in the situation, for there are very many factors in the institutional framework which encourage preservation of the status-quo. The scientist has a large investment in his extensive training. He has also spent quite a few years viewing himself as a research scientist pursuing certain lines of work, and it is not easy to change his self-concept, role-models, and values by turning to activities further removed from his science. Furthermore, the institutionalized nature of modern science makes for much inertia. There is likely to be some organization, consisting of the scientist's graduate students, post-doctoral fellows, and technicians, which implies a host of commitments. A continuing stream of students wishes to do research leading to a Ph.D. degree. They need to be given suitable research problems, and the most likely problems are along the lines of previous work. A similar situation prevails in obtaining contracts or grants. It is necessary to write contract proposals specifying the work to be done. Once again, the easiest thing to propose, and the one most likely to lead to the award, is research along lines similar to previous work. If the scientist really wanted to take time off to supplement his training and to pursue new work, he would face a much more difficult task.

On the other hand, the status quo, even when no longer very satisfying, can yet lead to many satisfactions. A secure established position offers many rewards and, even when research has become more sterile or less congenial, there are usually many ways of keeping busy fruitfully. Although a field may have become less exciting, considerable prestige accrues to the person who is expert in it. Indeed, specialization in some narrow area is one of the most successful ways of adapting to a rapidly changing science and to the inability of keeping in the forefront of knowledge. It makes for recognition, since requests for articles, lecture invitations, and consultant positions are likely to be addressed to the narrow expert whenever his field seems pertinent in any other context. On the other hand, in an age of increasing specialization, a scientist whose work spans a large variety of subjects runs the distinct risk that no one group of scientists will view his whole work and thus recognize his merits consistently.

The elite scientist is in a somewhat more hazardous situation. Endowed with an active curious mind and imbued with the self-concept of the creative scientist, he is particularly prone to feel dissatisfied when he finds his specialization running dry of important problems. He may, of course, be willing to settle for research activities less brilliant than those of his youthful years. Indeed, he may often follow this course and carry on his work as a highly gifted journeyman in a field which has lost its glamour and excitement. There is, however, another road potentially open to him: he may change his research significantly. If he switches into a relatively new and unexplored field, he is more likely to make important discoveries and to deal with questions of fundamental significance than in his own more fully worked-over—and possibly highly competitive—field. Furthermore, he may bring an unusual background of experience which may lead to fruitful originality in the new field. Lastly, fields differ in intrinsic difficulty and in the relative importance of intellectual brilliance versus accumulated experience in bringing about useful contributions. Thus a scientist may prolong his useful creative life by switching to a field which is somewhat less demanding and where the age of peak productivity tends to be greater than in his original field. . . .

. . . Switching fields is not easy and there exist no accepted channels whereby the ordinary scientist can contemplate changing his research activities significantly without encountering severe obstacles. The very complexity and increasing specialization of modern science implies that the scientist must acquire much unfamiliar knowledge before he can be creative in a new field. He will also have to compete against the young people trained *ab initio* in that discipline; compared to them, he is likely to be working under a permanent handicap of significant lacunae in his knowledge of subject matter. Furthermore, to what extent is the scientist likely to be welcomed as an older man? How easily can he acquire the necessary training in institutions primarily geared to the education of young people? If he does emerge retrained in the new field and wants to begin doing research in it, will he have sufficient independent prestige to acquire necessary funds, research space, and students? Will the people in the new discipline accept him as a member of their group? For example, with what university department will he be affiliated? To these problems must be added a number of other obstacles. We have already mentioned the scientist's vested interest as an authority in his previous field, and his many commitments to the students and staff dependent on him. He is also likely to have family responsibilities. Under these circumstances, how does he solve some of the more immediate administrative and financial questions? Will he be given sufficient leave of absence to retrain himself in a new field? Does he relinquish all the contracts and grants which have supported his previous research? Can he get the necessary stipends or fellowships to support him during his period of retraining?

Here again the answers depend significantly on the status of the scientist in his previous field. If he is a member of the elite who has acquired considerable prestige as a result of his previous accomplishments, then he can exploit this prestige to facilitate plans for switching research areas. He is then more likely to get the necessary stipends and to be welcomed within the new discipline. But if the scientist is of lesser stature, then his mobility is much less and the questions raised above become much more formidable.

LEAVING RESEARCH

Yet many scientists, and especially the elite perhaps, will abandon doing active research at some point of their career. As we have noted, scientists tend to feel increasingly less creative in their own research. At the same time, the expansion of science has led to a vast expansion of alternative opportunities available outside of research proper. There are not only the traditional teaching activities, but numerous positions—such as department chairmanships or deanships—have become open in the universities which have grown in size and number and have tended to bestow an increasing proportion of their administrative positions upon scientists. Then, of course, there are a large number of industrial positions ranging from section heads to laboratory directors, plus a great variety of government jobs as consultants, advisors, and commission members. Such non-research positions become, in essence, additional steps added to the more traditional scientific career ladder.

These newer career steps are supported by the very institutionalization of science; i.e., people are needed to fill those positions. The steps are also justified by perfectly reasonable rationales quite apart from considerations of increased salary, prestige, or power; for it can be readily argued and believed that these non-research jobs are extremely important. Especially for the kinds of positions available to elite scientists, good justifications can be made in terms of "important and necessary work"—even if it is not the work of creative discovery. . . .

It is clear that administrative positions may become increasingly attractive especially when the scientist is getting to feel less creative and less satisfied with his own research activities. Furthermore, his turning to such alternative administrative roles is significantly easier than switching to distinctly new research. Not only is the former path less arduous and risky, but it is facilitated by a number of accepted channels open to the individual who wishes to make the transition to such administrative tasks. Again, this is particularly true for the elite.

The transition, once made, is highly irreversible. It is very unlikely that the scientist will, after several years of outside activities, return to research; under the present conditions of rapid discovery he will have lost contact with the mainstream of his science. The transition requires thus an adaptation to a permanently new pattern of life, one which makes demands on the scientist's administrative and political abilities. It also requires a reorientation of values and aspirations. The scientist, raised in a tradition of science where the great discoverer is preeminent, does not find it easy to abandon his ego-ideal as active researcher and is prone to internal conflicts in his new roles. The older eminent scientist, although filling some key position important to the development of science in his country, may yet not find his office deeply satisfying. Looking back upon former days of active investigation, he may view his present activities as a "lesser task." Thus the justifications of the non-research position may not even convince the person himself, let alone his colleagues. Some scientists, commenting on colleagues who have become saddled down with administrative responsibilities, claim that these men "asked for it," that it is for them a "socially accepted escape from freedom . . . a decorous way of concealing that they are burned out." . . .

For the scientist who has internalized the norms of discovery, and especially if he has earlier proved himself an outstanding discoverer, the transition between active research work and the shift into non-research activities is thus fraught with personal difficulties and great demands on adaptability. The transition marks a critical turning point in the scientist's life and career: his "scientific menopause."

TRENDS FOR THE FUTURE

The rate of scientific discovery will, no doubt, increase even further as new fields of research continue to spring up and, after a brief time in the limelight of current fashion, in turn become well-explored, "classical," and replaced by more exciting fields. The pressures on the scientist are thus likely to become even more severe. Yet these pressures are not just felt by isolated individuals; they are becoming an intrinsic and, at least partly, recognized characteristic of the modern scientific enterprise. Correspondingly we venture to predict that the channels for coping with these pressures will also become increasingly institutionalized.

For example, it is likely that expectations regarding the scientific career are gradually going to change. The scientist himself, as well as his employing organizations, may come to accept as a normal part of his career that he will not remain active in research indefinitely, but will turn to other activities. This would imply a certain amount of age grading of the roles open to scientists, the younger being predominantly those entrusted with the task of carrying on research. After some time the role models acquired by students in their early training would change accordingly so as to encompass prominent scientists who perform tasks outside of research. This would tend to reduce the inner conflicts faced by older scientists when they ultimately turn to non-research positions. At the same time, the institutional expectation that research careers are short-lived would lead to more readily accessible opportunities whereby older scientists might make the transition to such non-research positions.

Of course, as remarked earlier, there is already emerging an increasingly large and significant number of scientists involved in supra-research and administrative functions: not only ordinary scientific administrators but also elite scientists who are concerned with policy decisions at the highest levels of government or industry. Most of these persons have themselves considerable past research experience. Indeed, the elite contains many scientists of great distinction and forms a very tight group of influentials. They have even been described as a "self-selecting core group that intercommunicates" and as an "oligarchy" who play a "game of musical chairs" with the available high level positions. The prestige of the older scientists in these non-research positions derives partly from their past research accomplishments and partly from their present positions of power and influence. The esteem accorded to these scientists by the scientific community itself is not necessarily the same as that bestowed by the larger community. But, at least as far as the latter kind of esteem is concerned, one can confidently anticipate that such a group of elder scientists, who occupy increasingly positions of influence and perform useful functions, will increase its prestige standing in the society at large.

Under these conditions one can readily conceive that, faced with the cumulative pressures generated within their research activities on the one hand and with some of these prestigious alternatives on the other, older scientists may find the latter positively attractive . . . perhaps even seductive. Indeed, on the horizon one can see indications that the very research ideal of science may come to change its meaning. Some aspects of the transformation are already quite visible. Research has become less the exclusive province of great men, but has come to encompass a wide range of activities—from the pure to the applied and from the outstanding to the uninspired—carried out by a large pool of "scientific manpower." The more important potential transformation, however, concerns the status of research itself within science. Thus, whereas formerly research reigned supreme within science, it may come to share its supremacy with other activities like certain types of elite administration or policy making. . . .

Since, however, prestige is a perishable commodity, even the outstanding research scientist is more than ever likely to move quickly into prized non-research positions in order to maintain his earlier hard-won prestige. Otherwise he may retain his personal reputation as an historically great figure, but be discounted on the current scene.

B. External Factors Influencing Performance

Introduction

Classically, two sets of variables which influence scientific performance have been distinguished: the first investigates the effects of sociocultural or environmental factors on work; the second studies the effects of the internal or psychological factors within the scientist himself. Characteristic of the first group are conditions in the context or milieu in which the research is conducted, the relationships, organizational interactions, and constraints which affect what is done and how it is done. Conditions within the institution of science itself, its practices, values, or procedures which establish limits or restrictions on scientific behaviors are often included. The studies in this section illustrate some of the investigations which have examined the ways in which milieu affects performance.

Pelz's article touches on a number of sociocultural or environmental conditions which appear in analysis of more than 11 research environments to affect performance. These conditions include relations with supervisors or administrators, interactions with colleagues, opportunities for independence in activity and decision making, and the nature of the task. Comparative study of more than 1300 highly successful and lesser successful scientists in various research environments shows that in all of these areas, practices can be developed which on the one hand are security-giving and the base from which confident performance grows, and at the same time, challenging. Pelz's contribution is important because it emphasizes that the security-giving characteristics of the environment alone do not necessarily encourage creative effort, nor on the other hand do characteristics which are singularly and exclusively challenging. Rather, a combination seems to be the most productive. While this may seem contrary to the popular conception of the importance of absence of tension for creative work, it should be noted that Pelz's formulation is in line with the clinical notions about "the right amount of anxiety."

The sophisticated perspective that Pelz brings to his data also has been found in the articles by Barber (1961) and Merton (1968a) which point to roles of certain aspects of scientific ethic as both conducive to and encouraging resistance to innovative thinking. In line with Kuhn's (1962) original work which showed how the adherence to certain scientific paradigms becomes a deterrent to their challenge, Barber shows that a healthy skepticism is a necessary part of the scientist's intellectual attitude. However, at the same time, it can function as a resistance toward original ideas. This skepticism has delayed recognition of real breakthroughs in science. The Pelz material also offers a nice parallel to the investigations of Cole and Crane (Section IV D) and Merton (1968b) which elaborate how the system of science itself operates in such a way to make discoveries by certain individuals more plausible and acceptable to the scientific community than the same or like contributions by others.

Pulling out a sequence of data on the same population about which Pelz reports, Andrews studied the influence of a number of specific characteristics of the environment and psychological characteristics of the individual upon creativity. Using the Remote Associates Test as the measure of creativity, he found that this test did not correlate with scientific performance unless certain sociopsychological variables were taken into consideration. For the creative individuals whose work performance demanded really restructuring tasks, the strength of motivation as well as communication channels and various other environmental considerations had to be taken into consideration in order for the creative aptitudes to be efficient in predicting performance.

Reflecting on differences in research environments, Gordon and Marquis questioned whether a traditional environment or an academically marginal setting (such as a nonuniversity-affiliated clinic or hospital) is more conducive to innovation and arrived at a most startling conclusion. Experts' evaluations of the innovativeness of 245 projects indicated that research conducted in academic social science departments was clearly less innovative than similar research in academically marginal institutions. It was hypothesized that the ease with which consequences of research can be assessed in marginal institutions accounts for the greater innovation. An organizational comparison of the visibility of consequences and the extent to which administrative influence on research activity limited freedom, revealed almost three times more projects evaluated as highly innovative in the high visibility-high freedom condition than in the low visibility-low freedom condition.

The organizational structure which defines the research environment may have an orientation, goal, value system, which is not identical with, nor completely compatible with, that of science. Glaser notes the apparently divergent orientation of the scientist who draws his perspectives from both the goals of his particular organization and from science. In contrast to earlier authors who see "local" and "cosmopolitan" scientists as two distinct groups in science, Glaser attempts to demonstrate that "local-cosmopolitan" can be a dual orientation of highly motivated scientists. This dual orientation is derived from institutional motivation as a determinant of *both* high-quality basic research and accomplishment of nonresearch organizational activities. When there is similarity between the goals of science as an institution and the goal of the particular organization, the dual orientation exists. However, when conflict between the two goals is present, scientists may be either local or cosmopolitan in orientation.

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Creative Tensions in the Research and Development Climate

Donald C. Pelz

What kinds of climate in research and development organizations are conducive to technical accomplishment? What is the optimum degree of freedom versus coordination? of pure research versus practical development? of isolation versus communication? of specialization versus diversification?

To find some answers, my colleagues and I studied 1300 scientists and engineers in 11 research and development laboratories. Since the answers in different kinds of settings might vary, we included five industrial laboratories, five government laboratories, and seven departments in a major university. Their objectives ranged from basic research to product development.

Among the findings appeared a number of apparent inconsistencies. The optimum climate was not necessarily some compromise between extremes. Rather, achievement often flourished in the presence of factors that seemed antithetical.

Some examples are given below and summarized in Table 1 (*1*). As we pondered these findings, it seemed possible to fit many of them under two broad headings. On the one hand, technical men were effective when faced with some demand from the environment—when their associates held divergent viewpoints or the laboratory climate required disruption of established patterns. These might be called conditions of challenge.

On the other hand, technical men also performed well when they had some protection from environmental demands. Factors such as freedom, influence, or specialization offer the

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scientist stability and continuity in his work—conditions of security.

It seemed reasonable to say that the scientists and engineers of our study were more effective when they experienced a "creative tension" between sources of stability or security on the one hand and sources of disruption or challenge on the other. The term was suggested by T. S. Kuhn in a paper entitled "The essential tension: tradition and innovation in scientific research" (2).

Necessity is said to be the mother of invention, but our data suggest that invention (technical achievement) has more than one parent. Necessity might better be called the father—since necessity is one form of challenge, a masculine component. The role of mother is, rather, some source of security. When both are present, the creative tension between them can generate scientific achievement.

METHODS

The findings were not obtained by polling scientists concerning what climate they preferred. Rather, we obtained measures of each man's scientific performance, including his scientific or technical contribution to his field of knowledge in the past 5 years, as judged by panels of his colleagues; his overall usefulness to the organization, through either research or administration, also as judged by his colleagues; the number of professional papers he had published in the past 5 years (or, in the case of an engineer, the number of his patents or patent applications); and the number of his unpublished reports in the same period.

The performance measures were modified in several ways. Since distributions of papers, patents, and reports were skewed, a logarithmic transformation was applied to normalize them. Systematic variations with level of education, length of working experience, time in the organization, and type of institution were removed by adding constants so as to equalize the means. Each scientist, that is, was scored relative to others with similar background.

Characteristics of the climate were obtained on a carefully tested questionnaire. The two sets of data (on performance and on climate) were analyzed to find those conditions under which scientists actually performed at a higher or lower level.

Since optimum conditions might differ in different settings, all analyses were replicated within five subcategories: Ph.D.'s in research-oriented laboratories; Ph.D.'s in development-oriented laboratories; non-Ph.D.'s in research-oriented and in development-oriented laboratories (for convenience the latter have been called "engineers"); and non-Ph.D.'s in laboratories where 40 percent or more of the staff members held a doctoral degree (because of the limited influence and promotional opportunity of these non-Ph.D.'s we have called them "assistant scientists").

SCIENCE VERSUS APPLICATION

For the first illustration, consider a tension not between factors of security and challenge but rather between science-oriented and product-oriented activity. The respondent estimated the proportion of his technical time (that is, time spent on research or development, as opposed to administration or teaching) that he allocated to each of the following five "R & D functions":

Research (discovery of new knowledge, either basic or applied):
- General knowledge relevant to a broad class of problems ___%
- Specific knowledge for solving particular problems ___%

Development and invention (translating knowledge into useful form)
- Improving existing products or processes ___%
- Inventing new products or processes ___%

Technical services (either analysis by standardized techniques or consultation and trouble-shooting) ___%

Some interesting trends appeared. For instance, Ph.D.'s in both research-oriented and development-oriented laboratories were judged most effective, on the basis of several criteria, when they devoted only half their technical time to research as such (first two categories above) and the rest to activities described as development or technical services. Similarly, Ph.D.'s in development-oriented laboratories were most effective when they spent only one-quarter or one-third of their time on activities labeled "development."

. . . Even in laboratories devoted to pure research the best performers carried on four functions; they did not concentrate on research alone, but spent some time on development or service functions. Performance dropped if Ph.D.'s or assistant scientists tried to perform all five functions, although engineers flourished under this condition.

Effective scientists, in short, did not limit their efforts either to the world of pure science or to the world of application but were active in both (see Table 1, tension 1).

Is this involvement with both worlds a genuine tension? I am inclined to think so. As time invested in one increases, investment in the other must decrease. Demands for solution of practical problems can interfere with long-range research.

Why, then, should such a tension be creative? Several writers have proposed that a creative act occurs when a set of elements not previously associated is assembled in a new and useful combination. Diversity in technical activities may broaden the range of elements from which the scientist or engineer can draw in synthesizing new combinations.

Other findings reinforced the importance of diversity. Individuals performed better when they had two or three "areas of specialization" within their scientific discipline, rather than one. The Ph.D.'s did their best work not when they devoted full time to technical activities but when they spent about one-quarter of their time in either teaching or administration.

In the framework of challenge versus security, diversity in the task may also be viewed as a source of disruption and hence a condition of challenge. For data on specialization versus diversity, see Table 1, tension 3.

INDEPENDENCE VERSUS INTERACTION

Scientists place high priority on freedom. To measure this need, an index of "motivation from own ideas" was constructed, from self-reported (i) stimulus by one's previous work, (ii) stimulus by one's own curiosity, and (iii) desire for freedom to follow one's own ideas. This score—the index might also be labeled intellectual independence—was analyzed in relation to the four performance measures within each category of scientific personnel. A series of positive correlations appeared. Among the 36 correlation coefficients, 25 were positive ($r = +.10$ or larger) and none were negative; this was one of the most stable trends in the analysis, and was consistent with other research. As stated by Anne Roe (3), "almost all studies of scientists agree that the need for autonomy, for independence of action, is something that seems to be particularly strong in this group."

In what seemed an inconsistency, however, effective scientists did not avoid other people; they and their colleagues interacted vigorously. High performers conferred with their most important colleagues several times a week or daily; they regularly conferred with several colleagues in their own section and often with ten or more elsewhere in the organization.

In our speculative framework, independence or self-reliance is a source of security. Interaction with colleagues is a source of challenge, for they may criticize and prod. The high contributor experienced a creative tension between independence and interaction (Table 1, tension 2).

The skeptic may ask, Are the two conditions antithetical? In terms of their occurrence in our data, not necessarily. Yet in common experience it is often difficult to maintain one's independence under social pressure. As Ralph Waldo Emerson put it over a century ago in his essay "Self-Reliance": "It is easy in the world to live after the world's opinion; it is easy in solitude to live after our own; but the great man is he who in the midst of the crowd keeps with perfect sweetness the independence of solitude." The aphorism fits our effective scientists today. In the midst of the crowd they retained—with enough sweetness to be creative—the independence of solitude.

AGE, SPECIALIZATION, DIVERSITY

In one analytical study we considered the question, Under what conditions can younger or older scientists, respectively, do their best work? Andrews and I had speculated that younger scientists already face challenge because their work is new; mainly they need security. Older scientists, we thought, possess security and mainly need challenge. To test these ideas we correlated several measures of climate against performance within successive age brackets.

The findings were far from simple. The overall conclusion, however, was that, among younger and older scientists alike, *both* security and challenge were required for achievement.

Table 1. Eight Creative Tensions

Security	*Challenge*
Tension 1	
	Effective scientists and engineers in both research and development laboratories did not limit their activities either to pure science or to application but spent some time on several kinds of R & D activities, ranging from basic research to technical services
Tension 2	
Effective scientists were intellectually independent or self-reliant; they pursued their own ideas and valued freedom . . .	. . . But they did not avoid other people; they and their colleagues interacted vigorously
Tension 3	
a) In the first decade of work, young scientists and engineers did well if they spent a few years on one main project . . .	. . . But young non-Ph.D.'s also achieved if they had several skills, and young Ph.D.'s did better when they avoided narrow specialization
b) Among mature scientists, high performers had greater self-confidence and an interest in probing deeply . . .	. . . At the same time, effective older scientists wanted to pioneer in broad new areas
Tension 4	
a) In loosest departments with minimum coordination, the most autonomous individuals, with maximum security and minimum challenge, were ineffective . . .	. . . More effective were those persons who experienced stimulation from a variety of external or internal sources
b) In departments having moderate coordination, it seems likely that individual autonomy permitted a search for the best solution . . .	. . . to important problems faced by the organization
Tension 5	
Both Ph.D.'s and engineers contributed most when they strongly influenced key decision-makers . . .	. . . but also when persons in several other positions had a voice in selecting their goals
Tension 6	
High performers named colleagues with whom they shared similar sources of stimulation (personal support) . . .	. . . but they differed from colleagues in technical style and strategy (dither or intellectual conflict)
Tension 7	
R & D teams were of greatest use to their organization at that "group age" when interest in narrow specialization had increased to a medium level . . .	. . . but interest in broad pioneering had not yet disappeared
Tension 8	
In older groups which retained vitality the members preferred each other as collaborators . . .	. . . yet their technical strategies differed and they remained intellectually combative

In the youngest age categories (up to age 34), positive correlations appeared between technical performance and length of time the scientist or engineer had spent in his main project. Devoting 2 or 3 years to one undertaking is a source of security. It enables the young man to build contributions in which he can take pride. But, at the same time, young non-Ph.D.'s were effective when they had several areas of specialization, and young Ph.D.'s did better when they were *not* preoccupied with "digging deeply in a narrow area." A diversified task provides challenge (Table 1, tension 3*a*).

After age 40, a somewhat different set of measures accompanied high performance. Older individuals achieved only when self-confident—when motivated from their own ideas and willing to take risks. After age 50, achievement was also linked with an interest in probing deeply. These factors both suggest security. On the other hand, achievement after 50 was also linked strongly with interest in mapping broad features of new areas (Table 1, tension 3*b*). Thus, among older scientists, positive correlations appeared between performance and *both* penetrating study and wide-ranging study. The tension in this case was genuine; self-ratings of the two interests were found to be negatively correlated.

One wonders whether, in the creative tensions discussed thus far, the opposing conditions occur simultaneously or successively. Does the effective scientist pursue one narrow specialization at the same time he is exploring several new frontiers, or does he alternate between these postures? Does he retreat one month to his own ideas and engage in dialogue the next, or does he do both at the same time?

Our data contain no means of distinguishing. My hunch is that many creative scientists are flexible; they are able to alternate between contrasting roles.

THE INDIVIDUAL AND THE ORGANIZATION

We saw previously the importance of desire for independence. But to desire independence does not mean that one *is* independent. We therefore measured the individual's freedom to choose his own research or development tasks by asking who exerted weight in deciding what his technical goals or assignments were to be. The more weight exerted by the technical man himself, relative to that exerted by his chief, his colleagues, or higher executives or clients, the greater his perceived autonomy. The measure appeared valid: it was highest for Ph.D.'s in research, and lowest for "assistant scientists."

Now the more autonomy an individual has (the more weight in selecting his own assignments), the greater should be the stability and continuity of his work—the greater his security. And we found that, as autonomy increased, so did performance—up to a point. We were puzzled, however, to observe that when Ph.D.'s in both research-oriented and development-oriented laboratories had more than half the weight in choosing their goals their performance dropped, whereas in the case of non-Ph.D.'s, as their autonomy increased their performance continued to rise. Why?

In one search for answers we examined an organizational variable: the tightness or looseness of coordination within the department, measured by nonsupervisory scientists' ratings of the coordination within their section and supervisors' ratings of coordination between sections. (Individual autonomy and departmental looseness are of course interrelated, but within a given department the freedom of individuals can vary.) A loose organization does not make demands on its members; it provides high security with little challenge.

We found first that, in the most loosely coordinated departments, highly autonomous individuals actually experienced *less* stimulation, from either external or internal sources. They withdrew from contact with colleagues; they specialized in narrow areas; they even became less interested in their work. In these settings, maximum autonomy was accompanied by minimum challenge.

Yet in the most loosely coordinated settings, we also found, it was essential that the person be challenged if he were to achieve. It was here that the strongest correlations appeared between performance and various stimulating factors: diversity in the work, communication with colleagues, competition between groups, involvement in the job.

In these loosely coordinated settings, the most autonomous individuals were able to isolate themselves from challenge. A nondemanding organization permitted them to withdraw into an ivory tower of maximum security and minimum challenge. There they atrophied (Table 1, tension 4*a*).

