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**Leading Edges  
in Social  
and Behavioral  
Science**

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# Leading Edges in Social and Behavioral Science

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EDITED BY  
R. DUNCAN LUCE  
NEIL J. SMELSER  
DEAN R. GERSTEIN

Working papers from the ten-year outlook on research opportunities,  
a project of the Committee on Basic Research in the Behavioral  
and Social Sciences, Commission on Behavioral and  
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Behavioral Sciences and the Social Science Research Council.

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# Preface

The thirty papers that constitute this volume evolved under very special circumstances. In 1983, the Committee on Basic Research in the Behavioral and Social Sciences (National Academy of Sciences/National Research Council) was asked by the National Science Foundation to help define priorities for increased federal investment in behavioral and social science research. Early in its preparations for this "ten-year outlook" on research opportunities, the Committee polled some 2000 behavioral and social scientists throughout the country, asking them to identify the most promising areas of research in their fields.

With the benefit of about 600 responses, the Committee delineated thirty topical areas that appear to occupy the leading edges of behavioral and social science research in the United States. For each area, the Committee appointed an expert working group of five to ten scholars and asked the group to prepare a 20-page statement outlining the main directions of new research on that topic. These working papers eventually informed and were incorporated into the Committee's final outlook report, *The Behavioral and Social Sciences: Achievements and Opportunities* (Washington, D.C.: National Academy Press, 1988).

The editors of that report, in consultation with the leadership of the Russell Sage Foundation, came to believe that the working papers themselves were of sufficient value to be published as a companion volume. Such a collection of papers would have the merit of exploring more intensively each of the relevant topical areas and providing specific references to the underlying scientific literature.

The papers that follow are remarkable from a number of standpoints. First, they are almost all interdisciplinary in character, reflecting the nature of the topics chosen and the composition of the working groups. Second, they represent a genuine and direct effort by leading scholars to reach consensus on the leading edges of research in the behavioral and social sciences. And third, these papers provide more detailed

guidance than did the summary report for those readers wishing to delve deeply into one or another of the topics.

Most of the working papers printed here were completed in 1986. The task of gathering the papers, obtaining the cooperation of each working group, and devising a format in which these disparate studies might appear was considerably facilitated by the efforts of the staff of the Russell Sage Foundation, without whose assistance this project would not have reached completion.

*R. Duncan Luce*  
*Neil J. Smelser*  
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PART

**I**

MIND AND BRAIN



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# 1

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## Sensory and Perceptual Processes

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When you look around, you see a variety of objects at various distances, some still and some moving, some transparent and some vividly colored, some partially obscured by others, and some not. All is perfectly ordinary and seen without effort if light is good and your visual system is normal. Your cat or dog sees these things, too, although somewhat differently from the way we do. And so does the fly that evades the swat. And listening to a musical recording, you have little trouble hearing separate instruments playing separate parts. If someone speaks over that background, people with normal hearing have little difficulty in understanding the words.

It is all so commonplace, but we do not know exactly how the brain does any of these things. No one yet knows enough to program a computer to pick out objects from a scene, to isolate a violin in an orchestral passage, to separate speech from noise and partition it into words. It has been estimated, for example, that computer implementation of complex visual processes could require billions of computer operations per momentary image. In fact, the difficulties of artificial perception are as yet insurmountable for anything but greatly restricted sets of stimuli because stimuli (e.g., the patterns of light on the retina) are inherently ambiguous. Enabling machines to do the complicated perceptual tasks that humans do would surely affect the way people live as much as the telephone, the thermostat, or radar have done. And we know these tasks can be done because our brains do so, continuously and apparently effortlessly. Of course, a good deal of our brain is occupied in this task. Something between a quarter and two-fifths of the total human cortex is devoted to vision alone (e.g., Van Essen, 1985).

Understanding such abilities is the focus of current perceptual research. The new studies are multidisciplinary, full of innovative theoretical work, highly dependent on sophisticated experiments with animals, and increasingly facile in using computers to develop theoretical models and simulate experimental tasks. The research involves psychologists, biologists, physiologists, physicians, graphic artists, physicists, engineers, and computer scientists.

An important part of the study of human perception is currently concerned with discovering the constraints used to resolve stimulus ambi-

guity in the hope that these constraints may then be used by machines. Similarly, those studying machine perception often suggest ideas that may apply to human perception. Developments in neurophysiology often result from behavioral analyses of sensory and perceptual processes, which provide evidence for functional distinctions that can be found in the workings of the brain.

Work with various species of experimental animals is playing an important role in learning how brains accomplish the tasks of seeing and hearing. By recording the electrical behavior of single nerve cells and studying the wiring diagrams of how these cells are arranged in the brain, researchers are beginning to understand the architecture of perceptual systems and how they process information. There are multiple processes arranged in a hierarchy of serial stages and parallel streams (e.g., Cowey, 1985; Van Essen, 1985). In addition to monkeys, animals such as cats, rabbits, goldfish, and barn owls have yielded valuable information about sensory systems. The appropriateness of the species depends upon the particular question being asked and the similarity of the relevant aspect of its perceptual system to the human system.

Overviews of many of the problems of perception and possible solutions can be found in Hochberg (1988) and Marr (1982). The two massive volumes of the *Handbook of Perception and Human Performance* (1987) edited by Boff, Kaufman, and Thomas summarize the current state of empirical knowledge about sensory and perceptual processes with a bias toward topics deemed particularly applicable to applied research and development. Recent work on taste and smell can be found in Roper and Atama (1987).

A number of examples of recent developments are described below. These must be considered examples only. Not only were they chosen by a limited group of people but they were chosen as much for what we hope is their easy describability to persons outside the field as for their intrinsic scientific merit.

## Seeing

### *Color Constancy*

When working in a room illuminated by the sun during the day or by artificial light during the evening, we notice little change in the color of objects. A blue shirt looks blue, quite independent of the light shining on it. Yet this invariance that people take for granted cannot be repli-

cated by color photographic media or other artificial devices at present. The difficulty is that the light arriving at our eyes or at a camera from such objects is very different in the two cases because it confounds the ambient illumination of the scene with the surface reflectances of the object. Artificially constructed devices cannot at present separate these components and, for example, recognize the same surface color under different illuminations. Recent analyses by quantitative psychologists of how this task could be done (again made possible by the availability of modern computers) have led to an algorithm that can, under certain conditions, perform an exact recovery of the surface reflectances as well as of the incident illumination (Maloney and Wandell, 1986). This algorithm is likely to be useful in artificial devices that must be responsive to objects' colors (e.g., in quality control or cameras) and can provide insights into why, for example, color constancy should improve as the number of distinct objects in a scene increases and why one of the three classes of human photoreceptors is less densely packed in the retina than are the other two.

### *Receptor Mosaic*

The initial neural stage in vision is the set of photoreceptors, the first of five neural layers in the human eye. The pigments in these photoreceptors are their most fundamental property for color vision. (On the basis of behavioral evidence—only recently confirmed by anatomical, physiological, and chemical evidence—it has long been thought that three photopigments for daylight vision must exist.) The spatial arrangement of these photoreceptors—which can be quite regular—is their most fundamental property for pattern vision. Results from behavioral experiments and mathematical calculations, augmented by anatomical results, have added substantially to our understanding of how the arrangement and spacing in the photoreceptor mosaic influence and limit pattern vision (e.g., Hirsch and Hylton, 1984; Miller and Bernard, 1983; Williams and Collier, 1983; Yellott, 1983).

One line of research uses patterns produced directly on the retina by interference between laser lights. When the interference-fringe pattern consists of very fine lines, the observer sees it as broad wavy lines due to the beating between the period of the line pattern and the period of the receptor mosaic itself. Reports of this appearance and the way it changes with alterations in the parameters of the stimulus (the spatial frequency and orientation of the lines) can be used to estimate the interreceptor spacing at various places in the retina and also the actual geometric arrangement of the receptors in the receptor

mosaic (Williams, 1985). This method has great potential for easily identifying anomalies in the receptor mosaic of patients exhibiting visual pathologies.

### *Spatiotemporal Analyzers*

We have learned, as a direct result of intense interaction between neurophysiological and behavioral studies aided by technological advances, that the initial stages of visual processing do not merely analyze the light point by point. Rather, one important low-level stage consists of multiple spatiotemporal analyzers that act in parallel to process the pattern of light at the eye. The physiological substrate for these may be the single neurons in areas V1 and V2 (the lowest levels of primate visual cortex). Each analyzer responds preferentially to stimulus components within some narrow range of size and orientation (technically, to components within some range of two-dimensional spatial frequency); the component may also have to be moving in a particular direction. (These analyzers are sometimes said to perform a crude spatiotemporal Fourier analysis, but that phrase is misleading unless carefully interpreted.) Precise quantitative models based on these analyzers can now explain human detection and identification of low-contrast visual patterns; we may soon be able to account for early shape processing. An overview of this work on multiple analyzers—with many further references—can be found in Graham (1985) and Graham (in press).

### *Design Evaluation and Image Augmentation*

A direct practical application of quantitative models embodying these multiple parallel spatiotemporal analyzers is the evaluation of visual displays such as flight simulators, video terminals, warning lights, and information signs. The great advantage of a model in such applications is that it can be used at design time, before the device is actually built, to evaluate the performance of the potential device.

As mentioned above, artificial vision (by machine or computer) is still far from feasible. What has proved feasible—due to our increased understanding of the early stages of visual processing—is the use of computers to aid human perception. Under computer control, for example, inherent inadequacies of electrical and optical visual imaging systems are compensated for. Information that is particularly useful to the human perceptual system may be enhanced at the expense of less useful information according to principles that have been dormant (and largely unquantified) in the Gestalt literature (see Metzger, 1953) and are now

being developed in computational vision (see Barrow and Tenenbaum, 1986; Pratt, 1978; Rosenfeld and Kak, 1982). Developing such image augmentation techniques further should prove particularly useful in perceptual tasks such as reading X-ray plates, satellite reconnaissance displays, tracing circuit diagrams, following blueprints and maps—tasks that are slow and error-prone without augmentation—and may soon lead to more economical transmission of visual images over phone lines (e.g., Sperling, 1980).

### *Depth and Motion*

Any point in the two-dimensional pattern of light at the eye could, in principle, have been produced by some point in the world at any distance away from the viewer. This ambiguity between two-dimensional images and the three-dimensional scenes from which they come is a most critical source of ambiguity for both human and machine perception. In human vision, the constraints that resolve ambiguity are commonly called depth cues and have guided computer-vision attempts to recover three-dimensional information from shading, motion, stereo, texture, and so on (e.g., Cavanagh, 1987). Much further work is needed, however, particularly on how humans or machines may integrate these cues with one another and on how to use them to obtain complete three-dimensional descriptions (Hochberg, 1987).

### *Global Motion Analyzers*

In addition to the low-level analyzers that are sensitive to the direction of motion of stimulus components, recent behavioral results have revealed higher-level motion analyzers. These are sensitive to the global direction of a pattern's motion even when the individual spatial-frequency components of that pattern are moving in other directions. These behavioral results suggest an investigation of differences among neurons that are selective for direction of motion. As it turns out, the low-level neurons in V1 and V2 are sensitive to the direction of component motion (even in cases where the human observer sees global motion) while higher-level neurons are selective for the direction of global motion (Adelson and Movshon, 1982; Movshon, Adelson, Gizzi, and Newsome, 1984).

### *Moving Visual Illusions*

Higher-level visual processes act on the outputs of lower-level visual processes to produce our perceptions of objects. Properties of these



higher-level processes may be revealed in their apparent failures, that is, in visual illusions. Although many perceptual psychologists have held for decades that illusions arise only in static artificial situations, we now know—greatly helped by the availability of computers and visual displays—that illusions are prevalent in the perception of moving scenes by moving observers. Instead of perceiving the moving three-dimensional world in just that organization which preserves rigidity, images that could be seen veridically only as nonrigid objects are actually seen as nonrigid and in incorrect motion and orientation (e.g., Schwartz and Sperling, 1983).

These illusions, while contributing to our understanding of visual process, also have direct practical implications. Illusions may be the underlying cause of many vehicular accidents on the ground or in the air. On the positive side, many of these illusions imply that large distortions in visual displays could be tolerated or even go unnoticed by an observer.

### *Perceiving Objects*

Mere fragments of objects and events suffice, in normally cluttered scenes, to produce appropriate perceptions. Somehow, the processes of visual analysis fill in the big picture from a collection of details. One reason why this occurs readily in natural scenes is that people tend to see correlated movement of disconnected parts as a single moving object partially masked by another object.

Some understanding has begun to emerge of what might be called a "natural visual language," a way to describe the meaningful elements in a visual scene economically and appropriately as perhaps the human perceptual system does. The natural components of an object may be recognizable in an image on the basis of clues like deep concavity of edges. The number of possible natural components may be very small, perhaps about thirty-six canonical shapes (e.g., Biederman, 1985). From a recognition of these primitive components, the object itself may be recognizable.

A stage of processing in between the multiple spatiotemporal analyzers that act in parallel (the substrate for which may be VI) and this small vocabulary of component object shapes may be revealed by the asymmetries in search tasks. Searching for a circle with a break in it that is located in a field of complete circles, for example, is much easier than searching for a complete circle among circles with breaks (Treisman, 1987; Treisman and Gormican, 1988).

The importance of contours in object perception has been assumed for decades, but recently an elaboration has been suggested in which there are two boundary systems: the "boundary contour system," which

computes the boundaries of perceived objects (whether or not the boundary itself will be perceived, as it will not, for example, when an object is occluded by another), and the "feature contour system," in which visible percepts are developed (Grossberg and Todorović, 1988). The idea of two boundary systems explains a number of previously puzzling results and has been rigorously embodied in a cooperative-competitive neural network model capable of making testable predictions.

In any case, humans can recognize objects extremely well even when presented so quickly that most of the objects are forgotten a very short time later (e.g., Intraub, 1984). Progress on how human observers so quickly segregate objects from background and then recognize the objects should aid in many situations—for example, in the design of factory robots that must make complex identifications of objects in order to carry out their function (without selecting the wrong object or inadvertently hurting someone).

## Hearing

Recent advances in auditory physiology and in computer generation of sound, combined with increased interest in the complex acoustic patterns of the environment, have strongly influenced and aided recent research in auditory behavior.

### *The Active Ear*

One example of the fruits of interaction among these areas is "Kemp's echo," the discovery that the ear (probably the inner ear or cochlea) not only receives sounds but also produces them, both in response to a click or brief tone and spontaneously (Kemp, 1978). The precise source of these emissions is not yet known, but their discovery has already spurred clinical studies of the relation of these emissions to tinnitus. These emissions are one of several phenomena leading to a new view of the ear as an active device (perhaps not unlike an electronic musical instrument) that may amplify and tune the physical forces that strike it.

### *Auditory Distance Perception*

A person negotiating traffic, no less than a barn owl hunting prey, needs to know how far away the source of a sound may be. Recent research has shown that this knowledge depends on a relation between the intensities of the direct and reverberant (echoing) components of a sound.

### *Infants' Hearing*

As we all know, human learning of speech takes place in infancy and early childhood. Some discoveries have been made about the development of hearing during this period (e.g., Trehub and Schneider, 1985). For example, human infants' ability to detect tones seems to be comparable to that of adults, perhaps even superior for very high frequencies. But the ability to localize sounds in space develops during the first six months of life.

### *Auditory Patterns and Perceptual Learning*

It is with auditory patterns that most of our daily hearing is concerned, from the delights of music and the stimulation of conversation to the everyday necessity of crossing a street without getting killed. But, until recently, relatively little research was done with auditory patterns (partly because of the difficulties of generating them in a controlled manner for experimental use). One recent approach to understanding the processing of complex sounds has been to measure a listener's ability to discriminate among tones embedded in temporal patterns of tones. Discrimination proves to be very much worse for a tone embedded in a pattern than for a tone in isolation—until a listener has had an opportunity to learn what the tone should be from hearing the pattern in highly predictable circumstances (e.g., Watson and Kelly, 1981). Such studies of perceptual learning bear on the problem of how a child learns the sound patterns of its language.

A complementary line of work measures listeners' sensitivity to the overall shape of the function relating sensitivity to auditory frequency (the spectrum) for complex auditory stimuli. Such sensitivity is clearly necessary for speech perception, but has only recently been studied quantitatively by using synthetic auditory stimuli with different spectral "profiles" (Green, 1983).

### *Grouping of Musical Sounds*

One benefit of the now-spreading study of auditory pattern recognition has been an increased understanding of music. Musical keys and scales are highly structured, and musicians, mathematicians, and philosophers have long speculated about the underlying nature of that structure. Recently, rules of counterpoint and orchestration have been explained not as arbitrary requirements of particular musical styles but as the result of some basic tendencies of the auditory system to group sounds in certain

ways as a step on the road to auditory pattern recognition. Researchers some time ago postulated that a set of ratios corresponding to a helical geometric structure underlies tonal perception. Recently, far more systematic studies using trained musicians as respondents and multidimensional scaling to analyze the results have led to a modified understanding of the geometric relations (e.g., Krumhansl, 1985). Stimulated by the success of linguists in analyzing language as an activity governed by a system of rules, psychologists also have made a successful beginning in finding rules that become internalized in the mind of a listener in the course of growing up in his or her musical culture. They have demonstrated how these rules affect the perception and memory of the listener (Bharucha, 1984; Deliege, 1987). Still other research is uncovering the general rules by which listeners segregate one auditory stream (one conversation perhaps or one melody line) from another (e.g., Bregman, 1981).

### *Hearing Aids*

A growing body of research attempts to understand in more detail the psychophysical abilities of hearing-impaired listeners (e.g., McFadden and Wightman, 1983) and to design hearing aids that do more than merely amplify the frequencies for which the patient's detection threshold is abnormally high.

### *Speech Vocabularies*

A large receptive vocabulary (already some 14,000 words in a six-year-old, middle-class, American child, and roughly 100,000 to 150,000 words in an educated adult) seems to be possible because the sound pattern of every language draws on a few dozen meaningless units (the phonemes, or consonants and vowels) to construct a very large set of meaningful units (words, morphemes).

### *Shingled Speech*

Yet, the acoustic flow of speech cannot be readily divided into a sequence of segments corresponding to the segments of linguistic description (phonemes, syllables, or words). The reason for this is that we do not speak phoneme by phoneme, syllable by syllable, or even, for the most part, word by word. At any instant, several articulators (larynx, tongue, velum, lips, jaw) are executing a complex, interleaved, rhythmic pattern of movements reflecting the influence of several neighbor-

ing segments. The consequence of this shingled pattern of movement is, of course, a shingled pattern of sound, such that any particular acoustic segment specifies more than one linguistic segment, while any particular linguistic segment is specified by more than one acoustic segment.

### *Speech Recognition*

Closely related to this discrepancy between acoustic and linguistic segments is a lack of invariance. The acoustic structure of any perceived segment—phoneme, syllable, or word—varies widely with a variety of factors: phonetic context, stress or emphasis, rate, style of speech (e.g., formal versus casual), and individual speaker. The lack of invariance leads to a host of constancy problems that any speech recognition system—human or artificial—must solve. How does the recognizer perceive something as constant when the actual stimulus is variable?

For artificial speech recognition, several pragmatic solutions are currently being pursued. For example, automatic speech recognition systems often attempt to finesse the problem by developing templates for whole words (for which the range of variability is obviously less). If selected words are spoken slowly, in a limited number of contexts, by a few speakers, recognition may be quite good. Current automatic speech recognition systems have attained vocabularies of 1,000 to 5,000 words, and, with increases in computational power, we may expect increases in vocabulary. However, all these solutions are largely brute force solutions, and dramatic improvement is more likely to come from a deeper understanding of speech motor organization and how this organization is reflected in the speech signal.

Another approach entails the construction of large data bases of natural speech (Pisoni, 1985). One goal here is to compile an exhaustive description of the nature and range of acoustic variation as a function of phonetic context, rate, stress, and so on, in a particular language. Another goal is to collect detailed information concerning the phonetic structure of words in a lexicon and their probabilities of occurrence as a means of constraining the search space in word recognition. Knowledge gained from such data bases may then be incorporated into models of word recognition in fluent speech.

If such models are to be scientifically rewarding, it is important that they be not only computationally but also psychologically and linguistically adequate. One of the few attempts to develop such a model (McClelland and Elman, 1985) had a lexicon of only 211 words. But the model is remarkable for its attention to short-term phonetic detail, its capacity to use contextual variability as a source of information rather

than of noise, its simulation of a wide range of behavioral effects observed in psychological studies, and even the insight it lends into the emergence of certain linguistic "rules." Work of this kind rests on interdisciplinary collaboration among psychologists, linguists, and computer scientists.

### *Speech and Motor Control*

There is growing evidence that the true perceptual primitives of speech may not be phonemes (consonants and vowels) at all but patterns of movement—vocal gestures—from which consonants and vowels emerge. For example, a fifteen-month-old child, attempting the word "pan," may correctly open and close its lips, raise and lower its velum, raise and front its tongue, but in the wrong phasal relations, thus emitting an utterance that may be roughly transcribed as "mbi." (Speech perception is a function, in most normal individuals, of the left cerebral hemisphere, in which motor control of the speech apparatus is also vested; see, e.g., Kolb and Whishaw, 1980. Children's phonological errors are a rich source of information; see, e.g., Locke, 1983.)

The increased interest in speech, displayed by students of motor control in recent years, is, therefore, an important development (e.g., Grillner, Lindblom, Lubker, and Persson, 1982). The field of motor control itself is evidently in a state of rapid theoretical development, with considerable controversy concerning the roles of central pattern generators, feedback mechanisms, and self-organizing processes in the coordination of complex, multicomponent motor systems. Many of the general problems faced by the field find particular instantiations in the study of speech. Speech, perhaps the most complex of all motor behaviors, may prove to be a valuable testing ground for the developing theories of movement science (Kelso and Tuller, 1984).

### **Taste, Smell, and the Skin Senses**

Taste, smell, and the skin senses (touch, temperature, pain) are linked in a variety of ways. Anatomically, taste and olfactory receptors differentiate from skin cells. Functionally, there are interactions in terms of the stimuli that excite the receptors. For example, touch, warmth, or cold can stimulate some taste neurons, some odorants stimulate tactile receptors in the nasal cavity, and some chemical stimuli excite certain pain receptors.