What about the more demanding organizations—those of moderately tight coordination? Why was autonomy an asset here and not a handicap? We found that autonomous persons here had more diversity in their work, not less. One can speculate that in these departments the technical man had to face problems important to the organization; personal freedom enabled him to find the best solutions. Again a creative tension: the organization itself presented challenges; autonomy provided security for solving them (Table 1, tension 4*b*).

INFLUENCE GIVEN AND RECEIVED

The question used to measure autonomy also indicated the weight exerted by other persons in the choice of an individual's assignments. The "decision-making sources" were grouped into four categories: The individual, his immediate supervisor, his colleagues or subordinates, and higher executives or clients. We scored for each scientist how many of the four sources were said to have had at least some weight (10 percent or more) in selecting his technical goals.

Now, to discuss one's projects with persons in several positions is to run the risk of criticism and disruption. The more sources there are involved in decisions, the greater is the likelihood of challenge.

For the scientist to allow other people some weight in his assignments does not, however, mean that he is powerless. He can *influence* the decision-shapers, and influence provides security.

We divided respondents into those who felt they exerted strong influence over key decision-makers and those who felt they exerted little. Responses on this item appeared valid; the highest influence was reported by Ph.D.'s in research laboratories, and the lowest by assistant scientists.

The results were clear: both Ph.D.'s and engineers performed well when all four sources had some voice in shaping their goals but when, at the same time, the individual could influence the main decision-makers. From this arose creative tension 5 (Table 1): influence received from several others (challenge) combined with influence exerted on others (security).

The reader may ask, To what extent are the receiving and giving of influence antithetical? In conventional views of bureaucracy, each is seen as restricting the other; the size of the "influence pie" is considered a constant, so that if superiors have more, subordinates have less. Likert (*4*) argues, however, in a fashion compatible with our results, that the total amount of influence is not fixed. When everyone exerts more—when total control rises—performance is likely to improve.

But why should participation enhance the scientist's performance? Mainly, I suspect, because it helps him to avoid the narrow or trivial, to select tasks of *significance*, either to the organization or to science. Diverse contacts may also turn up unrecognized problems, or suggest new approaches to old ones. Finally, the interest of others in the scientist's work will enhance his own involvement in it.

"DITHER"

Another way in which a man's colleagues can provide challenge is through questioning his ideas. An apt label was borrowed by Warren Weaver (*5*) from British colleagues who built into antiaircraft computing devices a "small eccentric or vibrating member which kept the whole mechanism in a constant state of minor but rapid vibration. This they called the 'dither.' . . . We need a certain amount of dither in our mental mechanisms. We need to have our ideas jostled about a bit so that we do not become intellectually sluggish."

A scientist's colleagues may jostle his ideas if they and he approach a problem differently. To test this hypothesis, we measured similarity or dissimilarity between the scientist and his colleagues in several ways. One method was subjective—the respondent's perception of how his own technical strategy resembled that of his co-workers. Other measures were objective, in the sense that we examined the approaches reported by the respondent and by each of his colleagues and numerically scored the similarity among them.

How much dither or disagreement is healthy? In our data the answer depended on the kind of dither. One objective measure concerned the source of motivation—whether one's superior, the technical literature, or some other source. Scientists who responded to the same sources were somewhat more effective—perhaps because they had similar interests.

On three other measures we found the opposite to be true. Scientists and engineers did somewhat better when they saw themselves as different from colleagues in technical strategy, and when, as scored objectively, they differed from colleagues in style of approach (when, for example, the individual stressed the abstract, his colleagues the concrete) or differed in career orientation.

How to reconcile this paradox? In some preliminary data obtained by Evan (*6*) for industrial R & D groups, the teams he found most effective reported personal harmony or liking among members, but intellectual conflict. Colleagues who report the same sources of motivation as the scientist's own probably provide personal harmony and support—a form of security. When they argue about technical strategy or approach, they provide dither or challenge (Table 1, tension 6).

GROUP AGE

Another portion of our analysis concerned the age of groups—the average tenure of membership in a given section or team. A reasonable hunch is that, as a group gets "older," security is likely to rise and challenge is likely to diminish. If this is so, what conditions are needed to maintain vitality as the group ages?

To study this question, Wallace P. Wells identified 83 sections or teams in industrial or government laboratories (ranging in number of members from 2 to 25, with a median of 6). He averaged the measures for scientific contribution and usefulness of members in each group and adjusted the averages to rule out the effects of individual age, percentage of Ph.D.'s, and type of setting.

When he plotted the adjusted measures against group age, Wells found that group performance generally declined as group age increased, although usefulness was highest for groups with an average tenure of 4 to 5 years.

Why the decline after 5 years? In a search for clues, Wells examined several measures of the group's climate in relation to its age. . . . The average preference for "deep probing of narrow areas" (a source of security) rose steadily as group age increased, while the interest in "broad mapping of new areas" (a source of challenge) dropped. . . .

Not all older sections declined in vitality; some continued to be both useful and technically creative. Why? Wells examined other measures of group climate. One he called "cohesiveness"; a group scored high on this measure if its members listed other members of the team as their main colleagues. If group members prefer one another as collaborators, they are undoubtedly secure.

Wells found that in older groups (average group age, 4 years or more), cohesiveness was correlated strongly with usefulness and technical contribution. That is, if an older team continued to be cohesive, it stayed effective. Also, those older groups whose members communicated freely with one another performed better than younger ones did.

Yet the climate in effective older groups could hardly be called relaxed. On the measure of felt similarity to colleagues in technical strategies, Wells found that, in older groups, the more dissimilar the approach was, the higher was the performance.

One other measure proved surprising. Scientists rated the "hesitance to share ideas" within their section (for convenience we have called it "secretiveness"). Usually such hesitance was absent or mild. When some of this feeling was present in new groups, it was a handicap; it hindered their work. But this feeling *enhanced* the performance of older groups.

On reflection, this contrast makes sense. A new, insecure group must suspend criticism while it searches for new ideas. An old, secure group, on the other hand, will profit from criticism. If it stays effective it is not a club where one can lower his intellectual guard. On the contrary, there is competition in ideas; members sharpen their wits and marshal their evidence before speaking. Such a climate indicates challenge rather than insecurity.

Creative tension 8 (Table 1)—intellectual combativeness among colleagues who value each other—resembles tension 6. To prefer one's section members as collaborators is a sign of personal support, while the atmosphere of combativeness indicates intellectual conflict.

PRACTICAL IMPLICATIONS

Before considering practical implications I should raise the question, What is cause and what is effect? Does a combination of security and challenge help to generate achievement? Or do scientists who achieve experience more security and sense of challenge?

My own speculation is that a feedback loop exists. Usually a high performer has not only ability but also personality traits of curiosity and confidence. He is attracted to diverse problems and to contact with colleagues (a source of challenge) and at the same time insists on freedom and a voice in decisions (conditions of security). He thus exposes himself to conditions which in turn stimulate him to achieve. If this is the case, might lower achievers surround themselves with a similar climate and so enhance their own performance? Can R & D managers help to create such environments? I believe they can, and offer the following suggestions.

CONDITIONS OF SECURITY

An important quality (see Table 1, tension 2) is self-reliance and pursuit of one's own ideas. But in a development-oriented laboratory the manager cannot give each man a free hand; how then can he build an individual's pride in his own work? One way perhaps is to insure that once or twice a year each man produces a product which bears his own name—even if this requires that a jointly prepared document be broken into parts. It was disturbing to find in our sample that two out of five non-Ph.D.'s in research had not published a single paper in 5 years; among engineers the figure was four out of five. Half the engineers had not a single patent to their credit in the past 5 years, and one out of five had not authored even an unpublished report. How can a scientist feel confident of his own ideas if he has no output in which to take a fatherly pride?

Consider how the method of rewarding performance may affect self-reliance. Typically a single chief assigns tasks, judges results, evaluates performance, and recommends promotions. What better way to stamp out independent thought? To build self-reliance there must be multiple channels for recognizing achievement. Make sure that each subordinate has a chance once or twice a year to explain his work to colleagues *outside* his group. In review sessions with executives or clients, include the engineer who is doing the work and let him do some of the talking.

Another security factor is autonomy—substantial weight exerted by the individual in choice of assignment (see Table 1, tension 4). Such weight does not mean, however, that the individual should be completely on his own. From a further analysis (not reported above) it appeared that a technical worker in a development-oriented laboratory performed best when he and his supervisor *jointly* determined assignments. For Ph.D.'s in research laboratories, an effective condition was joint determination by the scientist and his colleagues. Assignment by the supervisor alone was the worst condition in all settings.

Security can be provided by the opportunity to influence others who decide one's assignments (Table 1, tension 5). Organizational structure plays a part here. Such influence is probably weaker in a many-leveled impersonal organization where each level has a veto. The individual's voice counts more in an organization of flat structure with fewer levels, where there is a chance for face-to-face contact with the people who shape his assignments.

Security increases with the length of time an individual spends on a given project (tension 3), particularly in the case of the younger man. Give him a year or two to dig into his main project, instead of shifting him every 3 months. He must have time to build a solid contribution.

One's colleagues can also be a source of security. In forming teams, managers can put together individuals who have similar sources of motivation—who are interested in the same kinds of problems (tension 6).

As R & D teams get older they can remain productive if they stay cohesive (tension 8). The supervisor can encourage cohesion by giving credit to the group rather than to himself. He can build mutual respect by publicizing the contribution of each member. He can strengthen teamwork through promoting competition with other groups in the solution of technical problems.

CONDITIONS OF CHALLENGE

Scientists and engineers performed well not only when they had continuity and stability but also when they were challenged by demands from their environment. Frequent contact with one's colleagues (tensions 2 and 5) can be an important source of challenge. Such contacts can stimulate the individual in many ways. They can point to significant problems, suggest new approaches, or correct errors in a present approach.

How can the R & D manager encourage fruitful interaction? Often simply by knowing who in the organization or the field is doing what; he can steer the scientist to others who can give or use help. He can invite the individual to talk to a seminar, set up study teams and evaluation groups, pose problems which require consultation for their solution.

To encourage friendly disagreement, the R & D manager can invite members of an older group to look for flaws in each other's presentations (tension 8). When forming a new project committee he can include individuals who like each other but who use different strategies (tension 6). Periodic regrouping of teams—always with the consent of the persons involved—may help in maintaining a vital atmosphere.

Specialization lends security but diminishes challenge; some degree of diversity is required (tensions 1, 3, and 4). The manager should beware of letting some individuals focus exclusively on research, others exclusively on development. He should encourage his staff to tackle some jobs in both areas.

A younger scientist needs more than one area of specialization (tension 3*a*). In addition to a main continuing assignment, give him each year a second, shorter assignment which demands that he learn a new skill. Keep the older man's interest in broad areas strong by tempting him with problems on the pioneering edges of his field (tension 3*b*). Set up refresher courses; arrange sabbatical exchanges with a university.

Teams as well as individuals can become too specialized and lose interest in pioneering (tension 7). The R & D manager should not assume that one group has become *the* expert group in a specific area. As problems in this area arise, occasionally he will give one of them to a different team. He will challenge the expert group now and then with a task outside its specialty.

In the short run, such a policy may not be the most efficient way to manage a laboratory. It may cost more and take more time. But in the long run it will make for breadth and flexibility, and these will continue to open doors for creative advances. . . .

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Creative Ability, the Laboratory Environment, and Scientific Performance

Frank M. Andrews

Previous research on creativity has studied the characteristics and backgrounds of creative people (see, for example, studies by Barron,[1] Taylor,[8] and Roe[7]). There have, however, been few attempts to examine conditions which facilitate the translation of creative ability into creative performance.

WHY THE ENVIRONMENT MATTERS

Without pretending to be able to describe the entire creative process, we can perhaps identify gross aspects of it. Somehow a person first must get an original idea which is useful for the problem at hand. (We define creative ability as a person's capacity for getting such ideas.) Then he must be willing to make it known to others. This may be risky, particularly when the new idea is contrary to accepted ways of doing or thinking about things, or when the suggestion that something could be done better implies criticism of one's superiors or colleagues. Thus a person may be *unwilling* to present an original idea.

Even if a person is willing to present it, he may be *unable* to do so. For a variety of reasons, the idea may not "get through" to others. Others may not take the person's idea seriously because he lacks status, reputation, or formal training. Or perhaps there may be no way for him to communicate the idea—e.g., to a superior who is several rungs up the hierarchy.

Finally, creative ability may fail to pay off because an original idea is not appropriate for the situation. The situation may not be open to the shifts which a creative idea would require, i.e., it may be *inflexible*. For example, after several years a research project may have its methods and goals clearly defined. The job is to achieve these goals rather than set off in new directions.

Thus the creative process involves the social environment as well as a capacity to think of ideas which are original and useful. Environmental factors affecting the flexibility of the situation and a person's willingness and ability to make ideas known to others were expected to determine whether creative ability was related to technical performance.

DESIGN OF THE STUDY

To explore these ideas, information was obtained from about two hundred scientists and engineers in a wide variety of R&D settings: a university, two industrial firms (glass technology, pharmaceuticals), and five government laboratories (basic research in physical sciences, weapons, guidance, animal diseases, and uses of agricultural products.). The creative ability, performance, and laboratory environment of each man were measured, as described next.

Creative Ability

Of the numerous ways which have been proposed to measure creative ability, we chose a test developed by Mednick, the Remote Associates Test (RAT). In several studies based on widely different groups of people (architecture students, psychology graduate students, school children, and IBM suggestion award winners), Mednick and his associates[5, 3] have found that people earning high scores on the RAT tended to be rated as highly creative. Furthermore, Gordon[2] has found that in an industrial R&D setting scientists who scored high on the RAT were more likely (than low scorers) to write research proposals, had a higher percentage of proposals which won contracts, and tended to win larger contracts.

The RAT is a timed paper-and-pencil test which requires the person taking it to find a fourth word which can be associatively linked with three other words. For example, given the three words "rat," "blue," and "cottage," the answer would be "cheese" (rat-cheese, blue-cheese, cottage-cheese). Mednick argues that the mental process required for answering this

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type of item is the same as that used in being creative. . . .

Several studies have shown that scores on the RAT were not strongly related to intelligence.[3], [4] In addition, in a chemical firm, Gordon[2] found that the RAT was a better predictor of success in obtaining patents than two other tests often used to measure creativity, the Harris Simberg AC Test for Creative Ability and the Rokeach Dogmatism Scale.

Performance

Four different measures of performance were obtained. Two were based on others' judgments, two on the man's self-reported output.

For the first judgment, peers and supervisors who felt knowledgable about the man's work were asked to assess his *scientific or technical contribution* (the extent his work had contributed to general scientific or technical knowledge in his field over the past five years). Typically, five judges assessed each man's contribution. Each judge made his assessment independently, and we subsequently combined their answers (which tended to show good agreement) into a single score for "contribution."

Each judge also assessed each man's *usefulness* (the extent his work had been useful or valuable in helping his R&D lab carry out its responsibilities over the past five years). Here also, the judges were in reasonably good agreement, and we combined their information into a single score for "usefulness."

Several indications of actual output were also examined. Each respondent was asked how many published papers, patents (or patent applications), and unpublished reports he had produced over the past five years. (Various checks—such as comparing reported output with company records, or with answers to the same question given several months later—convinced us respondents were reasonably accurate in reporting their output.)

As is usually found for scientists and engineers, these performance measures varied systematically according to the respondent's background. Not surprisingly, scientists with long experience, long seniority, and long academic training tended to outperform their younger, less trained colleagues. Since these background effects could mask relationships between creativity and performance, all measures were compensated by adding or subtracting appropriate constants. This produced performance measures which expressed how well or poorly a person performed *relative* to others with similar backgrounds.

The interrelationships among these criteria of performance were about as expected. The usefulness and similarity scores tended to show some similarity, but only about half the variation in one could be attributed to the other (median correlation = 0.7). The output scores were only mildly related to the judgments of performance and to each other (median correlation = 0.2).

Laboratory Environment

Information about the scientist's laboratory environment was obtained from his answers to a lengthy questionnaire. The specific questions used in the present analysis are detailed below.

CREATIVE ABILITY AND TECHNICAL PERFORMANCE

First attempts to relate creative ability and technical performance were disappointing. For each of five different types of scientists (described below) the correlations were inconsistent and close to zero. Some scientists who got high RAT scores performed well, but about an equal number did poorly. Thus creative ability—considered alone—was a poor predictor of scientific achievement.

Intriguing results emerged, however, when the laboratory environment was also considered.

SITUATIONS WHICH AFFECTED THE PAYOFF FROM CREATIVE ABILITY

Time in Area

One factor which affected the payoff from creative ability was the length of time the scientist had specialized in his content area. Creative ability proved to be more highly related to performance (i.e., was more helpful) for scientists who had specialized in their areas for only a *short* time than for those with long experience in an area. Table I shows the data.

Since the analysis which led to these results was complex, Table I is also complex. A description of the table will indicate the methods used. Scientists were divided into five groups which previous research had shown to be rather different (Pelz and Andrews[6]). These groups were derived by considering three factors: a) formal training (Ph.D.'s were different in many respects from non-Ph.D.'s), b) orientation of the lab toward research or development (based on activities most valued by laboratory management, not on what any given individual actually did), and c) percent of co-workers holding Ph.D.'s (non-Ph.D.'s could rise to the top in labs where fewer than 10 percent held doctorates, but faced a promotion ceiling when this figure was 40 percent or more). Since a majority of the non-Ph.D.'s in development labs where fewer than 10 percent held doctorates had been trained in engineering specialties, we called this group "engineers." The "non-Ph.D. scientist" group were non-Ph.D.'s in research labs where few held doctorates. "Assistant scientists" were non-Ph.D.'s in

Table I

Relationships are shown between creativity, as measured by the RAT, and four criteria of performance —adjusted for length of experience. The five major groups were divided according to the length of time the scientist had been working in his major area of specialization, and correlations (*r*'s) computed separately for these sub-groups. The predominance of positive relationships among the newcomers (Part *a*) indicates that creative ability tended to "pay off" for them. Among the old-timers (Part *b*), however, high creative ability tended to go with *low* performance. Part *c* takes the same data and shows the *difference* between the two correlations. The predominance of positive differences indicates that creative ability helped the newcomers more. (If the correlation or difference was at least ±0.15, it is shown as a "+" or "—"; relationships statistically significant at the 5 percent level are indicated by double symbols. See text footnote 4 for significance of trends.)

		*Papers or patents**	*Reports*	*Contribution*	*Usefulness*	*Scorecard*	
a) Relationships for those a *short* time in area of specialization†						"+"'s	"—"'s
Ph.D.'s in development labs	(*N* = 21)	+	+	+	0	3	0
Ph.D.'s in research labs	(*N* = 7)	0	+	+	+	3	0
Engineers	(*N* = 17)	+	+	+	++	5	0
Assistant scientists	(*N* = 19)	0	—	+	0	1	1
Non-Ph.D. scientists	(*N* = 13)	+	—	+	0	2	1
Totals						14	2
b) Relationships for those a *long* time in area of specialization†							
Ph.D.'s in development labs	(*N* = 31)	0	0	0	0	0	0
Ph.D.'s in research labs	(*N* = 13)	+	0	+	0	2	0
Engineers	(*N* = 29)	0	0	—	—	0	2
Assistant scientists	(*N* = 30)	0	0	0	—	0	1
Non-Ph.D. scientists	(*N* = 20)	0	—	——	——	0	5
Totals						2	8
c) Difference: *r* for *short* time minus *r* for *long* time							
Ph.D.'s in development labs		+	+	+	—	3	1
Ph.D.'s in research labs		—	+	+	+	3	1
Engineers		+	+	++	++	6	0
Assistant scientists		0	—	+	+	2	1
Non-Ph.D. scientists		0	0	+	+	2	0
Totals						16	3

Note: *N*'s vary slightly due to missing data.

* For engineers patents were used instead of papers.

† For Ph.D.'s a "short" time in area was 0–9 years; a "long" time, 10 or more years. For other groups these figures were 0–5 and 6+ years, respectively.

Ph.D.-dominated labs (either research or development); although they were professionals, most occupied subordinate positions.

As shown in Table I, each of these five groups was divided into two subgroups according to the time the scientist had been specializing in his most proficient area. (Questions 2 and 3 used for determining this are shown in the above box.) Then within each of these subgroups, the relationships between creative ability (RAT score) and each performance measure were examined.

For Ph.D.'s the average amount of time spent in the main area of specialization was roughly 10 years. Thus those who had spent 0–9 years were *relatively new in their area.* Non-Ph.D.'s tended to have specialized more recently than Ph.D.'s; in each non-Ph.D. group six or more years experience constituted old-timers.

The correlation between RAT and output of papers among development Ph.D.'s who were relative newcomers was mildly positive ($r = 0.17$); high creative ability went with high output. Since this figure exceeded an arbitrary criterion of ± 0.15, it is shown in Part *a* of Table I by a "+" sign. For the old-timers, however, this relationship was mildly negative ($r - 0.11$); since this was less than the criterion, a 0 appears in Part *b*. Had the relationships for the newcomers and the old-timers been the same, we would have concluded that time in area had no effect. But the relationships were different: payoff from creative ability tended to be higher for those who had specialized only a short time. A rough indication of the size and direction of this effect was obtained by subtracting the correlation for the old-timers from that for the newcomers. This difference was +0.28 and is shown in Part *c* by a "+" sign.

As shown by the scorecard in Table I, there was a tendency for creative ability to enhance the performance of newcomers (note predominance of "+'s" in Part *a*) whereas it may even have hurt old-timers' performance (Part *b* is mainly 0's and "–'s"). These findings are combined in Part *c*, where the predominance of positive differences indicates that creative ability tended to pay off more (or hurt less) for those who had specialized in their area for a relatively short time.

Of course, many of these correlations and differences were rather mild. But when the same trends were observed for a variety of performance measures and were *replicated* for five different groups of scientists, we concluded that they were sufficiently general to be interesting. Before considering their implications, let us turn to some further data.

Time on Project

If time in area affected the relationship between creative ability and performance, would other "time" factors show similar results? A set of questions asking about the time the scientists had worked on his most important project provided a means of checking. . . .

Again the tendencies were for creative ability to be more useful to the newcomers. Relationships between creative ability and performance tended to be positive among scientists who had worked on their projects 0–2 years (Table II, Part *a*), and negative for those 3 or more years (Part *b*).

Could these findings have been an artifact due simply to younger people (many of whom would be newcomers to an area or project) making better use of their creative abilities? This was checked by dividing the groups into relatively young and old subgroups and examining the relationships between creative ability and performance. Age proved to have no consistent effect on these relationships. We also looked at career levels, and found that creative ability was as likely to be helpful to the senior-level scientist or research supervisor as it was to their more junior colleagues. (Data not shown.) Thus findings shown in Tables I and II seemed not to be attributable to differences in age or career level.

We concluded creative ability was most likely to pay off in increased performance when a person was a relative newcomer to an area or project, regardless of his age or career level.

Why? The time a person had been active in an area or on a project may have been one indication of the flexibility of his working situation. We had expected that creative ideas were more likely to be relevant in situations where there was still room to head in new directions. The findings in Tables I and II seemed to support this hunch: the flexibility of the situation seemed an important factor in determining the climate for creativity.

Table II

Creative ability paid off more for those who had been on their main project a short time than for those who had been involved longer. As in Table I, correlations were computed (*r*'s) between creative ability and four criteria of performance separately for subgroups determined on the basis of years spent on the main project. (If the correlation or difference was at least ±0.15, it is shown as a "+" or "—"; relationships statistically significant at the 5 percent level are indicated by double symbols. See text footnote 4 for significance of trends.)

		*Papers or patents**	*Reports*	*Contribution*	*Usefulness*	*Scorecard*	
						"+"'s	"—"'s
a) Relationships for those 0–2 years on project							
Ph.D.'s in development labs	($N = 21$)	+	+	0	+	3	0
Ph.D.'s in research labs	($N = 8$)	0	+	+	+	3	0
Engineers	($N = 36$)	0	0	0	0	0	0
Assistant scientists	($N = 30$)	0	—	+	+	2	1
Non-Ph.D. scientists	($N = 22$)	+	—	—	—	1	3
Totals						9	4
b) Relationships for those 3+ years on project							
Ph.D.'s in development labs	($N = 33$)	—	—	0	—	0	3
Ph.D.'s in research labs	($N = 11$)	+	0	+	0	2	0
Engineers	($N = 12$)	0	+	—	—	1	2
Assistant scientists	($N = 28$)	0	0	—	——	0	3
Non-Ph.D. scientists	($N = 11$)	—	—	—	—	0	4
Totals						3	12
c) Difference: *r* for *short* time minus *r* for *long* time							
Ph.D.'s in development labs		+	+	+	+	4	0
Ph.D.'s in research labs		—	+	+	+	3	1
Engineers		0	—	+	+	2	1
Assistant scientists		0	—	+	++	3	1
Non-Ph.D. scientists		+	0	0	0	1	0
Totals						13	3

Note: *N*'s vary slightly due to missing data.

* For engineers patents were used instead of papers.

Coordination in Chief's Group

Another factor which consistently affected the payoff from creative ability was the degree of coordination in the group headed by the respondent's chief. Creative ability tended to be most useful in relatively uncoordinated groups. Using the same methods as previously, but subdividing the groups according to coordination (see above box), produced the results shown in Table III.

Note that in loosely coordinated groups (Part *a*) creative ability either helped or made little difference, whereas in tightly coordinated groups (Part *b*) creative ability tended to depress performance, if anything. Part *c* shows that these trends were similar in all five groups.

If coordination within the chief's group was important, what about other indications of coordination—such as coordination with colleagues, or in the lab as a whole? A careful look showed that these forms of coordination did not affect relationships between creative ability and performance. This suggested it was not coordination *per se* which stifled creativity, but a scientist's inability to introduce new ideas into his immediate work situation.

Although one cannot say for sure, the results in Table III may have been another manifestation of the "flexibility phenomenon." Although high coordination may have helped a team reach an objective, coordination may have limited the team's ability to make the changes required by original ideas.

Table III

Creative ability was more likely to enhance performance when activities within the chief's group were only loosely coordinated. The relationships between creative ability and four measures of performance tended to be positive for scientists in loosely coordinated groups (Part *a*), but tended to be negative for those in tightly coordinated groups (Part *b*). (If the observed correlation or difference was at least ±0.15, it is shown as a "+" or "—"; relationships statistically significant at the 5 percent level are indicated by double symbols. See text footnote 4 for significance of trends.)

						Scorecard	
a) Relationships for those in *loosely coordinated* groups[†]		*Papers or patents*[*]	*Reports*	*Contribution*	*Usefulness*	"+"'s	"—"'s
Ph.D.'s in development labs	($N = 23$)	0	+	0	0	1	0
Ph.D.'s in research labs	($N = 11$)	+	+	+	+	4	0
Engineers	($N = 27$)	0	+	0	0	1	0
Assistant scientists	($N = 23$)	0	0	0	—	0	1
Non-Ph.D. scientists	($N = 19$)	0	0	0	0	0	0
Totals						6	1
b) Relationships for those in *tightly coordinated* groups[†]							
Ph.D.'s in development labs	($N = 29$)	0	—	—	0	0	2
Ph.D.'s in research labs	($N = 7$)	0	0	0	—	0	1
Engineers	($N = 18$)	0	0	—	—	0	2
Assistant scientists	($N = 30$)	—	—	0	0	0	2
Non-Ph.D. scientists	($N = 12$)	+	—	—	—	1	3
Totals						1	10
c) Difference: *r* for *loose* minus *r* for *tight*							
Ph.D.'s in development labs		+	++	+	0	4	0
Ph.D.'s in research labs		+	+	+	+	4	0
Engineers		—	+	+	+	3	1
Assistant scientists		+	+	0	0	2	0
Non-Ph.D. scientists		—	+	0	+	2	1
Totals						15	2

Note: *N*'s vary slightly due to missing data.

[*] For engineers patents were used instead of papers.

[†] For Ph.D.'s and non-Ph.D. scientists "loose" was defined as "slight" or less, for other groups this was "moderate" or less. Scientists who indicated higher levels of coordination were considered to be in relatively tightly coordinated groups.

Influence

When the questionnaire was designed, we had not fully formulated our ideas about the importance of scientists being willing and able to make original ideas known to others. Nevertheless two items which asked about the scientist's influence in his lab seemed relevant. Scientists who were able to influence those around them might be more inclined to accept the risks inherent in proposing new ideas. Also, their colleagues and superiors might be more likely to accept these new ideas. Thus influential scientists might be both more willing and more able to make original ideas known to others than those with low influence. Hence, influential scientists should get higher payoff from creative ability.

. . . Table IV shows what happened when scientists were subdivided on the basis of a measure of the influence they exert over decisions affecting their goals.

As shown by the positive differences in Part *c* of Table IV, our expectations were supported for three of the groups—the Ph.D.'s in development or research labs, and the non-Ph.D. scientists. Influence enhanced the payoff from creative ability. Effects of influence were inconsistent, however, for engineers and assistant scientists.

A parallel item asked about the ability of the scientist to influence the person with most weight in deciding his *resources*. Results using this measure of influence were roughly similar to those shown in Table IV.

Further investigation showed, however, that getting high payoff was not simply a matter of a scientist having freedom to do whatever he pleased. The groups were split on still another item—the scientist's autonomy (his *own* weight on decisions about his goals). This factor produced no consistent effects on the relationship between creativity and performance. A scientist with great freedom might or might not get high gains from creative ability. The important thing was his ability to influence *other* people who affected his work. This was what one would expect if creativity is affected by a person's willingness and ability to make original ideas known to others.

Communication

Another factor we expected would be important in enhancing creative performance was communication. Without adequate communication, potentially new ideas would go unrecognized. Also, communication might itself stimulate the generation of new ideas.

It was a problem to know how to measure the "adequacy of communication," for communication occurs in many ways. One factor

Table IV

Being able to influence the person who has most weight in determining one's technical goals enhanced the payoff from creative ability for Ph.D.'s in research or development and non-Ph.D. scientists; the effects of influence were mixed, however, for engineers and assistant scientists (see Part *c*). As in previous tables, we compared the strength of the correlations (*r*'s) between creative ability and four criteria of performance in subgroups of the respondents. (If the correlation or difference was at least ±0.15, it is shown as a "+" or "—"; relationships statistically significant at the 5 percent level are indicated by double symbols.)

a) Relationships for those with *high* influence†		*Papers or patents**	*Reports*	*Contribution*	*Usefulness*	*Scorecard* "+"'s	"—"'s
Ph.D.'s in development labs	(*N* = 40)	0	0	0	0	0	0
Ph.D.'s in research labs	(*N* = 5)	+	+	+	+	4	0
Engineers	(*N* = 34)	0	0	0	—	0	1
Assistant scientists	(*N* = 15)	+	—	—	—	1	3
Non-Ph.D. scientists	(*N* = 22)	0	0	0	0	0	0
Totals						5	4
b) Relationships for those with *low* influence†							
Ph.D.'s in development labs	(*N* = 15)	0	0	0	0	0	0
Ph.D.'s in research labs	(*N* = 14)	0	0	0	0	0	0
Engineers	(*N* = 15)	—	0	0	+	1	1
Assistant scientists	(*N* = 41)	0	—	0	0	0	1
Non-Ph.D. scientists	(*N* = 11)	—	——	—	—	0	5
Totals						1	7
c) Difference: *r* for *high* influence minus *r* for *low*							
Ph.D.'s in development labs		0	+	0	+	2	0
Ph.D.'s in research labs		+	+	+	+	4	0
Engineers		+	+	—	—	2	2
Assistant scientists		+	0	—	—	1	2
Non-Ph.D. scientists		+	+	+	+	4	0
Totals						13	4

Note: *N*'s vary slightly due to missing data.

* For engineers patents were used instead of papers.

† For research Ph.D.'s "high" influence was defined as "great" or more, for all other groups this was "considerable" or more. Scientists who indicated lower levels than these were considered to have relatively low influence.

examined was the frequency with which a scientist contacted his chief on work-related matters; another was the frequency of contacting colleagues. These produced effects in the expected direction for engineers: creative ability was positively related to performance if engineers contacted their chiefs daily, or their colleagues several times a week; but relationships were zero or negative among engineers with less frequent contact. For other groups, however, there was no marked effect.

In another attempt to measure adequacy of communication, the groups were split according to the number of talks or reports prepared by the scientist. If a person wrote more than the average number of reports for scientists of his type and experience, then perhaps his ideas would have a better than average chance of being communicated. This measure produced effects in the expected direction for two groups: research Ph.D.'s and assistant scientists. Creative ability was most useful to those who wrote an above-average number of reports. Results for the other groups were mixed, but not consistently opposite to expectation.

Thus high communication seemed to enhance the payoff from creative ability for three groups. The inconclusive results obtained for the other two groups may have reflected a need for more sensitive measures of communication.

SUMMARY AND IMPLICATIONS

This paper concludes in the format of a conversation with a research director.

> You folks have cited many "factors" which you looked at. What do you consider your main findings?

Okay, let's summarize. First, creative ability, as we measured it, did not relate to any measure of performance for any group of our scientists. The reason, however, was because some scientists were in situations where creative ability seemed to hurt their performance. The situations which seemed to enhance the payoff from creative ability were the following: working on a project or specializing in an area for a relatively short time; being part of a work team where coordination was not high; being able to influence important decision-makers; and having good facilities for communicating new ideas to others. These findings suggested that a good climate for creativity was a situation where scientists were willing and able to make their ideas known to others and where there was sufficient flexibility for change to be possible.

> You mentioned there were situations in which creative ability *hurt* a man's performance. Do you really mean that?

In at least one sense we do. Under some conditions the men with high creative abilities did less well than those with lower talent.

Consider a hypothetical example of how this might occur. Imagine a man with high creative ability finding himself in a very restrictive situation. When he gets one of his potentially creative ideas, he feels he would be risking his job if he were to suggest it; or he does suggest it, but finds no one will listen. He can't "turn off" his flow of ideas. So he becomes disappointed and dejected. If he doesn't leave the job, he may slow down, feeling that his talents go unrecognized. Soon he is doing less well than his unfrustrated colleagues. In this sense, his creative ability is hurting his performance.

> I am still puzzled why your RAT test didn't always relate to performance if it is supposed to measure creative ability. After all, science is supposed to be a creative endeavor.

Our best explanation is that creative ability can hurt as well as help, depending on the situation. When all situations were lumped together, the minuses canceled out the plusses. Another hunch is that some R&D labs may not actually want as much creativity as they claim. If so, creativity won't be rewarded. The man with the *new* approach won't have a chance to report or publish it. He won't earn higher status, nor even the respect of colleagues.

> You haven't said much about where you found the most creatively able people; did they tend to work in particular kinds of situations? I am wondering if they are really a "different breed of cats" from those who are less creative. . . .

We looked at this and found that scientists who scored high on the RAT test answered the questionnaire pretty much like those who scored low. There was no indication that creatively able people selected themselves into certain kinds of situations and avoided others.

> If I believe your findings, what would you suggest I do to make use of them?

Good climates for creativity seemed to be those where people were willing to make new ideas known to others, where they were able to do so, and where there was flexibility for accepting changes. Thus according to this study, anything you can do to change the situation in these directions should encourage good use of creative ability. If a man has worked on a project or in an area for a long time, perhaps he should shift to a new speciality. Maybe he should move to a new group where he would have some opportunity to influence events if his present group doesn't allow this; and make sure that it is easy for a person to try out his new ideas on others.

As a final suggestion, consider the reward system. Many labs say they want creativity, but give the biggest rewards to those who are productive along well-established paths. If you want creativity, does it get rewarded?

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Freedom, Visibility of Consequences, and Scientific Innovation

Gerald Gordon and Sue Marquis

In the literature on scientific organization there are a great many speculative statements stressing the relationship between freedom and scientific accomplishment. Wolfle, among others, postulates that the greater the freedom accorded the scientist, the more creative his research. Wolfle states, for example: "One of the most certain ways in which society can promote excellence in science and other areas of scholarship is by building strong universities and insisting that creative scholars be given time, facilities, and freedom of choice to carry out the studies that seem most likely to extend fundamental knowledge and understanding."[1] However, one finds little of an empirical nature which bears on the role freedom plays in enhancing creative scientific accomplishment. Rarer still are systematic attempts to relate

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[1] In Dael Wolfle (ed.), *Symposium on Basic Research* (Publication No. 56 [Washington, D.C.: American Association for the Advancement of Science, 1959]), p. 279.

structural variations in freedom to scientific accomplishment.[2]

To investigate the relationship between freedom and creative scientific accomplishment, we examined the authority patterns in four types of research settings: universities, medical schools, hospitals, and health agencies. Data were obtained by a mail questionnaire sent to the directors of 245 projects. The universe of inquiry consisted of all projects studying the social-psychological aspects of disease listed in the *Inventory of Social and Economic Research in Health* from 1954 to 1960.[3] Questionnaires for 223 (91 per cent) of the projects were returned. Of these projects, 64 were conducted in academic social science settings, 57 in health agencies, 38 in medical schools, and 20 in hospitals and clinics. Among the four settings comparisons were made of the proportion of project directors who had no administrative superior, the proportion who did or did not discuss the specific areas of research with their administrative superior, and the extent of influence of the administrative superior over given areas of research. The following pattern of freedom emerged: the health institutions (health agencies and hospitals) were the least free with maximal executive authority, and the educational institutions (universities and medical schools) were the most free with minimal executive authority. Tables 1 and 2 are illustrative of the degree of freedom in these institutions.[4]

If Wolfle is correct and maximal freedom does lead to highly creative scientific accomplishment, then of these four institutions one would expect the university and medical school to be most innovative. Ben-David, however, referring to the work of Koch, Pasteur, Villemin, Devaine, Freud, and others, argues that academically marginal settings provide greater impetus for innovation than do purely academic settings.[5] Gilfillan, in *The Sociology of Invention,* also supports the position that marginal research settings lead to innovation.[6] If Ben-David and Gilfillan are correct, and contrary to the prediction derived from Wolfle's argument, one would expect that social research would be least innovative when conducted in academic social-science settings and most innovative when conducted in the more academically marginal medical school, hospital, and health-agency settings.

To resolve this contradiction, evaluations of the innovativeness of projects conducted in each of the above settings were compared. In order to obtain these evaluations, the director of each project was asked to designate the

Table 1. Summary of Influence of Administrative Superior concerning the Allocation of Funds, Hiring of Personnel, and Formulation of the Research Design

		Percentage		
Institution	*N*	*Administrative influence*	*No administrative influence*	*No answer*
Health agency	57	63	35	2
Hospital	20	50	45	5
Medical school	38	31	68	1
University	64	39	58	3

Note—The difference between the educational institutions (medical school and university) and the health institutions (health agency and hospital) is significant at $p < .05$, z-test.

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[2] For a detailed discussion of the literature on scientific accomplishment, see Anne P. Folger and Gerald Gordon, "Scientific Accomplishment and Social Organization: A Review of the Literature," *American Behavioral Scientist,* VI (December, 1962), 51–58.

[3] Health Information Foundation, *An Inventory of Social and Economic Research in Health* (Editions III–IX [New York: Health Information Foundation, 1954–60]).

[4] For a fuller description of the methodology and data-gathering process see Gerald Gordon, Sue Marquis, and O. W. Anderson, "Freedom and Control in Four Types of Scientific Settings," *American Behavioral Scientist,* VI, No. 4 (December, 1962), 39–43.

[5] Joseph Ben-David, "Roles and Innovations in Medicine," *American Journal of Sociology,* LXV, No. 6 (May, 1960), 557–68.

[6] S. C. Gilfillan, *The Sociology of Invention* (Chicago: Follett Publishing Co., 1935), pp. 88–91.

Table 2. Percentage of Projects in Which the Administrative Superior Influenced Allocation of Funds, Hiring of Personnel, and Formulation of the Research Design

	Percentage		
Institution	*Funds*	*Personnel*	*Research design*
Health agency	74	60	54
Hospital	50	45	55
Medical school	32	32.5	21
University	42	31	47

major report(s) resulting from his study. We then summarized the report(s) indicated. After we validated the use of ratings of summaries instead of ratings of the major reports themselves,[7] the summaries were placed in booklets of twenty-five randomly selected summaries for evaluation. The evaluators were persons chosen as leaders in medical sociology by the members of the Section on Medical Sociology of the American Sociological Association. The mean number rating each project was 4.5.

For each project the evaluators indicated their rating of each of four criteria on a ten-point scale. As a guideline in making the ratings of innovation, the evaluators were asked to indicate:

> How innovative you feel the research is—the degree to which the research adds to our knowledge of illness through the development of new theory or findings not explicit or anticipated in previous theories or findings and/or adds to the development of new methods of research. The reference points for this rating should be what you feel is the general level of innovation for studies dealing with the social-psychological aspects of illness.

According to their rating the projects were divided into fifths—quintile 1 indicating the lowest ratings of innovation and quintile 5 the highest ratings of innovation.

A comparison of the percentage of the projects in the fifth or most innovative quintile clearly revealed that research conducted in academic social-science departments was judged less innovative than in the other three institutions (Table 3). Only 8 per cent of the projects conducted in the university were in the most innovative quintile compared to 18 per cent, 32 per cent, and 30 per cent, respectively, for the health agency, hospital, and medical school.

Table 3. Percentage of Projects in Each Institution in Most Innovative (Fifth) Quintile

Institution	*No. of projects*	*Percentage in fifth quintile*
Health agency	55	18
Hospital	19	32
Medical school	37	30
University	60	8

Note—Difference between university and health agency, $p<.10$; between university and hospital, $p<.02$; between university and medical school, $p<.02$ (z-tests).

To determine if the nature and type of research carried on in the different types of institutions rather than the differences between the institutions per se accounted for the differences noted, the following factors were controlled: variables studied, researcher's discipline, disease interest, diffuseness or specificity of the research problem, theoretical or problem orientation, and purpose of the research. In all cases the pattern of innovation previously noted remained constant. Table 4 is illustrative of the consistency of the pattern.

The findings support Ben-David and Gilfillan's belief that marginal research settings stimulate innovation. But the question remains: How and in what manner do marginal settings stimulate innovation? Ben-David provides insight into this question, stating: "Practice . . . is an invaluable guide in locating relevant problems—rather than finding illusory ones, which happened not infrequently in the history of academic thinking—and in adapting existing methods or devising new ones. The problems of practice are always real, and it usually possesses a tradition which is the result of a long collective process of trial and error and which may suggest the way toward new theory and

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[7] The extent of agreement between the ratings based on the summaries and the rating based on the major reports was twice that expected by chance ($p<0.025$). For further information see Gerald Gordon, "The Problem of Assessing Scientific Accomplishment: A Potential Solution," *IEEE Transactions on Engineering Management*, Vol. Em-10, No. 4 (December, 1963), 192–96.

Table 4. Percentage of Projects, by Research Purpose, in Most Innovative (Fifth) Quintile for Each Setting

	Health agency		*Hospital*		*Medical school*		*University*	
	*N**	*%5Q*†	*N*	*%5Q*	*N*	*%5Q*	*N*	*%5Q*
Delineation of problem	19	16	4	25	9	67	19	5
Causal relationship	9	11	3	33	15	33	16	12
Factors relating to problem solution	11	45	10	40	7	0	13	0
Problem solution	5	20	1	0	1	0	3	0
Evaluation	7	0	2	0	1	0	4	0
Methodology	2	50	0	0	2	0	3	33

* *N* is the number of projects with a given research purpose in each institution.
† "%5Q" refers to the percentage of these projects in the fifth or most innovative quintile.

new methods."[8] Thus, following Ben-David's reasoning, research in the academic setting is less innovative in comparison to the hospital, medical school, and health agency because there is less direct contact with the population affected by the research, the patients, and with the practitioners, the doctors, to whom the research is most relevant. The more contact an individual or organization has with the population served, the easier it is to measure or assess the actual or probable consequences of research. Practice, we feel, is "an invaluable guide in locating relevant problems [and] suggests the way to new theory and methods" because it increases the visibility of consequences of research.[9]

The location of relevant problems, however, does not necessarily mean creative solutions will follow. Both Barber[10] and Kuhn[11] have documented the resistance to innovation within science. Their evidence indicates that in terms of the repercussions of both failure and success it is often more dangerous to innovate that to follow habitual or accepted research paths. Kuhn, in fact, argues that generally the reward pattern of normal science induces conformity rather than originality.[12] This perhaps explains our finding that medical sociologists in the relatively free environment of the university were less innovative than their colleagues in the other institutions. Given a research environment that allows freedom of choice, many scientists choose safe rather than dangerous but original research paths. If the resistances to innovation are not overcome, the effect of facilitating conditions such as freedom obviously is reduced. For some researchers, inner curiosity and other internal drives may overcome the resistances to innovation. But for many scientists innovation or non-innovation is *dependent* upon external constraints.

We believe that the visibility of research consequences, in addition to aiding in the location of relevant problems, is one of the more important factors in overcoming the resistance to innovation.[13] For instance, in an organizational setting where the owner of an organization or his representative can accurately evaluate the findings of a project in terms of organizational goals, he can encourage the researcher who shows high probability of solving such problems. Also he can reward the researcher in relation to the extent to which the researcher aids in problem solution. As a consequence, the researcher is motivated to seek solutions to difficult but "relevant problems" in preference to less relevant but easier problems. In seeking a solution to the difficult problems the researcher at times must abandon traditional methods and thinking. This would appear to be as true for the academic as for the non-academic researcher. Kuhn, for instance, has observed that "the novel theory seems a direct response to crises."[14]

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[8] Ben-David, *op. cit.*, p. 558.

[9] *Ibid.*

[10] Bernard Barber, "Resistance by Scientists to Scientific Discovery," *Science*, CXXXIV, No. 3479 (September, 1961), 596–602.

[11] Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962), p. 150.

[12] *Ibid.*, p. 80.

[13] The discussion of consequences and freedom is based on the theoretical perspective developed in Gerald Gordon and Selwyn Becker, "The Entrepreneurial Theory of Formal Organizations" (mimeographed working paper, University of Chicago, 1965).

[14] Kuhn, *op. cit.*, p. 75.

On the other hand, where it is difficult for an administrator to relate the results of research to the attainment of organizational goals, the difference between the competent and the better solution becomes blurred and it is more difficult for the administrator to reward according to the extent to which the researcher attains the organizational goals. As a consequence, the constraint toward seeking the best solution is reduced, and extra impetus to go beyond competent but traditional research methods is lacking.

Marginal institutions, such as the medical school, hospital, and health agency, allow for more visibility of consequences than the university because they are in closer contact with the population served by the research. Further, academic goals such as increasing the general fund of knowledge tend to be more nebulous than the goals of institutions primarily concerned with practice or profit. The more obscure the goal, the more difficult is the assessment of consequences. If, as we hypothesize, visibility of consequences is a major impetus to innovation, then the relationship between marginality and innovation noted by Ben-David, Gilfillan, and ourselves arise, in part at least, from the greater visibility of research consequences in the marginal settings. Conversely, it would be predicted that without such an impetus the effect of facilitating conditions such as research freedom will be reduced. To test both hypotheses we attempted to determine the effect of variations in visibility of consequences and freedom upon innovation for each of the four types of research organizations investigated. This analysis was based on the assumption that, no matter how readily consequences can be assessed, they are not visible until someone assesses them.

The projects were classified according to visibility of consequences and freedom on the basis of the responses of the project directors to the following questions:

> Whom did you consider to be your administrative superior—the person to whom you were in any way responsible and who bore some responsibility for your research activity? (if no one, check here ______)

Those who had an administrative superior were asked if they discussed the following points with him:

- *a*) Research methodology employed in the study
- *b*) Purpose of the study
- *c*) Definition of the problem
- *d*) Interpretation of the research findings

The project directors were also asked to indicate on a ten-point scale from 1 (not at all) to 10 (completely) the extent to which their administrative superior influenced:

- *a*) Allocation of the research funds
- *b*) Hiring of personnel
- *c*) Formulation of the research design

The projects were divided into three major groups:

1. Projects in which the project directors either stated they had no administrative superior or they did not discuss their research with their administrative superior (Low visibility of consequences + Freedom)
2. Projects in which the project directors had freedom to specify their research procedures and they discussed their research with their administrative superior (High visibility of consequences + Freedom)
3. Projects in which the project directors stated that they had an administrative superior with whom they had discussions and who consistently influenced procedures (High visibility of consequences + Limited Freedom)

If our hypotheses regarding the effect of visibility of consequences is correct, then in the marginal institutions of the three types of authority patterns, type 2 would tend to maximize innovation while types 1 and 3 would minimize innovation. However, as previously discussed, in the university the organizational setting limits the visibility of research consequences, and therefore even if assessments are attempted, visibility of consequences tends to be low. Consequently, we would expect that variations in authority pattern would have much less impact upon innovation in the university than in the marginal institutions.

The consistency of the trends in the data in the direction of our hypotheses is striking. For example, as predicted, where consequences are difficult to assess, the amount of freedom accorded the scientist had little effect on inno-

Table 5. Percentage of Projects, by Authority Pattern in Each Setting, in Most Innovative (Fifth) Quintile

	No research discussion		*Research discussion with freedom*		*Research discussion with little freedom*	
Institution	*N*[*]	*%5Q*[†]	*N*	*%5Q*	*N*	*%5Q*
Health agency	6	0	31	19	15	13
Hospital	3	67	11	36	4	0
Medical school	14	7	20	45	3	33
University	23	9	21	10	12	8

[*] *N* is the number of projects with a given authority pattern in each institution.

vation. As can be seen in Table 5, in the university where it was postulated that under all conditions consequences are relatively obscure, not only was there much less innovation than in the other three settings but the authority patterns appeared to have little effect on innovation. Looking at the authority patterns in the other settings where consequences are visible and freedom is accorded the scientist, the predicted pattern of innovation is seen. The one exception to the pattern (project directors with no administrative superiors in the hospital) occurs in a cell with only three cases. A difference of one project would change the relationship.

Table 6. Percentage of Projects for Combined Institutions and Authority Patterns in Most Innovative (Fifth) Quintile

	Authority pattern			
	Ideal[*]		*Non-ideal*	
Setting	*N*	*%5Q*	*N*	*%5Q*
Marginal	62	31	45	13
Non-marginal	21	10	35	9

[*] Visibility + Freedom.

Significance of Differences (z-Tests)

	p
1. University–Ideal versus Marginal–Ideal	<0.03
2. University–Non-ideal versus Marginal–Ideal	< .006
a) University–No Discussion versus Marginal–Ideal	< .02
b) University–Little Freedom versus Marginal–Ideal	< .05
3. Marginal–Non-ideal versus Marginal–Ideal	< .02
a) Marginal–No Discussion versus Marginal–Ideal	< .05
b) Marginal–Little Freedom versus Marginal–Ideal	<0.06

Where the three marginal institutions were grouped together as shown in Table 6, it was found that the percentage of innovation under the ideal authority pattern was two and a half times that under the non-ideal conditions (31 per cent under the ideal condition, 13 per cent with no administrative superior or no discussion, and 14 per cent with administrative influence). The difference between the ideal and non-ideal conditions is significant at $p < 0.02$. The ideal condition in the marginal institutions (High visibility + Freedom) has three times as many highly innovative studies than occurred under any condition in the university. This difference is significant at $p + 0.02$. On the other hand, for those marginal institutions where visibility of consequences was low, the difference between the marginal and academic institutions was less than 4 per cent. Nevertheless, even the non-ideal marginal situations tended to be slightly more innovative than the academic institutions—indicating that, in addition to visibility of consequences, other factors associated with the marginal settings may also be positively related to innovation.

In sum, it is not possible to make a blanket statement relating maximal freedom to innovation, but rather maximal freedom is conducive to innovation only when there is an impetus to innovate. It further appears that the institutional settings in which research is conducted—in particular the visibility of the consequences of the research in relation to the goals of the owners of the institution—has a significant effect on inducing innovation. Our findings also challenge the assumption that, except in unusual instances, academic environments stimulate creative scientific achievement.