The history of investigation into these three senses also shares certain features. In all three senses there was early speculation that specialized

receptors would subserve each basic quality of sensation, followed by a period of disillusionment with specificity. (For example, individual taste neurons were not responsive only to stimuli having a common taste quality to human subjects.) The pendulum has swung again, however (e.g., Pfaffman, 1974). The most recent coding theories for taste maintain that each neuron signals only one taste quality upstream (e.g., saltiness). Other stimuli that excite that neuron (e.g., warmth) are really inadequate and produce only the normal sensation evoked by that neuron's best stimulus. Similarly, studies of both the skin and olfactory systems have suggested more order and specificity than was thought (e.g., Gesteland, Yancey, and Farbman, 1982; Kenshalo, 1979).

### *Sweetness*

The "sweet tooth" is found in all mammals. We know that sweet substances have widely diverse chemical structures. Is there one part of the molecule common to all sweeteners that could be the fundamental sweet unit? The answer is a qualified "yes." When dissolved, molecules of a sugar like glucose twist in such a way that three sites on the molecule form a triangle of particular size and shape. The three sites are believed to form bonds with three complementary sites on the taste membrane. This theory accounts for the sweetness of a large array of molecules.

### *Individual Differences*

However, there must be more than one such triangle to account for genetic differences among individuals. Many high school biology students are tested for their ability to taste PTC (phenylthiocarbamide) as bitter; two-thirds of the population do, one-third does not. An inability to taste PTC seems to be a simple Mendelian recessive characteristic. We have recently learned that tasters of PTC (those with at least one dominant gene) taste some sweeteners as more sweet than nontasters do. The differences between tasters and nontasters vary depending on the sweetener tasted. Tasters and nontasters clearly live in very different taste worlds.

### *Development and Plasticity*

The human fetus has some taste ability before birth. For example, if sweet substances are introduced into amniotic fluid, the fetus will swallow. However, the ability to taste salt appears to develop later. In adults on low sodium diets, NaCl, or sodium chloride, comes to taste saltier than it used to (e.g., Beauchamp, 1987). This very fortunate change

means that after a while on a low sodium diet, an individual can get a pleasant salty taste with less salt. This example of plasticity for the taste of NaCl raises questions about the effects of other diet or bodily changes (e.g., illness, pregnancy) on the taste of foods.

### ***Aging***

There is a common but incorrect belief that taste is lost with age. In fact, age takes its greatest toll on olfaction (smell) while taste remains relatively unimpaired (Stevens, Bartoshuk, and Cain, 1984). This erroneous belief results, in part, because food placed in the mouth produces many sensations: taste, smell, touch, temperature, and pain (in the case of spicy food). The loss of olfaction with age can endanger lives: The odorous substance added to natural gas (it has no odor of its own) to warn users of leaks becomes useless as olfaction is lost. (That particular odor is also one of several odorants to which some individuals are insensitive from birth.)

### ***Tactile Aids for the Deaf***

For the deaf, two approaches of considerable interest employ nonacoustic presentations of sounds. One strategy (Reed, Durlach, and Braida, 1982) aims to transform speech into vibratory or electrical patterns on the skin of the speech perceiver. In the second strategy—the Tadoma method, which has evolved within the community of persons who are both deaf and blind—the speech perceiver uses his or her hands to monitor the facial motions of the talker. The success of this latter method demonstrates that tactile information is adequate for speech reception. Current research is directed toward understanding the success of the Tadoma method and developing tactile aids that operate on acoustic waveforms and thus do not require direct physical contact.

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## Psychobiology of Learning and Memory

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How the brain codes, stores, and retrieves memories are among the most important and baffling questions in science. At the cellular level, there are two fundamental types of information coding. One is the familiar genetic code, shared by organisms from virus to man. In higher organisms, millions of bits of information are coded in the DNA of the cell nucleus. Over the course of evolution, a quite different kind of information coding has developed—the cellular encoding of acquired information in the brain. This coding is no less remarkable than the genetic code.

The generic term for acquired information coding in the brain is “memory trace.” The fundamental difference between the genetic code and memory code is, of course, that each individual human’s memory store is acquired through experience and learning. It is the biological substrate for the growth of knowledge and civilization. The uniqueness of each human being is due largely to the memory store—the biological residue of memory from a lifetime of experience.

The development, elaboration, and growth in size of the human brain over the past 3 million years are unprecedented in evolution. Indeed, some biologists now feel that a new principle or mechanism of evolution may have come into play, a mechanism that involved the rapidly growing ability to learn, with its consequent development of culture and transmitted knowledge. The cellular roots of this ability to learn can be traced to simpler organisms. The basic phenomena of associative learning, ranging from Pavlovian conditioning to operant learning, seem to exhibit similar fundamental properties from invertebrates to rats to humans. In the past generation, understanding of the biological basis of learning and memory has undergone a revolution. It is now clear that various forms and aspects of learning and memory involve particular systems, networks, and circuits in the brain. At least for simpler forms of learning, it is now both theoretically and technically possible to define these circuits, localize the sites of memory storage, and analyze the cellular and molecular mechanisms of memory.

The roots of this new understanding lie in several different disciplines often collectively termed psychobiology, which is notable for its multidisciplinary analysis of complex problems. From psychology has come a

clear characterization of the behavioral properties of learning—what learning and memory are—and a developing conceptual and theoretical analysis of the nature of the associative processes that form the basis of learning and memory. From network analysis we are learning how memory and cognitive properties can emerge as collective properties of systems of neurons. From the broad field of behavioral neuroscience has come the recognition that identifiable neural memory systems and circuits in the brain can be characterized and analyzed. From neurobiology we are learning about the cellular, biophysical, and molecular mechanisms that underlie elementary forms of associative learning in simple neural circuits in invertebrates as well as vertebrates.

The success of this collective approach has been a source of great optimism in the field; fundamental insights into the physical basis of memory will continue to be achieved over the next few years. Indeed, many of us feel that the field of memory research is now in the critical “breakthrough” stage. The present would seem to be the most exciting phase in the history of the field. These insights will be of significance not only from a basic scientific perspective but also from an applied or clinical perspective.

In the following report we will illustrate some of the recent advances that have come from the psychobiological approach, indicate relevant future directions where important breakthroughs can realistically be expected, and suggest applications of this new knowledge to the improvement of the human condition.

## Recent and Future Advances

### *Human Memory and the Brain*

Complaints about memory are common to students, aging adults, and neurological and psychiatric patients. There is good reason for this. The term memory is applied to many diverse processes of learning, retrieving, and reconstructing information. Disorders of human memory are found not only in amnesia but also in aphasia, anomia, dementia, apraxia, depression, and other disorders. There are also other methods to explore the time dynamics of processes in the human nervous system that may underlie memory. Event-related potentials recorded from the scalp are known to be related to changes in efficiency of performance in repeating an operation. It is likely that imaging methods will also allow exploration of operations involved in storing memory.

At the other end of memory performance is the feat of the chess master who may store 50,000 different chess positions. There is work in progress that seeks to dissect the basis of such expert memory, train it, and understand its components. We now realize that a large number of mature readers reach a level of skill in word recognition during silent reading not far below that of the chess master. In recent years computational models of memory retrieval have provided a basis for decomposing memory tasks, like those involved in access to familiar words, into a number of component mental operations.

Our ability to isolate the many codes present in reading and other memory-dependent tasks is basic to understanding the neural systems underlying performance. This type of research should lead eventually to a clarification of the processes of both routine and creative performances. It may provide new methods of formatting information for memory that will aid both in designing artificial intelligence systems and in training human intelligence.

Of the many questions that can be posed about memory, one of the most fundamental concerns how many types or varieties of memory exist. Throughout much of the history of experimental psychology, memory has been viewed as a unitary entity that consists solely of the forming and strengthening of associations. Although alternative ideas have been expressed from time to time, it is probably safe to say that until recently a majority of researchers accepted the notion of a unitary memory, and thus attempted to explain memory in terms of a single set of general principles.

In recent years, however, the results of both psychological and neurobiological investigations have suggested that memory is not a unitary or monolithic entity. A growing number of investigators have instead proposed that memory is composed of dissociable "types," "forms," or "systems" that fundamentally differ from one another. Research that explores the nature of and relation between these distinct memory types or systems has the potential to alter our basic conceptions concerning the nature of memory, and therefore represents an exciting and important direction for future research. The general idea that there are multiple forms of memory has also been investigated in a growing number of studies concerning nonhuman memory, and hence constitutes an issue of interdisciplinary interest.

A clear example of evidence for a nonunitary view of memory has been provided by studies showing that subjects' performance on certain types of memory tests can be facilitated by a recent exposure to an item (e.g., a word), even though they have no conscious memory of having seen the item before. This phenomenon is known as a *repetition priming*



*effect.* Several studies have demonstrated that the magnitude of priming effects is uncorrelated with performance on standard memory tests in which subjects are required to recall and recognize recently studied words. It has also been found that experimental manipulations that have large effects on recall and recognition do not affect priming, and that experimental manipulations that affect priming do not influence recall and recognition. Taken together, these results suggest that priming effects may be mediated by a type of memory system that differs from the one that underlies conscious recall and recognition. Our current understanding of the basis of priming effects is quite rudimentary and merits intensive investigation in the future.

A second, related area of research concerns the study of preserved memory abilities in brain-damaged patients who suffer from organic amnesia. Amnesic patients have a severe inability to recall and recognize recent events, and have difficulties learning new facts and many other kinds of information. However, such patients possess some relatively intact memory and learning abilities. On certain memory tasks, amnesic patients can perform as well as normal subjects, even though they may have no conscious memory for having performed the task before. One source of relevant evidence is that amnesic patients show normal repetition priming effects, in spite of poor recall and recognition. A second kind of evidence is that amnesic patients can acquire various motor, perceptual, and cognitive skills in a normal or near-normal manner. This evidence provides support for the general proposition that there are distinct and dissociable types of memory: some kinds of learning can proceed normally even when the brain structures that mediate conscious remembering (hippocampal and diencephalic structures) are damaged.

### ***Behavioral and Theoretical Analysis***

In the last decade tremendous changes have taken place in the behavioral study of learning in nonhuman organisms. Many of the advances have occurred in the study of elementary associative learning processes. There has been an unusually fertile interaction between the emergence of phenomena shedding dramatic new light on the nature of associative processes and the development of theories providing an explanation of those phenomena. Phenomena such as overshadowing, blocking, conditioned inhibition, the role of contingencies, and potentiation have revolutionized our conceptions of conditioning. Formal theories have increasingly converged on a quantitative account of these phenomena, and new techniques have facilitated these developments. In the minds of

many, basic associative learning in animals is now on the verge of fulfilling its original promise of being a model system for the analysis of learning processes in general.

Behavioral studies of learning have also taught us much about biological "preparedness," the biological constraints on learning and memory. Organisms ranging from invertebrates to humans readily learn to associate distinctive tastes with subsequent illness—the taste aversion learning paradigm. Yet they cannot learn to associate distinctive tastes with immediate threat of physical injury, an event to which sights and sounds are readily associated. And of course, sights and sounds cannot be associated with subsequent illness. Evolution has shaped the brain to learn best those things that tend to be adaptive. Ethological studies have also illuminated other important aspects of learning in the natural state, for example, foraging behavior. Such work has led to distinctions among possible categories of learning, such as between "working" and "reference" memory.

Imprinting in certain species of birds provides yet another example of biological preparedness, serving as an adaptive mechanism for parental and species identification. Song learning in birds that have a species-typical song is perhaps the most striking example of biological preparedness in learning. The young birds apparently have a song "template" encoded in the brain such that they learn best the species-typical song; yet if exposed only to an atypical song they will learn a compromise. Birds, such as the canary, that learn a new song each year appear to have developed an extraordinary biological adaptation that includes growth and subsequent atrophy of brain tissue each year in association with song learning. Song learning may prove a very useful model for possible biological substrates of that most cognitive aspect of human learning and memory—language acquisition.

The next decade will likely see three sorts of developments in the behavioral and theoretical analysis of learning. First, there will be continued refinement of quantitative theories of association based on existing conditioning preparations. This will include the analysis of the learning about new sorts of stimuli, such as spatial stimuli and contextual stimuli. Analysis of these more complex learning structures is actively underway in several laboratories.

Second, we will see the broadening of the analysis of associative structures to new instances of learning. Particularly important in this broadening will be the role learning plays in operant behavior and in the natural life of the organism. Many researchers feel that in the interests of developing tractable model systems, we have neglected the function that learning plays in the natural behavior of organisms. We are

seeing an increasing interest in the success at accounting for noncanonical but functionally important instances of learning. Also important in broadening the scope of the study of learning will be the development of behavior model systems that are especially tractable for a neural analysis.

Third, we will see increasing interest in various instances of nonassociative learning. The last decade has seen increasing biological interest in such instances of learning as habituation and sensitization. Neural analysis has proceeded rapidly, but we still need systematic behavioral analysis of these types of learning so as to provide a quantitative data base worthy of neural analysis.

It is clear that as neural accounts of learning develop they will depend heavily upon the highly developed study of learning at a behavioral level. The last decade has shown the positive impact that attention to the behavioral level of analysis can have on neural studies. Indeed, with the current rapid rate of technical advances in neurobiology, it is quite possible that progress in the neurobiology of learning will be limited mainly by our rate of developing the appropriate behavioral analysis.

A relatively new aspect of the psychobiology of learning and memory has to do with theoretical or computational modeling of learning and of memory circuits and networks in the brain. As memory circuits are empirically defined in both invertebrate and vertebrate nervous systems, it becomes essential to determine quantitatively what these circuits and their associated neurobiological processes are in fact capable of doing. This can be achieved only by mathematical/computational modeling. There is increasing evidence that more complex phenomena of memory and cognition can emerge as collective properties of neural networks—properties of large interacting systems of neurons that individually can change their response characteristics. Changes on a cellular level must be related to learning and memory storage on a network level. The problem of the organization of such networks is a theoretical/mathematical question of enormous complexity. Although biological elements are involved, this is not entirely a biological problem. For this reason the theoretical/mathematical analysis of such networks is of major importance. It is also essential that these analyses proceed in the closest interaction with empirical studies. In recent years, the analysis of one such complex network, an approximation to the visual cortex, was in agreement with a large number of experimental results. Such quantitative modeling and mathematical analysis will form strong bridges between artificial intelligence, associative learning theory, and empirical studies of memory networks in the brain.

### *Animal Models of Learning and Memory*

The recent development of primate models of human amnesia offers much promise. It should be possible to identify the neural systems involved in acquiring and storing different kinds of memory, to specify more clearly what the functions of these systems are, and to undertake investigations at the cellular/neurophysiological level that have a clearer connection to functional-behavioral questions than ever before.

Important advances have been made in understanding neural analogues of plasticity in such higher brain structures as the hippocampus (e.g., long-term potentiation). Alterations of the strength of synaptic interaction can be induced artificially and measured, and it may also be possible to observe the spontaneous occurrence of these changes as a consequence of the animal's own behavior during the course of learning. A basic problem in understanding the role of higher brain structures in information processing has to do with characterizing what groups of neurons do (rather than one neuron at a time). New techniques are being developed that will permit the simultaneous monitoring of the activity from many brain cells. With such techniques, each cell can be individually examined, and its interactions with other cells determined, while animals are engaged in freely moving behaviors that place significant demands on their cognitive capacity. The goal of this approach is to study the same ensemble of cells over extended periods of time, under different behavioral conditions, in an effort to understand the dynamics of cellular interactions leading to different cognitive functions. This empirical work relates closely to current theoretical/mathematical analysis of the emergent properties of neural networks.

In the past decade, important conceptual advances have been made in neurotransmitter/neuromodulator processes in learning and memory. The background for advances in this area is being provided by enormous progress in defining the neuroanatomical organization of neurochemical systems and their physiological functions. Certain transmitters, for example, NE, seem to act as global modulators of neuronal function. Both norepinephrine NE and acetylcholine ACh may play particular roles in "critical period plasticity," as in the development of ocular dominances in the visual cortex. At present our knowledge about the role of neurotransmitters/neuromodulators in learning and memory in the mammalian nervous system is based primarily upon the effects of systematically administered drug treatments, an approach that provides inadequate differentiation of the distinctive components of neurochemical systems that are now being recognized, such as ACh or opioid peptides.

It has recently been recognized that in neuropeptide systems different patterns of activity may include modifications in the processing of

peptides, resulting in end products that possess differences in their neurochemical actions. In addition, when neurochemicals are co-localized, patterns of input can simultaneously induce different effects on the processing of each neurochemical system that resides within the neuron. It should be emphasized, however, that progress in understanding neurotransmitter/neuromodulator mechanisms in learning and memory will depend upon an ability to target specific populations of neurochemically identified neurons within neural systems identified as playing critical roles in learning and memory.

In the past few years, unexpectedly rapid progress has been made in identifying essential memory trace circuits that code, store, and retrieve associative memory in the mammalian brain. Two basic aspects include learning and memory of discrete, adaptive behavioral responses and learning of autonomic responses. As in other aspects of the field, this progress has been due in large part to the systematic exploration of animal models. In the case of discrete behavioral responses learned to deal with aversive events, much of the essential circuitry, the "necessary and sufficient" circuits for learning and memory, have been identified, and evidence grows that the memory traces themselves are stored in the cerebellum. Eyelid conditioning in mammals has been a particularly productive model here. Similarly, much of the essential circuitry coding learning of cardiovascular-conditioned responses has been identified, using such models as pigeon, rabbit, and baboon.

It is very likely that these essential circuits, which code, store, and retrieve memories for basic associative learning in the mammalian brain, will be fully identified in the next few years. This will permit a much deeper understanding of how the mammalian (and human) brain functions as an information-processing system, which in turn may result in an entirely new approach to artificial information-processing systems. The problem of identification and localization of essential memory trace circuits, the barrier of "localization," has now been breached. As the essential memory trace circuits are fully identified, it will be possible to localize the storage sites and make substantial progress toward analyzing the mechanisms of memory storage, particularly those underlying long-term or permanent memory. This in turn could yield many practical benefits in the context of disorders of memory.

### *Cellular and Molecular Mechanisms*

An area of great current interest and excitement focuses on the question of basic cellular and molecular mechanisms underlying learning and memory. With the advance of modern biophysical and biochemical ap-

proaches, significant progress has been made in specifying possible substrates of memory in terms of particular ionic conductances and molecular events. From work in both mammalian and invertebrate preparations, one emerging picture is that memory storage involves the reduction of one or more species of potassium conductance in nerve cells, which can profoundly alter the way those cells both receive and transmit information. This process is in turn thought to be brought about by a "second-messenger"-mediated phosphorylation of specific substrate proteins, which can give rise to long-lasting neuronal changes. Second-messengers (such as calcium or cyclic adenosine monophosphate (AMP) are attractive molecules because they themselves can serve as the agent for short-term memory, while their intracellular effects can include possible genomic regulation involved in lasting cellular changes. The attractive feature of this collective work is that the molecular mechanisms emerging as important in these vertebrate and invertebrate systems are highly conserved in evolution, offering the promise that they may be of general significance.

In the near future significant progress can be expected from work in invertebrate preparations in specifying possible cellular pairing mechanisms involved in classical conditioning. In *Hermisenda*, pairing is thought to occur by means of cumulative depolarization in photoreceptors induced directly by light and synaptically by the simultaneous offset of light and rotation. In *Aplysia*, pairing is thought to occur by means of activity-dependent enhancement of the same process that underlies sensitization (presynaptic facilitation). Both systems are sufficiently well developed that progress in specifying details of these acquisition mechanisms should be rapid.

A second advance concerns molecular mechanisms of memory. In *Aplysia*, *Hermisenda*, and *Locust*, critical changes involve reduction of  $K^+$  currents. In *Aplysia*, *Hermisenda*, and *Drosophila*,  $Ca^{++}$  or cAMP-mediated phosphorylation of substrate proteins modulated by biogenic amines are implicated in memory. The next few years will surely provide further clarification of the generality of these findings and their applicability to mechanisms of memory storage in the mammalian brain.

The next major breakthrough may well center on long-term memory. This is likely to involve some genomic change (perhaps triggered by a second-messenger) that can modify, for example, the kinds of receptors or channels that are inserted into membranes over the long run, and even produce long-lasting structural changes in synapses and neurons. Suggestive evidence for this last possibility now exists for both invertebrate and mammalian systems.

## Applications

In a general sense, the more we understand about the neural basis of learning and memory the more we will know about the human mind and brain. This increased basic knowledge will have important practical applications in fields ranging from learning disabilities to the design of artificial intelligence systems. Following are a few specific examples, some remote and some of more immediate promise.

### *Education*

Education is a multibillion-dollar industry in the United States. The study of learning and memory provides basic research to support developments in education. Improvements in our understanding of how memories are formed and retrieved, how they relate to one another to build semantic structures, and of their relation to attention can help guide our efforts to achieve more efficient learning among both normal and handicapped populations. Studies of acquired deficits in amnesia, aphasia, and dyslexia are already helping us to clarify the specific problems in learning skills such as reading. At the other end of the spectrum is the memory of experts. Efforts to understand the act of retrieval in experts and novices may clarify how information can be organized for efficient retrieval and provide support both for teaching expertise to humans and for designing expertise in computer systems.

### *Normal Aging*

Since learning/memory systems are not invariant throughout the life span, it will be important to use a multidisciplinary approach to document normal brain and behavioral change. Thus, recent findings suggest that stimulation-induced synaptic plasticity in hippocampal neurons does not persist as long in old as in young adult animals. It is against this background of normal development that the major age-related dementing illnesses should be characterized. For diagnostic purposes, it is crucial to be able to discriminate between normal developmental alterations and early stages of true pathological states. As we get closer to an understanding of the mechanisms of normal brain change with age, inter-ventive therapies could be developed for these less devastating changes.

### *Human Memory Disorders*

Organic memory disorders have debilitating effects that extend to virtually all aspects of a patient's day-to-day existence. It is not yet known

whether effective interventions can be developed to lessen the impact of amnesia. One potentially promising approach is suggested by the recent research on preserved learning abilities in amnesia. Since we know that amnesics can acquire some kind of new knowledge normally, and since we also know something about the conditions that favor its acquisition and expression, it may be possible to develop techniques that promote the acquisition of knowledge that is helpful or necessary for everyday functioning. Priming techniques might be used to teach patients names of new acquaintances, facts and ideas that are important in an educational context, or even more complex forms of knowledge that are needed to return to gainful employment. Research that explores such possibilities could be profitably pursued in conjunction with theoretically oriented work on preserved learning. The development of a useful technology of knowledge acquisition for amnesia patients is likely to depend upon further understanding of the nature of the knowledge that can be acquired through primary or other preserved abilities. Alzheimer's Disease, a debilitating and progressive neurological condition affecting 5 percent of persons over 65 years of age, has at the present no known cause or cure. But it seems likely that as our understanding of the brain substrates of memory increases, treatments may be developed.

Psychobiological studies of memory can be expected to illuminate the pathophysiology of other major neurological conditions that cause cognitive impairment, such as epilepsy and the effects of anoxia. Particular cell layers in the hippocampus seem to be most sensitive to the damaging effects of seizure, ischaemia, and anoxia. Memory problems are the prominent sign of these conditions. Cellular/neurophysiological studies, directed at the hippocampus, may help develop the drugs or other treatments that can reduce vulnerability of the hippocampus to injury; models of anoxic or epileptic injury in experimental animals may result in behavioral tests to study such conditions in humans; and careful behavioral analysis of human amnesic patients, some of whom have this condition, may provide tests for sensitive early detection of memory problems and a way to quantify it so that the effects of positive treatments can be readily measured.

### **Mechanisms of Support**

Study of the psychobiology of learning and memory is in a unique "breakthrough" phase at present, a rare event in any field of science. Workers at all levels from theoretical modeling to molecular mecha-



nisms share a strong sense of excitement. Essential memory trace circuits are being defined, we are beginning to understand molecular/biophysical substrates, and theoretical analysis is proceeding apace.

From psychology has come a clear characterization of the behavioral properties of learning and memory and a developing conceptual and theoretical analysis of the nature of the associative processes that form the basis of learning and memory. Analysis of human memory suggests that there are several types or varieties of memory; examples include priming and selectively preserved memory abilities in amnesics. Perhaps the most fundamentally important aspect of the field has been the development of highly productive animal models of learning and memory processes, ranging from behavioral studies of the categories of learning and memory to primate models of human amnesia to identification of essential memory trace circuits in the mammalian brain and localization of the memory traces to neural analogues of plasticity in higher brain structures to a growing understanding of the neurophysiological, neurochemical, molecular, and biophysical mechanisms of memory in both invertebrate and vertebrate neural systems. Theoretical and computational modeling of learning and memory circuits and networks has proceeded apace, as has mathematical analysis of phenomena of memory and cognition that can emerge as collective properties of neural networks.

We anticipate significant and in some instances relatively immediate applications of this rapidly expanding understanding of learning and memory and the biological substrate in a variety of areas. This is in fact the scientific field that provides basic research for the entire field of education. Improvements in our understanding of how memories are formed and retrieved will guide our efforts toward improved education among both normal and handicapped populations. An increased understanding of the properties of expert memory will provide the basis for teaching expertise to humans and for the design of expertise in artificial intelligence systems. Psychobiological studies of memory will illuminate and ultimately provide a basis of treatment of memory disorders associated with such conditions as amnesia, seizures, ischaemia, and anoxia. An increased understanding of the neuronal basis of memory deficits in normal aging will help in developing techniques for enhancing performance and hopefully for the ultimate treatment of such severe disorders of memory as occur in Alzheimer's disease. Finally, as our understanding of the molecular and biophysical basis of memory deepens, it may be possible to develop dramatic new biological/biochemical approaches to the treatment of human memory disorders and possibly even to the ultimate enhancement of human memory and cognitive capabilities.

The psychobiology of memory is a uniquely interdisciplinary field. Key aspects range from behavioral studies in humans and animals to theoretical modeling and run the entire gamut of the neurosciences to chemistry and physics. No one person can be expert in all or even several aspects of the field. But it is essential that interdisciplinary training, collaboration, and interaction be increased. We suggest a series of mechanisms to provide such interactions, listed below in order of priority:

1. An approach that could make use of existing support mechanisms would be to establish interdisciplinary institutes at selected universities, using the vehicle of institutional program grants. Such funding should be targeted at theoretical modeling, behavioral/cognitive science, psychobiology, and neurobiology to focus on a particular memory circuit, network, or aspect of learning and memory. Such institutes could also provide a base for particularly expensive equipment, such as imaging devices. Thus, an institute with a focus on brain substrates of cognition in humans could house a human-head-sized NMR; one with a focus on learning and memory circuits in an animal model could house a smaller NMR or other imaging system; an institute with a focus on mathematical analysis and computational modeling might house or have access to a supercomputer or one of the new experimental parallel processing systems.

2. Perhaps most novel would be the establishment of institutes across groups of universities. Such institutes could provide the basis for the most rapid advances in specific projects. For example, the identification, localization, and full analysis of memory trace circuits require the expertise and resources of investigators working at many different levels of analysis. A group of investigators with a focus on a specific memory circuit may not be optimally assembled from within a single institution.

3. Summer training institutes focusing on behavioral/cognitive/theoretical aspects of brain research should run programs for students and young investigators with some degree of expertise in neurobiology and psychobiology. Several extremely successful model summer training institute programs already exist in neurobiology, for example, at Woods Hole and Cold Springs Harbor.

4. Interinstitutional pre- and postdoctoral fellowships would make it possible for graduate students, postdoctoral fellows, and young investigators to work and train in laboratories at more than one institution so they can receive training at more than one level.

5. Basic animal research centers, each perhaps with a focus on a particular model system/category of learning to provide long-term interdisciplinary support for the most promising and productive of the current model system approaches.

6. Establishment of clinical research centers with foci on particular aspects of human learning and memory disorders.

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## Information and Cognitive Sciences

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## Introduction

The reasons for investigating human cognition are manifold and pressing. For example, one of the most serious problems facing this country today is adult illiteracy. Current economic trends suggest that people with inadequate cognitive skills will probably find it even more difficult to compete for jobs in the future than they have in the past. To cite a specific instance, the ubiquity of computers and computer-based systems has established the importance of computer literacy for a large segment of the population.

In fact, information technology has the potential for providing new and qualitatively different ways of representing information for human assimilation. Effective exploitation of that potential requires a fuller understanding than we now have of human perceptual and cognitive capabilities and limitations.

Finally, it is surely not an overstatement to suggest that ineffective reasoning is one of the fundamental sources of the difficulties that face individuals, organizations, and nations. A better understanding of how and why reasoning goes astray is needed, as is the development of more effective approaches to the teaching of reasoning skills.

The cognitive sciences have made significant recent advances in understanding human cognition. Chief among them is the discovery of underlying structure where none was previously apparent. "Memory," for example, has been shown to be a collection of separate structures and processes. Our additional knowledge about the organization of cognition has already had an important impact in education, clinical settings, industry, and research in artificial intelligence. Future work will increase the precision of our description of how humans process information, and produce more fine-grained analyses of the structure of the processing system.

The discussions that follow do not constitute an exhaustive catalog, but they do highlight some of the primary topics of interest to contemporary cognitive scientists.

## Attention

### *Selective and Divided Attention*

Most of the time we attend to the object at which our eyes point. Fixating objects of interest is, after all, a major function of eye movements. Recent research has shown, however, that we can look "out of the corners of our eyes"—that is, without moving the eyes, one can concentrate attention on an object in the periphery of the visual field (e.g., Jonides, 1981). The effect of doing so is much like actually fixating the object: information about it is processed faster and more accurately, and other objects in the field suffer slower and less accurate analysis. This ability to shift attention covertly exemplifies central control over the allocation of our cognitive resources, with consequences for the information that gets processed from the outside world. Consider also our ability to listen to one conversation to the exclusion of others occurring in a crowded room. To accomplish this, there must be exquisite control over the object of attention even though there is no peripheral mechanism that will gate out the unwanted information. Both examples illustrate the importance of attentional mechanisms in channeling of information for further processing.

The mechanisms used to focus attention continue to be the subject of intense study. Current issues include the stimulus characteristics that elicit shifts of attention, the effectiveness of mechanisms that gate out irrelevant information, the effect of familiarity with particular signals on their attention-capturing properties, how quickly attention can shift from one stimulus to another, and the role of internal shifts of attention on later changes in eye position. Studies of these issues have led to information processing models of selective attention; these models have contributed to research on neurophysiological mechanisms underlying selective attention, based on measures of single-unit neural activity and on the study of patients with attentional pathologies.

A second line of research concerns the extent to which we can divide attention among several sources of information at once. This is clearly a question of practical import since there are many situations in which observers (such as pilots and nuclear power-plant operators) must monitor several information sources for a significant event. Recent experiments indicate that whether the information from any one source is degraded depends on the class of signals, but not on whether they are in the same or different sensory modalities (Shaw, 1984). The use of precise measurements of processing time and computer-generated displays

and further advances in processing models will keep this a lively topic of research.

### ***Multiple-Task Performance***

Can we do two things at once? If so, under what circumstances? These have been central questions in the study of complex performance throughout the history of cognitive psychology. Clearly, the bottleneck in many complex tasks is with the human operator, not with the machinery with which the operator interacts. How is it that sometimes we can perform two tasks simultaneously nearly as well as we can each individually, but other times not? How is it, for example, that we can normally drive a car and converse with little interference, but not read and converse? In order to understand such complex multiple tasks, we must continue to study skilled and unskilled subjects in complex task environments.

One of the more exciting developments in recent research on this topic has been the study of automaticity and its development in complex task situations. Various researchers have established, for example, that mere practice at a task is not sufficient for the development of automaticity in performance. The practice must be of a particular type, in which stimuli and responses are tightly tied together in a nonvarying way from one presentation to another (e.g., Shiffrin and Schneider, 1977). Under these conditions, even such difficult tasks as reading and taking dictation can be accomplished simultaneously with remarkable efficiency (Spelke, Hirst, and Neisser, 1976). Once future research, both empirical and theoretical, establishes the conditions that promote the development of automaticity, there will be ample opportunity to apply these conditions to natural situations in which operators are responsible for the execution of multiple tasks.

The study of multiple task performance will be facilitated by the convergence of various techniques that are now being applied to the problem. One technique allows investigators to have subjects engage in two tasks simultaneously and to examine the quantitative trade-offs in performance on the two tasks with great precision. Another technique involves the measurement of brain potentials during participation in a dual task situation. This latter technique, and other measurements of brain activity as it is tied to environmental events, promise to provide the opportunity to identify brain mechanisms responsible for various sorts of cognitive activity.

### *Processing Complex Displays*

Research focused on the processing of complex displays has made substantial progress in recent years in identifying the mechanisms that are important to this processing. One exciting discovery is that attentional allocation is crucial to the identification of objects in visual displays. To many observers it seems obvious that quick inspection of a display, with little attention or effort applied to its processing, yields a veridical perception of the objects contained therein. Not so. Research has shown that one must attend to each object individually for the features of a display to be "mentally glued" together properly into the objects that are actually present in the display (e.g., Treisman and Schmidt, 1982). Viewing a complex display while attention is diverted elsewhere may well result in perception of the gross features of the display, but these features may not conjoin into the objects that were actually present. A red ball presented with a green block might give rise to the perception of a red block and a green ball if attention is not concentrated on each of the objects. And the more features that must be conjoined to properly synthesize the percept of a display, the longer it takes to perceive it. This and other research begins to outline the importance of attentional processes in object perception.

The role of attention in complex scene perception extends well beyond the study of object perception as well. How are locations in a visual display selected for attentional allocation? What is the role of familiarity with the regularities in scenes on the order in which parts of scene are examined? How are eye movements guided to important regions of a display? How is all the information that is collected about a scene over time integrated into a coherent perception of the scene? These and other issues are at the heart of current research, and their answers are important to the understanding of complex tasks such as comprehending the complex readouts in the cockpit of a modern jetliner or in the control room of a nuclear power plant.

Future research on these and other related questions is likely to be fruitful for several reasons. First, there are multiple experimental techniques that have already proven successful in addressing these problems (e.g., measurement of the time to process a display, measurement of eye fixation behavior while viewing displays). Second, attention to these problems is coming not only from experimental psychologists, but also from the computer science community. For example, there is emerging research in which the phenomenon of illusory conjunctions of features, described above, has been simulated on an artificial intelligence system.

Also, there is artificial intelligence research concerned with the integration of features within complex scenes. A synergy between the psychological and computational approaches to these problems will accelerate progress in the next several years.

## Learning and Memory

### *Learning of Facts versus Procedures*

A fundamental problem in the theory of human knowledge is the relation between knowledge of facts and principles on the one hand, and knowledge in the form of skills and procedures on the other. There are many examples of the distinction. For example, medical students learn many facts and principles in physiology and other sciences; they also must learn the skills required for conducting diagnostic interviews and examinations and choosing treatments.

Recent research concerned with this distinction suggests that the two kinds of knowledge are acquired and retained in distinct memory systems with quite different properties. The system for skill, which evolved earlier, reflects "habits," and functions according to conditioning principles—that is, the mental operations underlying successful actions are strengthened by rewards. This system does not necessarily involve conscious awareness, and it remains intact in human amnesia. The more recently evolved system does involve conscious awareness, deals with facts and propositions rather than with procedures, and enables the person to access specific past experiences.

It would be tempting to think of these two learning systems as prelinguistic and linguistic, but the distinction is not that simple, as recent research has shown. Participants in a recent study learned a list of uncommon words like "assassin" and "universe" and were later tested for retention in two rather different ways. The first test was recognition memory—the person had to decide which of the words in a long list had been learned previously. The second test made no mention of "memory"; subjects were simply asked to complete word fragments such as

\_SS\_SS\_ \_.

It was found that the effect of learning on recognition memory declined strongly over a week, whereas its effect on the ability to complete word fragments declined very little. Thus two tasks involving language appar-

ently are under the control of different memory systems. Memory about the occurrence of words in a specific situation depends on the conscious system of factual knowledge, while completing a word from partial cues is independent of conscious recollection.

### *Strategies of Memorization*

People differ greatly in their success in remembering factual and other information, and often think of themselves as having "good" or "poor" memories. Research shows that memory processing is significantly affected by the kind of activity that a person engages in when the information is presented, and the way in which the information is related to knowledge that the person has. In one series of experiments, subjects in different conditions either attended to superficial features of words (e.g., does a word contain a specified letter?) or to the words' meanings (e.g., is the word synonymous with a specified word?). Later recognition memory for the words varied from 15 percent to 80 percent simply as a function of the different types of mental operations performed during learning. The words, the time allowed for study, and the time between study and test were held constant.

The activity of attending to meanings of words probably improves memory of their occurrence by integrating the information in the study experience with an existing body of knowledge that both relates the studied words to other known words and enables construction of a mental representation that distinguishes the study experience from previous experiences of the same type.

The importance of a systematic knowledge base for memory was also demonstrated by research in which individuals practiced daily for several months to improve their ability to remember arbitrary sequences of digits. Recalling a sequence of digits is often used as a test of intelligence, and capable adults can usually recall sequences of 6–8 numerals presented one per second. One person who practiced the task for several months learned to recall sequences of more than 80 digits presented one per second. He did this by constructing representations of the digits in sets that could be times to run different standard distances (the person was an active amateur runner), and developing a way to organize these short "chunks" of digits into larger groups in a structure with four or five levels that could be retrieved reliably, albeit with considerable effort.

These findings show that success in memorizing depends on knowledge that a learner can acquire and activities that he or she can perform.

### ***Question-Induced Distortions in Memory***

We usually assume that when we ask a question, a person who answers truthfully reports information from his or her memory. This assumption underlies the legal use of eyewitness testimony. Recent research has cast serious doubt on this psychological assumption. Linguistic analyses have identified important presuppositions that play a significant role in all understanding, including the understanding of questions. When a question is asked, knowledge is presupposed that makes the question meaningful. Recent psychological research has shown that such knowledge is often constructed by the listener if it does not already exist in his or her memory. The result can be a significant change of the information in the person's memory brought about by the question.

For example, in several studies concerned with eyewitness testimony, Loftus and her associates found that memory of an event can be distorted by the way a question about the event is phrased. After seeing a film of a car accident, witnesses gave substantially higher estimates of speed when they were asked "How fast were the cars travelling when they smashed together?" than when they were asked "How fast were the cars travelling when they bumped together?"

Other studies have shown that the original memory can be modified when a question presupposes the existence of an object. Thus a witness might be asked "How fast was the car travelling when it passed the white barn?" even though no barn had been present in the original event; nevertheless, the witness will often report seeing a barn when later recalling the initial experience. Understanding the limits and vagaries of such real-life remembering is of obvious importance in legal procedures and in other situations where much depends on accurate recollection. The findings also have important theoretical implications for understanding how memories can be influenced by the retrieval environment.

### ***Spatial Cognition: Imagery***

If an earthquake caused a particular skyscraper to disintegrate, which other buildings would be in jeopardy? Or, which tree should be chopped down so that it would fall to form a bridge over a nearby stream? These kinds of questions could be answered by building an actual model, but only with considerable time and expense. A better solution would be to program a computer to run a simulation or, even better still, to train people to run such simulations in their heads. It is interesting that the human mind seems to have evolved the ability to perform just such simulations when reasoning about physical problems.



Consider a classic finding of Shepard and Metzler (1971): They showed subjects pairs of blocklike forms, and asked whether the members of each pair were identical irrespective of orientation. As illustrated in the figure, the time subjects required to make this judgment increased linearly with the disparity in the orientations of the two forms, just as if the subjects "mentally rotated" an image of one form to the orientation of the other. This finding is remarkable because images are not actual objects that must obey the laws of physics. Because they cannot move with infinite velocity, real objects must pass through intermediate points along the trajectory as they change orientation; but images are not objects, and hence need not necessarily obey physical laws. Rather, it appears that our imagery mechanisms are built so that imaged events are constrained to mimic aspects of actual physical events. However, images do not necessarily mimic all aspects; for example, rotating objects in images need not show effects of acceleration or inertia. It is an empirical question as to which properties of physical transformations are preserved in images, one that can only be answered by further research.

Research in this area is expanding along two fronts: First, more research into the basic properties of the imagery system is underway. For example, it has been found that body parts (such as arms and legs) are more easily "mentally rotated" around joints than around an unnatural axis; the implications of this finding have yet to be explored in detail. (Is visual imagery tied into the motor system?) Second, more research is directed at understanding the uses of our imagery abilities. In addition to being important for spatial reasoning, imagery has been shown to have a role in learning, memory retrieval, language comprehension, and perception. For example, people who have more vivid images have been found to read concrete prose more slowly than people who have less vivid images, which suggests that vivid imagers take time to "mentally illustrate" as they read. There is no difference in the time required by vivid and nonvivid imagers require to read abstract prose.

The study of imagery has already begun to be productive in three areas of application. First, it has led to a much more sophisticated view of the components of imagery ability, and the basis of differences between individuals. Recently it has been discovered that people's intuitions that they are globally "good" or "bad" at imagery are vastly oversimplified. Imagery is not a unitary, undifferentiated phenomenon, but consists of distinct subabilities, and people can be good or bad at a given component of imagery ability independently of any of the other components. Second, imagery research has led to an increased understanding of the cognitive deficits that can result from brain damage. With the aid

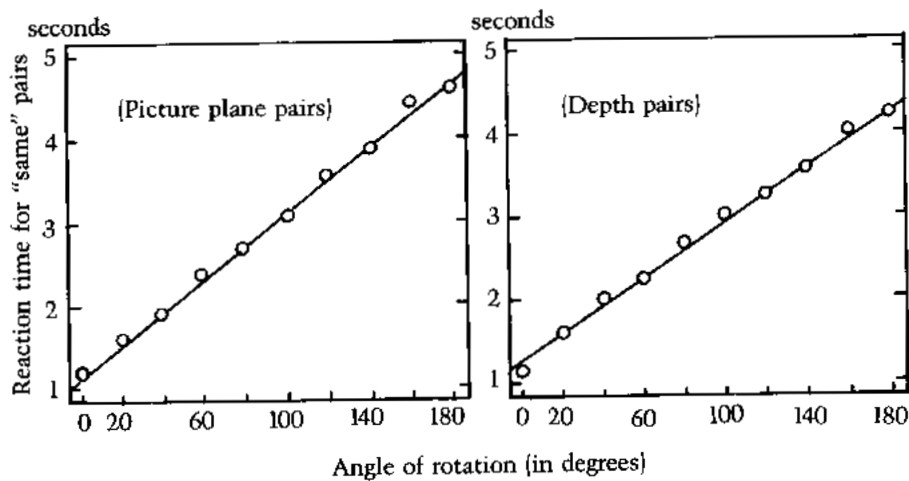
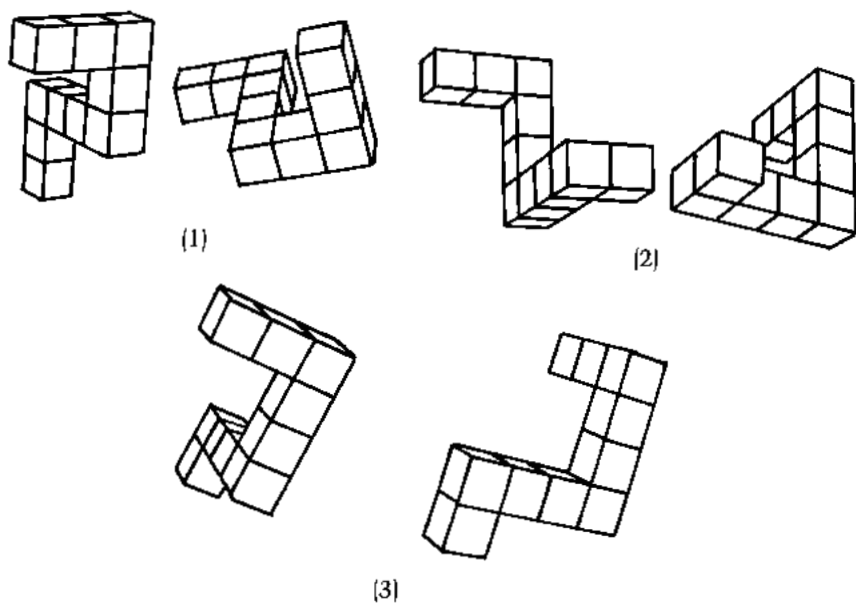
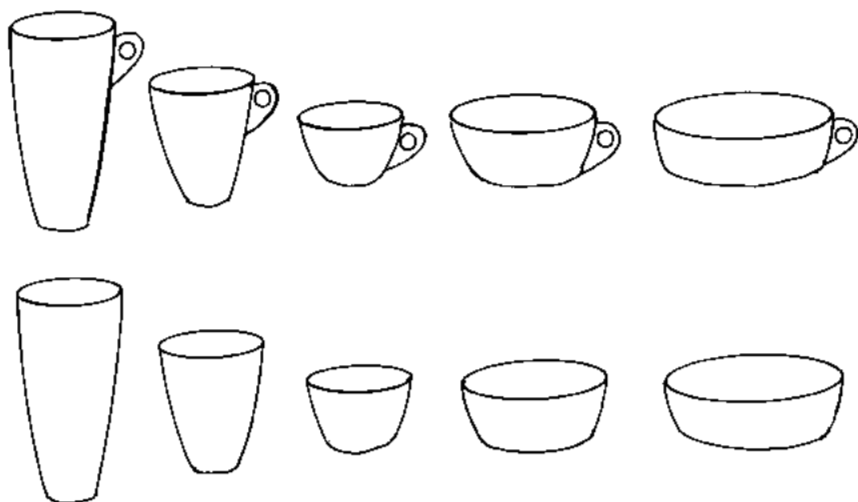


Figure 3.1

of componential analysis, Farah (1984) has found that image generation is selectively affected by specific types of brain damage. Componential analysis has further shown that one of the critical areas implicated in imagery is in the left, not in the right, hemisphere of the brain, contrary to previous thinking. Third, imagery research in psychology is beginning to have a direct impact on research in artificial intelligence, where researchers concerned with developing machines that can perform spatial reasoning tasks such as in navigation are building computer programs that use humanlike imagery processes.