The evidence presented by Ben-David, Gilfillan, and ourselves in regard to the effect of marginal settings on scientific accomplishment refers primarily to new and developing areas in the social and natural sciences rather than to well-developed scientific areas. On the other hand, recent evidence by Pelz and Andrews indicates that our hypothesis in regard to the relationship between visibility of research consequences, freedom and innovation is valid for established as well as emerging research disciplines.[15] It is possible, however, that in mature sciences the existence of highly developed theoretical frameworks serve both to identify meaningful research problems and to make the consequences of research highly visible. If this is true as a science developed, the effect of marginal settings upon innovation would decrease. But extending Kuhn and Barber's thesis, one would argue that the more entrenched a theoretical framework, the greater would be the resistance by academicians to research calling into question the theoretical framework.

Speculation along this line leads to further questions in regard to the differential effect of academic and marginal settings upon scientific accomplishment. For instance, is the university a supportive environment for the highly creative, internally driven researcher? Does the effect of marginal and university environments on innovation differ when research appears to have some immediate relevance to society and when the relevance of the research appears less immediate? Do sciences in different stages of development require different environments? These questions just begin to scratch the surface of the highly complex pattern of activities subsumed under the name "science."

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[15] Don Pelz and Frank Andrews in *Scientists and Organizations: Productive Climates for Research* (New York: John Wiley & Sons, 1966). Pp. 35–53 present additional evidence confirming our hypotheses in regard to the relationship between visibility of consequences and scientific accomplishment. They state, "Gordon and an associate, Selwyn W. Becker, suggest that innovation is more likely where 'consequences are visible' (represented in his data by a superior who keeps in touch), but where at the same time the researcher has freedom (is not dominated by the superior). In our data [physics labs, R & D labs, chemical labs, etc.], involvement of several decision-making sources should increase visibility of consequences; and the individual's influence guards against domination by one superior. Under this combination, we found performance to be highest."

The Local-Cosmopolitan Scientist

Barney G. Glaser

Several studies in the sociology of occupations and of organizations have concluded that some professionals in organizations tend to assume a "cosmopolitan" orientation that manifests itself in their working for professional goals and the approval of colleagues throughout their professional world, in focusing on a professional career, and in a concomitant lack of loyalty to and effort for the organization. Other professionals tend to assume a "local" orientation that manifests itself in their lesser commitment to the profession, in more concern with the goals and approval of the organization, and in focusing on an organizational career.[1] With the growing movement of

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[1] The terms "cosmopolitan" and "local" were first used by Merton to describe different types of community leaders (Robert K. Merton, *Social Theory and Social Structure* [Glencoe, Ill.: Free Press, 1957], pp. 387–420). For a formulation of cosmopolitan and local as organizational types see Alvin W. Gouldner, "Cosmopolitans and Locals: Toward an Analysis of Latent Social Roles," *Administrative Science Quarterly*, II (1957–58), 281–306, 444–80; see also Alvin W. Gouldner, "Organizational Analysis," in Robert Merton, Leonard Broom, and Leonard Cottrell (eds.), *Sociology Today* (New York: Basic Books, 1959), pp. 410–19. For particular studies, see Peter M. Blau and W. Richard Scott, *Formal Organizations* (San Francisco: Chandler Publishing Co., 1962), pp. 64–74; Leonard Reissman, "A Study of Role Conceptions in Bureaucracy," *Social Forces*, XXVII (1949),

scientists into research organizations, there has been some interest by sociologists of science in studying the many problems and strains generated by the often conflicting professional and organizational demands and practices that, in turn, generate the adoptive cosmopolitan and local types of orientations.[2] A partial list of these problems might include varying incentive systems, differential emphasis on publication of research results, types of authority and supervision related to the professional need of autonomy, divergent and conflicting influences on work situations, assignments and research problem choices, budgets of time and money, kinds of compatible work groups, focus of performance, multiple career lines and commitments.

The major goals of many research organizations, particularly industrial research organizations,[3] are, of course, not consistent with the major institutional goal of science: advancing knowledge by basic research. They often emphasize goals of application, product development, and expert service. The scientist seeking a professional career (one based on pursuing the institutional goal) in an organization of this type becomes a "cosmopolitan," by and large directing his efforts to professional goals, rewards, and careers. Insofar as the cosmopolitan is always looking within the community of research organizations for better professional positions and conditions[4] and has little "local" loyalty to inhibit his mobility, the result is a high organizational turnover. A professional career may be impeded by a too long stay in the industrial context. Indeed, insofar as the industrial organization needs basic research, it becomes detrimental for it to try to induce the cosmopolitan to focus his efforts on the major organizational goals—product development, application, and service—since that refocusing may reduce the quality of his basic research contributions.[5]

Whereas studies of industrial research organizations have usually found scientists who have either a primarily local *or* cosmopolitan orientation, I shall try to demonstrate a local-cosmopolitan orientation among highly motivated scientists in an organization devoted to the institutional goal of science. The congruence of goals reduces in considerable measure, if not completely, the strains between organizational and professional requirements that tend to generate distinct local and cosmopolitan types. My principal criterion for ascertaining the general orientation of these investigators will be the directions of their work effort. First, I investigate the general performance-reward process of science; then I investigate the efforts of those who do well in their scientific performance to meet organizational demands. From these findings on their professional and organizational contributions, I infer that the orientation of these scientists is both local *and* cosmopolitan. I conclude with a discussion of the implications of this formulation for the developing theory about local and cosmopolitan orientations of professionals in organizations.

The data for the analysis consist of answers given to survey questionnaires in 1952 by the total resident research staff (332) of a large government medical research organization devoted to basic research. In addition, some letters and documents give further information

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p. 308; Theodore Caplow and Reece J. McGee, *The Academic Marketplace* (New York: Basic Books, 1958), p. 85 and *passim;* Harold Wilensky, *Intellectuals in Labor Unions* (Glencoe, Ill.: Free Press, 1956), pp. 129–53; Warren G. Bennis *et al.*, "Reference Groups and Loyalties in the Out-Patient Department," *Administrative Science Quarterly*, II (1958), pp. 481–500.

[2] William Kornhauser, *Scientists in Industry* (Berkeley: University of California Press, 1962), esp. chap. v; Simon Marcson, *The Scientist in American Industry* (New York: Harper & Bros., 1960); Donald C. Pelz, "Some Social Factors Related to Performance in a Research Organization," in Bernard Barber and Walter Hirsch (eds.), *The Sociology of Science* (New York: Free Press of Glencoe, 1962), p. 357; Herbert A. Shepard, "Nine Dilemmas in Industrial Research," *Administrative Science Quarterly*, I (1956), 346; Hollis W. Peter, "Human Factors in Research Administration," in Rensis Likert and Samuel P. Hayes, Jr. (eds.), *Some Applications of Behavioral Research* (Paris: UNESCO, 1957), p. 142; Clovis Shepard, "Orientations of Scientists and Engineers," *Pacific Sociological Review*, Fall, 1961, p. 82. Robert Avery, "Enculturation in Industrial Research," *IRE Transactions in Engineering Management*, March, 1960, pp. 20–24; Fred Reif, "The Competitive World of the Pure Scientist," *Science*, CXXXIV (1961), 1959.

[3] Kornhauser, *op. cit.*, p. 133; Leo Meltzer, "Scientific Productivity in Organizational Settings," *Journal of Social Issues*, No. 2 (1956), p. 38; Marcson, *op. cit.*, pp. 81–82, 104; Shepard, *op. cit.*, p. 347.

[4] Kornhauser, *op. cit.*, p. 130.

[5] *Ibid.*; see also Shephard, *op. cit.*, and Pelz, *op. cit.*, p. 358.

on the organization. My demonstration will be an effort to *explore* for plausible relations between variables, *not* to develop a strong case built on hard fact. While secondary analysis is well suited for exploratory work, to achieve the latter with old data is probably impossible. Accordingly, I shall use somewhat crude indexes and consider small differences that are consistent, highly suggestive, and that lead to an integrated picture of the local-cosmopolitan process. Since I am only suggesting, not testing, my language will not be riddled by the qualification rhetoric required in more rigorous demonstrations; my inferences will be designed to guide future research on local-cosmopolitan theory along (I believe) useful lines; and my primary effort will be to generalize as opposed to describing a real situation in detail.

THE PERFORMANCE-REWARD PROCESS

Motivation. In the institution of science perhaps the most important goal for the *typical* scientist is to advance the knowledge in his field by some form of basic research. A scientist, especially in training but throughout his career, is constantly reminded by colleagues that it is his job to advance knowledge by some increment, large or small. He internalizes the goal, and becomes, using Parson's term, "institutionally motivated" to achieve it.[6] Therefore, before we know anything about the distinctive personality of this or that scientist, we can hypothesize that to some degree he will be motivated to advance knowledge by virtue of his professional training and that his research performance will tend to vary directly with the degree of his institutional motivation.

Insofar as the research scientist is motivated to advance knowledge, both his *research work* on problems, hypotheses, and methods as well as his *results* are centrally involved because he has the potential for advancing knowledge at either stage.[7] Irrespective of failures in results, he may have been quite original in his research work, and vice versa, he may have run rather a routine project into a contributing result.

As a measure of motivation to advance knowledge, I have selected the following two items that tap the (*a*) work and (*b*) result stages of the advancing knowledge process.[8]

> "How much do you want? How important is (it) to you?"
> (*a*) Freedom to carry out my own ideas; chance for originality and initiative.
> (*b*) Contributing to basic scientific knowledge.
>
> Degree of importance: (1) utmost, (2) considerable, (3) some or little, (4) no opinion.

Over half the investigators felt both freedom and contributing were of the utmost importance. Each item was dichotomized between "utmost" and the remaining categories since this was where the direction of association consistently changed in cross-classification with criterion variables. The two items were fairly strongly related (coefficient of association = .70). Investigators were considered to have high motivation if they felt both freedom in work and contributing results were of the utmost importance. Fifty-six per cent (186) of them were in this category. Among those of lower motivation, 27 per cent (89) were high on one item and 17 per cent were low on both items.

For further analysis I dichotomized the index into high and low, distinguishing those

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[6] Institutional motivation has been dealt with extensively in: Talcott Parsons, ***Essays in Sociological Theory*** (Glencoe, Ill.: Free Press, 1954), chaps. ii, iii; Merton, *op. cit.*, pp. 214, 531, 555, 558–59; Robert K. Merton, "Priorities in Scientific Discovery," ***American Sociological Review***, December, 1957, pp. 640–41. It should be noted that advancing knowledge as I deal with it here is institutional, a part of a normative pattern, not a mode of orientation that is simply natural to man. Thus, I make the distinction between institutional motivation (motivation based on internalized norms and goals) and typical human motives (assertive, friendly, ambitious, egotistic, etc.) as elements of concrete motivation.

[7] Advancing knowledge is a process that, for any one scientist, is composed of many events. This process has at least two broad stages: research work and research results. Bernard Barber, in talking of "inventions and discoveries," says "they have two aspects, that of process and that of products, and these aspects must be distinguished" (*Science and the Social Order* [Glencoe, Ill.: Free Press, 1952], p. 193).

[8] I follow the procedure for index construction outlined and discussed by Paul F. Lazarsfeld, in Merton, Broom, and Cottrell (eds.), *op. cit.*, chap. ii, pp. 47–67; in "Evidence and Inference in Social Research," ***Daedalus***, LXXXXVII, No. 4 (1958), 100–109; and with Wagner Thielens, ***The Academic Mind*** (Glencoe, Ill.: Free Press, 1958), pp. 402–7.

who were high on both items from all others. Three justifications for this are: (1) In many cross-classification checks, the middle group proved to be more like those low on both items than those high on both items. Therefore, the index is reducible on statistical evidence.[9] (2) We only need a dichotomized variable to establish general relations between variables. (3) The dichotomization is at the median, saving cases for necessary cross-tabulation.

Performance. The performance score (developed three months after the survey data were collected) consisted of the assessments by colleagues in the work situation of each investigator's current research.[10] Each assessment was based on five criteria: (1) originality and creativeness, (2) wisdom and judgment, (3) rigor of thought and precision of methods, (4) persistence, industriousness, and efficiency, and (5) contribution to the work of others. Three criteria (2, 3 and 4) focus directly on the research work, and two (1 and 5) focus mainly on research results. Thus, this index is based on the same aspects of advancing knowledge as the motivation index. Bearing out my hypothesis on the positive relation between motivation and performance, 19 per cent more of the highly motivated scientists (compared to those with less motivation) have been judged by their colleagues to have high quality performance.

Recognition. Concomitant with the development of institutional motivation is the expectation of reward for achievement of the institutional goal.[11] The strong institutional emphasis of science on this achievement-reward pattern is noted by Merton: "originality can be said to be a major institutional goal of modern science, at times the paramount one, and recognition for originality a derived, but often as heavily emphasized, goal."[12]

The institutional emphasis on professional recognition holds for the research organization under study.[13] A memo to all personnel described the promotion process as follows:[14] The immediate supervisor recommends the investigator to the institute director for promotion. If the latter agrees, he recommends the investigator's case to the promotion board. The board then thoroughly examines the investigator. A sample of his publications is read; prior and current supervisors are asked about him; and his qualifications are judged in terms of the following criteria: (1) quality of work he has been engaged in, (2) capacity to develop, (3) capability in relation to other investigators, (4) reputation in his field, (5) personal characteristics and ability to get along with others, and (6) ability in the non-scientific work associated with his present and prospective position. If he passes this examination, he is recommended for promotion to the director of the organization, who follows the advice of the board in most cases.

The first four criteria clearly relate to the investigator's professional recognition by focusing on his past, present, and potential ability to advance knowledge. I have shown elsewhere that professional recognition is also positively linked with getting along with others and with accomplishing non-scientific work.[15] Given the

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[9] On reduction of property space see Alan Barton, "The Concept of Property Space in Social Research," in Paul F. Lazarsfeld and Morris Rosenberg (eds.), *The Language of Social Research* (Glencoe, Ill.: Free Press, 1955).

[10] This performance score cannot be construed as a measure of recognition, since, to be sure, the scientists were not made aware by the research team of their colleagues' evaluations. The essence of recognition is that it is a *known* reward for one's work. For a complete discussion of the construction of this index of research performance see Donald C. Pelz *et al.*, *Human Relations in a Research Organization* (Vol. II; Ann Arbor: Institute for Social Research, University of Michigan, 1953), Appendix C; and *Interpersonal Factors in Research* (Ann Arbor: Institute for Social Research, University of Michigan, 1954), Part I, chap. i, Appendix A.

[11] See Parsons, *op. cit.*, pp. 53–54, 143–44, 230–31, 239, for the formulation that institutional norms reciprocally define relations between two classes of people or two positions.

[12] Merton, "Priorities . . . ," *op. cit.*, p. 645.

[13] This is not the only government medical research organization that bases promotions on professional recognition. There would seem to be many others. Meltzer reports for his national sample of 3,000 physiologists that publication productivity for those in government was the same as those in the university and that publication was as strong a factor in promotions in both contexts (Meltzer, *op. cit.*).

[14] Charles V. Kidd, "Resolving Promotion Problems in a Federal Research Institution," *Personnel Administration*, XV, No. 1 (1952), 16.

[15] See my *Organizational Scientists: Their Professional Careers* (Indianapolis: Bobbs-Merrill, forthcoming), chaps. vi and vii, and see below for the relation of performance process to accomplishing non-scientific work.

emphasis on professional recognition for advancement, it seems reasonable to assume that this reward (recognition) for achievement will maintain motivation for further achievement.

The promotion process clearly indicates the importance of two types of professional recognition: (1) the immediate supervisor's evaluation and (2) publications. Therefore if each type of recognition is measured and combined in an index, we can approximate completeness in measuring both the fundamental range of professional recognition required by the organization, and an important patterned form of professional recognition for research work and results. Thus all three indexes are based on the two broad stages of advancing knowledge.

The questionnaire did not include information on actual supervisor's evaluation nor did it include information on actual publications (extent or quality). It did include two items that measure *felt* recognition from supervisors and in publications. They are:

A. "How do you feel about the way your chief makes *evaluations* about the *quality of work you are doing?*" (1) accurate, (2) partly accurate, (3) no attempt, (4) no answer.

B. "In scientific or other professional papers about work to which you have made some contribution, *is proper credit given to your own contribution* by means of authorship or acknowledgment?" (1) always, (2) usually, (3) seldom, (4) no opinion.

Over half the investigators feel they receive adequate recognition from the supervisor (53 per cent say "accurate") and in publications, whether by authorship or acknowledgment (72 per cent say "always"). To construct an index of felt professional recognition I have dichotomized each item between the highest category and all others. This dichotomization occurs as close to the median as possible, and at a statistical breaking point. In many cross-classifications of each item with other variables, the direction of association consistently changed between the highest category and the remaining categories. In combining these two variables into an index of felt recognition, 44 per cent of the investigators are high on both items; 37 per cent of the investigators are high on one item; and 19 per cent are low on both items. I have dichotomized the index between high and all others (low) for the identical statistical and substantive reasons earlier applied to the motivation index.

As suggested, professional recognition tends to maintain institutional motivation in this organizational context. Nineteen per cent more of those scientists who feel they have achieved high recognition (compared to those with low recognition) are highly motivated to advance knowledge.[16]

Process. The next step is to show in one table the following process: recognition for advancing knowledge (which indicates past performance) tends to maintain motivation (a time sequence based on common observation), which in turn tends to result in high quality research performance (measured three months later). This will give us the basic links of the circular, general performance-reward process in science: research performance leads to professional recognition, which maintains motivation to advance knowledge, which in turn leads to more performance.

In Table 1 the magnitude of association between recognition and performance is *diminished* when the intervening effect of motivation is removed. Therefore, high motivation tends to be a link between attaining recognition and accomplishing high quality research performance, thus tentatively demonstrating the performance process.[17] As a social pattern, this circular process will continue if the performance measured here results anew in recognition.

At this point I wish to suggest that, besides research performance, it is also possible to predict behavior associated with research on the basis of intensity of institutional mo-

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[16] For other evidence that recognition supports motivation see Donald C. Pelz, "Motivation of the Engineering and Research Specialists" (*General Management Series*, No. 186 [New York: American Management Association], p. 30). He reports that for a national sample of 3,000 physiologists, the number of publications and acknowledgements is positively related to intensity of motivation.

[17] Various sources exist for a full discussion of Lazarsfeld's elaboration analysis of which this is the MI type. For the primary source see Paul F. Lazarsfeld, "Interpretation of Statistical Relations as a Research Operation," in Lazarsfeld and Rosenberg (eds.), *op. cit.;* see also Lazarsfeld and Patricia L. Kendall, "Problems of Survey Analysis," *Continuities in Social Research*, eds. Lazarsfeld and R. K. Merton (Glencoe, Ill.: Free Press, 1950), and Herbert Hyman, *Survey Design and Analysis* (Glencoe, Ill.: Free Press, 1955), chap. vii. . . .

Table 1. Recognition, Motivation, and Performance

	Recognition (per cent)		
	Average	*Less*	*Difference*
High performance	56 (144)	44 (188)	+12
Proportion with high performance and:			
High motivation	60 (96)	53 (90)	+ 7
Low motivation	46 (48)	37 (98)	+ 9

tivation. This is borne out by one indicator of research behavior: the amount of time in a typical work week the scientist puts into "performing my own professional work (or work under the guidance of my chief) such as research, professional practice, professional writing, etc." Fifteen per cent more of the highly motivated investigators work 21 hours a week or longer on personal research. Furthermore, 11 per cent more of those who work 21 or more hours a week on their own research have a high quality performance score.

In combining motivation, personal research time, and performance, Table 2 demonstrates that the highly motivated investigators will tend to put more time into their own research work, and that this time, in turn, will tend to result in higher quality performance. The magnitude of association between motivation and performance is *diminished* when the intervening effect of personal research time is removed. This finding adds a subsidiary link to the performance-reward process. . . .

SCIENTISTS AS ORGANIZATIONAL MEN

As a link in the performance process, time in own research has direct relevance to the research organization. Insofar as this process supposedly results in the continual fulfillment of the institutional goal of advancing knowledge, one might be tempted to say that this is favorable for the organization since this is why the research organization has been created. *But is the process favorable?* Scientists in any organization have other activities and duties, besides their own personal research, that must be accomplished as part of their organizational commitment. The typical investigator cannot be his own scientist all week long, as is indicated by the fact that the median number of hours put into "own professional work" in a typical week is 29.8.

The question, therefore, arises as to whether investigators with high motivation sacrifice their other organizational commitments for their personal research because of strong desires to advance knowledge.[18] If they do, and since this factor is a link in the performance process, then perhaps the above findings have unfavorable consequences for the organization. This process may require too much time for personal research, which may be disruptive for the organization as regards the scientists' fulfilling their other organizational commitments.

Table 3 provides one answer to this question. The extra time that the highly motivated scientists put into their own research is carried forward, as their weekly time schedule accumulates, with no sacrifice to other professional and organizational activities or commitments. The longer hours put into their own research (15 per cent difference) are maintained by highly motivated investigators as time is consecutively added on (1) for other professional productive work (14 per cent difference), such as performing services for others and working with close colleagues; (2) for nonproductive professional work (21 per cent

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[18] That this is an important consideration for the organization is indicated by one of the six criteria used in evaluating the scientists for potential promotions: "Writing or editorial ability, effectiveness on boards and committees, ability to organize his and others' work, administrative judgment and other traits relevant to his performance on his current job and the job for which he is being considered" (Kidd, *op. cit.*). This criterion indicates that the scientist's worth to the organization is based also on the non-scientific work he has been asked to do.

Table 2. Motivation, Research Time, and Performance

	Motivation (per cent)		
	High	*Low*	*Difference*
High performance	57 (186)	38 (146)	+19
Proportion with high performance who put:			
Twenty-one or more hours per week into own research	60 (142)	43 (89)	+17
Less than twenty-one hours per week into own research	48 (44)	35 (57)	+13

Table 3. Motivation and Work Activities

Consecutive addition of hours per week spent on various work activities	*Motivation (per cent)*		
	*High**	*Low*†	*Difference*
1. Own research: twenty-one or more hours	76	61	+15
2. Plus other professional productive work: thirty-six or more hours	63	49	+14
3. Plus non-productive professional work: forty-one or more hours	69	48	+21
4. Plus other organizational activities for total work week: fifty-one or more hours	65	48	+17

* $N = 186$.
† $N = 146$.

difference), such as attendance at meetings and seminars, reading and dealing with people other than close research associates; and (3) for a total work week (17 per cent difference), which includes all other organizational activities beyond their professional ones.

In fact, in response to the question, "How much time per week are you now spending on activities which could be shifted to other people or eliminated without impairing your present scientific or other professional work?" more highly motivated investigators suggested that less time be shifted to other people. Thus, in line with not sacrificing organizational work for their own research, the highly motivated investigators are less ready than those with low motivation to shift any additional work load or organizational life upon other men. Indeed, it would have been understandable if they had been more ready to shift activities not directly pertinent to their professional pursuits to other personnel, since they are motivated to advance knowledge, and any activity that intruded into this effort might appear burdensome. *It would seem, then, that high institutional motivation tends to make these scientists both hard-working investigators and hard-working organizational men.*

THE DISTINCTION BETWEEN COSMOPOLITAN AND LOCAL

This finding suggests that those scientists who are highly motivated to advance knowledge will be assets to the organization in two ways: (1) achieving the organizational goal, which is the same as the institutional goal of science, and (2) meeting non-scientific organizational requirements that take time from research. Thus, the organization will tend both to persist and to maintain its prestige (through accumulated individual successes) within the community of scientific organizations. The latter aim is very important for attracting and recruiting more capable, highly motivated scientists. Persistence and maintenance of prestige through achievements of the institutionally designated goal need not always be related. There are numerous examples in the literature that show that attempts to meet requirements for persistence can subvert organizational goals.[19]

This finding—that both research and nonresearch activities seem important and compatible to highly motivated scientists—*indi-*

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[19] The foremost example is Philip Selznick's *TVA and the Grass Roots* (Berkeley: University of California Press, 1953).

cates, by the criterion of direction of work efforts, that these scientists are both cosmopolitan and local oriented. They are oriented to achievement of the institutional goal and honorary rewards, and hence toward professional colleagues everywhere and toward success as members of their profession. They are also oriented to their responsibilities within the organization that provides them with the facilities for advancing scientific knowledge and thus gaining recognition, and with a prestigeful base for that cluster of organizational rewards called a promising career.

Further data support the presence of this dual orientation among highly motivated scientists. As hard-working cosmopolitans oriented to all professional colleagues they are more interested in contacts outside the organization as sources of information, in a move (if necessary) to a university environment (however, motivation does not account for more plans to move), and in belonging to an organization with prestige in the scientific world. Also, they feel a greater sense of belonging to and involvement with professionals within the organization. With respect to the professional or institutional goal, any suggestion of a change from basic research as the only organizational goal to its coexistence with applied research will be cause for concern.

As hard-working locals, the more highly motivated investigators desire an important job in the organization and association with persons who have high status and important responsibilities. In addition, more of them have a strong sense of belonging to the organization and are interested in higher level jobs that are most compatible with the institutional goal. That is, they tend to be interested in the supervision of subordinate scientists rather than in supervision of the organization.

In sum, this congruence of organizational and institutional goals generates a local-cosmopolitan scientist when the scientist is highly motivated to advance basic knowledge. Devotion to both profession and organization is, in this case, not incompatible, as it tends to be for scientists in industry.