### *Categorization*

How are categories defined in the mind? How do we recognize instances of categories? In spite of the apparent simplicity of these questions, we lack answers that are completely satisfactory. Yet these questions are fundamental to an understanding of thinking, since we reason about things by using our knowledge of them, and our knowledge is, in large measure, knowledge about categories of things. To know, for example, that Socrates is mortal, we must first decide that he is a man. How do we do it? How do we decide whether a new object is a member of some category or not?



*Figure 3.2*

A traditional view holds that categories are defined by a set of properties that an object must have if it is a member of a category. Scientific categories (e.g., mammal) often have this characteristic. However, categories in natural language generally do not. The category of "cups" is an interesting example. The figure shows a number of objects. Each has a different constellation of properties. Some tend to be seen as cups, and some tend to be seen as mugs, vases, or bowls. What makes something a cup? Interestingly, no single property is absolutely essential, as Labov (1973) has shown. Having a handle helps; having sides that bow inward helps, etc. An elongated cylinder without a handle and containing a flower is clearly not a cup, but something can still be a cup if it has straight sides, or if it lacks a handle, or if it has a flower in it, as long as it has most of the other properties typical of cups.

From a wealth of findings such as these the idea arose that category membership is not all-or-none but is a graded function of the typicality of the properties of an object relative to other members of the category. Some theories propose that we store the typical properties in a mental representation of a single "prototype" of the category; others propose that we store mental representations of many individual exemplars and compare new examples to those stored in memory. The evidence suggests that categories that are learned without conscious attention to discovering which properties are critical for membership—the categories of natural language—are best characterized by models with stored exemplars; even with explicit attention to discovering which properties are critical, there is evidence of an important role for memory of specific examples.

Beyond these basic facts there remains a huge amount to be learned about categories and categorization. Are there different kinds of categories? There is considerable evidence that some categories such as "cup" or "lamp" are somehow more basic than others, such as "furniture" or "floor lamp." How do we create new categories? Some studies have begun to examine how we combine what we know about individual categories to form new categories, such as "pet cockroach" or "blue banana." Still other studies explore the ways in which categorization influences our memory for objects. If I am told an object is a cup, I will remember it differently than if I am told it is a mug; in the latter case I will remember it as having properties more similar to mugs. These and other studies illustrate the importance of categorization in thinking, and provide us with clues about how representations of categories and their members develop and change with experience.

## Higher Mental Processes

### *Problem Solving*

Suppose you are in a room where two cords are hung from the ceiling. Their length is such that when you hold one cord you cannot reach the other. Your task is to tie the ends of the cords together. Also in the room are the following objects, which you may use: poles, clamps, pliers, extension cords, tables, chairs (Gick and Holyoak, 1983).

Of people posed with this problem, 39 percent produced a solution that relied on using an object plus one cord to form a pendulum, setting it in motion so that the two cords could be grabbed together (Maier, 1930). One would think this solution sufficiently unusual so that if this problem were preceded by a similar one the type of solution would "transfer" to the cord problem. Not so. People are startlingly insensitive to the pertinence of an analogy in a situation of this sort. It is not that they do not understand the solution; they simply do not apply it (Gick and Holyoak, 1983). Why aren't analogies used more productively in problem solving? Under what conditions will analogies be used effectively? These are among the questions about the transfer of knowledge from one situation to another that are the focus of current research on problem solving.

Investigation of issues such as transfer requires theories of how knowledge is represented. Such theories are being developed for several knowledge domains, spawned largely by investigation of problem solving in constrained situations. The collection and analysis of individual protocols from people as they solve problems, the measurement of time to solution, eye movement data, and the examination of patterns of errors have all contributed a wealth of information on which new computational models of knowledge representation depend.

The development of such models has in turn led to the study of problem-solving skills in the classroom. Domains of study have included physics, geometry, algebra, and elementary mathematics. One of the developments from these lines of research has been a clearer understanding of the sorts of tacit knowledge that contribute to the solution of complex problems, such as skills of problem understanding and pattern recognition. This research bears on the design of instruction in problem solving.

### *Reasoning and Judgment*

Consider the following problem: The U.S. is preparing for the outbreak of an unusual Asian disease which is expected to kill 600 people. Two alternative programs to combat the disease have been proposed.

If Program A is adopted, 200 people will be saved.

If Program B is adopted, there is a one-third probability that 600 people will be saved and a two-thirds probability that no people will be saved.

Which of the two programs would you favor?

Now consider the following alternatives for the same problem.

If Program C is adopted, 400 people will die.

If Program D is adopted, there is a one-third probability that nobody will die and a two-thirds probability that 600 people will die.

In a typical sample of people, 72 percent favored Program A over Program B, but 78 percent favored Program D over Program C. These represent opposite policy judgments for precisely the same problem. Why are people so heavily influenced by the mere wording of the alternatives?

Studies of reasoning and judgment have concentrated recently on understanding puzzles such as the one above. In this case, it seems that behavior is best characterized by the principle that humans tend to be risk-seeking for losses, but risk-averse for gains. The wording of the alternatives biases subjects to perceive them in terms of gains or losses.

Results of this experiment illustrate an important discovery about human reasoning: People tend to concentrate on concrete wording and examples in their analysis of problems, not on the underlying abstract principles. The general conclusion is consistent with research dealing with a variety of problems. For example, studies of reasoning about physical phenomena such as the motions of objects show that even after successfully completing a course of physics, many people continue to reason informally and incorrectly, using schemes based on previous experience, rather than applying formal principles and formulas that they have learned (e.g., Caramazza, McCloskey, and Green, 1981). And people are strikingly more successful with deductive logical problems if they are phrased in terms of familiar situations than if they are phrased in abstract notation.

The study of reasoning is now largely concerned with the processes that lead people to make judgments, rather than with a simple comparison of people's judgments with those derived from some normative rule. In the next several years, this research should uncover the details of reasoning heuristics. This could then lead to a better understanding of decision making in important policy contexts, and to corrective prescriptions for flaws in reasoning.

### *Expertise*

Persons become experts by virtue of their experience and knowledge, but what is the knowledge that experts have? Empirical studies of experts have provided some answers to this question, and more answers are likely to come from further research.

An interesting case is expertise in playing chess, which was thought by many to involve ability to analyze long sequences of moves to anticipate long-term consequences of alternatives. This belief influenced the design of computer programs for playing chess, a seminal domain in the development of artificial intelligence systems. Chess programs were designed to exploit the capacity of computers to hold large quantities of information in memory without loss, and therefore to conduct deep searches of the space of possible sequences of moves. However, when expert chess players were studied, evidence of deep searches was not obtained. Instead, it was found that chess masters are distinguished by having very large capacities for recognizing patterns and relevant abstract features of board positions (Chase and Simon, 1873; deGroot, 1965). Expertise depends much more on having large "vocabularies" of patterns that occur in game situations, and the ability to understand the "meaning" of board positions in terms of strategic possibilities such as threats and opportunities for strengthening a position.

The conclusion to be drawn from the study of chess masters is that they are expert in recognizing relevant features of chess patterns, and they recognize the applicability of various strategies. This same conclusion is applicable to the study of problem solving in physics. Novices tend to concentrate on the kinds of objects appearing in problems, whereas physics experts understand problems in terms of general principles (such as conservation) that the problems illustrate (Chi, Feltovich, and Glaser, 1981). The empirical results for physics and chess make it clear that expertise is better characterized as an understanding than as exquisite development of search and calculation routines. Further research must now capitalize on this discovery by developing expert sys-

tems that use sophisticated pattern recognition operations (e.g., Berliner and Campbell, 1984; Wilkins, 1980).

Current research contradicts the early view that expertise consists mainly of a large quantity of knowledge, and that the major task of expert system technology would be to find ways to store large amounts of knowledge so it could be retrieved when needed. There are indications that this view is fundamentally inadequate. Its main weakness is that it fails to recognize the importance of general concepts and strategies that are included in experts' understanding of situations and problems. One example occurred in an attempt to use an early example of a successful expert system in medical diagnosis (Shortliffe, 1976) as a basis for teaching medical students the knowledge they need to perform the same diagnostic task. The "expert" system's knowledge had to be totally reorganized so that it conformed more adequately with general concepts, principles, and strategies that expert diagnosticians actually use in their work (Clancey, 1983).

### Reading Overview

Reading research has recently focused on the psychological processes that are used in understanding the conceptual basis of texts, including technical and scientific texts. In addition, there have been large advances in understanding the nature and sources of difficulties in reading. The important new findings indicate:

1. When a reader interprets each successive word of a text, he attempts to interpret it immediately rather than using a "wait and see" strategy of interpretation.
2. The limitations on the reader's short-term memory capacity are circumvented by retaining information that is the most recent and most centrally related to the topic. Difficulties arise at the conceptual level because of lack of knowledge of the concepts or their organization.
3. The inefficiency of a few basic processes, such as word recognition, is a bottleneck for some children and adults, particularly those who have specific reading problems (dyslexia).

These findings have implications in the areas of scientific and technical documentation, display technologies, and instructional technologies. Knowing what causes a bottleneck in the processing of a text allows writers to produce text that is optimally suited to the characteristics of



the comprehension processes. Technical documentation and scientific texts can be optimized by using appropriate linguistic constructions, properly related to accompanying diagrams, graphs, and charts.

New reading technologies can take advantage of high-resolution video graphics displays, to present text, diagrams, and animations. Computer-displayed text is already a common medium among white collar workers and the university community, and will increase only with the advent of greater communications pathways (like MCI mail) and networks (bitnet, ARPAnet, etc). The flexibility of the new display technology will permit the format, sequencing, and rate of the displayed material to be matched to the users' capabilities.

The theoretical research in reading has integrated a large body of empirical findings into a theory of the reading process, often expressed as a computer simulation, a program that reads text and understands, while displaying similar processing characteristics to human readers. The value of such a theory is that it provides a basis for instructional innovations and diagnosis and remediation of reading difficulties. The focus on information processes has been supported by experimental methodologies that reveal the temporal characteristics of the processes, such as eye fixation experiments that measure the amount of time spent on each word of a text, and other chronometric techniques that examine the effects of various text properties on the reading time for a given sentence.

In the instructional domain, the research developments have produced refinements in educational practice that result in improved reading skills in children. For example, in some projects, children who are poor readers have been taught particular cognitive strategies that have been discovered in the adult skilled reader; one such strategy is identifying and retaining the most central information from the text. Children given this training show large and general improvement in comprehension. Other studies are implementing new programs to teach word recognition, taking advantage of computer-based technology for practice, drill, and individual tailoring of the curriculum.

### *Word Identification*

Humans, unlike any artificial system constructed so far, often seem to have available just the right knowledge to use in a given situation, and the mass of irrelevant information just naturally keeps out of the way. However, recent research has shown that in fact the irrelevant information is not simply ignored. That is, common sense tells us that if we read a sentence such as "Thieves broke into the vault of the bank and

stole a million dollars" we are not aware of the alternative meaning of "bank" as "river bank." But in fact, we do activate this knowledge.

The discovery that the initial stages of concept identification are independent of context was made possible by sophisticated new methods. For example, if a person is asked to decide whether a string of letters is a word or not, having just recognized an associatively related word speeds up this decision. Thus, if after "bank" a subject is presented with "money," the response will be about 40 msec faster than if an unrelated word had preceded "money." However, in the context of the sentence above, if we asked subjects to respond either to "money" or "river" after the word "bank," a priming effect would be obtained for both words: both meanings of "bank" were activated. After about 300 msec, only the contextually appropriate meaning remains effective.

The discovery of such counterintuitive properties of our word recognition system is particularly important if we want to use the human system as a model for building artificial word recognition devices. Before we can use the human system as a detailed guide for building artificial systems, we must know what is being activated when a word is read, and how other knowledge in the system is used to elaborate the "core" concept. Considerable progress is being made on these and related questions, and the day may not be far off when we have a good understanding of how the almost miraculous flexibility of human knowledge use is achieved in this domain.

### *Comprehending and Learning from Text*

Much of what we learn, in school and out of it, we learn from reading. A crucial step forward in our understanding of text comprehension occurred when psychologists turned their attention away from properties of the text in isolation, and began to develop models of the interaction between the comprehender and the text. We have now progressed to the point where computer simulations of this interactive process can point out exactly where and why a given reader (with known information-processing capacities) encounters difficulties in a text, and what could be done to avoid or ameliorate these problems. An important factor in text comprehension turned out to be short-term memory capacity: The reader has the difficult task of constructing a coherent mental representation of a text while being limited to processing only a single phrase or sentence at any point in time. Thus, short-term memory limitations require sophisticated processing strategies from the reader, as well as a favorable organization of the text.

Theoretical claims that short-term memory may be an important bottleneck for comprehension processes (as well as other thinking and prob-

lem-solving activities) appeared to be contradicted by the observation that standard tests of short-term memory (e.g., digit span) were notoriously uncorrelated with comprehension skills. However, it was recently shown that if, instead of evaluating a person's short-term memory capacity in a different context, one asked how much excess capacity a reader had available during the actual process of reading, the resulting estimates were highly predictive of reading skills. Thus, the development of refined testing procedures and processing models of comprehension go hand in hand.

However, although we can make a text comprehensible and memorable for a given reader, this does not necessarily create effective conditions for learning: People can comprehend (superficially) a text, remember it, and even summarize it—but still be unable to use the information acquired. For example, they can read an instruction manual and not be able to know what to do. Learning requires integrating the information from the text with previous knowledge, forming a "mental model" of the situation—which can be quite different from a "mental model" of the text itself (which might permit one to remember and summarize the text). We have much to discover about how people learn to use the information acquired from a text in new situations, and this will be the focus of much future research.

### *Neuropsychology of Sentence Comprehension*

After the basic results of the nineteenth century, further progress in the understanding of language disorders caused by brain damage was extremely slow. But in the past ten years there have been remarkable advances. The reason is that it is impossible to achieve very much understanding of an abnormality of a function unless one already knows something about the processes that operate when normal individuals carry out such functions. Over the past ten to fifteen years detailed models describing how people normally carry out various linguistic tasks, such as producing speech or reading, have been developed.

The combination of interests from cognitive psychology, clinical psychology, and neurology has resulted in the emergence of a new subject, cognitive neuropsychology. It has two aims: (1) to apply what we know about normal cognitive processing to the interpretation of cognitive disorders; and (2) to use data from studies of people with disorders of cognitive processes to test, refine, and extend theories of normal cognitive processes.

The most recent development in the field concerns the correction of the impairment of sentence understanding suffered by many aphasic patients. Models of normal sentence comprehension indicate that sentence

understanding entails three operations: You have to parse the sentence into grammatical components such as subject and object (syntax). You have to retrieve from your knowledge of language the specific meanings of the nouns and verbs (semantics). Then you have to map semantics onto syntax. Thus, an aphasic patient might have problems in understanding sentences for any of three reasons: impaired parsing, impaired semantics retrieval, or impaired mapping.

Current work involves pinpointing each patient's difficulty by separately assessing how the patient can perform each of these three aspects of sentence processing. The type of treatment used with a given patient can then be tailored to the type of impairment causing the comprehension difficulty. One study along these lines has been completed. It was possible to show that this patient's problem was neither in parsing nor in semantic retrieval, but in mapping. Therapy therefore focused on restoring the patient's ability to map semantics onto syntax. He was trained to do this using only active-voice written sentences describing spatial locations, e.g., "The box is in the bucket." After two weeks of such training, he improved from being at chance to being perfect at understanding such sentences. If the effect of this training had been to restore his ability to map semantics onto syntax, then he should now be able to understand spoken sentences, passive sentences, and sentences about actions rather than spatial locations, even though these kinds of sentences were never used in his training. This was so: performance was perfect with all of these sentences. There was also some improvement in his ability to speak grammatically. The success depended on applying a model of normal sentence comprehension to pinpoint the patient's deficit very precisely, and then directing therapy specifically at the pinpointed impairment.

### **Human-Computer Interaction**

The per capita number of electronic circuits in the United States is estimated to have been about 3 in 1965; the comparable number as of 1980 was about 10,000; by 1990 it is expected to be about 2 million. This phenomenal growth is due to the explosive increase in the population of people who use computers daily in their jobs or at home, and to the increasing use of computing devices in automobiles, household appliances, wearable or pocket items, and children's toys. Nearly everyone is already a user of computing resources in one way or another; and in the future the number of ways people interact with computers can only increase.

The challenges and opportunities for research relating to person-computer interaction are many. How will the use of computer-based message systems affect the ways people communicate? What kinds of tools will be needed to help people manage the enormous amounts of information to which they will have access? Under what conditions will speech be preferred for person-computer interaction? To what extent can programming be used to teach thinking and problem-solving skills? The list is easily extended.

### *Computer-Based Information Representation*

How information is presented for human assimilation is constrained by the available media. The book, which has served so well both to store and convey information, will undoubtedly be a major medium for many years to come. It is a static medium, however, and limited in its ability to represent processes and dynamic concepts. Motion pictures, including those generated by time-lapse photography, slow motion, and animation, have some advantages over the book in this regard. But this medium has limitations of its own, and lacks some of the conveniences of paper media. Neither books nor motion pictures provide a convenient basis for development of interactive or adaptive representation.

Computer-driven displays and video disks provide an opportunity to explore new and qualitatively different ways of presenting information. In particular, they can support dynamic, multimedia, interactive presentation schemes. The feasibility of more flexible and versatile information presentation methods gives new urgency to the question how information should be packaged for human use. It is easy to imagine applications to newspapers, maps, blueprints, circuit diagrams, and instruction manuals, for example. The details of their design, however, are neither self-evident nor readily deducible from existing handbooks; realization of these technological possibilities will require a better understanding of what aspects of a presentation method influence the assimilation and retention of information.

### *Human Understanding of Machine Intelligence*

Efforts to develop computer programs that exhibit "intelligence" have been going on for several decades. Recently this work has captured the imagination of the public and the business community, and the intensity of the effort to begin applying artificial intelligence to practical problems has greatly increased. Computer-based systems of the future will likely have increasing "cognitive" capabilities. How will such systems be per-

ceived by their users? Users develop conceptualizations or "models" of the systems they use, which may be more or less accurate representations of the systems' inner workings. A model that is inaccurate is not necessarily useless, but some kinds of inaccuracy may make it harder for the user to achieve his or her goals. Technologically naive users may develop anthropomorphic models of system capabilities and impute to such systems more human abilities and characteristics than they really have. How to ensure that users' conceptions are reasonably consistent with the facts and that their attitudes and expectations are realistic are important questions for research.

### *Psychological Aspects of Expert Systems*

"Expert systems" emulate human specialists in specific knowledge domains. In some cases they are intended to function in place of a human expert; in others their role is that of an assistant, often in situations where information-processing demands would otherwise overload the human.

Motivation to develop expert systems has several bases: human experts are too few to be available whenever and wherever they are needed; expert systems can have abilities shared with no humans (e.g., the ability to compute very rapidly); and an expert system can be patterned after the best of the experts in its field.

Such systems have been developed within several domains, notably computer systems configuration, locomotive repair, medical diagnosis, and oil exploration. A few systems are in operational use; most are still considered experimental. The utility of such systems has been demonstrated, however, and much effort is going into both improving existing systems and developing others in new domains.

This work presents several challenges to cognitive psychology. The development of an expert system requires discovering what constitutes expertise in a domain, representing that expertise in a form that is usable by a computer program, and specifying the logic for applying the information. Discovering what expertise is has proved to be difficult because much of the knowledge an expert has seems to be readily accessible only in certain contexts. There is a need, therefore, for research on the process of explicating expertise.

Second, the interest in developing expert systems provides a better opportunity to understand what expertise is and how it is acquired. Human expertise has been the focus of some research attention in recent years. The intense interest in building expert systems for practical purposes provides an opportunity for a coupling of such research with sys-

tem building. Research on human expertise will yield information the system builders need; efforts to build systems will force attention to issues that might otherwise not be identified.