LOCAL-COSMOPOLITAN THEORY

This dual orientation of highly motivated scientists is especially important since, with few exceptions, the research literature characterizes *scientists* as either cosmopolitan or local. They are presented as two distinct types of scientists whose orientations and activities are, if not directly opposed to each other, not related. Shepard, in discussing dilemmas in industrial research, has said, "The research staff itself is likely to be divided into what Robert Merton calls the 'cosmopolitans' and 'locals.'"[20] In his book on industrial scientists, Marcson reports that "it is possible to distinguish between two types of laboratory staff people—professionally oriented and organizationally oriented."[21] Peter reports on a seminar on problems of administering research organizations, "In the first two of the seminars, some time was spent discussing another bimodal distribution of scientists, those described as 'cosmopolitan' and those called 'locals.'"[22]

I suggest that cosmopolitan and local can also be seen as two dimensions of orientation of the same scientist, each activated at the appropriate time and place as determined by the organizational structure within which he works. The question now arises as to whether or not there is a conflict between my findings of a cosmopolitan-local orientation and the body of literature that treats the two orientations as distinct. Is one view more correct than the other? If we ask the question, "Under what conditions has each distinction emerged?" then we find that each of the views is accurate and applicable to the particular organizational situation under analysis.

The distinction between cosmopolitan and local scientists emerged during the study of research organizations in which the institutional goal of advancing knowledge is more or less in conflict with a major organizational goal of applying knowledge. For example, in reviewing industrial research organization studies, Shepard states that the scientist's "motto" is "How much do we know about this?" whereas the businessman's motto is "What is the value of this to the company?"[23] This conflict results in a "problem person" in the cosmopolitan and in a "good employee" in the local.

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[20] "Nine Dilemmas . . . ," *op. cit.*

[21] *Ibid.*, p. 18.

[22] *Ibid.*

[23] *Ibid.* A conflict in goals is also the criterion for separating local and cosmopolitan scientists used by Marcson, *op. cit.*, and Peters, *op. cit.*

Scientists take sides in the conflict according to their goal priority; hence the social scientist studying the organization uses this criterion to divide scientists into two groups. The cosmopolitan group makes trouble for management in primarily pursuing the institutional goal and career, and the local group creates little problem in primarily pursuing the company goal and career. In sum, this distinction is a device for understanding organizational problems such as communication of results, turnover, multiple career lines, differential incentive systems, needs for loyalty versus expertise, and so forth.[24]

Cosmopolitan and local as dual orientations of the same scientist emerged in our analysis of a research organization that emphasized the institutional goal. As there was little or no conflict between goals, there was no necessity to take a priority stand, or of being split into groups. Because of this congruence of goals, a local orientation helps to maintain the opportunity to pursue research and to have a career at a highly prestiged locale, both thoroughly consistent with the cosmopolitan orientation.[25] In using the notion of dual orientation, we end up talking of organizational benefits, not problems.

Further, I have found this dual orientation among highly motivated scientists, whereas Shepard, as well as the other authors cited, talks of all scientists. Thus, the two conditions that generate the emergence of either groups of cosmopolitan and local scientists, or scientists with a cosmopolitan-local orientation, are (1) compatibility of the organizational with the institutional goal, and (2) highly motivated scientists versus all scientists.

One of the exceptions to viewing local and cosmopolitan scientists as different groups in the literature on scientists is the "mixed type" offered by Kornhauser.[26] The "mixed type" is oriented to both company and profession and is interested in "facilitating the utilization of technical results." This applied orientation existed under the conditions of a conflict between the institutional goal and the company goal and is an accommodation seemingly in favor of the company. Thus, to date we have two general types of local-cosmopolitan scientists arising under different sets of specific conditions: (1) the basic research local-cosmopolitan and (2) the applied research local-cosmopolitan.

Table 4 locates the various general orientations of scientists to organization and/or profession likely to be generated by the two cited conditions: (1) congruence of institutional and organizational goals and (2) degree of institutional (or professional) motivation.

Table 4. Scientists' Orientation

Institutional and organizational goals	*Professional motivation*		
	High	*Medium*	*Low*
Same	Basic research Local-cosmopolitan		Local
Different	Cosmopolitan	Applied research Local-cosmopolitan	Local

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[24] That the distinction between types of scientists has much potential use in the analysis of problems surrounding the research organization's need for both loyalty and expertise is forcefully brought out in Gouldner, "Cosmopolitans and Locals," *op. cit.*, pp. 465–67.

[25] Blau and Scott, *op. cit.*, pp. 70–71, in comparing county agency caseworkers and Bennis' data on professional nurses, note that opportunities for a professional career in an organization coupled with restricted opportunities in competing organizations generate local orientations among professionals. Whether they still remain cosmopolitan or not was not discussed. Their analysis is, therefore, consistent with mine on the local dimension.

[26] Kornhauser, *op. cit.*, p. 122. Another exception is Avery's (*op. cit.*), "The career question confronting the technical man is not, typically, whether to commit himself wholly to localism or cosmopolitanism. Rather he is likely to be constrained to try to extract advantages from both sources." Gouldner (*op. cit.*), and Blau and Scott (*op. cit.*), also have mixed types in their tables but do not discuss them in text. They focus on the distinct groups. Caplow and McGee also note a mixed orientation among professors in high-prestige university departments (*op. cit.*), p. 85 (see also Warren G. Bennis, *op. cit.*, pp. 481–500).

Last, the concern among the scientists in this study over the potential organizational emphasis upon the applied research goal suggests a few hypotheses about possible changes. If the organization starts to emphasize applied research, those highly motivated to do basic research may give up the basic research cosmopolitan-local orientation and become a definite group of cosmopolitans. The professional motivation of some may drop a little and then they are likely to become applied research local-cosmopolitans. The potential conflict between institutional and organizational goals may generate these changes, which then could result not only in the loss of benefits to the organization cited in this paper but also in the accumulation of problems cited by those writers who have developed the distinction between cosmopolitan and local as two types of scientists.[27]

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[27] For an analysis of the generation of cosmopolitan and local factions because of a change in goals see Paula Brown and Clovis Shepard, "Factionalism and Organizational Change in a Research Laboratory," *Social Problems*, April, 1956, pp. 235–43.

C. Activities of Scientists External to Professional Work

Introduction

One of the interesting issues from the viewpoint of understanding the scientific man as an individual is the extent to which his unusual investment and commitment to work colors the other aspects of his life. Is his intense devotion to work also found in nonwork activities? What behaviors and attitudes does he show as a father and a husband? How does he spend his time outside the lab? What interests and hobbies does he pursue?

Actually few studies have been done on these aspects of his activities. What is known is that there is comparatively little interest and investment in nonwork relationships and activities. In fact, the main components of the scientist's sense of identity is drawn from his role at work. Apparently investigators, noting this characteristic in scientists, have also felt little pressed to study these nonwork-related activities. However the studies show that this is not an unimportant area since there are certain consistent relationships between work and nonwork behaviors that suggest that there is a good deal of feedback among the various aspects of a scientist's life. Also, certain changes in the scientist's definition of what he should be doing that are evident on the contemporary scene have antecedents in some of his earlier postures in regard to how he votes, whether he is an active church member, and what he does on his vacations.

In the first selection Eiduson has singled out from interviews with forty academic scientists, data on his sense of identification in his after-laboratory hours with other members of the university community. The segment represented here is devoted to his patterns of leisure and play. Interestingly, these data tell something about the continuity of early childhood interests over time. Leisure time activities are also described by the scientists with much of the intensity and enthusiasm associated with work. Eiduson suggests that play activities have taken on this positive affective component because they are rationalized by scientists as directly contributing to creative performance.

The Kerr, Newman, and Sadewic study is old, outdated, and methodologically hampered by an only 40 percent response rate to a questionnaire on retrospective worries and concerns. It is included here, however, because it is the only study that seems to have attempted to systematically deal with the types of worries that social scientists have and with the effects of age on their anxieties. The sample consisted of 103 older psychologists, median age 54. Economic worries were the most frequent, with a median remembered-peak at the age of 30. Kubie (1954), too, has found that unanticipated economic worries plague the young scientist, as much as do other psychological tensions. From ages 18 to 26 the typical respondent reported worries about idealism and personal development, but by age 30 a shift to problems of economic and other realistic survival worries had occurred. The shift to political problems came after age 40 for most subjects. One wonders how accurately these reflect today's scientific culture.

Lehman and Shriver consider religious beliefs, scientific eminence, and academic discipline. Lehman and Witty (1931) found that only 25 percent of distinguished scientists listed a religious affiliation in autobiographical sketches, as compared to over 50 percent for men in the general population. In the scientific groups, certain liberal Protestant religions, e.g., Unitarianism, were overrepresented; the more traditional Protestant religions and Catholicism were underrepresented. The Lehman and Shriver article provides a contrasting view of religious identity in social and behavioral, and physical and natural scientists. The cognitive aspects of religion, for example, were found to be much more developed in the social scientists than in the other groups.

A number of articles in the literature are concerned with political attitudes of scientists. The selections here include only two, the article by Turner, Spaulding, and McClintock on attitudes of social scientists and the Ladd article, which is extremely contemporary, dealing with attitudes toward the Vietnam war. In a 1963 study, Palmore had found a shift in both the class origins and political orientations of early and present day sociologists. The founders of sociology from the lower class were leftists, while founders from the upper classes were conservative and moderate. In contrast, modern sociologists prove to be overwhelmingly Democratic, although there are differences in the voting behavior dependent on class origins in the contemporary sociologists. In the Turner selection presented here, academic sociologists who were members of the American Sociological Association were predominantly Democratic, in support of Palmore's work. Their family backgrounds were normally conservative and involved relatively prestigious occupations, in comparison with professions in general. Although certain differences in demographic factors, such as religion and age, were found between Democratic and Republican sociologists, the authors believe that the overwhelmingly Democratic affiliation cannot be explained by background variables, but is due more to professional training and socialization.

Ladd's article, which compares scientists in various academic disciplines regarding opposition to the war in Southeast Asia, also found that social scientists were more liberal and active than their physical scientist counterparts. They were more likely to actively oppose the Vietnam war by signing public petitions. In the extent of their involvement they were closely followed by political scientists. Study of the geographical representation of persons involved in antiwar activities also found that scientists in the northeastern universities were disproportionately overrepresented, while southern and Catholic institutions were notably underrepresented. Though in some ways social scientists and political scientists had similar involvements, there were interdisciplinary differences in their ways of opposition.

From these studies which are aimed at conveying the focal points in the investigation to date on the nonprofessional activities of scientists, the major question is the relevance, saliency, and importance of these activities, and particularly the interaction, if any, of the nonprofessional and professional roles. For the older generation of scientists with their strong tendencies toward isolation and toward the denial of everything that interfered with persistence and devotion to work, the relative unimportance of outside activities is fairly easily accepted and understood. Whether the same would be true if contemporary scientists, "gentlemen scientists," were now subjected to study—especially in the light of the vigorous debates about scientific role and responsibility that are consequent to attacks on the relationship between technology and societal development, is as yet unknown. This seems extremely important to investigate, especially since so many aspects of scientific activity are implicated, as well as the basic norms, conventions, and ethics of science itself.

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The Scientific Life Style

Bernice T. Eiduson

In this section vocational identity is used to see how it influences the other aspects of scientists' lives, their nonvocationally preset roles. Drawing upon the interview data, I shall look at the research scientist in his functions as the head of a household, as a member of the community in which he lives, and as a person at leisure and play.

The purpose of spotlighting the scientist in these ways is twofold: first, to draw a more complete picture of the scientific man—to show that his is not wholly a one-sided existence; and second, to study how his identification as a scientist, with its characteristic orientations and ways of acting and reacting, leaves a mark on his nonscientific roles. . . .

Unfortunately, the data about the nonscientific aspects of these men's lives are not so comprehensive as the work data—again something that could have been predicted. Some scientists were a little reluctant to tell about their private affairs, and some were less skilled in talking about their private affairs than they were about their jobs. In some men, I felt that what seemed to be hesitation was actually repression and impoverishment in these areas. I think, however, that the interview data are sufficient to suggest the circuitous ways in which the scientific style finds nonscientific expression.

THE SCIENTIST AS A FAMILY MAN

Thirty-six of the 40 scientists studied are married, and all but two of these have children. Of the single men, one has been married twice.

On the one hand, the scientists are examples of the middle-class culture often seen in the university town; and yet, in some respects their outlook and way of life are urbane and upper class. Generally they lead quiet, unpretentious, even-tenored, rather steady lives. They come home regularly for dinner, and spend the time before and after dinner with their children and wives; pursue their hobbies systematically; and, more often than not, spend the evenings reading journals, novels, magazines, or watching television. Most live on a fairly modest scale and do not spend much for theaters or entertainment. Their children grow up in middle-class neighborhoods in which they reside for long periods of time. Some few receive a private education, but most attend public schools. Home life is respectable and comfortable, and one would have to look very hard to find examples of unusual, uncontrolled, or deviant living patterns.

On the other hand, their lives are not uneventful. They travel a good deal, spend sabbaticals abroad, and, to some extent, they develop broad and cosmopolitan interests and tastes; they seek out the intellectuals in foreign countries, and even develop some facility in adapting themselves and their families to new tongues and environments. . . .

Their lives are not conventional in the usual sense, nor are they extremely unconventional. Yet, despite the fact that their home lives are interesting and varied, one gets the impression that these do not reflect their most exciting selves.[1]

For one thing, the interviews reveal that the scientists are not able to give the most significant parts of themselves to home and family. They feel that most of their individuality and personality comes out in their work, and what they as individuals can contribute uniquely is drawn out in studies and laboratories. This is why so much of their sense of identity comes from their professional roles, and, by contrast, this is why their roles at home hold relatively little interest for them; and why they give it in turn little of their best. As a result, their wives and children know those aspects that encompass the commonplace and pragmatic, but they know very little of those aspects of the scientists' personalities that are concerned with the personal and the intimate. A number of men whom I interviewed mentioned that they had never told anyone so much about themselves as they had told me,

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although the interviews were not so sustained nor so intensive that extremely intimate material was proffered. Thus, it would seem that even the personal is more readily expressed in the framework of science than it is in relationships at home—or at least those aspects of the personal from which the scientific men derive their sense of identity.

Also, the data show that the roles at home and work remain fairly well isolated from each other. To some extent, the difficulty in communicating science to the nonscientist is involved in this; in this respect the wives and children are no different from other lay persons. "What happened at work today, dear?" usually boils down to the intradepartmental gossip, who was at what committee meetings, and a few of the worries about students and funds. A number of men told me how little their children know or understand their work, even though they make an effort to show them their laboratories, and try to tell them what being a scientist is like. They know their children mouth some rather advanced scientific words and concepts, and yet the scientists are aware that the youngsters have little real understanding of what science is like, and particularly of what they do for a living. Some men try hard to give their families some of the color of the life they enjoy, because as a few admit, they are uncomfortable in loving something they are unable to share with those who are closest to them. . . .

Others tend to compartmentalize their different activities carefully, which often sets the pattern for the rest of the family. It is not surprising to see the isolating tendencies—which they have used effectively at work—employed in separating work concerns from home concerns. And wives help effect successful isolation by taking over major responsibility for the running of the home and the welfare of children. They also frequently become the family representative in all community and school affairs. The scientists find a great deal of support among their academic colleagues for letting their wives take over these things, for this is the most frequent pattern of division of family responsibility in the university community. . . .

Most of the scientists work in the garden, paint their houses, take their children camping. Some enjoy these things, but others do them dutifully, feeling they are obligations which keep them from spending more time in the laboratory, which they would prefer. Some are so pulled toward their work that they determinedly, but guiltily, leave their wives with the daily chores, but find themselves doing much more than they really have to in what little time they are at home. Others exaggerate the immediate press of work so that it frequently becomes the excuse for not taking the trips that they are not too interested in, or going to the parties that bore them, or staying at home with the "cats, dogs, and infants."

A number mentioned spontaneously that they knew their devotion to work kept them from recognizing interpersonal problems that they did not want to face. One chemist described the absence of overt conflict with a first wife with whom he found he had nothing in common. He felt that he was quite unaware of this for a long time because he was so absorbed in his work. . . .

Because scientists frequently travel to meetings and occasionally go abroad, vacations hold less luster for them than they do for the wives, who look to the summer for a glimpse of scenery that the kitchen walls simply do not provide. Some men describe real "vacation neuroses," to extend Freud's "Sunday neuroses," and literally cannot stay away from work for more than three to four days at a time. Those with wives who cannot stay away from their children for longer than that, find that they have a happy match indeed.

Many scientists described their wives as being bright, intellectual, and eager to use their own talents, although some were dismayed at their own ambivalences toward their wives' strivings. . . .

Scientists say their most difficult role is being a parent. Some described themselves as being too lax, too permissive; others were distressed because so little of themselves seemed to have been transmitted to their children. A number who had adolescent or prepubertal youngsters were dismayed to find that the children flitted about from one interest to another, and did not become absorbed in anything for more than brief periods. This behavior existed in a variety of families; it did not seem to matter whether the parents took a laissez-faire attitude toward their children's interests or whether they consciously tried to stimulate their children to emulate them. None of the men felt that his children were as intensively involved in anything as he had been at their ages; even sons who were already in adult work were described as having more varied and extensive involvements, rather than selected, intensive ones. . . .

LEISURE AND PLAY

It seems that one can separate the scientist at work from the scientist at play only by such a mundane criterion as what he does for a living.

Scientists are peculiarly individualistic in some of their habits. They do not look forward to long vacations "away from it all"; often, they cannot bear to be away for very long, or if they do go, they take work with them, so that even the beauty and grandeur of the Sierra Nevadas are insufficient to distract them from current work. Some never let science interfere when the snow is right for skiing. They do not merely relax after work, for whether they lift weights or garden, they work at this methodically, regularly, with effort and precision; and in their hobbies, many are dilettantes, in Russell Lynes's meaning of the word. They mentally separate work and play, put very different values on them, and there is seldom any question about which one has priority. Time assumes a premium for them as do few other variables, for as one put it, "Although we cannot all be Einsteins just by working twenty-four hours a day, we do have a better chance of becoming great and important scientists." And they know that the geophysicist who puts in twenty-four hours a day does, in fact, the work of three scientists. Therefore, they are forced into devising methods that will admit play into their superego-dictated work patterns, and, with the resourcefulness we have come to expect from these men, they have arrived at some clever and effective ways to "bribe their consciences."

What they know as the nature of the creative process becomes their ally in this contrivance. The ways of creativity are tortuous and have a large component of the unpredictable and the unconscious. This offers a natural permissiveness for indulgence in nonrealistically oriented activities, in the immature, and even in the nonsensical. There are too many stories about great scientific discoveries nurtured by skillful neglect for anyone to deny that their impulses "may need," as one organic chemist has put it, "an unhampered run now and then." Some scientists say that play activities make them work better and think better:

> My impression of scientists that I have observed who have an undiluted devotion to their science is that they're not nearly as creative as those who have outside interests and occasionally think consciously at least about some other matters. This is purely subjective. As I say, I ask myself sometimes too whether my attitude in this particular matter is a matter of rationalization, because I don't like to do this myself. I like to do some other things occasionally and am very much interested in whether this really cuts down on my creativity. I feel fairly confident at the present time at least that it doesn't cut down on my creativity, but my feelings are subject to change.

Others describe periods of change from work as "periods during which we get keen again," and sometimes they interject an hour or two during the middle of the day for swimming, baseball, or tennis, or for a noon concert, or for a stroll through the university art gallery. For many, this change is more than rest—it is a "restoring of creative health." Sports, they say, are "good" and they are necessary if one is to minister to the needs of one's "creative ego." . . .

The sharpening of sensitivities and motivations by alternating activities does not direct every scientist's attention to leisure-time interests. For some, the different aspects of work that their professional activities entail serve the purpose. The distinction here is usually between work demanding originality, and the more routine kinds of work, such as administration and teaching.

Because there is effort, enthusiasm, and discipline put into the "play" activity, there is little of the "letting down" feeling. The tempo of work is hard to shrug off. One man said, "I work hard, stick to it seriously, become tense and driving, and I need the same kind of hardness and violence in exercise."

The tensions cling, too. One man speaks of the discouragement he feels at times, and uses exercise to get out of these moods. He says:

> There are certain things that help a lot—getting a lot of exercise does. It makes these periods much more tolerable. In fact, real vio-

> lent exercise can really wash it out. Going skiing can. It can just wipe out anything. When I go skiing I usually don't think about things in the laboratory, but I think about personal things the first couple of days. Skiing wipes out everything, but eventually I begin to think of work again and become eager to get back.

Others let their frustrations out in modern woodsheds, which are usually equipped with electrical tools. One man, who takes his writing as seriously as he does his research, says that by being able to move between two such important activities he "always remains optimistic."

Few leisure activities do not have at least one advocate among this small group. Many participate in sports: golf, basketball, handball, fishing, camping, hiking, mountain climbing. Others read a great deal; some do photography, collect records, play musical instruments; some are carpenters and woodworkers, and gardeners; others write science fiction, poetry, and popular articles. In this group there are some tournament bridge players; two stamp collectors have maintained collections from childhood; two "play" in the stock market; two are painters; one is an archeologist, and there are a few who like to dance and to watch television.

Some describe how they have moved from one interest to another at various times in their lives, achieving some of the horizontal mobility which is so difficult to experience in work where expertness is demanded. . . .

Scientists report little of the boredom that seems so prevalent today, and that has become one of the major complaints of people seeking psychiatric help. Play is not their way of sinking happily and guiltlessly into complete passivity, nor do they become exposed to the anxieties that effortlessness sometimes brings. The scientists do not seem to envisage play as providing these things, nor do they seem to want them; instead, they expect play to provide the new challenge, the different task, the experience that offers the use of different personal resources.[2] Play does not imply surcease of activity or less challenge, but rather difference in activity and challenge. The physicist who can let himself go in physics really lets himself go in play. Though this might suggest that there is more regression permitted in play and that this furnished the difference, I seriously question that some of these leisure activities are more regressive or show deeper kinds of regression than take place in really creative thinking, in the fantasies, daydreams, and in the "paranoid leaps" that precede intellectual product. Play seems to supply not a less structured or a less controlled kind of behavior but rather a behavior that provides a different kind of excitement or stimulation from that of work. Not having one's living dependent on what one does in play encourages many different kinds of activities and different kinds of ego involvement. The feeling of rest and relaxation from work, and the "resharpening of oneself," lies in the recruiting of different aspects of personality, the use of different skills and resources and personal tools. The difference in kind is what the scientists search for in play.

I have been impressed that what scientists choose to do in their leisure often reflects the fantasies and partially fulfilled gratifications of childhood. Play gives the opportunity for re-experiencing some of the satisfactions of "pure" sensory experiences, and it provides enjoyment with no immediate consideration of responsibility or need for intellectual sophistication. One chemist said he particularly liked golf because of "the pure pleasure you get from the feel of your muscles when you swing the clubs"; another took pride in the precision with which he could carve furniture legs. Play also seems to give to scientists a second chance for the longed-for pleasures of boyhood. . . .

One realizes how little lag, pause, and circuity there has been between childhood and adulthood for some of these men. Play today seems as much a dawdling with the mysteries and uncertainties as is any kind of experimental work that they set up on the laboratory bench —perhaps more so. They seem to have grown up too quickly, "wised up too fast," to use an expression of Kenneth Boulding's; perhaps their excellent intellectual abilities account for this. In their present stake in play, there may unconsciously be some wish to "wise down again," although they probably realize very perceptively that one "wises *up*," and that one can never really "wise down" again.

It would be surprising not to find a few men who have a great deal of difficulty turning to play, since there is such a premium on hard work. In general, I found that it is the scientist whose work pattern is filled with conflict who is harassed by conflicts in play. The chemist who works in such irregular spurts that he never accomplishes as much as he could, describes feeling guilty when he reads fiction in the evening while the journals lie unopened. The physicist who has to be reassured about his abilities by somebody else, or urged to put his ideas on paper before the challenge in the problem itself takes over, literally has to change environments in order really to let himself play; everything in his home setting seems too confined, too constricted. Others compulsively set out their leisure activities, feeling with Sidney Hook that, "If the development of the powers of cooking, fishing, and roller skating get in the way of the development of the powers of reading, writing, and problem-solving, then the first must yield."[3]

A few fear becoming too much taken with play. One chemist describes how carefully he controls his interests:

> I do comparatively little—some professional football and amateur sports, but mostly I read. I haven't been to the movies since I've been back from Europe, I'm sure. I listen to classical music on the radio and recordings. I go out to various people's houses for dinner one night a week, or just sit around and talk—very little in the way of formalized leisure. I'm afraid to get a television set because I know damn well I'd watch it a lot if I did. I really am afraid, because I enjoy watching television, and there are a lot of good things. If there is something I really want to see, I'll go to somebody's house and see it.

. . . In some sense, the scientist is an anachronism in the light of modern conditions. It is legitimate and desirable that he meditate at work, that he follow his whim and fantasy, that he defend himself against time, that he even "kill it"—this is the scientific condition. Therefore, what play provides for him is difference—the difference in experience, in feel, in the use of different resources and skills. Play is the way he flexes his muscles. It is the way he keeps in mental condition and resharpens himself. It is the way—when he is most remote from his other scientific colleagues—in which he reconditions his individuality, that aspect of all his qualities which makes him at one with them.

NOTES

1. The novels about scientists, such as E. Lipsky's *The Scientists*, New York: Appleton, 1959, and C. P. Snow's *The Search*, New York: Scribner, 1955, seem to have as their goal presenting the researchers in a more human light. They substitute the laboratory and university background for more conventional settings, and I was left with the feeling after reading them that scientists are no different from anyone else, except that they were a little brighter in school. This would seem a rather oversimplified situation if one thinks for a moment that in their work settings these men essentially create—at least symbolically—new cultures and then live in them, while at home they are more the recipients of the culture that is superimposed upon them.
2. Cf. R. Denny, "The Scientific Corps: A Sixth Estate," *Confluence, 3:*220, 1954.
3. In Sidney Hook, "The Ends and Content of Education," *Daedalus, 88:*7–24, Winter 1959.

Lifetime Worry Patterns of American Psychologists

Willard A. Kerr, Harry L. Newman, and Alfred R. Sadewic

As the first of a series of projected studies of the "lifetime" worry patterns of the members of specific American occupational groups, this research employed the voluntary, anonymous questionnaire technique.

A letter-questionnaire requesting cooperation and listing twelve potential fields of worry was prepared and mailed with attached post-

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paid answer-reply card to a random sample of 250 successful American psychologists who were born before 1903. "Successful" for the purpose of this study is defined as being employed in a psychological capacity, and, with a few exceptions, holding fellowship status in the American Psychological Association. Ages of the 103 respondents ranged from 46 to 81, the median age being 54. Each psychologist was requested to state the age or age level at which any or each of the potential worries had been most of a problem to him.

Inspection of Table I reveals economic worry to be the most frequently reported of the twelve potential worries. Nine out of ten psychologists state a specific age or age level of economic worry, the median age of such worry being 30.

Since economic worries appear to reach their maixmum at age 30, a first hypothesis that seems reasonable to account for this result is that a maximum number of respondents just happened to be age 30 during economic depressions. In order to evaluate this hypothesis, . . . the respondents were divided into four age groups. Approximately one fourth were found to be in each group. The frequency distributions of occurrence of economic worry of the four age groups were then plotted and the years of national economic depression were also indicated. Results reveal that the median or modal ages of economic worry do not occur in economic depression periods much, if any, more frequently than would be expected by chance. Psychologists aged 45–49 for example, reported a peak economic worry load at age 31 which was in the depression year of 1932, but those psychologists aged 50–54 also reported a peak economic worry load at age 31 and that was the relatively prosperous year of 1927. . . . There is some tendency toward greater economic worrying during depressions, but the tendency toward peaking of economic worry between ages 29–34 persists regardless of the business cycle.

Actually two other of the twelve worries studied seem psychologically relevant to economic worry—job security and lack of confidence of personal business success. This belief is supported by data . . . which reveals the modal point of worry for all three worries at age 30; also, as shown in Table I, the quartile deviations of these worries are practically identical.

The central tendency data of Table I provide a probable chronological sequence of twelve of the major worries which seem likely to arise in the lifetime of typical American psychologists. This sequence begins with worry about personal appearance maximizing at age 20, overlapping markedly with worries related to sexual morality which peak at age 23.

Proceeding from his early twenties amidst concern over personal appearance and sexual morality, the psychologist at age 26 apparently becomes more worried than at any other time over lack of confidence in making a good impression in meeting people and over religious

Table I. Statistical Summary of Reported Worry Experience of 103 Male Psychologists Born Before 1903

	Per cent expressing worry	*Median age at time of worry*	*Mean age at time of worry*	*Interquartile range*
1. Appearance	52	20	20.8	16–31
2. Sexual morality	54	23	24.5	18–30
3. Lack of confidence of making a good personal impression in meeting people	65	26	24.7	19–39
4. Religious or philosophical convictions	88	26	28.2	21–41
5. Economic worries	91	30	32.0	23–38
6. Lack of confidence of business success	63	31	33.3	26–38
7. Job security	75	32	34.6	26–38
8. Peace of mind	67	35	34.8	26–47
9. Willing to return to job at less pay	50	36	40.3	32–46
10. Political convictions	82	38	37.8	29–48
11. Health	57	38	40.5	28–50
12. Giving up important hopes or ambitions	56	40	38.5	28–45
13. Marital difficulties	42	41	40.2	34–48

convictions. At age 30, a notable change in the worry load seems to occur with peak emphasis on economic worry—which incidentally is the same age as the mean age of marriage of the respondents. At age 31, maximum lack of confidence of personal business success is encountered, and age 32 finds greatest worry over job security. General worry over peace of mind lacks any notable age peak, but the median age of realization of lack of peace of mind is 35.

At age 36 the maximum tendency apparently is felt to take a job of less pay. This would seem to indicate a psychological breaking away from the economic worries of the preceding six-year period. The shift from personal economic worries is further reflected at age 38 and thereafter by a continuing increase in emphasis on political convictions.

Forty-two per cent of the respondents indicated marital difficulties as being sources of worry; the median age for these worries is reported as 41.

Two variables yielded interesting bimodal distributions. "Giving up ambitions" reaches a small peak at age 22 and a high plateau at ages 39–45. Health as a worry reaches a high plateau at ages 30–35 and another plateau proceeding on from age 45.

Reliability of these worry patterns at the oldest age levels undoubtedly is least because of diminishing numbers of cases.

SUMMARY

Many difficulties are inherent in this type of research, especially the unknown effects of the memory factor, the biases of the 40 per cent who cooperated, and the possible tendency to emphasize past rather than present worries, therefore, all conclusions indicated by statistical analysis must be regarded as highly tentative; nevertheless, within the limitations of the study, certain conspicuous trends in age-worry pattern characteristics are apparent.

1. Of the twelve potential worries reported by 103 older psychologists, economic worry was reported with greatest frequency.
2. Economic worries apparently reach their peak near age 30, this tendency persisting in periods of both economic depression and prosperity.
3. As reviewed in perspective, the worry characteristics of the typical older psychologist responding with reference to age of maximum worry fall into the following sequence: age 20—personal appearance, age 22—giving up ambitions, age 23—sexual morality, age 26—making a good impression in meeting people and concern over religious or philosophical convictions, age 30—economic problems, age 31—personal business success, age 32—job security, age 33—health, age 38 and thereafter—political convictions, age 41—marital difficulties, age 42—giving up ambitions, age 45 and thereafter—health.
4. When aged about 18–26, the typical respondent reported chiefly worries of idealism and personal development, but by age 30 the worry emphasis has shifted to problems of survival and hard reality. By age 38 the economic survival worries have eased somewhat as is shown by maximal and continuing emphasis upon political convictions with political concern gradually increasing into later years. This later life relative emphasis upon political problems indicates a practical combination of earlier idealism and subsequent experience with reality.

Academic Discipline as Predictive of Faculty Religiosity

Edward C. Lehman, Jr. and Donald W. Shriver, Jr.

Discussions of religion and the American university usually dwell on the questions of possible conflict between science and religion and whether a person can adhere to traditional religion and still be an intellectual.[1] While the early approach to these issues was highly po-

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[1] Rodney Stark, "On the Incompatibility of Religion and Science: A Survey of American Graduate Students," *Journal for the Scientific Study of Religion*, 3 (Fall 1963), pp. 3–20; Andrew M. Greeley,

lemical,[2] recent studies concentrate on the more analytical problem of discovering conditions under which persons are more or less likely to participate in religion. These investigations typically compare academicians with nonacademicians to test the hypothesis that involvement in higher education is predictive of irreligiosity.[3]

The present study seeks to investigate variations *among* academicians rather than to make gross comparisons of collegians with noncampus persons. Here the specific problem involves a search for conditions associated with academia under which academicians themselves are more or less likely to be religious. Reliable data on this question are relatively scarce. The few relevant studies to date tend to concentrate on type of school (parochial or secular) and quality of undergraduate and graduate education.[4]

The variable with which this paper deals is *type of academic discipline*, a profession-relevant factor which has been the subject of considerable speculation but relatively little empirical analysis. The remainder of the paper consists of a brief review of positions taken on the issue, presentation of a potentially explanatory construct called "scholarly distance from religion," and a review of a pilot study in which the construct was employed.

ACADEMIC DISCIPLINE AND RELIGIOSITY

Several theoretical positions regarding type of discipline and involvement in religion recur in the literature. One stance, usually an implicit assumption carried over from a popular stereotype, holds that all university personnel tend to be basically irreligious.[5] As a lay stereotype the position is not hard to understand, but its continued implicit presence in scholarly writing is strange, given the empirical evidence amassed to the contrary.[6]

A slightly different monolithic view acknowledges differences in academicians' religiosity but either ignores or consciously tries to erase possible variations in involvements according to academic discipline. A recent study argues, for example, that all modern scholars are really scientific scholars, in an attempt to account for relative uniformity of religiosity among graduate students in different majors.[7] Again, however, studies of *faculty* indicate clear variations in religiosity among disciplines.[8] It is apparently only in studies of graduate students that differences in religiosity are independent of discipline, an observation which may be important in itself.[9]

The Scientist—Nonscientist Dichotomy

The most prevalent position for differentiating between academic disciplines in relation to religion focuses on the scientist. He is pictured by many as the irreligious atheist who, intentionally or not, corrupts the faith of his unsuspecting students and readers. The discussion is by no means one-sided in this regard,[10] but the prevailing opinion is that science and religion do conflict logically and methodologically. "At the root of the controversy are two

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"The Religious Behavior of Graduate Students," *Journal for the Scientific Study of Religion*, 5 (Fall 1965), pp. 34–40, "Religion and the Intellectuals," *Partisan Review* (PR Series #3, 1950); and Fred Thalheimer, "Continuity and Change in Religiosity: A Study of Academicians," *Pacific Sociological Review*, 8 (Fall 1965), pp. 101–108.

[2] Leuba, for example, a psychologist who first studied the problem empirically in 1914, felt that no self-respecting scholar would be involved in traditional religion, an orientation which unfortunately shows rather clearly in his analysis of data. See James H. Leuba, *The Belief in God and Immortality* (Boston: Sherman, French & Co., 1916), esp. p. 213 *et passim*. For general discussions of the controversy, see A. D. White, *A History of the Warfare of Science with Theology* (New York: Dover Press, 1960); and Ian G. Barbour, *Issues in Science and Religion* (Englewood Cliffs, New Jersey: Prentice-Hall, 1966).

[3] For a summary of the basic issues involved in this research, see Thalheimer, *loc. cit.*

[4] *Supra*, footnote 1.

[5] Greeley, for example, labels *academicians in general* as secularists, *op. cit.*, p. 34.

[6] Leuba, *op. cit.*, pp. 203 ff; James H. Leuba, *The Reformation of the Churches* (Boston: Beacon Press, 1950), pp. 44–49; R. H. Edwin Espy, *The Religion of College Teachers* (New York: Association Press, 1951), pp. 43 ff; and Gordon Allport, *et al.*, "The Religion of Post-War College Students," *Journal of Psychology*, 25 (1948), pp. 10–29.

[7] In this regard see Stark, *op. cit.*, p. 4.

[8] Leuba (1916), *op. cit.*, pp. 254 ff; Leuba (1950), *op. cit.*, pp. 45–47; and Espy, *op. cit.*, pp. 139–140.

[9] Stark, *op. cit.*, p. 10; and Greeley, *op. cit.*, p. 37.

[10] See for example, Harold K. Schilling, *Science and Religion* (New York: Charles Scribner's Sons, 1962).

different normative systems which have two different theories of knowledge, two different approaches to reality, two different methods of extending knowledge, and two different attitudes of mind."[11] One also reads that the "true scientist" is an unbeliever[12] and that the worlds of scientific scholarship and religious faith are "mutually exclusive."[13]

From such premises some observers have argued that scientists tend to be less religious than nonscientists. "The scientist's devotion to the scientific method has latent consequences which sometimes intrude upon his nonscientific statuses and roles and cause him to reject religion."[14] Certain empirical studies indirectly seem to support such a statement. Leuba found both in 1914 and in 1933 that scientists he studied tended to be irreligious.[15] A more recent study of 64 scientists by Roe indicated that while all of them were from basically religious backgrounds, less than five percent of them were personally involved in religion at the time of the study.[16] Nevertheless, Leuba's study does not compare scientists with other academicians, but rather compares scientists of different levels of achievement. He also contrasts scientists with laymen. The question of interdisciplinary comparisons is still open.

Scholarly Distance from Religion

This paper takes the position that the scientist-nonscientist stance is an oversimplification. A more fruitful basis for classifying academic disciplines in relation to religion is *scholarly distance from religion*, a construct which refers to the extent to which a discipline's institutionalized activity includes scholarly study of religion. Disciplines can be ordered on this dimension. Certain fields are characterized by little scholarly distance from religion. They constitute sociocultural infrastructures in which faculty objectively study religious phenomena as a part of their routine activity as scholars. Where religion itself is often the object of explanation in a discipline—i.e., religion is a dependent variable—scholarly distance from religion may be said to be low. In some other disciplines religion usually is studied more indirectly. It is not the phenomenon to be explained per se but rather is considered a factor which must be taken into account at least occasionally. Here religion more often is viewed as a possible independent variable, and scholarly distance would be considered greater. Finally, scholarly study of religion is in no way characteristic of a third category of disciplines, in which case scholarly distance would be high.

Scholarly distance from religion, then, refers to the extent to which an academic discipline itself approaches religion from the traditional scholarly perspective—i.e., applies the norms of objectivity, tentativeness, criticism, and doubt specifically to religious thought, religious behavior, religious literature, religious groups, etc. It does *not* mean whether the ideas comprising a given field are said to have logical implications for religion. . . .

Analyzing faculty religiosity from the perspective of scholarly distance from religion is different from the scientist-nonscientist approach in several ways. In the first place, the concept of scholarly distance cuts across the scientist-nonscientist scheme. Disciplines characterized by little scholarly distance from religion include sociology and psychology which are usually considered social sciences, as well as English literature and philosophy which are categorized as humanities. Fields with somewhat greater distance from religion would include economics as a social science and architecture which is not a social science. Disciplines of maximum distance from religion are typified by physics and/or agronomy.

Concentrating on the scientist leads one to focus unnecessarily on methodological differences among disciplines and on the question of logical compatibility between religion and science. The construct of scholarly distance from religion, on the other hand, avoids the brash assumption that academicians behave more logically than others. It focuses more on sharing occupational norms (logical or illogical) which call upon the practitioner to apply

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[11] David O. Moberg, *The Church as a Social Institution* (Englewood Cliffs, New Jersey: Prentice-Hall, 1962), p. 334.

[12] This stance is found in both Leuba's 1916 and 1950 works.

[13] Stark, *loc. cit.*

[14] Moberg, *op. cit.*, p. 333.

[15] Leuba (1916), *op. cit.*, pp. 253 ff; and Leuba (1950), *op. cit.*, pp. 45 ff.

[16] Anne Roe, *The Making of a Scientist* (New York: Dodd, Mead & Co., 1952), cited in Stark, *op. cit.*, p. 5.

the scholarly perspective of skepticism and criticism (by whatever methodology) directly to religion itself.

Degree of scholarly distance from religion should be useful for explaining observed variations in faculty religiosity. One would expect to find a higher degree of religious involvement among faculty in disciplines characterized by high scholarly distance than among scholars in fields of low distance. In a situation of low distance, where the "ancient and ardently taught . . . obligation to question, to suspend belief in received knowledge . . ."[17] is institutionalized specifically in relation to religion, certain factors are present which do not necessarily appear where distance is high. To the situation of one's having internalized the scholarly perspective or of routinely using scientific methodology in one's work is added the fact that the academician does not decide alone whether to focus one or both of these stances on religion. The norms of his discipline prescribe the step.

The institutionalization of the scholarly perspective as the appropriate approach to religion probably will affect religiosity partly because of two important social-psychological consequences. In the first place, the person in this circumstance is virtually forced by the norms of his discipline to place religion in the same attitude structure[18] as other objects of study. Minimum distance tends to prevent cognitive differentiation. The anthropologist, for example, would be less able to think of religion as "something different" from other cultural forms he scrutinizes and tries to explain than would the physicist. The former discipline involves the study of religion specifically; the latter does not. Persons in physics and other like fields are characterized by a great deal of scholarly distance from religion and can keep religion psychologically segregated from the rest of their lives more easily; i.e., from the scientific and scholarly outlooks associated with their vocations.

In the second place, social interaction among peers in disciplines involving little or no scholarly distance from religion will provide less social support for religious beliefs and attitudes than might be had among colleagues in disciplines of greater distance. Where a discipline's prevailing norms involve scholarly explanation of one or more religious phenomena, one would be less likely to experience social support for adherence to traditional religion. If anything, the social structure would be more likely to support detachment.

The concept of scholarly distance from religion seems to order the variations in religiosity characterizing different disciplines in earlier studies of faculty.[19] It is fruitful, therefore, to pursue its explanatory utility further.

RESEARCH DESIGN

To study these questions in a preliminary fashion, the authors surveyed a stratified systematic sample of permanent, full-time faculty ranking assistant professor and above at a southeastern state university during the spring of 1966. The sample was stratified according to broad types of discipline, namely, social science-humanities, nonagricultural natural science, and agricultural natural science. Each stratum was sampled in such a way as to obtain a total sample of approximately *100* respondents, one-third of which would come from each type of discipline. This procedure yielded *104* names distributed basically as planned. Graduate students in sociology, psychology and economics conducted personal interviews with the respondents. The final sample consisted of 99 completed interviews which was considered representative of the target population.

The interview schedule included items designed to measure religiosity, a variable which at best is difficult to approach empirically. The authors decided, first of all, to focus on *traditional religion,* instead of "religion in general" or whatever types of religion were found among the faculty (e.g., traditional or esoteric, other-worldly or humanistic). The subject of most debate seems to be traditional involvements. *Religiosity,* therefore, is defined as *involvement in traditional Judaeo-Christian religion.*

Furthermore, following Glock and Stark, religiosity was conceptualized as a multidimen-

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[17] Thalheimer, *op. cit.*, p. 101.

[18] For a concise discussion of the concept "attitude structure" see Theodore M. Newcomb, *et al.*, *Social Psychology* (New York: Holt, Rinehart & Winston, 1965), chap. 5.

[19] Leuba (1950), *op. cit.*, p. 45; and Espy, *op. cit.*, pp. 69, 136–140.

sional phenomenon.[20] The study employed four of their five rudimentary dimensions in order to deal with different ways in which involvement in religion is manifested. The dimensions included are: (1) *ideological*, or beliefs which the followers of a given religion are supposed to share, (2) *experiential*, religious feelings or experiences, (3) *ritualistic*, or practices such as workship and prayer, and (4) *intellectual*,[21] referring to knowledge of basic doctrines or literature.

The data-collection instrument included items to measure each of these dimensions. The ideological dimension was operationalized by asking the extent to which the respondents accepted the broad outlines of traditional Judaeo-Christian theology. These tenets were shown to the respondent on a card. Responses were combined so as to produce a five-point scale ranging from unqualified acceptance at one extreme, through accepting it but with reservations on peripheral ethical statements, accepting it with reservations on the core theology, basically rejecting it but with some parts meaningful to the person, and to unqualified rejection.

Degree of ritualistic involvement was measured by combining frequency of church participation, frequency of prayer, and frequency of reading the Bible. Possible scores ranged from *0* to 9.

The cognitive dimension was measured by means of a ten-item matching quiz. The respondents were asked to identify four items from the Biblical period (Moses, Easter, Good Samaritan, and David), three items from the period of historical development (Reformation, Augustine, and John Wesley), and three items from the contemporary period (Karl Barth, Thomas Altizer, and John XXIII). The number of correct answers was considered an indication of the respondents' relative knowledge of the Judaeo-Christian tradition.

The experiential dimension was operationalized by asking the respondents whether they had had certain traditional religious experiences. The items used were rather crude in the light of the complexity of religious experience. Nevertheless, the number of affirmative replies to these questions was used as an index of experiential religiosity. The possible scores ranged from *0* to *6*.

The four indices are used in the analysis below as ordinal judgment scales.

FINDINGS

Distributions of Scores

Analysis of these data supports previous findings of wide variations in religious involvements among faculty. Table 1 shows respondents' scores to be widely distributed on each dimension. The tendency is for relatively high scores on the ideological and ritual dimensions.[22] Scores on the cognitive (intellectual) and experiential items tend to be lower. These distributions are clearly contrary to an oversimplified monolithic view of faculty religiosity.

Interrelations Between Types of Religiosity

It is interesting to note the extent to which scores on these dimensions of religiosity are interrelated. The question has been raised as to whether a person who is highly involved on one dimension is also likely to be highly involved on other dimensions. Recent observers argue that there is no one-to-one relationship between types of religious involvement.[23] One recent study points out concomitant variations between Glock's dimensions.[24] Findings of the present study indicate systematic relations between scores on some dimensions, but in no case is the relationship very strong. . . . A matrix of rank-order correlation coefficients

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[20] Charles Y. Glock and Rodney Stark, *Religion and Society in Tension* (Chicago: Rand McNally & Co., 1965), chap. 2.

[21] The term "cognitive" is employed in preference to "intellectual," although the referent dimension of religiosity is the same. The cognitive dimension was first introduced by Yoshio Fukuyama, "The Major Dimensions of Church Membership," *Review of Religious Research*, 2 (Spring 1961), pp. 154–161.

[22] It is difficult to compare these distributions with those of other studies of faculty and with those of the United States population as a whole. For some rough points for comparison, see Thalheimer, *op. cit.*, pp. 103–104.

[23] Gerhard Lenski, *The Religious Factor* (New York: Doubleday & Co., 1961), pp. 11 ff.

[24] Joseph E. Faulkner and Gordon F. De Jong, "Religiosity in 5-D: An Empirical Analysis," *Social Forces*, 45 (December 1966), pp. 246–254. Faulkner and De Jong point out correlations between Glock's five dimensions of religiosity.

Table 1. Percentage Distributions of Scores on Four Dimensions of Religiosity

Ideological	Score	4 *Accept outright*	3 *Accept, but res. on ethics*	2 *Accept, but res. on dogma*	1 *Reject, but some meaningful*	0 *Reject outright*					
	Percent	51	2	29	13	4					
Ritual		*High ritual score*								*Low ritual score*	
	Score	9	8	7	6	5	4	3	2	1	0
	Percent	22	20	11	8	6	6	7	5	9	5
Cognitive		*Much knowledge*								*Little knowledge*	
	Score	10	9	8	7	6	5	4	3	2	1&0
	Percent	11	7	11	14	13	20	13	5	3	2
Experiential		*Many experiences*					*Few experiences*				
	Score	6	5	4	3	2	1	0			
	Percent	6	18	19	22	23	9	2			

($N = 99$).

(Goodman and Kruskall's *gamma*), shows three dimensions interrelated—ideological, ritual, and experiential. Degree of involvement on any one of these dimensions is relatively predictable given the score on any other of the three. The ideological factor seems to be the most pervasive among them, and the experiential the least.

The cognitive dimension of involvement is not significantly related to the ideological, ritual, or experiential factors. One cannot predict belief, ritual behavior, or religious experience on the basis of knowledge of the tradition. This pattern is roughly as Glock predicted earlier, and it is consistent with both his own findings[25] and additional findings of this study presented below.

We shift now to the problem of explanation. Why do these patterns exist? Specifically, which of the positions (if either) outlined above regarding type of discipline is (are) predictive of the observed variations in faculty religious involvements?

Scientists and Nonscientists

Are scientists less religious than nonscientists? The present study sought to approach this question in two ways. On the one hand, the respondents were classified as scientists or nonscientists by the observer according to their stated discipline. Secondly, the respondents were asked explicitly whether persons in their profession usually consider themselves scientists. Thus the scientist–nonscientist dichotomy can be approached from the standpoint of both analytical classification and subjective classification.

The results of employing the observer's classification of disciplines are portrayed in Table 2. They do not support the hypothesis that scientists are less religious. Instead, we find the opposite tendency, although without statistical significance. The median ideological, ritual, and experiential religiosity scores tend to be higher for the scientists. Median cognitive scores are identical.

Basically the same results are obtained when one compares respondents' subjective classification of themselves on the science–nonscience dimension. These data are also shown in Table 2. In this case there appear to be no differences on the cognitive and experiential dimensions of religiosity between faculty who classify themselves as scientists and nonscientists. On the ideological and ritual dimensions, however, the median scores are again higher for the scientists than for the nonscientists, although again the differences are statistically nonsignificant. Therefore, whether we use the analytical or the subjective bases of classification, the analysis does not support the assertion that being a scientist is associated with rejection of religion.

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[25] This finding supports that of Faulkner and De Jong, *loc. cit.*

Table 2. Median Religiosity Scores of Scientists and Nonscientists According to Observers' Classification and Respondents' Subjective Report

	Dimensions of religiosity			
	Ideological	*Ritual*	*Cognitive*	*Experiential*
*By observers' classification**				
Scientists	4.0	7.0	6.0	3.5
Nonscientists	2.5	6.0	6.0	3.0
	$(p<.70)$†	$(p<.70)$	$(p<.98)$	$(p<.70)$
*By subjective report**				
Scientists	4.0	7.0	6.0	3.0
Nonscientists	2.0	6.0	6.0	3.0
	$(p<.20)$	$(p<.70)$	$(p<.95)$	$(p<.90)$

* $(N = 99)$.

† Computed by the median test. See Sidney Siegel, *Non-Parametric Statistics for the Behavioral Sciences* (New York: McGraw-Hill Book Co., 1956), pp. 111–116.

Table 3. Rank-Order Correlations* between Scholarly Distance from Religion and Four Dimensions of Religiosity

	Dimensions of religiosity			
	Ideological	*Ritual*	*Cognitive*	*Experiential*
Scholarly distance from religion	.50	.20	—.41	.25

* All of the coefficients in this table are statistically significant beyond the .01 level.

Scholarly Distance from Religion

Scholarly distance from religion was introduced into the analysis by placing faculty in the various disciplines into one of three ordered groupings according to the field's apparent involvement in the study of religion. Into the "low" distance category were placed those disciplines whose practitioners study religious phenomena per se as the object to be explained. Religious ideas, behavior, literature, groups and groupings, and values are the data calling for scholarly explanation. Fields in the sample which were considered having these traits were sociology, psychology, history, religion, English, and social studies. These disciplines can be said to study religious phenomena as a dependent variable, although they also view religion as an independent variable at times. The second category, labeled "moderate" distance, includes disciplines which occasionally deal with religion, but which regard it more as a possible independent variable—i.e., as a factor which may be taken into account but itself is not the object of study. They rarely seek to explain religion itself but focus on something else. This second category in this sample covers education, economics, political science, architecture, design, and modern languages. The position at the other extreme of the continuum, called "high" distance, includes those disciplines in which scholarly study of religion basically has no part. Such fields as physics, soils, chemistry, civil engineering—in short, all other disciplines in the sample—were grouped in the high-distance category.

One would predict a positive relationship between scholarly distance from religion and the ideological, ritual, and experiential dimensions of religiosity. One would expect a negative relationship between distance and the cognitive dimension—i.e., the more one scrutinizes religion, the more one should know about it.