Third, if efforts to develop expert systems succeed, there may be some unanticipated social effects. For example, insofar as experts derive status and other rewards from the fact that they have knowledge that is not readily accessible to most other people, the value attached to human expertise could decrease. Research directed toward anticipating the psychological and social effects of the development and proliferation of expert systems could help ensure a graceful accommodation to that change.

### *Tutorial Software*

Most of the educational software now in use in elementary schools provides drill and practice on basic skills, and the most common use of computers in high schools is in teaching students to write simple programs. These are positive contributions, but they do not significantly exploit the potential of cognitive theory and computational technology to enhance children's educational experience.

A second level of computer-based instructional software is now becoming available, with instruction focused on specific instructional problems using teaching ideas based on cognitive analyses. For example, in systems for improving reading skills, students are required to make decisions about the presence or absence of specific phonemic components in rapidly displayed words, or they are asked to indicate as many combinations of morphemic units as possible that form words in English. Also, systems aiding the acquisition of skill in writing provide information about a writer's text, indicating difficult sentence constructions and grammatical errors (in addition, of course, to misspelled words), so that teachers' attention can be given to significant rhetorical issues. Systems for instruction of strategic knowledge in high school mathematics show tree diagrams of students' problem-solving efforts in geometry proofs or solution of algebra equations, so that the students can reflect on the significance of decisions for achieving goals. Another system, based on analyses of students' misconceptions about force and motion in physics, provides a computational environment in which the commands available to the student produce motions that conform to Newtonian laws, and students can observe the consequences of those laws in a direct manner. Each of these is an example of a tutorial system that makes use of expert knowledge to provide the appropriate environment and feedback to a student engaged in problem-solving.

A third level of advanced intelligent tutorial systems is in development in a few laboratories and research centers. These systems incorporate models of student knowledge based on cognitive analyses of the instructional tasks, along with diagnostic procedures for assessing the state of a student's understanding and skill and procedures for tutorial decisions based on the diagnoses that are obtained. One example is a system for diagnosing elementary-school students' cognitive procedures for solving subtraction exercises, many of which involve systematic flaws or "bugs," involving fundamental conceptual deficiencies about the significance of place value and the reasons for procedural components (Brown and Burton, 1980).

Cognitive analyses of knowledge, reasoning, understanding, and problem solving have shown the potential for educational uses of information technology that could greatly enhance the intellectual growth of students. It is within our reach now to develop integrated learning environments that would encourage acquisition of meaningful relations between knowledge in different subject-matter domains and between formal and informal knowledge. Systems that analyze students' problem-solving procedures and strategies already exist (Brown and Burton, 1980; Burton and Brown, 1982), but these need to be integrated with students' understanding of concepts and principles in subject-matter domains. The technology and the scientific resources needed for significant advances are available. In the next ten years, development of intelligent tutorial systems and integrated learning environments can provide significant advances in educational productivity, as well as contribute to the progress of fundamental scientific understanding of complex processes of cognition and learning.

## Motor Control

### *Analysis of Action*

After years of neglecting the organization of motor behavior, psychologists have returned to it during the past decade. Their interest is complemented and invigorated by advances in robotics, long traditions within physiology and neurophysiology, recognition of human factors applications needs, and interests in motor prosthetics and in disorders ranging from stuttering to Parkinson's disease. Exciting and promising results have led to a recent spate of books, journal articles, and symposia. The work is facilitated by new technology for transducing motion



(optical, magnetic, ultrasonic, X-ray), for storing the results, and for analysis of large data bases of movements, sometimes aided by artificial intelligence methods.

Examples of questions being addressed are "What aspects of movement does the nervous system control?"; "In accomplishing an action, how does the system constrain the many degrees of freedom that are biomechanically available?"; "What determines the accuracy of reaching?"; "How are limbs coordinated?"; "How is serial order represented and realized in planned movement sequences?"; and "How do errors in speech and typing come about?"

Consider the hand reaching for a target, controlled by the interplay of elbow and shoulder rotations. Biomechanically the hand can approach its target along any of a vast number of paths; we might expect the actual path and the velocity profile along that path to reflect this complexity of control and to vary with the conditions of movement. Yet in the horizontal plane the path is essentially a straight line, and shows a single-peaked, bell-shaped velocity curve regardless of changes in speed and load (Abend, Bizzi, and Morasso, 1982). More generally it is the movement itself, and not the pattern of activity in individual muscles, that is invariant during compound arm movements (Soechting and Lacquaniti, 1981). Such simplicity suggests planning at the hand level rather than the joint level, with the system generating complex coordinated joint-angle changes to achieve simple hand trajectories.

Also in the domain of single actions, we have acquired a new understanding of the remarkably general logarithmic trade-off between the speed and spatial accuracy of limb movements (Langolf, Chaffin, and Foulke, 1976). The prevailing theory asserts that precision slows a movement because of an increase in the number of visually guided corrective submovements, with each submovement independent of precision. However, new experiments (supporting a new mathematical theory) show that the trade-off occurs without visual feedback, and that submovement speed varies with precision (Meyer, Smith, and Wright, 1982).

How are single actions combined into ordered sequences? Recent studies of speech and typing have revealed advance planning of entire sequences and hierarchical organization of actions in multi-action units. In rapid utterances, for example, we now know that the unit is not the syllable, or the word, but the stress group—a sequence of syllables containing a primary stress (Sternberg, Monsell, Knoll, and Wright, 1978). A hierarchical network model of speech production with activation spreading both "up" and "down" the network is supported by its ability to explain facts about slips of the tongue (Dell, 1985). New methods of

inducing speech errors under laboratory conditions have facilitated such research. In a similar spirit, a model of typewriting with parallel distributed processing that converts a sequence of discrete symbols into continuous and temporally overlapping movements of fingers and hands can explain many features of timing and errors in the performance of skilled typists, including how a stroke by one finger can be accompanied by movements of other fingers that position them for action two or three strokes later (Rumelhart and Norman, 1982).

This is then a period of ferment and progress in the study of action. The new findings will enhance person-machine interfaces (e.g., instrument panels and keyboards), skill training, implementation of artificial movement systems (prosthetics, manipulators, robots), and the diagnosis and treatment of movement disorders.

## Structure of Processing Systems

### *Mechanisms for Combining Information*

Perception, comprehension, thought, and action all require the combination of information from different sources. Consider three examples:

1. The figure demonstrates that the identification of an ambiguous form depends on the surrounding letters. The form itself limits the possibilities to A or H, but the surrounding letters determine which we recognize the figure as in each case.
2. In sentence comprehension, similar effects occur. Consider the sentence: "The haystack was important because the cloth ripped." This sentence generally seems meaningless when heard out of context. But when context is supplied (the single word "parachute" is enough), it can suddenly make a great deal of sense. Again, information from context must be combined with information from the sentence before comprehension can occur.
3. Perhaps as much as anything else, intelligent action requires the combination of information. The driver of a car who must do just the right thing in a threatening traffic situation, the quarterback who is scrambling and looking for the open receiver or the chance to gain yardage on the ground, and the committee chairman who must formulate just the right compromise, all must combine information from a number of sources to succeed. Novices are gener-

ally very poor at combining information, but one of the hallmarks of the expert is that he "knows just what to do" in response to a complex situation, in which many sources of information must be combined.

A class of models that capture the simultaneous use of multiple sources of information has recently been developed. These models are often called activation models, because each piece of information that plays a part in a particular mental act is thought of as active. Thus each piece of information exerts an influence on the direction of the mental process, just as each force on an object exerts an influence on the trajectory of the object. These models should be distinguished from more traditional models of thought processes, in which there is a single active processor that must examine each piece of information separately. A crucial difference is that the performance of activation models is improved and facilitated by additional sources of information, while the performance of traditional models becomes bogged down as more information is added.

One example is the interactive activation model of word perception (McClelland and Rumelhart, 1981). This model consists of a large number of simple processing units, some subset of which becomes active in the course of processing a word, such as the first word in the figure. "Visual feature" units represent line segments, "letter" units represent possible letters, and "word" units represent possible words. When a word is presented, feature units are activated; these activate compatible letter units, and these activate compatible word units. In turn, the word units feed back activation to the letter level, and the letter units feed back activation to the feature level. Since "THE" is the word most consistent with the first word in the figure, it becomes most activated; its feedback supports the "H" over the "A" interpretation of the ambiguous middle letter.

Activation models are being developed at the interface of psychology and computer science. They are useful theoretical tools for accounting for human performance, and they are useful computational tools for overcoming the limitations of traditional computational approaches. On the psychological side, the interactive activation model of word perception provides a detailed account of a large body of empirical data concerning the detailed time-course of the effects of context on letter perception, and has recently been applied to other areas in perception, memory, and language. On the computational side, AI researchers have begun to apply activation models to previously intractable problems in vision (Bal-

lard, Hinton, and Sjnowski, 1983), and have joined together with psychologists in extending the approach to the computationally challenging problems that are posed by the complexities of sentence structure.

Work with activation models is computationally intensive, because activation models must be simulated on conventional computers. Further development of these models will require more (and perhaps different) computational resources and training in the use of these resources. The investment would be well worthwhile. Activation models provide an exciting opportunity to help us understand these important processes, and to develop superior computational techniques for implementing them in artificial systems.

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## Language and Language Processing

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Language is the glue that holds human society together, that links past and future in a social, intellectual, and legal tradition. Humans have a capacity to express thoughts and to communicate in a way that distinguishes them from other species. That capacity depends upon a complex set of cognitive systems, served by a set of motor and perceptual skills that are elaborately developed and delicately timed. Every day, a normal human being utters and listens to thousands of words in the forms of novel sentences and conversational fragments (most of which have never been spoken or heard previously). Every word must be identified by the hearer from an ensemble of more than 50,000 forms stored in what may be called a mental dictionary or lexicon in less than a third of a second, and assembled into a structure that correctly represents the meaning intended by the speaker. This linguistic capacity arises in every child at about the same age regardless of intellectual level, educational background, or the particular language community of the child. The linguistic sciences comprise the technical fields that inquire into this process and into the human language faculty that makes this possible. They include the study of the human language faculty itself as well as the memory and processing systems and the neurological and physiological functions that support it.

The research aimed at discovering how these systems work is necessarily divided among specialized subfields of linguistics. A major goal of *theoretical linguists* is to construct models of linguistic structure ("grammars" that represent one's knowledge of language), identifying those aspects of structure that are part of the human biological endowment (i.e., are "universal") and those that are particular to individual languages. *Psycholinguists* study the real-time processing of language in speaking and the acquisition of language by children; and *neurolinguists* are concerned with the neurological structures underlying our linguistic abilities. Other subfields of linguistic science focus on the mathematical properties of language, how language is used for literary purposes, how it changes historically, the physical (phonetic) characteristics of the acoustic signal, the physiological mechanisms that produce the articulations of speech, and language in its social setting, to name just a few.



The fields of language science are about to expand in their intellectual, medical, and socioeconomic consequences. Three forces have brought this about:

1. Work in theoretical linguistics has, in the past twenty-five years, brought us closer to an understanding of the nature of linguistic universals and how they help to determine the form of grammars of individual languages (see especially Chomsky, 1981; Lightfoot, 1982; Newmeyer, 1986).

2. Recent results in neuroscience and experimental psychology have dovetailed with linguists' claims about the nature of language: studies of language breakdown in aphasia, of the hemispheric lateralization of the sign language of the deaf, of real-time language processing, and of the acquisition by children of their first language have, by and large, confirmed the abstract grammars constructed by linguists as real mental grammars seated in the human brain. And linguists, in turn, are increasingly drawing upon results in the neurosciences and experimental psychology in the construction and testing of their linguistic models, recognizing that models of language must be responsible to these kinds of linguistic data (for overviews, see Newmeyer, 1983, 1988a).

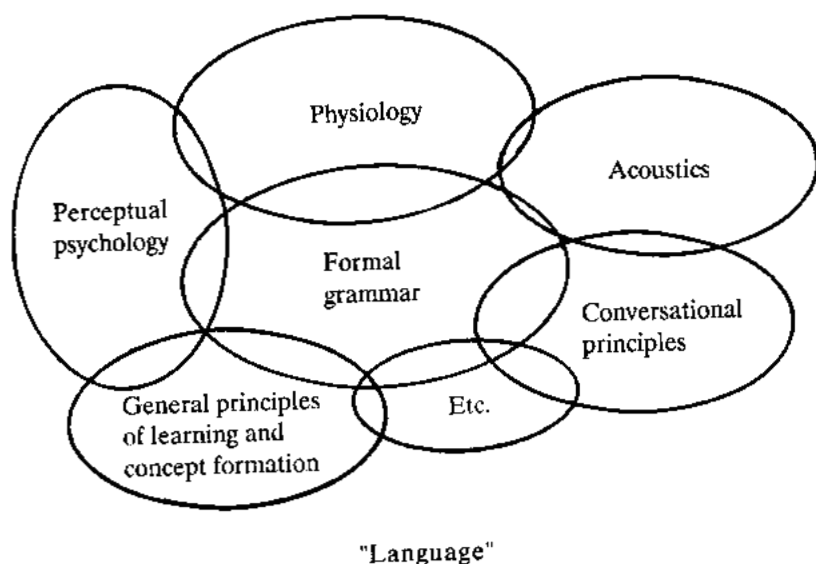
3. There is a new interplay between linguistics and the area of symbolic information processing. Technological advances such as computer-based natural language query systems and speech synthesis and recognition devices, for example, utilize conceptions provided by theoretical linguistics, while linguists are aware that their theories of language structure must be integrated into a comprehensive theory of information processing (Halvorsen, 1988).

We thus see a research program incorporating linguistic science, biomedical science, and information science as one of the most important intellectual and practical developments of the next decade.

### Results and Research Opportunities

The important advances in our understanding of the nature of language attained in recent years have been rooted in large part in a revised conception of the relationship between the various faculties involved in the totality of language. While it was once commonplace to view the grammatical properties of language as essentially derivative—a mere byproduct of general cognitive, physiological, and other nonspecific systems—there is increasing evidence in support of the view that linguistic capacity (or the grammar itself) forms an autonomous cognitive system, gov-

erned by its own set of distinct principles, which interacts with other cognitive systems in its development and use. This modular conception of language, which is outlined in Chomsky (1965:3-4; 1981: Chapter 1), Anderson (1981), Fodor (1982), and Newmeyer (1986: Chapter 7), can be portrayed schematically as in the figure below:



The modular conception of language not only has been borne out through empirical investigation, but it also underlies some of the more important discoveries about language in recent years and promises to yield further results in the years ahead. We can illustrate this point by looking at the results and research agendas of the following areas of linguistic inquiry.

### *Theoretical Linguistics*

The belief in the existence of a set of principles governing the grammars of all human languages until recently was based on little more than faith; now we have made great progress in the explicit characterization of these principles. The breakthrough came about as a result of a conceptual shift among theoreticians. Formerly, linguists regarded the grammar of a language as essentially a list of rules, each rule characterizing a particular construction (Lees, 1960 and Rosenbaum, 1967 exemplify

this). Hence, for English, a passive rule was proposed to account for sentences like *The ball was thrown by John*, a question rule for sentences like *Who did Mary see?*, an imperative rule for sentences like *Pick a card*, and so on. While such grammars provided a means of describing individual languages, they failed to reveal a great deal of similarity among languages—after all, passives, questions, and imperatives differ markedly from language to language. However, in the last decade linguists have come to take a more modular approach, as research has indicated that particular constructions are actually superficial manifestations of the interaction of simple, but highly abstract, underlying principles, which differ only slightly from language to language (for particularly good examples, see Rizzi, 1982; Huang, 1984; and Bouchard, 1984). The task of the theoretical linguist, then, has shifted from writing particular rules for particular languages to positing the general principles governing all languages. The importance of this shift cannot be overemphasized. It means that theoretical linguistics has made a major step in the direction of the “hard” sciences. Just as physics seeks to specify precisely the class of physical processes and biology the class of biological processes, the goal of theoretical linguistics is now to specify precisely the notion of “possible human language.”

A major research task for theoretical linguistics in the next decade will be to further test this hypothesis through intensive investigation of the world's languages. In addition to the research on languages related to English such as Dutch, French, Spanish, and Italian, preliminary investigations of languages such as Japanese, Chinese, Arabic, Hausa (spoken in Nigeria), Warlpiri (spoken in Australia), and others have given credence to the idea that much of linguistic diversity is due to different manifestations of the same underlying grammatical principles. However, a far greater percentage of the world's more than 4,000 languages (depending on one's definition of “language,” up to 8,000 have actually been suggested) require the attention of theoretical linguists to test the adequacy of this conception so that, if it is borne out, the grammatical parameters for particular languages can be fixed.

### *Child Language Acquisition*

The most productive research into the acquisition of language by children has assumed that acquisition is guided in part by a set of highly abstract principles of grammar, principles that to a large extent are biologically determined. Experimental evidence has shown that even the youngest children exhibit subtle knowledge of syntactic structure, phonology (the organization of the sound system), and semantics, which could

not have been learned by either induction, imitation, or instruction. Newborn infants respond differently to phonetic distinctions in human language for conveying information from those that are not, suggesting that aspects of the phonology are "prewired," so to speak, in all humans (Eimas, 1985). Likewise, the child's acquisition of grammar does not arise from gradually improved imitations of adult speech. On the contrary, from the rapid speed of acquisition, from the fact that even very young children are capable of producing and understanding an infinite number of sentences they have never heard, and from the fact that at early stages of acquisition, the rules of children's grammars are both complex and highly abstract, we conclude that children are biologically equipped for the language learning process (Lenneberg, 1967; Tavakolian, 1981; Hyams, 1986; Roeper, 1988; Caplan, 1988). And, significantly, children the world over, whatever language they happen to be learning, go through the same stages of acquisition, regardless of whether in their culture they are encouraged to speak and rewarded for doing so, or whether their first utterances are simply ignored (Wanner and Gleitman, 1982; Gleitman et al., 1988 survey the language acquisition literature).

The case for a biological basis for grammar has also received support from the fact that, in extraordinary cases, linguistic abilities may be dissociated developmentally from other cognitive abilities (such as means-end knowledge, classificatory skills, symbolic play). It used to be commonplace among psychologists to believe that once the child had reached a certain stage in the development of the latter abilities, the former would follow automatically. But a number of case histories show that this is not true. The most dramatic involves "Genie," a girl who underwent extreme isolation and deprivation until she was thirteen years old (see Curtiss, 1977). Genie, in effect, faced the task of first language acquisition as a teenager. A decade later, despite the fact that most of her general cognitive skills had become relatively well developed, her grammatical abilities were still rudimentary. And conversely, there have been reports of children who have complete mastery of linguistic structure but no ability to utilize it in communication (see Blank, Gessner, and Esposito, 1979; Curtiss, 1988). Such circumstances support the hypothesis that there are distinct principles governing syntactic development.

Research in the next decade will be directed at adducing evidence for the specific principles shaping the acquisition of grammar, which will then be compared to those arrived at by theoretical linguists on the basis of the investigation of diverse languages. This interplay between acquisition studies and cross-linguistic studies can lead to greater understanding of the biological foundations of language.

### *Real-Time Language Processing*

As far as the real-time processing of language is concerned, modularity has come to be established as the presupposed general condition (for overviews, see Frazier, 1988, Tanenhaus, 1988). An important question of concern is the inventory of modular systems and their interaction in speech production and comprehension. What are the parallels between comprehension and production, and to what extent do these two systems rely on common computational resources, and refer to a "common grammar?"

A specific issue now being explored in psycholinguistics concerns the conditions governing the real-time resolution of lexical and phrasal ambiguity, ambiguity such as *she was standing by the bank*, where the dual meaning depends on the dual meanings of "bank," and *John likes Mary better than Sue*, respectively (Tanenhaus et al., 1979). There is a productive controversy over the role that semantic and pragmatic (i.e., real-world knowledge) factors play in influencing the syntactic analysis assigned to what we are hearing or reading, a controversy that has given rise to a systematic effort to establish the range and limits of semantic and pragmatic effects. We have learned that some syntactic processes are indifferent to the semantic analysis of the sentence and others reflect real-time semantic processing. Establishing the precise boundaries of these will contribute greatly to our understanding of linguistic systems in particular and cognitive systems in general (for counterposed views on this question, see Forster, 1979 and Marslen-Wilson and Tyler, 1980).

Until recently the experimental procedures for the exploration of these questions were in their infancy. Today, by contrast, we have a broad range of methodological possibilities, including the flexible use of time-compressed speech and of rapid serial visual presentation for precise determination of the time course of language processing; the use of a variety of reaction time measures such as lexical decision, naming, phoneme/word/syllable/sentence monitoring, and timed judgments of well-formedness and plausibility. Parallel experimental methods for studying the production process are also available, particularly in regard to the ability to conduct computerized analysis of pitch contours, the speech signal itself, pausal phenomena, and onset time (these techniques are reviewed in Flores d'Arcais, 1988 and Garrett, 1988).

### *Neurological Aspects of Language*

Joint work between linguists, neurologists, and neuropsychologists has forged the new interdisciplinary field of "neurolinguistics," which has

already advanced both linguistics and neurology (Blumstein, 1988 overviews this work). It was once commonplace among neurologists to think of language as little more than strings of words. Hence, they were incapable of dealing adequately with the various types of aphasia (the breakdown of language under conditions of brain damage). Now, equipped with conceptions from theoretical linguistics, including its modular conception of language, neurologists and neuropsychologists are able to identify the particular components of language affected in different aphasias. For example, it has been found that certain aphasia patients have the ability to recognize the syntactic structure of a sentence (i.e., they can distinguish well-formed sentences such as *The dog was chased by the cat* from deviant sentences such as *The dog was chased the cat*), but they lack the ability to use structural information to recover the sentence's meaning. This characterization of the sentence comprehension deficit promises to have positive consequences for therapy and has already led to the development of a remediation program focusing on the use of structural information (Linebarger et al., 1983).