The analysis tends to support the hypotheses. The results are shown in Table 3. As scholarly distance from religion increases, scores on the ideological, ritual, and experiential dimensions also tend to increase. The less the repondent's discipline includes explanatory study of religious phenomena, the more likely he is to accept traditional religious dogma, report having religious experiences, and participate in acts of religious ritual. The relationship is strongest on the ideological dimension and weakest on the ritual dimension. This difference is possibly due to the probability that little academic distance from religion will affect one's ideology more than one's ritual behavior, the latter often being influenced also by community norms prescribing church attendance.

As predicted, scholarly distance and cognitive scores are related in the opposite way. As scholarly distance increases, knowledge of religion decreases.

INTERPRETATION

It was argued above that scholarly distance from religion would affect religiosity partly by inhibiting cognitive differentiation and by removing social support for religious thought and behavior. Scholarly distance from religion should be related positively to both the likelihood of cognitive differentiation and the prevalence of social norms in support of religious involvements among colleagues in the same discipline. Similarly, cognitive differentiation and social support each should be related to religiosity.

It was possible to approach this interpretation in the analysis but indirectly. The interview schedule included a question designed to elicit the respondent's perception of the amount of conflict between religion and science. Assuming that statements of no conflict would tend to accompany cognitive differentiation and that perceptions of very serious conflict would be associated with relative absence of such differentiation, responses to this item can be used as indirect indications of the extent to which the respondents had science and religion cognitively differentiated.

Respondents were considered as having indicated their perceptions of religion-relevant norms among their colleagues by stating whether they thought most of their peers in their field basically accept or reject the Judaeo-Christian tradition. Perception of negative norms suggests a lack of social support for religious involvements, while perceiving positive norms indicates the presence of social support.

Table 4 contains a matrix of correlations between these variables. The analysis indicates that scholarly distance from religion is related to cognitive differentiation and perception of norms as expected. Religiosity is also related to cognitive differentiation and perception of norms on most dimensions of religiosity, especially the ideological and ritual. The correlations involving the experiential and cognitive dimensions are very weak. Between the cognitive dimension and perception of positive norms there appears to be no significant relationship.

Controlling for cognitive differentiation and colleague social support in the relationships between scholarly distance from religion and religiosity scores also produces results consistent with the idea that the former two variables partly explain the original relationships. The partials (in Table 5) indicate, first of all, that when cognitive differentiation is held constant, the coefficients move toward zero. The coefficients on the experiential and ritual dimensions diminish to below the magnitude at which the zero-order correlation would be statistically significant at the .05 level. The figure for the cognitive dimension reduces, but somewhat less so. The coefficient involving the ideological dimension diminishes the least.

Holding colleague social support constant produces similar shifts, but the reductions are generallly less pronounced, except for the correlation involving the ritual dimension which reduces to zero. These patterns suggest that the

Table 4. Rank-Order Correlation Coefficients Relating Measures of Cognitive Differentiation and Colleague Social Support to Four Dimensions of Religiosity and Scholarly Distance from Religion

	Ideological	*Ritual*	*Cognitive*	*Experiential*	*Scholarly distance*
Cognitive differentiation*	.75	.48	—.11	.29	.35
Social Support†	.55	.29	.00	.11	.40

* ($N = 99$).

† ($N = 92$).

All coefficients are statistically significant beyond the .01 level, with the obvious exception of the one which resulted in .00.

Table 5. Partial Correlations between Scholarly Distance from Religion and Four Dimensions of Religiosity, Controlling for Cognitive Differentiation and Colleague Social Support

	Dimensions of religiosity			
	Ideological	*Ritual*	*Cognitive*	*Experiential*
*Controlling for cognitive differentiation**				
Scholarly distance	.41	—.03	—.23	.04
Controlling for colleague social support†				
Scholarly distance	.42	.00	—.40	.19

* $(N = 99)$.
† $(N = 92)$.

concept of social support is less useful than cognitive differentiation for interpreting the relationship between scholarly distance from religion and faculty religiosity.

One important issue remains. How is one to interpret the relationships observed causally? There has recently been considerable discussion of this general problem. Some observers, typified by Stark,[26] feel that academic involvements affect religiosity, the effect being a tendency to depress religious commitments. Others, following Greeley,[27] insist that the opposite is the case—i.e., persons with little interest in religion tend to gravitate toward academic pursuits more than those with greater religious inclinations. Findings by Thalheimer tend to support Greeley's position.[28] This issue is relevant to the present study in that the relationships discussed above could be interpreted as indicating either that scholarly distance from religion affects religiosity or that religious inclinations lead one to select a certain type of discipline.

There is some evidence in support of the selectivity hypothesis in this study. Respondents were asked to indicate how frequently both their parents and they themselves attended church when they were children. Answers to these questions give some indications of religious background. It appears that parental religiosity is related to faculty religiosity, especially on the ritual, experiential, and ideological dimensions. Parental religiosity is also related to scholarly distance from religion, although the correlation is very small. These patterns suggest that faculty from more religious backgrounds tend both to be more religious and to enter disciplines of slightly higher scholarly distance from religion than academicians from less religious homes.

However, when the influence of parental religiosity is controlled in the correlations between scholarly distance and the four dimensions of religiosity, the relationships still hold. A comparison of partial correlation coefficients with zero-order coefficients reveals that the strength of the correlation involving the ideological dimension has not changed at all, that there is but a slight reduction for the cognitive and ritual dimensions, and that the correlation on the experiential dimension increases slightly. These results may be partially a consequence of the skewed distribution of scores on parental religiosity—i.e., 82 percent reported their parents as having attended church at least twice a month. In either case, however, the data suggest that parental influence does not explain away the findings. The lack of change in the partials is possibly due as much to the weak relationship between parental religiosity and scholarly distance from religion.

Perhaps much of the problem lies in the way the issue of antecedence has been put. To date the selectivity question has been stated largely in "either-or" terms. More probably the situation is one of "both-and." In the first place, it makes little sense to deny that persons are unlikely to enter fields of endeavor that hold no promise of meeting their personal needs. That this happens is quite true. But it is doubtful that (other things being equal), where the person has a real choice and is aware of the potential consequences of his decision, he will select an occupation that will be dissonant with his existing attitudes.

By the same token, it seems unreasonable

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[26] Stark, *loc. cit.*

[27] Greeley, *loc. cit.*

[28] Thalheimer, *loc. cit.*

to deny that there probably will be some feedback. The zero-order and partial relationships observed above suggest that disciplines of low scholarly distance from religion are characterized by norms which operate so as to inhibit personal commitment to traditional religion. Once a person is in such a discipline, he probably will come to share norms specifying a scholarly approach to religion. To the extent that he comes to view religious phenomena from the scholarly perspective, he probably will experience difficulty retaining religious allegiances, especially to traditional religion.

SUMMARY AND CONCLUSIONS

. . . It would be premature to draw firm conclusions from the analysis presented above. Rarely do pilot studies test hypotheses satisfactorily. The present study is no exception. This is especially so given the relatively small sample employed. Nevertheless, the findings are consistent with the hypothesis that scholarly distance from religion is a useful construct for explaining variations in faculty religiosity.

The study implicitly raises at least two additional questions. First, the research was carried out at a state university which came into being as a land-grant institution. Would the same findings emerge from a study of different types of schools? It would be informative to compare the patterns of religiosity which characterize land-grant colleges, other large state universities, private four-year colleges, large privately endowed universities, and church-supported schools. In the second place, the university sampled in this study is located in the Southeast. To what extent are the findings generalizable to schools in other regions? To the extent that institutions of higher education reflect the ethos of their regional subculture, as they probably do at times, it would be profitable to compare faculty religiosity in different parts of the country. Hopefully these issues can be addressed in future research.

Political Orientations of Academically Affiliated Sociologists

Henry A. Turner, Charles B. Spaulding, and Charles G. McClintock

Studies of many specialized subdivisions of the American electorate are needed to expand and deepen our knowledge of political behavior. Much of significance appears to be lost in the use of the very broad analytical categories often applied to the national and community samples so frequently used in the study of political phenomena.[1] For instance, the general category of persons with professional and technical occupations (sometimes even including businessmen) usually produces an aggregation which is on the average conservative in orientation and which votes relatively frequently for Republican political candidates.[2] Yet observation and analysis suggest that at least certain groups of professional persons are actually not at all conservative on the average and are largely oriented toward the Democratic Party.

Because sociologists seemed likely to be one of the professional groups showing a significant difference in political orientation as compared to the professions considered as a unit, an analysis of certain aspects of their political behavior was organized around 6 general hypotheses which suggested: (1) that American sociologists who are members of the faculties of colleges and universities are predominantly Democratic in political affiliation and preference; (2) that the proportion of Democrats has increased since the 1920's; (3) that the explanations offered by the sociologists themselves and certain comparisons between them and other groups would reveal these predominantly Democratic orientations of the sociologists to be related more closely to the dynamics of their professional roles as students and practitioners than to special background factors; (4) that the Democratic majority would have a tendency to hold more liberal attitudes than the Republican minority; (5) that the remaining Republicans, as compared to the Democrats, would display on the average sig-

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nificant differences in backgrounds, personal characteristics, and experience—of types often associated with Republicanism in the general population; and (6) that those who had changed parties would be more politically active.

Procedures. In order to pursue these hypotheses, a questionnaire was developed and pretested on two groups of college and university professors. In the early fall of 1960, the revised questionnaire was mailed to a systematic sample of the fellows and active and associate members listed in the 1959 *Directory* of the American Sociological Association. The sample of 486 persons was to have been drawn entirely from those sociologists affiliated with colleges and universities, but a few others were inadvertently included. Of the 486 addressees, 318 returned questionnaires, and 298 of these were used in the analysis. . . .

In this instance the 298 respondents can be assumed to be roughly representative of the 3 mentioned categories of American sociologists who are affiliated with the regular faculties of American colleges and universities. Distributions of the 298 respondents were compared with the distributions of the systematic sample in terms of geographic region and academic rank. The regional distributions of the respondents and the addressees turned out to be very similar, and the distributions of their respective academic ranks were only slightly less so. In addition, as will be shown, the results are consistent at key points with those of certain other studies of social scientists.

Validity of the First 2 Hypotheses. The data clearly support the general validity of the first 2 hypotheses. Table 1 demonstrates the tremendous preponderance of Democrats among teaching sociologists. Of the respondents, 77.9 per cent (232 persons) were either registered in the Democratic Party or expressed a preference for it, while only 10.4 per cent (31 persons) gave similar allegiance to the Republicans. A detailed examination of the questionnaires reveals that almost as many (8.7 per cent) appeared to be genuinely independent voters as claimed to be Republicans.

That the present preponderance of Democrats is, as suggested, the end product of a process of change going back to the 1920's is strongly indicated by the voting records . . . as well as by the reports of changes in party registrations. These records reveal the sociologists as having been about equally divided between Davis, Coolidge, and LaFollette in 1924, giving Hoover as many votes in 1928 as Smith and Thomas combined, providing Roosevelt in 1932 with almost as many votes as Hoover and Thomas combined, and then going more and more into the Democratic camp. Thomas retained a small following as long as he continued to run, and Wallace drew appreciable support in 1948, but after that election the Democratic vote has consistently exceeded the Republican by roughly 4 to 1, and third-party votes have nearly disappeared.

The general validity of the voting pattern of sociologists as shown by the data is indicated by the similarity of the findings of Lazarsfeld and Thielens concerning the elections of 1948 and 1952.[3] Combining the 1948 and 1952

Table 1. Party Preferences of Respondents

	Number	*Per cent*
Democratic Party preference	(232)	(77.9)
Registered as Demoorat	196	65.8
Nonparty registration, stated preference	17	5.7
Not registered, stated preference	19	6.4
Republican Party preference	(31)	(10.4)
Registered as Republican	27	9.1
Nonparty registration, stated preference	3	1.0
Not registered, stated preference	1	.3
Others	(35)	(11.7)
Registered as independent or equivalent	26	11.7
Registered, other and unclassified	8	2.7
Not registered (Prohibition Party preference)	1	.3
Total	298	100.0

votes for Democrats, for Republicans, and for other presidential candidates, it is revealed that of the votes cast by our respondents in those 2 years 73.4 per cent were for Democrats, 18.3 per cent for Republicans, and 8.3 per cent for other candidates. In the same 2 elections, the sociologists in the Lazarsfeld and Thielens sample voted 72.0 per cent for Democrats, 22.3 per cent for Republicans, and 5.7 per cent for other candidates. . . .

Professional Role vs. Background Factors, Activity, and Income. In accordance with the third hypothesis, the data suggest that the overwhelmingly Democratic orientation of the sociologists is associated more strongly with their professional development and roles than with differences in background factors, political activity, and income. Both the reasons selected by the sociologists as explaining their choices of party and comparisons of certain of their demographic characteristics with those of other groups support this conclusion. As one element in the questionnaire, respondents were asked to rank a series of 8 possible reasons for their choices of party. While each person was asked to rank all 8 reasons in order of importance, Table 2 is based upon the number of times that each item was selected as a first, second, or third choice. The answers clearly reveal the emphasis placed by the sociologists upon the rational factors of (1) party and leader policies and (2) information gained in their profession as opposed to the suggested background factors of economic status, parental influence, place of residence, ethnic or nationality group, or religion. . . .

The large preponderance of Democrats among our respondents is not the result of family inheritance of party. Sociologists who have become Democrats have broken the hold of family patterns more often than those who have become Republicans. This situation is clearly revealed in Table 3A which indicates, among other things, that 58.1 per cent of the Republicans of 1960 were descended from 2 Republican parents, while only 40.1 per cent of the Democrats had sprung from 2 Democratic parents. Conversely, only 19.4 per cent of Republicans claimed 2 Democratic parents, but the percentage of Democrats with 2 Republican parents was 31.9. The influence of the family is apparent, but its ultimate effect is reduced by the operation of those factors which orient the profession toward the Democratic Party.

Table 3B reveals that sociologists tend to originate in the families of persons with normally conservative attitudes. Eliminating from the analysis those who gave no answer and those whose fathers were farmers or farm workers, some 2 thirds (64.6 per cent) of the sociologists had fathers who were in the professional and technical categories, or who were managers, proprietors, or officials. On the same basis, 3 quarters of the fathers (75.2 per cent) were in white-collar groupings of all types.

A comparison of these data with those of other studies suggests that sociologists come on the average from families supported by relatively prestigious occupations somewhat more often than do professional persons in general. Of our total sample (298, including those with

Table 2. Most Important Factors in Party Choices of Respondents (in per cent)

	Democrats	*Republicans*	*All respondents*
Policies of party favored	26.6	19.4	25.2
Information gained in profession	22.1	15.1	20.8
Leadership and policies of a particular President	19.7	16.1	18.9
Economic status	7.6	11.8	7.9
Parents	5.5	6.5	6.6
Place of residence	2.9	7.5	3.5
Ethnic or nationality background	2.7	1.1	2.6
Religion	1.8	2.2	1.8
Other	2.7	3.2	2.8
No answer	8.5	17.2	10.0
	100.1	100.1	100.1
Number of respondents	(232)	(31)	(298)

Table 3. Background, Personal, and Institutional Attributes of Respondents[1] (in per cent)

Attributes	*Democrats*	*Republicans*	*Total sample*
A. Parents' preferred parties			
Both Democrats	40.1	19.4	36.2
Both Republicans	31.9	58.1	35.9
Both independents	6.5	—	6.4
Mother Republican, father Democrat	4.3	3.2	4.0
Father Republican, mother Democrat	3.4	6.5	3.7
Other	1.8	—	1.4
No answer	12.1	12.9	12.4
B. Respondents' fathers' occupations			
Professional and technical workers, etc.	32.8	32.3	31.2
Managers, proprietors, officials	24.1	22.6	23.8
Farmers and farm managers	12.5	19.4	12.8
Clerical workers	1.7	—	2.3
Sales workers	6.9	3.2	6.7
Craftsmen, foremen, etc.	9.9	6.5	10.7
Operatives	3.4	—	2.7
Service workers, except private household	5.2	12.9	6.0
Farm laborers and foremen	0.4	—	0.3
Laborers, except farm and mine	1.3	—	1.7
No answer	1.7	3.2	1.7
C. Fathers' denominational preferences			
Protestant	62.5	83.9	65.1
Catholic	12.1	12.9	12.1
Jewish	16.4	—	13.8
Other and no answer	9.0	3.2	9.1
D. Fathers' ethnic, nationality or racial groups			
None	51.7	71.0	53.7
Jewish	17.7	—	14.4
English	6.0	3.2	5.7
Irish	3.4	6.5	4.0
Negro	2.2	—	1.7
Italian	0.4	—	0.7
Other and no answer	18.6	19.3	19.8
E. Respondents' academic salaries			
Below $ 4,000	2.2	9.7	3.4
$ 4,000– 5,999	9.9	12.9	10.4
6,000– 7,999	37.1	22.6	35.2
8,000– 9,999	26.7	25.8	25.8
10,000– 11,999	12.1	22.6	13.4
12,000– 13,999	5.6	—	5.4
14,000 or more	6.5	6.5	6.0
No answer	—	—	0.3
F. Respondents' liberal-conservative attitude scores[2]			
0–48	0.5	18.5	3.1
49–55	3.0	11.1	3.5
56–62	2.0	25.9	5.0
63–69	10.4	—	11.5
70–76	14.4	18.5	15.4
77–83	18.8	11.1	17.3
84–90	27.2	7.4	23.1
91–97	20.8	—	17.7
98 and over	1.0	—	1.2
Incomplete	2.0	7.4	2.3

Table 3. (Continued)

Attributes	*Democrats*	*Republicans*	*Total sample*
G. Respondents' ages			
20–29	4.3	—	3.7
30–39	38.8	22.6	35.6
40–49	32.3	22.6	31.5
50–59	15.1	19.4	16.8
60–69	7.3	29.0	9.7
70 or older	0.9	3.2	1.3
No answer	1.3	3.2	1.3
H. Respondents' religious orientations			
Very religious	12.9	45.2	17.1
Moderately religious	31.5	45.2	33.6
Affiliated because of parental and other influence[3]	14.7	3.2	14.1
Not religious	37.9	6.5	32.2
No answer	3.0	—	3.0
I. Respondents' religious preferences			
Protestant	53.9	77.4	57.0
Catholic	9.5	12.9	10.1
Jewish	11.2	—	9.4
Other and no answer	25.4	9.7	23.8
J. Students in respondents' institutions			
0– 499	7.8	25.8	10.4
500– 999	7.3	9.7	9.4
1,000– 2,999	13.8	19.4	16.1
3,000– 4,999	11.2	3.2	9.4
5,000– 6,999	10.8	9.7	11.1
7,000– 8,999	10.3	12.9	9.4
9,000–10,999	9.9	9.7	8.7
11,000–12,999	4.7	3.2	4.7
13,000–14,999	2.2	—	2.0
15,000 or more	19.8	3.2	16.8
No answer	2.2	3.2	2.0
K. Support of respondents' institutions			
Primarily from a governmental unit	55.6	25.8	49.7
Primarily from non-governmental sources	43.1	74.2	49.3
No answer	1.3	—	1.0
L. Respondents' communities of origin[4]			
Open country	14.2	19.4	14.1
City or unincorporated community			
0– 4,999	11.6	25.8	14.1
5,000– 49,999	21.6	25.8	22.8
50,000–199,999	18.5	6.4	16.4
City			
200,000–1,000,000	10.8	3.2	9.4
More than 1,000,000	20.7	12.9	20.1
No answer	2.6	6.5	3.0
M. Graduate work in respondents' institutions			
Ph.D. in ten or more fields	49.6	35.5	45.3
Ph.D. in fewer than ten fields	49.6	64.5	53.7
No answer	0.9	—	1.0

Table 3. (Continued)

Attributes	*Democrats*	*Republicans*	*Total sample*
N. Respondents' academic ranks			
Instructor	8.2	12.9	9.1
Assistant Professor	24.1	16.1	23.8
Associate Professor	33.2	22.6	30.5
Professor	34.5	48.4	36.6
O. Respondents' home communities[5]			
Part of metropolitan area	51.7	35.5	49.7
Not part of metropolitan area	42.7	64.5	44.0
No answer	5.6	—	6.4

[1] All sections of Table 3 are based on 232 Democrats, 31 Republicans, and a total sample of 298—except for section O, as explained in the text.

[2] The higher scores are those of the more liberal respondents.

[3] The complete statement of this choice was "Affiliated with religion primarily because of parental or other influences, for sake of children, for social contacts, and do not consider it a major force in my life."

[5] The question was: "Is the preceding community (see note to section L) part of a metropolitan area consisting of a city of at least 100,000, with surrounding contiguously populated territory?"

fathers who were farmers, and unknowns), 64 per cent had fathers in the white-collar occupations. In Rogoff's 1940 sample from Marion County, Indiana, approximately 60 per cent of the fathers of professional persons were in those same categories,[4] while the NORC study of 1946 found 57 per cent of the fathers of its professional persons to have held white-collar occupations.[5] These apparent differences between the origins of sociologists and other professionals probably do not result from the fact that the study of the sociologists is of more recent date than the analyses with which it is compared. The time difference should have tended to produce relatively more diverse origins for the sociologists, rather than the more restricted occupational origins actually discovered, because the period encompassing World War II and subsequent years has probably been one of relatively increased occupational mobility.[6]

The information on religious and ethnic affiliation of the sociologists' parents (Table 3C and D), when compared with data on current national distributions, suggests that the Democratic tendencies of sociologists cannot be explained by these background factors. In 1960 the religious preferences of persons 14 years old and over in the United States were expressed in the following percentages: Protestant 66.2, Catholic 25.7, Jewish 3.2, other and no answer 4.9.[7] At the same time Negroes constituted some 10.5 per cent of the population. Obviously, Catholics and Negroes are seriously underrepresented among the sociologists, while persons of Jewish heritage are considerably overrepresented. Since all 3 of these groups are generally regarded as being heavily Democratic in politics, these disproportions in representation appear to largely offset each other. . . .

The fact that sociologists are so largely affiliated with the Democratic Party can scarcely be explained on the basis of salaries inferior to those of other faculty members. The available data suggest that sociologists fare reasonably well in terms of regular salaries when compared to other college teachers. . . . The salaries of other college and university faculty members undoubtedly constitute the most significant referent for a comparison with sociologists; but the figures on the money earnings of year-round, full-time professional, technical, and kindred workers of the United States reveal the probable source of some occupational frustrations for sociologists and therefore probably reveal at least one modest factor related to their tendency to vote Democratic. The median money earned for the indicated professional and technical males in the United States for 1960 was about $7,115 as stated in the *Current Population Reports*,[8] while the median of the salaries reported by the respondents (including both males and females) was in the neighborhood of $8,078 (see Table 3E). Even if an adjustment were made to correct the median for the apparent overrepresentation of the upper grades in the sample, the median for sociologists would still obviously be somewhat higher than $7,115 per year. The source of possible frustration is, however, clearly revealed in the same national survey which showed that full-time, year-round, male, professional, technical, and kindred workers, who were self-employed, had median *earnings* of $10,858 in 1960, while the median *total income* for our sample of sociologists was about $10,196.

In spite of the fact that they earn less than self-employed professional and technical persons, most sociologists obtain some income over and above their regular academic salaries. The facts . . . indicate that 81.9 per cent claimed such income from various sources.

The fact that sociologists show more tendency to vote Democratic than do social scientists in general may be *in part* explained by the fact that they come from families supported by slightly less prestigious occupations and that there are more persons of Jewish background among them. At least, a comparison with the Lazarsfeld and Thielens sample of 1955 so suggests. Of their broader sample of social scientists, 70.2 per cent came from families supported by white-collar occupations, and 4.4 per cent indicated their religious preference to be Jewish.[9] For our sample, the corresponding figures are 64 per cent and 10.1 per cent.

Differences in Attitudes. Whatever may be the reasons for the largely Democratic orientation of sociologists, it seems to be associated with a generally more liberal set of attitudes toward domestic socio-economic issues—as predicted by hypothesis 5. On a crude scale of liberalism-conservatism the Democrats rate markedly more liberal than the Republicans. This scale was constructed by asking each respondent to indicate the strength of his agreement or disagreement with each of 14 statements concerning governmental policy in the areas of balancing the federal budget, public aid in housing, "right-to-work" laws, supporting standards of living, creating jobs in hard times, public development of electric power, old-age pensions, collective bargaining, income tax rates, medical care, ownership and operation of industry, vocational training, preventing depressions, and general approaches to economic security. . . .

The rather sharp difference in attitudes between the Republican and Democratic groups is clearly revealed in Table 3F. That the Democrats are more liberal is shown by the fact that 67.8 per cent of them score 77 or more on the scale, while only 26 per cent of the Republicans score so high. This comparison is based on the responses of 202 Democrats and 27 Republicans. . . .

Characteristics of Republican vs. Democratic Sociologists. The analysis in terms of the fifth hypothesis shows that the tide of Democratic allegiance among sociologists has left a Republican minority marked on the average by distinguishable personal characteristics and occupying positions with identifiable attributes. It seems logical under the circumstances that the Republicans should be on the average older than the Democrats, and so they are (Table 3G). They are also more religious (Table 3H), more largely Protestant (Table 3I), and less active in political campaign participation. They tend to teach in the smaller, privately supported colleges and universities, and to have lived in smaller communities before entering college (Table 3J, K, and L). The various differences in distribution of responses between Democrats and Republicans indicated in this paragraph are all statistically significant at the 5 per cent level.

While a number of other discovered differences (Table 3M, N, and O) between the 2 aggregations are not statistically significant at the 5 per cent level, they may, nevertheless, indicate the presence of actually divergent characteristics between the 2 populations. . . .