At the same time, studies of aphasic language have become important to theoretical linguists and psycholinguists. Since the patterns of language breakdown must reflect the functional architecture of the preexisting, that is, normal, language system, these patterns can be taken as evidence for the componential structure of language in the normal state. To give one example of a promising research line in this area, the study of language breakdown across languages offers the potential for isolating basic, universal capacities underlying human language and for the identification of their neural substrates. Languages differ considerably in form and in structure. For example, English relies heavily on word order to convey information (cf. *Tom hit Bill* vs. *Bill hit Tom*), while other languages, like Serbo-Croatian, signal the same distinctions by the use of inflections (e.g., suffixes, prefixes). Is the nature of syntactic breakdown nevertheless the same, at some abstract level, across these different language types, and are the same brain loci implicated? Systematic investigation of these questions is relatively recent, but has already yielded fascinating data (see Luria, 1966; Panse and Shimoyama, 1973; Peuser and Fittschen, 1977). Studies of this nature should lead to a deeper understanding of these deficits and of the linguistic capacities that underlie them.

The study of the neurobiological correlates of language is also changing dramatically due to the development of powerful new techniques in the field of neuroscience. Perhaps the foremost development, in terms of its applicability to the understanding of human language and cognition, is the advent of fine neuroimaging techniques such as computer-

ized axial tomography (CAT), nuclear magnetic resonance (NMR), magnetic resonance imaging (MRI), and emission tomography. In the case of CAT and MRI, it is now possible to delineate the exact shape and location of both normal brain structures and of acquired or developmental brain lesions leading to language disorders. The equally dramatic technology of emission tomography using either positrons or single photons permits a dynamic measure of the level of metabolism or cerebral blood flow of given areas of interest in the human brain during cognitive experiments involving both language and nonlanguage (for discussion of results based on this new technology, see Kean, 1988).

### *Sign Language Studies*

Important results pertaining to the relationship between language and the brain have also been derived from the study of the sign language of the deaf, for example, the American Sign Language (ASL). One discovery is that ASL manifests all of the crucial properties of spoken language, including its modular structure and its two formal structuring levels (lexical units and sentences). Furthermore, as in spoken language, the principles involved are highly abstract, yet are learned by children without any formal instruction (Padden, 1988 reviews the linguistic literature pertaining to ASL).

One reason that ASL is such a powerful vehicle for investigating the nature of brain organization for language is that the means of conveyance of its linguistic structure is rooted in the visual rather than the auditory modality. The left cerebral hemisphere has been considered specialized for linguistic functions, and the right, for visual-spatial functions. Yet, it turns out that the left cerebral hemisphere is dominant for sign language. Not only does this confirm the fact that ASL is a "real" language, but it also has implications for the neurological basis of spoken language: the left-dominance for ASL shows that one theory linking the specialization of the left hemisphere for language to capacities for fine auditory analysis cannot be correct (for discussion, see Bellugi, Poizner, and Klima, 1988).

The increasing linguistic research on sign language can also tell us about the nature of and human capacity for language. Work has recently begun on sign language and linguistic universals. Since some researchers have linked language universals to characteristics of the vocal tract and the speech channel, the study of sign language can reveal how deeply rooted are the basic principles of language that are modality independent. To take another example, more linguistically informed analyses of sign aphasia will allow us to determine how the nature of

sign aphasia disorders parallels that of disorders in the spoken language, and how aphasic impairments are related to special processing requirements of particular language modalities. The objective here is not to understand sign aphasia *per se* but to utilize data from the breakdown of visual-gestural languages to better understand the nature of language breakdown in general (see Siple, 1978; Klima and Bellugi, 1979; Lane, 1984).

### *Symbolic Information Processing (SIP)*

A parallel interaction between linguistic theory and the area of symbolic information processing has developed in recent years. SIP is the interdisciplinary science concerned with the acquisition, representation, and communication of information, whose goal—linking linguistics with the fields of logic, psychology, artificial intelligence, philosophy, and computer science—is to explain the nature of the intricate relations that hold between languages, the world, and intelligent agents (whether human or computer).

Linguistic theory plays an important role in SIP, since the latter seeks to develop precise theories of how information is embodied in and manipulated by languages. The past work in language processing and artificial intelligence, which attempted to bypass the results of theoretical linguistics altogether, resulted in a dead end (similar to the early approaches to automatic machine translation and speech recognition). Increasingly, however, these fields have drawn on conceptions provided by linguistic theory, resulting, for example, in programs that process written English well enough to understand questions of some degree of complexity, to search for the answer to them in a computerized data base, and to respond in written English. The most successful of these programs all incorporate a formal grammar.

Linguistic theory, in turn, stands to benefit from research in this area, specifically in the case of the study of pragmatic inference. For a comprehensive overview of SIP research, see Halvorsen (1988).

### *Automatic Speech Synthesis and Recognition*

Linguistic theory has proved its applicability to technological needs in another respect: in the construction of devices that synthesize and “recognize” or “comprehend” human speech. While the former are at present considerably better developed than the latter, the coming decade will see tremendous advances in both as a result of collaboration be-



tween linguists, communication engineers, and computer scientists. Again, the benefits will be mutual. For example, phonological theory has been enriched through the systematic studies of the acoustic properties of speech, some of which have been carried out with technological innovation in mind. The computer revolution has made it possible to acquire in hours or days large phonetic data bases for studying the sound structure of language that would have taken months previously. As a result of the investigation of intonational and other phonetic properties of speech undertaken with the ultimate goal of constructing machines that talk and listen, we now have a better understanding of how the units of the linguistic system are related to acoustic signals. A promising area of this collaboration is the study of intonational phrasing. For example, the syntactic structure of a sentence typically does not "match" its division into intonation units, as shown by the fact that the major syntactic break occurs between the subject and predicate in the sentence *John decided to try to climb Mount Everest*, that is, between *John* and *decided*, whereas the major intonational break is typically between *decided* and *to*. An investigation of the disparity between major syntactic boundaries and intonation breaks will lead to new theoretical insights as well as better devices for producing and interpreting spoken language.

Speech synthesis and recognition projects are discussed in Reddy (1976), LaBerge (1981), Bristow (1984), and Halvorsen (1988).

The time is now right to integrate the research in theoretical linguistics, language acquisition, psycholinguistics, neuroscience, and symbolic information processing. Major advances in scientific fields are most often triggered by the attempt to find a systematic account of the relations between independently developed and superficially distinct domains. Such productive integration obviously requires significant and independently motivated theoretical structure on all sides of the research fence. That was not possible a short time ago, but it is possible today.

### Resources Needed

Despite the interdisciplinary turn that the work in the language sciences has taken, few individuals are adequately trained in more than one relevant discipline. It is unlikely that there exist a half dozen people in the world who hold degrees in both linguistics and neurology. Furthermore, traditional academic compartmentalization, which works to impede collaboration between scholars in different academic programs, presents an especially acute problem in the area of language and language processing: linguists, neurologists, psychologists, and computer scientists, for

example, not only often find themselves in different academic departments but also in different colleges within the university structure.

A great need, therefore, is for stable interdisciplinary centers in which the necessary collaboration can take place. These would permit individuals affiliated with other institutions an opportunity to visit and expand their professional skills. The establishment of ten or twelve such centers, half focusing on computation and formal systems, half on language-processing and biological systems, could produce dramatic results. This does not mean that there should be two distinct types of centers, each concentrating exclusively on its particular specialty. While their emphases would necessarily differ, the "formally oriented" centers could not ignore biologically dictated language-processing limitations, nor could the "biologically oriented" centers afford to ignore the formal and computational constraints that transcend the physical instantiation of cognitive systems.

One function of these centers would be to train linguists, neuroscientists, and information scientists in each other's disciplines. To that end, funds should be earmarked for one-, two-, and three-year fellowships, both for recent Ph.D.s and for those already established in their careers.

Interdisciplinary curricula for doctoral programs need to be developed as another way to break down the traditional barriers. One function of the centers described above could be to develop such curricula. In addition, or as an alternative method, funds for release time of faculty in the relevant departments in universities could lead to the development of interdisciplinary programs and jointly taught courses and seminars.

The goal of interdisciplinary training can also be achieved through existing summer institutes cosponsored by the Linguistic Society of America and host universities. These institutes have been held since 1928. Students and visiting scholars from throughout the world attend these institutes, which are taught by faculty of the host university as well as other leading linguists. Specially funded subinstitutes of various kinds have been held at previous institutes. Future institutes should include specially funded subinstitutes with a half dozen or so faculty positions to teach linguists principles of neurology, neuropsychology, artificial intelligence, computer science, etcetera. While such subinstitutes, being limited to six or eight weeks in a summer, would not obviate the need for the centers described above, they could serve the useful function of acquainting practicing linguists with possible avenues for their research that might otherwise have escaped them. In addition, these subinstitutes would permit ongoing discussion and seminars among the researchers involved in such interdisciplinary research.

Postdoctoral fellowships, in addition to those supporting faculty at the centers, would help young scholars, as well as those who have already achieved distinction, to "retool" and study in areas outside their own discipline, either at their own universities or at other institutions.

Another pressing need is for the establishment of computerized data bases, easily accessible to all interested scholars. Beginnings have been made in compiling banks of cross-linguistic child language data, speech error data, and aphasic speech data (complete with the patient's medical history and CAT scans). But as of now, these data banks are extremely fragmentary and not readily available. A coordinated centralized effort should be made to remedy this situation.

With respect to biotechnology and medicine, access to the tools for relating brain state to cognitive processes is currently tied to the hospital context and to the exigencies of patient treatment. Support should be provided to permit collaboration between the medical and the academic research communities in an atmosphere in which the objectives of basic research into normal cognitive functioning have high priority.

Finally, stable international connections, preferably in the form of institutional centers but at least through regular summer seminars, research workshops, and exchange of scholars, need to be established where cross-linguistic work can be coordinated and access to subjects, including children and aphasic patients, can be facilitated. Linguistics is the scientific study of human language; the greater access investigators have to the diversity of language, the better the ultimate goals of the discipline will be achieved.

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PART

**II**

**BEHAVIOR IN SOCIAL  
CONTEXT**





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# 5

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## Development of Cognitive and Social Competence

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The three fundamental questions in the development of cognitive and social competence are: (1) What is the original state of the organism? How should the competence of the newborn or young infant be characterized? (2) What is the subsequent course of development of competence? Exactly how does the initial state of the baby evolve into the competence of the mature organism? (3) What is the mechanism of the developmental transformation? An answer to the question of mechanism includes specifying the important biological, experiential, and social influences on development.

The study of infant competence has been marked by considerable progress in the last two decades. Infant studies have benefited from the development of methods and availability of equipment suited for these methods. A key methodological innovation, for example, was the habituation-preference technique: infants are known to prefer to look at novel stimuli. Thus if an infant is shown a stimulus such as the photograph of a particular face repeatedly it will tend to stop looking at it. When shown a novel face it will resume looking. The use of photographic equipment (for eye movement recording), videotape recording equipment, and computers has enabled fine-grain analysis of the habituation-preference behavior as the particular stimulus displays are varied. These techniques have produced an explosion of knowledge as to what infants can do.

These techniques document infants' ability to discriminate but leave open their appreciation of the meaning of the stimuli. Does the infant who demonstrates differentiation of a happy and a sad face "understand" the implications of the two expressions? What is the basis of such discrimination—some slight difference in facial muscles or an emotional gestalt? Sophisticated use of these techniques with subtle variation of stimulus displays are beginning to answer even these questions of meaning.

However competent infants are shown to be, it is obvious that they exhibit only the beginnings of the complex cognitive and social abilities of mature humans. The presence of the early competence both constrains and feeds the development of mature abilities. The increasing knowledge of the initial state of the organism has helped cognitive and

social researchers better describe subsequent development. In this respect they have turned to analysis of the structural and formal characteristics of knowledge domains. Such analyses permit precise statements of the relationship between early and later knowledge. One emphasis has been consideration of how children interpret their environments—their concepts and theories of the natural world. Another emphasis has been on “formal” content domains, such as numbers and physics, and skills like reading and writing, considered as “new knowledge.”

How these developmental changes come about is the most problematic of the three fundamental questions. What motivates developmental changes? What are the transition mechanisms? There has been some progress in specifying the experiential and biological correlates of development and more can be expected.

### State of the Art—Identification of Some Promising Directions

#### *The Origin of Competence*

During the last fifteen years significant progress has been made in characterizing the earliest competence of the child. Techniques were developed beginning in the 1960s to investigate the sensitivity of infants and even newborns. Prior to that time there was tacit agreement that William James's description of the world of the infant as “blooming, buzzing, confusion” was probably correct. The basic paradigm for studying infants was elegantly simple in principle. It capitalized on the tendency of an infant to orient itself toward an interesting or novel stimulus. Thus an infant might be shown an object such as a red star for some time. Then it would be shown the red star alongside a blue star. The infant would manifest its interest in the novel stimulus by looking first and/or longer at the stimulus with the new color. Such a result would at the same time demonstrate an infant's ability to discriminate between two colors and to remember the first color at least long enough to make the discrimination.

With the use of these techniques, infants' sensory capacities have been or are in the process of being systematically mapped out. For example, we now have detailed knowledge of infant color vision (e.g., Bornstein, 1975) and of the very early development of infant contrast sensitivity functions (e.g., Banks and Salapatek, 1981; Dobson and Teller, 1978). These techniques have quickly been extended to investi-

gate more complex and abstract aspects of infant perception and cognition. Of particular interest for perception is the early development of depth perception. A classical question for philosophers was whether the ability to perceive a three-dimensional world on the basis of inherently two-dimensional sensory input depended on extended early associative experience. Now research has established that by seven months of age an infant can use all the types of depth information adults use. However, there is a regular progression in the development of this ability. Infants first use depth information specified by motion, then stereoscopic depth information, and finally pictorial information (Yonas and Cranrud, 1985).

Through the use of the basic habituation paradigm, by judicious choice of the familiarization stimuli and pairing of test stimuli, it has been possible to demonstrate an infant's appreciation of the continuity of partially occluded objects behind the occluding surface (Kellman and Spelke, 1983), and of the difference between rigidity and elasticity of objects (Gibson and Walker, 1984). By familiarizing infants with a class of objects rather than a single object researchers have been able to demonstrate their early appreciation of classes and concepts. One very abstract example is the child's appreciation of number (Starkey and Cooper, 1980; Strauss and Curtis, 1981).

If infants are repeatedly shown, say, arrays of three objects of various types in various spatial arrangements, they will gradually lose interest and spend less time looking at them. Then if they are shown an array of four objects their interest recovers and they will again spend more time looking at this collection of objects with a different numerosity. Such evidence of sensitivity to even such an abstract property demonstrates only a detection of a difference and not any kind of appreciation of the meaning of number.

It is possible in some cases to get close to being able to infer appreciation of the meaning of a display. Consider an experiment in which infants were exposed orally to either a flexible spongy or a rigid hard pacifier (Gibson and Walker, 1984). After a period of familiarization with this object they were shown a visual display of an elastically distorting object and a rigidly rotating object. The infants preferred to look at the visual stimulus that was moving in a way different from the familiar object. Here the infants seemed to be sensitive to the correspondence between the tactual property of rigidity or flexibility and its visual counterpart. This seems to be very close to appreciating the meaning of the property of rigidity. Or consider a case where infants were habituated to the distance of an object when the spatial information was provided by pictorial depth information. They showed increased looking behavior

when the object distance was changed as specified by stereoscopic depth information (Yonas, Arterberry, and Granrud, 1987). Again the infants seemed to be detecting a correspondence between the pictorial and stereoscopic depth information. However, there is nothing formally similar about the stimulation in these two instances. What is similar is what they specify, namely, their meaning.

In the realm of social competency, dramatic newborn and infant precocity is beginning to be demonstrated. Convincing evidence for the differentiation and imitation of facial expression and very simple facial gestures has been presented (Melzoff and Moore, 1983; Field, Woodson, Greenberg, and Cohen, 1982). During the first six months of life there is also evidence of infants' ability to recognize classes of faces and particular faces.

Detection and discrimination of social stimuli are one thing but again appreciating its "meaning" is more impressive. Infants seem to develop such sensitivity at a very early age. Walker (1982), for example, projected side-by-side films of a person displaying two different emotions such as happy and sad. A sound track appropriate to one of the displays was presented midway between the two. Babies of five months of age preferred to look at the appropriate film strip, that is, the one that matched the sound track. This cross-modal identification suggests that the baby was responding to some possibly affective characteristic common to both the sight and the sound. There are other examples of infant appreciation of such meaning of both social and nonsocial stimuli. However, the research so far has produced only demonstrations rather than a systematic understanding of the structure of a baby's knowledge and perception.

There is also a gap between our knowledge of an infant's sensory capacities and our prediction of its perception of meaningful environmental properties. It was hoped, for example, that by approaching pattern perception through the study of linear systems analysis it would be possible to understand the perception of complex, meaningful stimuli in terms of sensitivity to simple stimulus dimensions. Linear systems analysis is a powerful technique of modern psychophysics used to characterize visual sensitivity. The basic idea is that any complex visual pattern can be generated by summing, according to Fourier's theorem, spatial sine wave gratings of appropriate frequencies and amplitudes (and in appropriate phase relations). If the human visual system operates like a linear system, the perception of complex patterns could be predicted on the basis of sensitivity to individual sine wave gratings. In short, perception of all manner of complex stimuli could be derived from a knowledge

of sensitivity to a smaller number of very regular sine wave patterns. The task for psychophysics then becomes the measurement of sensitivity to the various frequencies of sine wave gratings. This is accomplished by determining for each different spatial frequency the minimal detectable light-dark contrast. It turns out, for example, that for adults such contrast sensitivity is higher for gratings of intermediate frequencies than for gratings of low or high spatial frequencies.

During the last few years measurement of contrast sensitivity has been accomplished for infants by taking advantage of their preference to look at a patterned as opposed to a uniform stimulus. An infant is shown a sine wave grating with very low light-dark contrast paired with a uniform stimulus. The light-dark contrast of the grating is gradually increased until the infant shows a looking preference for the grating over the uniform stimulus. This procedure is repeated systematically with sine wave gratings of a wide range of spatial frequencies. From these data it should be possible to predict infants' perception of complex pattern stimuli, and to a considerable degree preferences for meaningless complex patterns can be predicted. However, these data alone do not account for infant sensitivity to the more abstract and meaningful characteristics of stimuli.

It is likely that the linear systems approach will at least enable researchers to set constraints on what stimulus patterns can be perceived and discriminated by infants (Dannemiller and Stephans, 1988). In addition, the linear systems approach may have considerable promise as a diagnostic device. If visual pathology is suspected, deficits in sensory functioning can be specified very precisely by identifying spatial frequencies where contrast sensitivity is reduced. The highest spatial frequency that can be discriminated from a uniform visual field of the same average luminance is a good measure of acuity (cf. Jacobson, Mohindra, and Held, 1981, 1982). The techniques used for studying infant perception and cognition are just beginning to be exploited for diagnosis of pathology and monitoring of treatment.

Finally, our understanding of infant perception and cognition is suffering from a kind of visual imperialism. A huge preponderance of our knowledge is derived from visual perceptual paradigms. There is some information based on auditory perception, particularly speech stimuli, but very little from any other sensory or perceptual input to infants.

The research on infant speech perception is particularly interesting in that it shows infants making sharp discriminations in acoustic continua at the very place where natural languages have phonemic boundaries. Thus, infants show a sharp boundary in discriminating between /ba/ and /pa/, which is specified by voice onset time, that is, the temporal delay

between onset of vocal cord vibration and expulsion of air from the oral cavity. More surprising, American infants have an analogous sharp discrimination boundary in a prevoicing region that is not a phonemic contrast in English but is in certain other languages such as Thai (Eimas, 1975; Aslin, Pisoni, Hennessy and Percy, 1981). These results have interesting implications for the development of speech perception because adults have considerable difficulty in making such discriminations when they are not phonemic in their language. This evidence and other preliminary research on the categorization of speech sounds by infants indicate discrimination of speech sounds but not use of this ability in organizing and processing speech input.

### *The Structure and Organization of Knowledge and Issues of Learning*

Progress in describing the structure and organization of knowledge has again been significantly aided by recently developed methods of assessing knowledge and rules that a child might possess. These include double assessment techniques (Wilkinson, 1982, 1984), information integration approaches (Anderson and Butzin, 1978), and rule assessment techniques (Siegler, 1981). To illustrate, in Siegler's rule assessment approach, errors on a diagnostic set of problems are used to help identify which of a set of rules children are using to solve a problem. The assumption is made that children's problem-solving behavior is rule-governed, with the rules becoming more sophisticated with age. Applied to a typical Piagetian problem such as predicting the direction of tilting of a balance beam, the simplest rule might be to consider only the number of equal weights on either side: if the number of weights is the same, predict balance; otherwise, predict tilt with the greater number. The next more sophisticated rule would be to rely on number of weights if the two sides have different amounts and predict tilt with the greater. If the two sides have equal weights, then consider the distance from the fulcrum. A diagnostic set of problems includes balance problems where the same configuration of weights is on both sides of the fulcrum, weight problems where equal amounts of weight are at different distances from the fulcrum on the two sides, etcetera. In an elegant way the pattern of errors eliminates possible rules that children of different ages are using. In analyzing the structure and organization of knowledge the emphasis has been on the quality rather than the quantity of knowledge a child might possess at any one time. Does the information form a coherent structure? Does it form a system of interlocking semantic fields? Or is it a piecemeal structure with many internal inconsistencies? Such an emphasis is reflected in recent work on the development of concepts,

which has stressed the importance of underlying theories in which concepts are embedded and from which they derive their meanings (e.g., Carey, 1985; Gentner, 1978; Keil, 1981).

Keil's work on ontological knowledge provides a good illustration of this approach. He has shown, for example, that the knowledge of adults about the basic categories of existence is hierarchically organized, with physical objects being subdivided into animate and inanimate objects, the animate objects being subdivided into plants and animals, and animals being subdivided into humans and lower animals (Keil, 1979). Evidence for this organization comes from judgments of adults about what could or could not be predicated about an object. Subjects were asked if it was correct to say, "The lady is not sorry," "The day is heavy," etcetera. Studying children as young as three and five years old, Keil (1981) found a similar hierarchical organization but with less differentiation than among adults. The adult organization reflects physical objects subdivided into animate categories (with subcategories of plant, animal, and people) and inanimate categories (with subcategories of natural objects and artifacts), while the child organization may distinguish only animate and inanimate objects. Keil's results imply a clear organization to children's knowledge. When children make classification errors, their attributions to the misassigned term are consistent. For example, one five-year-old who judged rocks to be alive indicated that rocks could have babies (pebbles), could grow, and could die.

Analysis of how such organizational structure of knowledge changes makes it clear that there can be local qualitative shifts in how concepts are organized that are consequences of changes in domain-specific theories. Carey (1985), for example, argues that changing patterns of induction of biological properties for various animals reflects changing theories about the nature of animals. Keil's work has shown that children undergo shifts in what properties they regard as critical to the identity of various natural objects such as animals and plants, as well as shifts in what they regard as the fundamental dimensions for organizing other conceptual domains such as kinship and cooking terms. In general, the shifts seem to be a consequence of the development of theories about the entities in a domain and how they should be organized and interrelated.

This work has benefited greatly from an allied line of work on causal reasoning (e.g., Bullock, Gelman, and Baillargeoni, 1982; Schultz, 1982), which has demonstrated that preschoolers can have rich causal intuitions about the mechanisms underlying various phenomena, not just in the physical but also in the social realm (i.e., attributions). This emphasis on causal information converges with the concept development work in that children's theories frequently have elaborate causal structures.



One reason for the fruitfulness of this recent research on concepts and causal theories is that investigators have turned away from the use of artificial stimulus sets composed of conjunctions and disjunctions of arbitrary features. The study of how children learn concepts such as "small red triangle" tells very little about how most concepts develop normally, since in natural cases those patterns are inextricably bound up in developing domain-specific theories. Advances in the ability to measure knowledge states in detail have led to renewed interest and initial successes in studying the relation between knowledge and learning. This renaissance in the study of learning is characterized by two features: one is a focus of attention on principled domains of knowledge, and the other is the possibility of formalizing the learning process itself.

With respect to learning in principled domains, it is possible to map out a child's current state of knowledge by using the various knowledge assessment techniques mentioned above. And by definition a principled domain provides a well-understood structure against which to assess the performance of the learner. These considerations have led some developmentalists to investigate learning in major academic domains. Educators and educational psychologists, of course, have been studying these areas for years. However, developmental psychologists are beginning to bring fresh perspectives that have already produced some notable advances. For example, in the area of mathematics alone there have been significant studies of counting (Gelman and Gallistel, 1978), early arithmetic skills (Ginsburg, 1982; Cooper, 1984), and geometry (Greeno, 1978). For many years educators have challenged cognitive developmentalists to apply theories and methods to studying how children acquire important skills. These challenges are starting to be met, but a great deal more research needs to be done. In many of these domains, such as mathematics, physics, biology, reading, and writing, the knowledge and skills are learned over a long age range and the techniques can be fruitfully applied to the learning and thinking of children from preschool to adolescence and adulthood.

When it is possible to measure knowledge states and characterize a principled domain, it is possible to develop a formal learning theory. This is nowhere better illustrated than in the area of language acquisition. Although ten years ago it would have been foolhardy to construct explicit theories of language learning, today there are some dozen formal or algorithmic models of the learning process. They come from artificial intelligence, in the form of computer simulations of language acquisition (e.g., Anderson, 1976), models of learnability from linguistic theories (Wexler and Culicover, 1980), mathematical studies of when learning is possible in principle (Gold, 1967), and explicit psychological models (Pinker, 1984).

In language acquisition these developments will help to resolve a number of controversies such as those concerning innateness, the role of semantic/conceptual input in rule learning, and the importance of the special speech mothers direct at their children. These require precise notions of the underlying constructs (e.g., semantic representation, innate rule schemas, representations of the input) and how they interact causally in the process of learning. In addition, explicit learning theories affect one's choice of research problems. For example, depending on the role one's model assigns to maternal repetitions or expansions of prior utterances, the detailed analyses of such behavior will be more or less important. Similarly depending on the formal model, laboratory studies of the child learning new words or rules under experimental control will be more or less interesting. Furthermore, explicit learning theories tie studies of children's behavior closer to the intellectual issues that inspired interest in language to begin with, such as the species-specificity and task-specificity of language, the importance of language-specific innate constraints, the relative contribution of child and environment, etcetera. Finally, theories of learning allow one to understand the developmental process. For example, they allow one to factor apart the developmental changes that must be attributed to learning, those that may be attributed to learning, and those that cannot be attributed to learning. In some cases they allow one to explain why some stages of competence must be mastered before others.

While this argument for formalization has been developed with respect to language acquisition, it can also serve as a model for other domains. A number of approaches to formalization show considerable promise. Production systems and parallel distributed process models are useful for characterizing procedural knowledge and problem-solving behavior (cf. Siegler, 1983) as are planning net theories (e.g., Sacerdoti, 1974; Hayes-Roth and Hayes-Roth, 1979). The formalisms of specific knowledge domains (e.g., number theory for numerical knowledge or geometry for spatial knowledge) are useful for characterizing particular knowledge states and perhaps the transitions between states. The goal in all cases is to make precise statements about assumptions and models of competence and changes in competence.

### *Emotional Development and Social Competence*

The elaboration of new methods has also contributed greatly to the study of emotional development in infancy. The early sensitivity of babies was alluded to in the section above on the origin of competence. There the recent reports on imitation by neonates (Meltzoff and Moore,

1983; Field and Walden, 1982) were referred to as was the cross-modal matching of a sound track to an appropriate emotional visual display (Walker, 1982). Other evidence includes the fact that infants become agitated in response to violations of usual and expected affective stimuli as when an adult frowns or maintains a static face. For example, Cohn and Tronick (1983) had mothers pose a depressed feeling state to their three-month-old babies as manifested by a slow and monotonous tone of voice, minimal movement of arms and body, and absence of touching the baby. These conditions resulted in more gaze aversion, protest behavior, and crying than when the mother behaved normally. A new "social referencing" paradigm has recently been used to study infant social-emotional sensitivity. This paradigm provides evidence for babies using the emotional expression or response of adults when they are in an uncertain or potentially fearful situation. A nice demonstration of this paradigm involves the response of one-year-olds to the visual cliff (Sorace, Emde, Campos, and Klinnert, 1985). When the depth on the deep side of the visual cliff was intermediate and the babies might or might not cross it they looked to their mothers and used the mothers' facial expression as a basis for guiding their behavior. When the mother posed a fear expression none out of seventeen infants crossed the deep side, while fifteen out of nineteen infants crossed when the mother posed a happy expression. Another example concerns the baby's response to strangers. Infants will modify their responses to strangers according to the emotional reaction of their mothers. Boccia and Campos (1983) showed that infants responded to a stranger with cardiac acceleration (characteristic of fear), less smiling, and more distress when their mother responded in an unfriendly manner to the stranger as opposed to when the mother reacted in a friendly way. It should be noted that this kind of evidence reflects not only sensitivity to the emotional expression of others but also use of that sensitivity in the service of regulating the infants' own behavior.

Significant progress has also been made in the understanding of the origin of emotional expression. This work has taken advantage of advances in the measurement of emotional expression. Several detailed schemes for describing emotional expression have been elaborated (see Ekman and Oster, 1979). Although these schemes were initially developed on the basis of adults, they have also been applied to infants (e.g., Hiatt, Campos, and Emde, 1979), and evidence is accumulating that infants differentiate a number of positive and negative emotions in their facial expression. In one study, Sternberg, Campos, and Emde (1983) found clear patterning of anger in seven-month-old infants. The emotion elicitor was the removal of a teething biscuit just before the baby placed

it in her mouth. Prior to removal of the biscuit only 3 percent of anger-related facial movements were observed; after removal the anger facial movements increased to 29 percent while nonanger components did not change. Izard and his colleagues (Izard, Hembree, Dougherty and Spizirri, 1983) have shown a similar patterning for anger in nineteen-month-old infants in response to the stress of inoculation.

In order to exploit the progress already achieved, such investigations need to be extended beyond infancy and the preschool years. Prior work has been largely laboratory-based, and research needs to be extended to natural settings to discover the role of affect in the emergence of social competence in ongoing social exchanges in peer-peer and parent-child interactions. Some recent work indicates links between social competence with peers (as assessed by a child's sociometric status among peers) and the ability to accurately recognize emotional expressions. Specifically, Parke and his colleagues (Parke, MacDonald, Bietel, and Bhavangri, 1987) have found that children who were better able to identify facial expressions were more popular with their peers. However, the ways in which emotional expression operates in interaction among peers to regulate social behavior and to lead to social competence are poorly understood. Short-term longitudinal studies bracketing periods of theoretically interesting change will help provide the needed level of analysis. For example, the period in which children begin to have meaningful peer encounters should be scrutinized. Such studies will also help pinpoint the antecedents of the emergence of these affective skills. Recent work has begun to uncover the role of the family in the socialization of affect, but this work has barely begun to map out the antecedents of emotional expression and regulation.

The organization and structure of knowledge are also manifest in the development of social competence as reflected in the study of emotional development. How do children come to control their own emotional expression voluntarily and to understand the situational determinants of emotion and signs of emotion in others? There has been considerable recent work on what children understand about the causes and experience of emotion (e.g., Gnepp, 1983; Harris, 1983; Harter, 1982). Results indicate that initially (at about three years of age) children distinguish primarily a positive and a negative emotional category with little differentiation in between. Over the next several years children begin to differentiate within the positive and negative categories in ways that are in some respects culture-specific.

One intriguing finding is that children only gradually (between three and thirteen years of age) come to realize that it is possible to simultaneously experience different emotions, such as being happy that one's

lost dog has come home but sad that the dog has been injured. At first only the positive or negative emotion is acknowledged. Later, children will typically say that the two emotions are experienced sequentially, and finally they will accept the possibility that the two emotions can be experienced at the same time.

### *Social Relationships and Social Systems*

One of the most evident social changes in development is the increase in number and kind of a child's social relationships. There has emerged a concerted research effort to describe the developing nature of social relationships. Part of this interest includes an expanded view of the important social agents in the child's environment both within and beyond the family. Starting with the analysis of initial attachment (Ainsworth and Bell, 1974; Sroufe and Waters, 1977), there have been substantial conceptual and empirical advances in the area of parent-child relations. During the past decade a new understanding of the distinctive as well as the overlapping roles of mothers and fathers has been achieved. A number of researchers (Lamb, Parke, Pedersen) have documented that fathers and mothers differ primarily in their play style, with mothers being more verbal and didactic, and fathers being more physical and arousing. These stylistic differences in turn are reflected in children's social and cognitive development, with the children of highly verbal mothers and physically playful fathers being rated as most socially competent with their peers (MacDonald and Parke, 1984). Some progress has been made in showing that both parents can serve as significant attachment objects and that both significantly influence children's social and cognitive development. Of particular interest is the greater recognition given to ways in which husband-wife relationships may in turn affect how parents execute their parental roles. High-quality marital relationships are associated with more satisfactory parent-infant interaction patterns (Pederson, Anderson, and Cain, 1980). In turn, the cognitive and social competence of children may be enhanced. Accompanying this shift is the recognition that the paths of influence on children are often indirect.

The embeddedness of families in larger social systems is also gaining recognition. Progress has been made in characterizing the type and amount of social contact outside the family, as well as the impact of differing degrees of connectedness to, utilization of, and satisfaction with social contact on individual family members and in turn on children's social-emotional development. For example, maternal social support, especially in the case of a temperamentally difficult infant, has been found

to be a predictor of the quality of mother–infant attachment. Crockenberg (1981) has found that secure infant–mother attachment will be attained with a difficult infant if the mother receives sufficient social and emotional support; without such support the temperamentally difficult infant develops an insecure relationship with its mother. Similarly, it has become clear that family and peer systems are linked. The quality of the infant–mother attachment has been found to be a significant predictor of later peer competence.

There has been an increase of information about the nature and role of relations between siblings and peer relations. The role of older siblings as models is now better understood. Another advance is how older siblings react to the birth of a new sibling.

The peer relations area has participated in the increased examination of infant behavior as studies continue to uncover the developing social skills of young infants and toddlers with their peers. Even in the first year of life, rudimentary social skills are apparent, although the frequency and complexity of infant–infant interchanges are limited. Hartup (1983) has noted that these early social skills reflect changes in the baby's interactions with its mother and with inanimate objects. Studies during the second year of life have revealed a high degree of similarity between the games toddlers play with adults and with peers, suggesting that an adult partner is not necessary to support such social exchanges.

In the preschool and elementary years a major focus has been on the relationship of social competence to status in the peer group. Interest in this problem has accelerated partly because of evidence suggesting that early peer status is predictive of later social and emotional adjustment. Among the advances on this problem are the following. First, there is evidence that different types of low status vary in their stability and in their association with later adjustment problems; for example, rejected children are more stable over time than neglected or popular children. Second, children's interaction styles predict status in newly formed groups (short-term longitudinal studies of the group formation process have made an important contribution here). Third, studies of social knowledge have examined the content of children's ideas, children's social problem skills, and developmental changes in the structure of children's thinking. There is evidence that social knowledge relates to status in the peer group. Goetz and Dweck (1980) have studied the relationships of children's attributional processes to their social coping styles. Children who attribute social rejection to their own personal inadequacies are less likely to cope effectively by changing their strategies. Another dimension receiving attention is children's goals for social interaction. Renshaw and Asher (1983) have found that unpopular children

do not necessarily differ in their knowledge about what to do in various social situations but differ in the way in which they construe goals in certain social situations. Popular children have more friendly, assertive goals than unpopular children, who, in turn, have more task-oriented goals. Fourth, intervention research focused on social competence is proving effective in promoting change in the social status of socially rejected children.

In sum, knowledge about the individual child and its interaction with other individuals, be they parent or peer, and even with institutions such as schools is rich (Minuchin and Shapiro, 1983). In some respects this very richness has led to the realization that our understanding of how the child functions in these relationships is incomplete. In trying to investigate social development, researchers first focused on the child as the passive recipient of social as well as physical stimulation. An advance was and is still being made as researchers have realized that the child, even the infant, is not completely passive in the interaction. This is most obvious in default where infants at risk may not elicit "appropriate" parenting behavior from adults. But even recognition of this interaction does not seem to capture the whole picture; the relationships are more complex. It has been noted, for example, that husband-wife relationships will affect how parents execute their parental roles, and conversely the child's behavior can affect husband-wife relations. Indeed, the mother-wife, father-husband, child-sibling . . . form an integral system. A number of investigators have called for the examinations of relationships as units (Sigel and Parke, 1987; Belsky, 1984). For example, attachment is conceived as a relational concept independent of either participant and in this way different from the earlier concept of dependency (Minuchin, 1985).

The thesis of this perspective is that it would be valuable to examine such systemic relations as units; they behave as integral wholes. A number of theorists have proposed the very general application of such a systems approach to the study of social development. The child is embedded in the family, the family embedded in a community, the community embedded in a society. These approaches have been elaborated provocatively but abstractly in somewhat different ways (e.g., Bronfenbrenner and Crouter, 1983; Sameroff, 1983). Empirically this kind of analysis has been used by family therapists to explain why some family systems seem to have a homeostatic resistance to intervention. Dramatic examples include cases where the improvement of one member is accompanied by breakdown in another. Minuchin (1985) characterizes this approach with a set of principles that emphasize the view that any system is an organized whole whose elements are interdepen-

dent, that patterns in a system are circular (or helical) rather than linear, that systems are homeostatic and maintain stability of their patterns, and so forth.

Notwithstanding this application to family therapy and some examination of children's affects on parental relations, empirical work on systems as such has lagged behind the theory. The problem may be that methods for examining systems (especially higher-order systems) are lacking. Even where researchers are concerned with wider impact they seem to fall back on effects on and of individuals. However, this may be about to change. The need for such approaches is now more widely recognized and researchers are groping toward better methodology. The special issue of *Child Development* (Kaye and Furstenberg, 1985) on family development and the child as well as the volume "Review of Child Development Research, Vol. 7: The Family" (Parke, Emde, Macadoo, and Sackett, 1984) represent the beginning of this promise.

### *Action and Skill Development*

More and more developmental researchers are becoming concerned with the relation between knowing and acting. This is reflected in the study of both cognitive and social development.

The study of metacognition is one area in which interest in this relation between knowing and doing has been manifest. Metacognition refers to a person's knowledge and understanding of his own cognitive processes and those of others (Flavell, 1976). What does one know about one's own mental activities and thought processes? Are there processes that one is explicitly aware of, and do they differ from those that one follows implicitly? There has been considerable advance in the past decade in our understanding and ability to isolate and measure metacognitive aspects of cognition (e.g., Brown, 1978; Campione, 1987; Wellman, 1985). Demonstrations abound concerning what young children do and do not know about their thinking processes and how this affects their performance.

This is an important research area because some notion of metacognition or executive control occurs in most developmental theories. We need to go beyond demonstration studies and develop more detailed and more powerful theories of metacognitive components in a variety of domains. Promising topics include task specificity and generality of metacognition; the causal relationship, if any, between metacognition and performance; the development of ability to interpret metacognitive information adequately; how implicit knowledge is transformed into explicit knowledge; and whether and how metacognitive aspects of performance can be instructed, and if so, what effect that has on performance.



Cognitive competence not only plays a role in cognitive activities such as learning and problem solving but also is central in more instrumental actions, which have typically been studied under the rubric of motor skills. The study of motor performance, in general, is exploding out of a relatively dormant period. Among the influences in this renaissance are the application of information-processing techniques and analyses to motor performance (Rosenbaum, 1980); the work in artificial intelligence concerned with developing relatively adaptable robots; the ecological approach to perception, which emphasizes the close connection between perception and action (Gibson, 1979; Turvey, 1977); and recent advances in the physiology of motor control concerned with the representation of motor commands and motor programs (Polit and Bizzi, 1978; Terzuolo and Viviani, 1979).

The cognitive aspects of development of action are seen first of all in the transition from involuntary to voluntary behavior. Newborn behavior seems to be comprised primarily of random movements, reflexes (Prechtl, 1984), and rhythmic stereotypes (Thelen, 1979). However, over the first year of life these gradually are transformed into voluntarily guided, intentional, and purposive actions. Some attempts have been made to define voluntary, intentional behavior (e.g., Bruner, 1973), although they are not entirely satisfactory. Nevertheless, in many cases it is possible to obtain reliable judgments about the intentionality of behavior.

In order to perform many actions, the child must accurately represent its own capacities in relation to various features of the world. Thus consider a toddler wanting to climb some stairs. The child must appraise the height of the step in relation to its capacity to crawl. Data have been obtained for the simple task of lifting weights of different sizes. The ability to adjust the initial force of the lift according to the size of the weight develops only gradually, with young children exerting unnecessarily large forces for the smaller weights and having to actively inhibit the movement (Mounoud & Hauert, 1982). What is the nature of the child's representation of such action-relevant properties of the physical world? How are they acquired? What is the relation of children's practical knowledge to their conceptual knowledge and does this relation change with age and experience?

No less important is the application of knowledge in the social realm. One crucial skill is interacting with others. Little work has been done in this area. For example, most of our knowledge of competence is based on situations where people act alone, struggling with a problem on their own. Increasingly, it is recognized that most of daily life involves working with or for other people. Other people are resources we can use: sources of advice and help. Others can also make problem solving diffi-

cult: they are sources of interference. A major issue, then, is the gap between our knowledge of some limited forms of competence and the variety of forms it takes in real life.

A related problem is our long-term dependence on a paradigm that emphasizes the way in which knowledge is self-constructed. The two basic ingredients are considered to be the inherent disposition or competencies of the individual and the information yielded by the individual's actions upon a world of objects or people. This paradigm has never provided much room for instruction, leaving us with little way of bridging child development material with material on instruction, on social learning through modeling, or on the way in which people package information for children. The paradigm is beginning to shift, with a marked rise of interest in peer-group instruction, in adult-guided learning, and in Vygotsky's developmental theory, which places major stress on gaining of knowledge through gradually decreasing support of adults or peers.

The techniques for investigating these processes are being developed in various studies of interactions among people on shared tasks, divisions of labor in joint work, effective and ineffective help seeking, and the acquisition of rules about the "ownership" of work. These approaches need to be brought together and extended. For example, theoretical and empirical efforts on self-construction and joint participation in learning could be fruitfully integrated.

### *Biological Mechanisms of Change*

The issue of mechanism of behavioral change can be analyzed at a variety of levels. One exciting current possibility is the investigation of the biological mechanisms of change. Several different biological approaches offer promising arenas for seeking mechanisms of change: ethology, behavior genetics, embryology, psychoneuroendocrinology, and neurobiology. These approaches have implications for behavior relevant to development of both social and cognitive competence.

The area of neurobiology will be used to illustrate the possibility of investigating the interaction of brain function and experience in development. A number of correlational approaches to the study of brain function are useful with humans. These range from correlating the behavior of clinical populations with the presence of brain anatomic or physiological abnormalities to correlating behavior or experience in an experimental setting with current brain activity as measured by noninvasive electrophysiological techniques such as EEG or cortical evoked potentials, and to the inferring of brain function from presence or ab-

sence of reflex behavior or asymmetries in sensory or motor function. One of the best known and most graphic examples of the developmental implications of the use of clinical populations for the study of brain function is Lenneberg's (1967) analysis of the effects of brain damage on language behavior. Damage to the left cortical hemisphere results in more or less permanent deficits in language function for a large majority of adults. However, persons who suffer even severe damage in that hemisphere before the age of five have essentially normal language function as adults. Apparently for such persons the right hemisphere has assumed language dominance.

An experimental approach to the study of brain function is possible with animal models. These obviously have very important implications for human development. An intriguing example of such implications is found in research on the development of stereoscopic vision. Based on the classic work of Hubel and Wiesel (1965), it has been shown that normal binocular experience is necessary for development of normal binocular functioning of cortical neurons in kittens. The normally binocular neurons (neurons which respond to stimulation of either eye) of kittens raised with artificially induced strabismus respond only to stimulation of one eye. Such treatments result in poor performance in behavioral tests of binocular functioning such as stereoscopic depth perception. Furthermore, there seems to be a critical period for having normal binocular experience (Blakemore and Van Sluyters, 1974). Such evidence has led to questioning whether there might be a sensitive period for development of normal binocular functioning in humans. This was investigated by examining patients who had corrective surgery at various ages for naturally occurring strabismus. The binocular functioning of such patients was measured by the interocular transfer of a visual aftereffect. From these data it was possible to infer the age at which normal visual experience was most critical. The evidence suggests a sensitive period between six months and two years of age (Banks, Aslin, and Letson, 1975).