The rough accuracy of our findings on the differences between the attitudes and social contexts of the Democratic and Republican respondents is indicated by the similarity of some of them to certain findings of Lazarsfeld and Thielens. Those authors used as criteria of liberalism or conservatism attitudes toward freedom of belief, speech, and organization.[10] They then noted that the more conservative social scientists were more likely to be Republicans[11] and were also more likely to be found in the smaller schools—especially those which were publicly supported, were teachers colleges, or had relatively strong denominational connections.[12] In addition, studies of the general populations of the nation or of communities have indicated tendencies for older persons and Protestants to incline toward the Republicans more than do their opposites; while persons of Jewish extraction, Negroes, and the inhabitants of large cities tend to favor the Democrats, as compared to voters without minority ethnic or racial identification and to those dwelling in smaller places.[13]

Party Changes of Political Activists. We had expected to find that those who had changed parties were more politically active than those who had not (hypothesis 6). This prediction was based on the general assumption that converts of various types are likely to be relatively zealous.[14] Our data suggest that there is some validity to the hypothesis. Of all respondents who reported that they had changed registrations one or more times, 57.5 per cent reported themselves has having engaged in some activity in a regular campaign; while of the remainder of our respondents, only 26.2 per cent indicated such activity. . . .

NOTES

1. See, for instance: Bernard R. Berelson, Paul F. Lazarsfeld, and William N. McPhee, *Voting: A Study of Opinion Formation in a Presidential Campaign* (Chicago: The University of Chicago Press, 1954), 55, 333, *et passim;* Angus Campbell, Gerald Gurin, and Warren E. Miller, *The Voter Decides* (Evanston, Ill.: Row, Peterson and Company, 1954), 72; Richard Centers, *The Psychology of Social Classes* (Princeton: The Princeton University Press, 1949), 62, *et passim;* Paul F. Lazarsfeld, Bernard Berelson, and Hazel Gaudet, *The People's Choice* (New York: Columbia University Press, 1948), 19; also various public releases of the American Institute of Public Opinion as reported in newspapers and magazines. While generally using the usual broad categories, Angus Campbell, *et al.*, indicate the usefulness of more detailed subdivisions of voters by occupation in *The American Voter* (New York: John Wiley and Sons, 1960), 482–483. Information on some scattered studies that separate various types of intellectuals from the general professional category will be found in Seymour Martin Lipset, *Political Man: The Social Basis of Politics* (Garden City, New York: Doubleday and Co., 1960), especially 313–318.
2. See, for instance: Centers, *op. cit.*, 63; Campbell, Gurin, and Miller, *op. cit.*, 72; Campbell, *et al., op. cit.*, 482–483. A Republican preference is considered an index of a conservative tendency; this assumption will be justified in a later part of this paper.
3. Information concerning the reported votes of 405 sociologists in 1948 and 1952 was kindly made available to us by Professor Lazarsfeld and John W. Meyer. The 405 sociologists constituted a part of their larger sample of 2,451 social scientists who were interviewed as a basis of the book cited as: Paul F. Lazarsfeld and Wagner Thielens, Jr., *The Academic Mind: Social Scientists in a Time of Crisis* (New York: The Free Press of Glencoe, 1958).
4. Natalie Rogoff, *Recent Trends in Occupational Mobility* (New York: The Free Press of Glencoe, 1953), 45. Percentages calculated by present authors.
5. Cecil C. North and Paul K. Hatt, "Jobs and Occupations: A Popular Evaluation," *Opinion News* (September 1, 1947) in Logan Wilson and William L. Kolb, *Sociological Analysis* (New York: Harcourt, Brace and Co., 1949), 473.
6. See Charles B. Spaulding, *An Introduction to Industrial Sociology* (San Francisco: Chandler Publishing Co., 1961), 102–106.
7. U.S. Bureau of the Census, *Current Population Reports* (Series P-20, No. 79). Cited in U.S. Bureau of the Census, *Statistical Abstract of the United States* (1961), 42. Percentages calculated by present authors.
8. U.S. Bureau of the Census, *Current Population Reports: Consumer Income* (Series P-60, No. 36), Table 7, 4.
9. Lazarsfeld and Thielens, *op. cit.*, 401. Percentages calculated by present authors.
10. *Ibid.*, 119–127.
11. *Ibid.*, 132.
12. *Ibid.*, 128.
13. Berelson, Lazarsfeld, and McPhee, *op. cit.*, 333–334.
14. For a discussion of the process of conversion, see Tamotsu Shibutani, *Society and Personality* (Englewood Cliffs, New Jersey: Prentice-Hall, 1961), 523–532, especially 527.

American University Teachers and Opposition to the Vietnam War

Everett Carll Ladd

Since the beginning of substantial American military involvement in Vietnam, the academic community in the United States has been the scene of dissent. Most public opinion surveys have found that Americans of high educational attainment, and generally of high economic status, have supported United States activity in Vietnam more strongly than those with less formal education and lower status.

Yet college and university teachers, who are among the most highly educated, have been found to be the most critical. In the spring of 1966, the proportion of academics[1] who thought that American involvement in Vietnam was an error was more than twice as great as that of college and university graduates in a national sample.[2]

In public protests, the universities have of course been very prominent. The "teach-in" is one of the more distinctive forms of protests to come out of the Vietnam war, and it comes strictly from the campus. Since 1967, protests against recruiting on campus by producers of war materials, against the Reserve Officers' Training Corps and against university research on behalf of the United States Department of Defense have become widespread, fusing with other protests against universities and against society at large. There have also been the petitions published in newspapers, listing great numbers of names of war opponents. In the flood of petitions calling for a change in United States policy in Vietnam published in the *New York Times* between 1964 and 1968, academics took the lead.

PETITIONERS AND SIGNATORIES

The act of petitioning governments is a venerable one, and has been provided for in numerous constitutional documents since Magna Carta; the right to petition "for a redress of grievances" is recognised in the First Amendment to the United States Constitution. But if petitioning is an old means for citizens to express grievances, the 1960s have seen a new variant. Protest petitions bearing massive numbers of signatures have appeared in newspapers, with the *New York Times* by far the most favoured. The petitions touched all sorts of issues, but between 1964 and 1968 those on Vietnam predominated. Not all the Vietnam petitions were critical of United States policy, but most were. And while other professionals also contributed petitions, academics published more than anyone else, and provided many more signatures.

The petition campaign was only one vehicle for protest, but it was a substantial one in terms of time, energy and financial support. More than 20,000 university-affiliated persons signed one or more of the petitions published in the Sunday editions of the *New York Times* between October 1964 and June 1968, as a formal public statement of opposition to the war. While there was an element of chance in whether an individual academic was invited to sign—because the method of solicitation was generally haphazard—the coverage was far-flung, and many of the petitions had a national circulation. The most active of the petition sponsors, the Boston Area Faculty Group on Public Issues (Bafgopi), formed in 1961 to protest against the Bay of Pigs invasion, has been managed by distinguished academics. The petition campaign deserves consideration as a major effort by academics to influence elite and mass opinion on an issue of central importance.

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[1] In this article, "academics" refers to regular members of the academic staff of universities and colleges with the rank of instructor and above.

[2] Armor, David J., *et al.*, "Professors' Attitudes toward the Vietnam War," *Public Opinion Quarterly*, XXXI, 2 (Summer, 1967), pp. 162, 172. The survey used in this comparison was conducted at about the same time as Armor's in 1966. Armor worked with a sample of academics at 17 colleges and universities in the Boston area. There is need for some caution in assuming that academic opinion in the northeast was the same or about the same as academic opinion in other sections of the country.

Those academics who signed petitions came disproportionately from universities and colleges in the northeastern part of the United States. Southern and Catholic institutions were notably under-represented.[3] Although there are about 2,400 "institutions of higher learning" in the United States, most of the petitioners came from the 100–150 institutions generally regarded as being in the higher or middle range. They were not all from the great universities, but most were from good ones. Teachers of agriculture, business and education —often described as "more conservative"— just did not sign. The petitioners were not disproportionately junior academics; in fact, full professors were better represented, in relation to their numbers, than their junior colleagues. Nor were the most frequent signatories, as many have assumed, the natural scientists and the humanists. Social scientists participated far more extensively than academics from any other field. The proportion of sociologists signing, for example, was nearly three times that of professors of English, nearly four times that of biologists (Table I).

I conducted a further study of the 640 psychologists, economists, political scientists, sociologists and anthropologists who signed the "Stop the Bombing" petition of January–February 1967. This study sought answers to the following questions: were these social scientists predominantly senior or junior, professionally active or inactive; was petition-signing their principal public expression of opposition to the American policy in Vietnam, or did they engage in other protests; if so, what other protests; what were their chief objections to the war; did the several social science professions differ significantly in their criticisms of American policy; and how had this group of critics of American policy in Vietnam reacted to the series of campus demonstrations in 1968–69 which were linked to—although with issues obviously not contained in—the war in Vietnam.

THE SOCIAL SCIENTIST PETITIONERS

The social scientists who signed the petition of January–February 1967 resemble very closely in rank and age American social scientists as a whole; they resemble, too, the larger body of petitioners. Senior social scientists are slightly overrepresented. The median age of the petitioners in 1967 was 39, of the political scientists alone, 39. The median age of the members of the American Political Science Association in 1967 was 35 (Table II).[4]

On the whole, the petitioners are a productive group—more so, certainly, than the body of American social scientists. They probably compare favourably with their colleagues at the universities of the top and middle range from which they come. Only 5 per cent. have not published at all, while 48 per cent. have published 10 or more scholarly articles, and 42 per cent. at least one book. Among the full professors, 80 per cent. have published 10 or more articles in their fields, and 65 per cent. at least one book (Table III).

Most of the petitioners hold degrees from the major graduate departments, teach at distinguished institutions and are productive scholars. They are also making progress in their careers. Between 1966–67 when they signed the petition and the spring of 1969 when they received the questionnaire, they achieved an impressive number of promotions. Nearly one-half of those below the rank of full professor were promoted (Table IV).

The data thus do not support the suggestion that those who took part in the petition campaign in 1966 and 1967 were generally young and academically less successful or unsuccessful assistant professors.

A large majority of American social scientists are supporters of the Democratic Party. One survey showed that 78 per cent. of the sociologists were Democratic, 10 per cent. Republican, with the remaining 12 per cent. in other categories, mostly independents. The distribution reported for political scientists is similar: 74 per cent. Democratic, 16 per cent. Republican and 10 per cent. other.[5] Another

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[3] See Ladd, Everett Carll, "Professors and Political Petitions," *Science*, 163 (28 March, 1969), pp. 1425–1430.

[4] Eulau, Heinz, "Quo Vadimus?", *P.S.: Newsletter of the American Political Science Association*, II, 1 (Winter, 1969), p. 12.

[5] Turner, Henry A., Spaulding, Charles B. and McClintock, Charles G., "Political Orientations of Academically Affiliated Sociologists," *Sociology and Social Research*, XLVII, 3 (April, 1963), pp. 273–289; and "The Political Party Affiliation of American Political Scientists," *The Western Political Quarterly*, XIII, 3 (September, 1960), pp. 650–665.

Table I. Distribution of Academic Signatories among Fields and Selected Subjects[a]

Field and subject	*Number of signatories (total = 3,037)*	*Percentage of all academic signatories*	*Profession representation index*
Social sciences	747	24·6	251
Sociology and anthropology	171	5·9	304
Political science	146	5·0	284
Psychology	213	7·3	281
Economics	110	3·8	211
Humanities	844	27·8	132
Philosophy	124	4·3	287
History	184	6·3	166
English	264	9·1	110
Languages	138	4·8	89
Physical sciences	489	16·1	122
Physics	231	7·6	253
Mathematics	185	6·1	127
Chemistry	61	2·0	54
Life sciences	556	18·3	117
Health fields	337	11·1	137
Biology	207	6·8	85
Fine Arts	149	4·9	60
Engineering	140	4·6	60
Education	94	3·1	18
Business	12	0·4	10
Agriculture	6	0·2	6

SOURCE: The data in Table I refer to 3,037 full-time academics with the rank of instructor and above, from institutions at which the petition received significant circulation, who signed a petition published in three parts in the *New York Times*, on 15 January, 1967, 22 January, 1967 and 19 February, 1967. This petition was selected for more intensive examination because it had the broadest circulation among academics throughout the country. The 3,037 academics who signed the petition were from 51 colleges and universities. Sponsored by the Boston Area Faculty Group on Public Issues, this petition had a brief appeal: "Mr. President: Stop the Bombing!" The "profession representation index" used in Table I is computed by dividing the percentage of the total full-time academic staff in a particular field into the percentage of all academics from that field who signed the petition. For example, 9 8 per cent. of academics in the United States are in the social sciences; 24·6 per cent. of those who signed the petition are social scientists. Dividing 9·8 into 24·6, we arrive at a profession representation index of 251 (2·51 × 100). An index figure of over 100 means that the field or subject is represented among those who signed the petition to a higher degree than it is in the academic profession as a whole. The data on academics by field were drawn from *Staffing American Colleges and Universities* (Washington, D.C.: US Government Printing Office, 1967), pp. 15–29.

[a] The rank for the social scientists is that which they held in the academic session 1966–67, when they signed the petition.

investigation showed that 70 per cent. of American social scientists are Democratic.[6] The social scientists who signed the petition of January–February 1967 are more overwhelmingly Democratic. Only 4 per cent. supported the Republican Party; only 5 per cent. voted for General Eisenhower in 1956, and 5 per cent. for Mr. Nixon in 1960 and 1968. Not one of the petitioners voted for Mr. Goldwater in 1964. Naturally, in 1968 the social scientists preferred Democrats who had strongly criticised the administration's Vietnam policies. Senators Robert Kennedy, Eugene McCarthy or George McGovern were the first choice of 77 per cent. of the respondents to my questionnaire. Not getting their preference, most of them finally voted for the Democratic nominee, Vice-President Hubert Humphrey. But a large proportion—15 per cent.—claim to have deserted the major parties and cast protest votes either for

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[6] Eitzen, D. Stanley, and Maranell, Gary M., "The Political Affiliation of College Professors," *Social Forces*, XLVII, 2 (December, 1968), pp. 145–153.

Table II. Social Scientists, and All Academics Who Signed the January–February 1967 Petition, by Rank[a]

	No.	*Professor*	*Associate professor*	*Assistant professor*	*Instructor*	*Total*
		%	%	%	%	%
Social scientist petitioners	497	33	31	27	9	100
Economists	82	31	30	29	10	100
Political scientists	114	34	32	25	9	100
Psychologists	170	36	32	24	8	100
Sociologists and anthropologists	131	29	30	31	10	100
All social scientists in US (1963)	16,390	30	26	31	13	100
All academic petitioners	3,037	32	25	31	12	100
All academics in US (1963)	133,089	25	23	31	21	100

SOURCE: The data on the distribution of all social scientists and all full-time academics by rank were calculated from Durham, Ralph A., Wright, Patricia S. and Chandler, Marjorie O. *Teaching Faculty in Universities and Four-Year Colleges, Spring 1963* (Washington, D.C.: US Government Printing Office, 1966), p. 61.

[a] The rank for the social scientists is that which they held in the academic session 1966–67, when they signed the petition.

Table III. Scholarly Output of the Social Scientists Who Signed the Petition, by Subject

	Economics (*no.* = *82*)	*Political science* (*no.* = *114*)	*Psychology* (*no.* = *170*)	*Sociology and anthropology* (*no.* = *131*)	*All* (*no.* = *497*)
No. of books	%	%	%	%	%
None	68	37	82	39	58
1	12	37	12	39	25
2	7	12	1	13	8
3	10	6	3	2	4
4 or more	3	8	2	7	5
Totals	100	100	100	100	100
No. of professional articles					
None	14	10	1	2	6
1–3	27	26	12	18	19
4–6	15	10	13	9	12
7–10	17	14	15	14	15
Over 10	27	40	59	57	48
Totals	100	100	100	100	100

Mr. Eldridge Cleaver, the Black Panther leader, or for Mr. Richard Gregory, the negro comedian. Another 6 per cent. abstained. Mr. Nixon with 24 supporters had less support among these social scientists than either Cleaver or Gregory, who had 48 and 25 respectively.

Senator Robert Kennedy was the first choice of 41 per cent. of the petitioners, Senator McCarthy of 28 per cent. Their respective supporters were not distributed evenly over the social science disciplines. The political scientists preferred Kennedy by a ratio of six to one, while a clear majority of sociologists supported McCarthy.

This difference between the political scientists and sociologists among the petitioners is not an isolated case; similar and related differences between these two groups of social scientists occurred in their attitudes towards other matters.

THE SOCIAL SCIENTIST PETITIONERS: OTHER ANTI-WAR ACTIVITIES

Were the social scientists who signed the protest petition of January-February 1967 generally active in anti-war activities? Or was their public expression of opposition largely limited

Table IV. Distribution of Petitioners of 1966–67 by Rank in 1969

	1966–67 (*no.* = *497*)	*1969* (*no.* = *497*)
	%	%
Professor	33	45
Associate Professor	31	32
Assistant Professor	27	20
Instructor	9	3
Total	100	100

to the signing of the petition? Most academics have not engaged in other protests. In the survey reported by Dr. David Armor, 95 per cent. of the academics of the sample had not spoken publicly against the war, and 90 per cent. had not been involved in any demonstrations.[7] The petitioners in our survey, however, have been generally active. Only one in nine limited his expression of opposition to petition signing. One-third said they had made critical speeches. About the same number had taken part in public demonstrations. Two-thirds frequently discussed the reasons for their opposition to the war with their students, while 73 per cent. provided financial support for anti-war organisations.

The differences among the disciplines in types of protest activity are substantial. The political scientists spoke against the war more than the other social scientists—in public speeches and in discussions with their students. The psychologists were apparently far more reluctant—or saw fewer opportunities—to express their criticisms to students. A notably higher percentage of the sociologists reported that they had engaged in acts of civil disobedience (Table V).

SOCIAL SCIENCE PETITIONERS: THE BASES FOR OPPOSITION TO THE WAR

The 497 social scientists who had signed the petition of January–February 1967, and who responded to our questionnaire, were asked what their principal objections to American policies in Vietnam were. Most answered in some detail; the range of objections was very wide. In analysing the responses, care was exercised to take into account not only the substance of the objections, but the rhetoric used in the objections. Adhering closely to the language and substance of the respondents' objections, I arrived at 41 objections which I considered distinct. Up to three were permitted for each respondent, which gave a quite precise delineation of each person's views. From these specific categories, I constructed five basic types of response. The combinations were not imposed on the data; they occurred to-

Table V. Anti-War Activities of Social Science Petitioners, by Department

	Economics (*no.* = *82*)	*Political science* (*no.* = *114*)	*Psychology* (*no.* = *170*)	*Sociology and anthropology* (*no.* = *131*)	*All* (*no.* = *497*)
	%	%	%	%	%
". . . engaged in any other overt opposition to American policies in Vietnam, besides signing one or more protest petitions."	93	90	82	95	89
". . . contributed money to anti-war organizations and activities."	78	67	70	79	73
". . . discussed reasons for opposition with students frequently."	65	80	49	71	63
". . . written critical letters, or expressed opposition informally to public officials."	63	65	54	57	59
". . . organized public protests."	28	38	28	34	32
". . . made critical speeches."	45	55	14	23	31
". . . engaged in acts of civil disobedience."	10	8	7	18	11

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[7] Armor, David J., *et al.*, *op. cit.*, p. 165.

gether in the answers of the petitioners. All three objections of 373 of the petitioners were contained entirely within one or another of the five basic types. The responses of 108 of the petitioners include elements of more than one basic type, but only two of these contained significant numbers. I describe below the specific objections contained in each basic type.

(1) *Moralist. American policy is immoral:* American actions have been immoral, have inflicted unjustified suffering on the people of Vietnam. The United States has practised mass murder and near-genocide. The United States, not the Viet Cong or North Vietnam, is the aggressor. The United States has behaved as an imperialist power.

(2) *Undemocratic. American policy has failed to support democratic principles in Vietnam:* The United States has not sustained the democratic processes it is supposed to represent. It has ignored the interests and feelings of the Vietnamese people, has used them as a shield. It has supported a corrupt, undemocratic and unrepresentative regime in Vietnam, has imposed a puppet government. It has opposed the genuine desires of the people of Vietnam for revolutionary change.

(3) *Internationalist. American policy is a violation of international law and proper internationalist procedures:* The United States has failed to respect the 1954 Geneva accords. It should not have engaged in unilateral military intervention, but instead should have relied upon the United Nations. In general, in those rare instances in which intervention in the affairs of other nations is justified, that intervention should occur only under the auspices of an international body.

(4) *Democratic. American policy threatens American democracy:* The Johnson administration lied to the American people; it violated the basic trust placed in a democratic government. Defence lobbyists have dominated Vietnam policy-making. The war is being fought for the benefit of generals and profiteers. The poor and needy of America have been forced to bear the burden of this war.

(5) *Realist. American policy is contrary to the national interest, burdened with strategic and technical errors:* The principal error was to intervene in the absence of a stable South Vietnamese regime. Huge miscalculations were made regarding the prospects for victory. The United States became engaged in a war it could not win, in a situation in which the United States had no vital interest. Limited support to South Vietnam would have been acceptable, but large-scale involvement is not. The errors of American policy are political, not moral. American behaviour shows that the United States does not understand the nature of the struggle, and has made miscalculations in foreign policy. The war has distracted the United States from the really important foreign policy concerns, such as a further *rapprochement* with the Soviet Union and steps towards disarmament.

The objections represented here as "Moralist" and "Realist" account for more than half of the respondents; or, if the combinations of types in which these appear are included, nearly three-quarters (Table VI).

The distribution of social scientists of the several disciplines among the principal sets of objections reveals striking differences, and again sociologists and political scientists are at opposite poles. The extent of their differences is easier to see when we examine the two major groups. . . .

The sociologist petitioners were mainly moralistic in their objections to American policy. The political scientists were much more inclined to refer to American interest, and to strategic and tactical mistakes. There were no significant differences in the types of objection among groups of petitioners distinguished by age, rank, quantity of scholarly output or university affiliation.

THE SOCIAL SCIENCE PETITIONERS ON CAMPUS DEMONSTRATIONS

Although the demonstrations and protests which occurred frequently in American college and university campuses in 1968 and 1969 had motives quite unrelated to the Vietnam war, their immediate targets were often connected with the war: for example, representatives of firms producing military equipment engaged in interviewing prospective employees on campus, research institutes receiving grants from the Department of Defense, etc. Despite the similarity of views with respect to the war, a large majority of the social science petitioners were critical of the demonstrations and

Table VI. Principal Objections to United States Vietnam Policies of Social Science Petitioners, by Subject

Types of response	*Economics (no. = 82)*	*Political science (no. = 114)*	*Psychology (no. = 170)*	*Sociology and anthropology (no. = 131)*	*All (no. = 497)*
	%	%	%	%	%
1. Moralist	18	15	28	40	26
2. Undemocratic	18	4	11	9	10
3. Internationalist	2	7	3	5	4
4. Democratic	1	5	8	5	6
5. Realist	28	57	21	14	29
Combinations of types of response					
6. Moralist-Democratic	10	5	12	7	9
7. Moralist-Undemocratic	11	3	10	8	8
8. Undemocratic-Democratic	2	0	3	2	2
9. Realist-Democratic	4	1	2	2	2
10. Undemocratic-Internationalist	4	1	1	1	1
Unclassifiable because of insufficient response	2	2	1	7	3
Totals	100	100	100	100	100

of the demonstrators. Many believed that the demonstrations arose from justifiable grievances, and did not assert that those initiating the demonstrations were solely at fault. But those who believed that the losses exceeded the gains outnumbered those who believed that there was on balance an advantage in the demonstrations by slightly more than two to one. Among petitioners expressing general disapproval, objections commonly took the form of (a) dislike of violence; (b) a belief that the demonstrations disrupted the university community; (c) fear of harmful reactions by such extra-academic groups as legislators—especially a loss of financial support; and (d) the view that however many things were wrong with universities and society, the means chosen by the demonstrators were not appropriate.

Once again, the sociologists and political scientists among the petitioners occupy the polar positions, the former providing the highest proportion of support for the demonstrations. Sociologists formed the only group in which supporters outnumbered critics. The younger members in all departments tended to support the student demonstrations more strongly than their older colleagues. On the whole, however, this group of critics of the war in Vietnam quite clearly were not enthusiastic supporters of the campus demonstrations.

CONCLUSION

The anti-war petitioners from the several social sciences, although generally similar in their professional standing and accomplishments, differed in their political views. The sociologists and political scientists were consistently the most dissimilar from each other. The petitioners from sociology departments were much more likely than their counterparts in political science to express their opposition to the war in moralistic terms. They supported McCarthy in 1968 rather than Kennedy, and were much more in favour of student demonstrations than were the political scientists.

A number of observers have from different standpoints commented on the disposition of sociologists in recent years towards a radical outlook. There seems to be little doubt that sociologists have become the most radical politically and the most hostile towards their societies of any of the social science professions.

Two closely related factors seem to be most important in accounting for this. On the one hand, in marked contrast to American political science which has emphasised a "hard-headed," "realistic" approach to power and its uses, there is an old oppositional tradition in sociology which is now resurgent. American political science lacks a tradition of alienation from the existing institutional system comparable to that from Marx to Mills and other contemporary sociologists.[8] It is this intellectual tradition, I think, which has attracted to sociology in greater numbers than to the other social sciences persons of an alienated disposition.[9] Subject-matter and selective recruitment seem to have contributed to a distinctive academic subculture.

The differences detected among the petitioners appear, then, as products of more general differences in the intellectual and moral traditions of the different social science disciplines. The latter, however, is a subject on which we have relatively little rigorously established information, and our understanding of the political orientation of academics is very much in need of systematic empirical investigation.

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[8] Professor Lipset has noted that sociology "was founded by reformers and continues to deal with topics which inherently remain a focus for discontent, *e.g.*, race, urbanism, stratification, poverty, power, crime, delinquency, etc." Lipset, Seymour Martin, "The Politics of Academia" (mimeographed), p. 25.

[9] Studies of prospective concentrators in sociology among first-year undergraduates and beginning graduate students in the 1950s found a very high proportion committed to social reform. See Selvin, Hanan C. and Hagstrom, Warren O., "Determinants of Support for Civil Liberties," in Lipset, S. M. and Wolin, Sheldon (eds.), *The Berkeley Student Revolt* (Garden City: Doubleday Anchor, 1965), pp. 512–513.

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Subject Index*

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* Material has been indexed according to the original vocabulary used by each of the authors represented in the book.