Neuroanatomical and electrophysiological techniques are beginning to be used to study development. The work of Goldman-Rakic and her colleagues, for example, on the developmental functioning of the prefrontal association cortex has considerable implications for cognitive development. This area of the brain seems particularly involved in the organization and continuity of behavior toward future goals. By examining in animals effects of experimentally produced brain damage at different ages it is possible to model the kinds of observations Lenneberg made with clinical patients. The functioning of specific parts of the brain and how it changes with age can be determined. The way in which

recovery of function does or does not occur can lead to possible therapeutic regimes (Goldman-Rakic, Isseroff, Schwartz, and Bugbee, 1983).

The use of such experimental approaches with animal models will be most valuable when used in convergence with noninvasive methods with humans. New methodology should make this easier. As noted, cortical evoked potential techniques are already being applied in situations comparable to behavioral habituation experiments. The use of nuclear magnetic resonance (NMR) and superconductive quantum inference devices (SQUID) offers exciting possibilities for the near future.

### **Infrastructure and Methodological Recommendations**

Implicit in all of the above recommendations is the need for infrastructure support and methodological emphases. We will make explicit recommendations for training and technological support and for support for cross-cultural and longitudinal studies.

#### ***Training***

If the progress in the topics described above is to continue, or better, to accelerate, it is imperative that the current generation of graduate students receive the requisite training. Such training cannot come from psychology alone but must involve the technical subjects that fostered the advances outlined. For example, in developmental psycholinguistics a major impediment to progress has been the paucity of trainees with skills both in linguistic theory and in experimental design and statistics; those with expertise in one tend to be backward in the other. The requisite training in some cases will involve different bodies of material for students of different aspects of development. For students of language acquisition, training in linguistic theory is imperative; for those interested in the development of logic or numerical competence, training in formal logic or the foundation of mathematics would be almost a necessity. Knowledge of expert systems from artificial intelligence might be very desirable for students interested in acquisition of skill and expertise, as would ethology and observational methodology for those interested in processes of social interaction, and neurophysiology and biochemistry for those interested in the psychobiology of behavior. All students should have a knowledge of experimental design and statistics and be familiar with adult psychology in the area of their specialization.

Cognitive students might also benefit from a knowledge of cognitive simulation and formalisms such as production systems and parallel distributed process models.

Where will the time be found for such training? One possibility is that the traditional graduate program in developmental psychology, extending horizontally across all aspects of development, be superseded by a vertically organized curriculum in which training in formal and technical subjects such as those listed above take the place of breadth within developmental psychology. A second possibility is the increased availability of focused postdoctoral opportunities for gaining specialized expertise of the kind described. The Sloan Foundation cognitive science centers may be a good model for this.

### *Technology*

Compared with other areas of psychology, the study of cognitive and social development is in a primitive state in terms of instrumentation and technology. Computer-driven graphics, computer-generated or -analyzed speech, real-time computer control of experiments, and sophisticated social interaction analyses from videotapes are far too rare in developmental research. Recently, under the leadership of Brian MacWhinney and Catherine Snow, a computerized archive of child-language transcripts with associated software for analysis has been established at Carnegie-Mellon University and the Max-Planck Institute for Psycholinguistics in Nijmegen, the Netherlands. Although the MacArthur Foundation and the National Science Foundation provided funding, the establishment of these data bases required collaboration of a major interdisciplinary group dedicated to finding new methods of collaboration and data sharing. Once established, the requirements of local capacity for data storage and computer power are quite modest by standards of other disciplines but beyond the reach of many developmental investigators. Finally, computer simulation is in many cases a prerequisite for development of subtle and sophisticated theories of the child's mind and learning. It is imperative that there be greater support for computer instrumentation in the major research centers and training programs in human development.

### *Cross-Cultural Studies*

Social, ethical, and practical constraints on experimentation often prevent investigators from making truly decisive tests of developmental hy-

potheses. Cross-cultural studies often provide one with the only hope of an independent variable. More such studies are needed. To be done properly, they should be inspired by interesting questions for which cross-cultural variation provides the crucial independent variable, and not simply be done out of curiosity or to test the generality of an effect, and be done, if possible, in cooperation with investigators from the relevant cultures. This would ensure that the study would be to everyone's benefit since a local investigator would be more appreciative of the sensitivities of the local culture and investigators from both cultures would learn and benefit from the collaboration.

### *Longitudinal Studies*

In a number of areas longitudinal studies are the method of choice. Longitudinal studies provide powerful methods for determining the antecedent conditions for developmental outcomes, for determining the motivating factors in developmental changes, and for arriving at ways to predict developmental outcomes. This is true for both cognitive and social development. The classical longitudinal studies in developmental psychology (e.g., Berkeley, Fels) were and are immensely useful, and it is time to take up this approach once again.

First, new application of the longitudinal method could focus on specific and detailed measures of variables of interest. For example, the representation and structure of children's knowledge in specific domains in the case of cognitive development and the attachment behaviors exhibited by children in the case of social development (see Egeland and Sroufe, 1981) would be good candidates for input into longitudinal analyses. Second, it is now possible to make permanent videotape records of target situations so that the original observations are much more available for reanalysis than before. Third, there is obvious use for short-term longitudinal studies to study interesting transition periods. The cost of these shorter studies should be manageable. Related to this and the previous point, it would be desirable to use observations of more dynamic characteristics at two points in time. For example, how is the process of social interaction at time 1 related to the interaction at time 2? Finally, new designs and techniques for analysis of longitudinal data have been developed. Time-sequential and cohort-sequential designs help eliminate cohort, age, and time of measurement effects, and path analysis and cross-lagged panel analysis help to infer causality from correlational data. While all the problems in the use of these methods have not been worked out they are very promising.

### Funding

Most of the infrastructure recommendations have implications for funding. Improving the level of technology and conducting cross-cultural or longitudinal studies are costly. In addition, the procurement of subjects for developmental research is more expensive as a basic cost than for many areas of behavioral research. Infant subjects have to be recruited and transported to the laboratory (or the laboratory transported to them). Infant studies of necessity are labor-intensive and the attrition rate is high. Laboratory schools offer an attractive source of subjects at older ages. But these are also expensive to operate, especially with potential subjects whose families are unable to pay the full cost of the school. The point is that funding agencies have to recognize that policy guidelines or rules of thumb to fund at levels around \$30,000 to \$60,000 a year may be unrealistic and reduce productivity.

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## Health and Behavior

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Several scientific and health care developments have led to an increased prominence of social and behavioral research in the health domain. At the turn of the century, the greatest contributors to morbidity and mortality in the United States were infectious diseases such as pneumonia and tuberculosis. Today, the leading causes of death are chronic diseases such as cardiovascular disorders and cancers. It is now well established that these disease states are caused by a confluence of biological, social, environmental, and behavioral factors (Hamburg et al., 1982; USDHEW, 1979a).

Several Institute of Medicine reports (Hamburg et al., 1982; Institute of Medicine, 1984) document the enormous costs to society of mental illness as well as addictive disorders and other health-damaging behaviors. The role of behavior in today's most pressing health problems challenges the traditional medical model, which views disease as a purely biomedical phenomenon, the consequence of specific agents or pathogens and bodily dysfunction. As a result, there has been renewed interest from the scientific, public health, and policy communities in a broader model of health and illness that would encompass psychological and social variables and their interaction with biological processes (Hamburg et al., 1982; Krantz et al., 1985; Matarazzo, 1980; Miller, 1983; USDHEW, 1979a; 1979b).

For example, cardiovascular diseases and cancers are not an inevitable consequence of aging and genetic make-up. Environmental and behavioral variables such as cigarette smoking, diet, poverty, and stress are involved in the pathogenesis of many of these disorders. Behavioral factors also are crucial ingredients in the effective treatment and management of disease. High blood pressure, a symptomless chronic condition with potentially serious consequences, can be pharmacologically controlled. However, the success of drug therapies depends on a patient's compliance with medication prescription, while other cases are treated with dietary and habit changes rather than with drugs. Behavioral techniques also have been applied to rehabilitation and to the reduction of postsurgical pain and complications.

Not only has research on health and behavior contributed to the health care disciplines and the understanding of disease but it has also

advanced basic understanding of interactions among biological, psychosocial, and behavioral processes. For example, over the past decade, research has suggested that rather than being an autonomous defense mechanism for combating foreign agents in the body, the immune system is sensitive to changes in the central nervous system and in endocrine functioning, such as those elicited by stressful events (e.g., Ader, 1981; Schliefer et al., 1983). Another example is the new understanding of basic mechanisms of addiction through recent research on psychosocial and biobehavioral factors affecting the cigarette smoking habit (e.g., Jarvik, 1979).

The documentation of important associations between social and behavioral variables and health outcomes is only the beginning of research on health and behavior. In contrast to a purely correlational approach, biobehavioral research has explored *mechanisms* linking behavioral processes to health and disease. An example is provided by recent evidence of the role played by psychosocial processes (e.g., emotional stress) in the development of cardiovascular disorders. In an effort to determine how these processes are influenced by behavioral events, behavioral scientists are developing animal behavioral models, and are devoting increased attention to the physiological processes (e.g., neuroendocrine activity and cardiovascular responsiveness) associated with stress that are implicated in the development of coronary heart disease. Also under study are the psychosocial processes that affect the decision of adolescents to begin smoking cigarettes, and the socialization of health habits and ways of recognizing and perceiving symptoms (e.g., Campbell, 1975). These and other examples illustrate the close interplay between research on basic mechanisms (modeling, attitude change, adolescent development) and the development of effective preventive interventions (e.g., Evans et al., 1981; Matarazzo et al., 1985).

In a chapter of this length, it is difficult to represent fully the breadth and scope of research in the rapidly developing domain of health and behavior. In the following section, we describe several representative areas of study that are likely to be at the cutting edge of future research.

## Promising Research Frontiers

### *Factors Perpetuating Health-Damaging Behaviors*

Considerable epidemiological research has revealed that a variety of behavioral factors are related to the development and maintenance of dis-

ease. Smoking is a prominent example of a health-destructive behavior that is influenced by an interaction of social, psychological, and biological factors. The process of smoking has three developmental stages: initiation, transition, and the development of high-rate, steady smoking. The initiation of smoking is related to a variety of social factors, including advertising and peer pressure. In addition, modeling of smoking behavior and tolerance to nicotine may be important in the initiation of smoking. After someone begins to smoke, the probability of continuing and increasing the rate of smoking relates to similar social, behavioral, and biological influences, though additional behavioral and pharmacological factors may be important during the transition phase. The level of nicotine in the cigarettes chosen may influence the transition from a beginning to a consistent smoker, while the capacity of nicotine to serve as a reinforcer (e.g., by improving performance or mood or by forestalling withdrawal symptoms) may stimulate further smoking. After smoking reaches a steady state, pharmacological properties become more important, and heavy smokers smoke to maintain a constant amount of nicotine. Psychological factors, such as stress, and learning mechanisms, such as conditioning, also continue to regulate smoking, in part because of an intertwining of behavioral and pharmacological processes. Finally, there is evidence that biobehavioral factors are involved in the common side effects (weight gain, irritability, sleep disturbance) that people experience when attempting to give up the smoking habit (Grunberg and Bowen, 1985). Thus, a comprehensive model of smoking and the development of effective techniques for prevention and cessation must take pharmacological, psychosocial, behavioral, and biological influences into account.

Overeating is another behavior that can be understood only by taking into account genetic, biological, and psychosocial variables. Unlike smoking, however, overeating must be defined and studied by using the person's own genetic and metabolic baseline, as opposed to some absolute standard, thus making the study of this phenomenon both intriguing and difficult.

The field is ready for studies of common processes in the initiation and maintenance of use of substances that are health-damaging (e.g., Levison, Gerstein, and Maloff, 1983). These studies would range from basic neural mechanisms (e.g., endorphinergic reward systems), through psychological variables such as learning and motivation, to sociocultural influences, such as peer pressure and media effects.



## *The Interaction of Brain and Behavior with Immune and Endocrine Function*

### *Psychoneuroimmunology*

During the past several decades impressive advances have been made in our understanding of the cellular and biochemical processes involved in the immune system, as well as of the functioning of the central nervous system (CNS) from the anatomical to the molecular level. Consideration of the integrative action of the CNS on a variety of biological systems has stimulated increased interest in the role of the CNS in relation to the immune system. In addition to concern with the neural modulation of immunity, there has been a renewed effort to understand behavior and immune function. Investigations of these phenomena may lead to the development of powerful models for analyzing the interaction of various body systems and for understanding the role played by environmental/behavioral variables in affecting health and disease.

A number of studies have begun to consider the regulatory effect of the CNS on immunity. These include a consideration of endocrine, neurotransmitter, and opioid influences as modulating processes as well as a direct neuronal link between the CNS and immunocompetent tissues. In addition to regulatory mechanisms, attention has also been directed to feedback processes between the immune system and the CNS (cf. Ader, 1981; Miller, 1982).

Studies investigating behavior and immune function in humans and animals have been reported by, for example, Ader (1981) and Schlieffer et al. (1983). The experimental strategies include a consideration of conditioning paradigms, the influence of stress, and the role of immune function in psychiatric disorders. Although there has been an increase in research in the general area of brain, behavior, and immunity, the findings to date have indicated that the problem is complex and in need of well-conceptualized and clearly delineated investigations.

Since the immune system plays a major role in the maintenance of the integrity of the organism by providing protection against disease, there has been an understandable and irresistible desire to relate brain and behavior effects on immunity to health and illness. These studies have been hampered by our still incomplete understanding of how CNS effects on endocrine and immune parameters are related to more distal outcomes such as disease or good health (see below). Further basic studies are therefore required to clarify more precisely the behavioral and biological processes that may be involved in the influence of brain and behavior on immunity, and to determine if immune changes associated

with brain and behavior influences are related to the onset or course of disease.

After decades of achievement, promising opportunities are now available to further our understanding of brain, behavior, and immune functions. However, the funding of research has not received the priority required to carry out investigation in these critical yet understudied areas. Until fairly recently, there has been a general lack of interest in the area, and scientific agencies have not been fully aware of the significance and importance of research concerned with brain, behavior, and immune functions. It has also become clear that the scientific review of research is hampered by a number of administrative and structural problems. These issues are considered further in a later section of this chapter.

#### *Other Physiological and Neuroendocrine Correlates of Behavior*

Recently, there has been increasing interest in studying the neuroendocrine correlates of stress in an effort to understand the physiological mechanisms by which environmental events affect the health of the body (cf. Baum et al., 1982). For example, recent biobehavioral research suggests that psychophysiological responsiveness (reactivity) to emotional stress may be a marker of processes involved in the development of cardiovascular disorders (Krantz and Manuck, 1984). Recent investigations have also examined sex differences in neuroendocrine responses to stress as well as the effects of occupational stressors on adrenocortical and adrenomedullary hormones (Frankenhaeuser, 1983). This area of research has been greatly facilitated by the availability of new bioassay and measurement techniques.

#### *Risk and Protective Factors in Mental and Physical Disorders*

We have already noted the important role of behavioral factors in the etiology of chronic diseases. For example, standard physical risk factors for coronary heart disease fail to predict many new cases of the disorder. This state of affairs has led to the identification and investigation of psychosocial risk factors such as certain behavior patterns, occupational stress, and the inadequacy of social networks (Ostfeld and Eaker, 1985).

With regard to psychopathology, a rapidly developing area of research is the role played by risk, vulnerability, and protective factors in the etiology, maintenance, and recovery from such diverse mental disorders as schizophrenia, affective disorders, antisocial disorder, and (in children) attention deficit disorder (cf. Masten and Garmezy, 1985). Risk factors are those elements in the individual, the family, and the environ-

ment associated with higher incidence rates of such disorders in population-based studies. Vulnerability refers to those palpable, specific markers that reflect an individual's susceptibility or predisposition to disease outcomes. The study of risk and vulnerability factors offers insights into the potential power of different predictive models of etiology of disease while providing, in turn, a rational basis for norms of preventive interventions with those individuals who are prone to the development of certain mental and physical illnesses.

Individual risk factors for mental disorders include those associated with genetic history, biochemical defects, demographic and social class variables, social and cognitive skill deficits, and deviations in other biological and psychological attributes. Familial risk factors include certain family interaction patterns, deprivational states, negative expressed emotion, and poverty (Masten and Garmezy, 1985). Environmental factors often include stressful life events, mobility patterns, cultural characteristics of the environment, and lack of social supports. The concept of psychosocial risk factors for coronary heart disease (CHD) derives from an inability of standard (physical) risk factors such as hypertension, elevated serum cholesterol, and smoking to predict many new cases of CHD (Ostfeld and Eaker, 1985).

#### *Molecular Genetics and Mental Disorders*

Schizophrenia and manic-depressive psychoses are two major national health problems for which research has yet to uncover the fundamental etiological factors responsible for their presence. Yet it is well known that these disorders run in families, suggesting a strong heritable component. Twin and adoptive studies have added supporting evidence. The growth of the neurosciences and the extraordinary recent advances in molecular genetics suggest that there may soon be progress toward understanding the fundamental genetic factors implicated in these disorders. The task now calls for discovery of those genes that serve as vulnerability indicators for major psychiatric disorders while adding greater specificity to the concept of "diathesis-stress" (the interaction of biological predisposition and environmental experience) as these factors contribute to the etiology of mental disorders. The latter goal may be accomplished by studies of genetic and environmental (familial and extrafamilial) factors of high-risk families, together with intensive biological and personality studies of affected and nonaffected first-degree relatives.

#### *Protective Factors*

The concept of a protective factor is a relatively recent outgrowth of research on stress-resistant individuals. These may be thought of as the

positive counterpart of risk factors, their presence being associated with a heightened incidence of positive outcomes in individuals for which negative outcomes (often on the basis of risk indicators) are anticipated.

Protective factors reflect one of the neglected areas of research in psychopathology. Presumably, future research will reveal those biological and psychological indices whose presence is associated with favorable outcomes in individuals exposed to deleterious conditions. An example can be drawn from perinatal risk studies. It has been demonstrated that at all ages the utilization of good prenatal care (a protective factor) modifies the association between low SES and a greater incidence of perinatal stress. Among protective factors that have been identified, there is the need to determine the interactive role of biological/behavioral processes in fostering favorable outcomes among otherwise high-risk individuals.

### *Contributions of a Life Course Perspective on Health and Behavior*

There is considerable evidence that mechanisms of aging interact with social and behavioral processes in influencing health outcomes. Moreover, in order to understand antecedents of health behaviors and their consequences across the lifespan, a longitudinal research perspective is necessary. Accumulating research points to four guiding principles by which to investigate the relationship between health, behavior, and aging: (1) aging is a life course process; (2) aging is a psychosocial as well as biological process; (3) aging is not universally and permanently fixed but changes with variations in society; and (4) because aging is not entirely immutable, it is subject to a degree of human intervention and control (e.g., Parron et al., 1981).

The rapid growth of the older population with its disproportionate share of health care expenditures highlights the importance of examining health-behavior relationships among the elderly. The refinement of new and powerful statistical techniques for analysis of longitudinal data lends further promise to life-span research.

Recent research has focused on understanding how age and gender interact with social and behavioral factors to affect health and functional status. Among the elderly, living alone has been associated with poor nutritional intake and poor health for older men but not for older women. Similarly, a direct association between bereavement and mortality is found for men but not for women (Stroebe and Stroebe, 1983). Other current research has focused on examining how the effects of social networks on health outcomes for the elderly are mediated by psychological mechanisms such as the perception of control (e.g., Langer

and Rodin, 1976). Exciting research is also being conducted on how age-related changes in physiological (i.e., endocrine and immune) functioning affect or are affected by interacting social and behavioral factors (Riley et al., 1987). These studies reveal the differential effects of environmental factors on the human biological substrate as it changes with age.

Normal age-related events are generally seen as manageable. Therefore, the life course timing of onset of chronic diseases and disabilities can affect coping and adaptation. There is speculation that children or adolescents with chronic diseases such as diabetes and cancer have more difficulty complying with medical regimens than persons who develop the disease as older adults. There is a related body of literature examining how the cumulation of stressors over a lifespan interacts with personal and social resources to influence health and behavior (Elliot and Eisdorfer, 1982).

### *The Institutional Context of Health Behavior*

It is widely accepted that behavior is influenced by the institutions in which people participate and by their roles within the institutions. There is every reason to believe that health-related behavior is no less influenced by institutional contexts than by other types of behavior. Family, occupation, and health care systems are particularly crucial in this regard. As we learn more about how these institutions affect health behavior, such knowledge may contribute to effective preventive interventions implemented on a social or macrosocial level.

The family is an especially important source of health behavior. Among the evident ways in which the family affects health behavior are smoking, food, nutritional, and alcohol use practices. Familial effects are exercised through more subtle processes of socialization as well. An illustration of such processes is provided by the work of John Campbell (1975), who revealed a correspondence between the recognition and interpretation of symptoms by mothers and their young children. However, mothers were much more likely to interpret symptoms as indicative of illness in their children than in themselves. The point is that health dispositions are acquired in the family, sometimes at an early age. It is a process that merits research attention, for the family stands as a potentially strategic target for prevention and intervention programs.

Occupation and job setting have long been recognized as important to health and illness. There may be features of the work or the work environment that pose a direct risk to health (Baker, 1985). Coal miners, asbestos workers, firemen, and many other workers are exposed to

agents or hazards that can be highly injurious to health and longevity. But in addition to direct threats, certain occupations and job conditions involving high demand levels and low levels of personal control have the capacity to affect health indirectly by arousing psychological stress and its biological substrates. This has been demonstrated in several studies here and abroad (e.g., Baker, 1985; Frankenhaeuser, 1983). Involuntary unemployment needs also to be considered in relation to health and health behavior. Many studies have shown the deleterious effects of job loss, such as work (Kasl, Gore and Cobb, 1975) revealing an increase in serum cholesterol following plant closings. None is more dramatic than the research that traces psychological depression as much as forty years after the economic depression of the 1930s and its job disruption. Clearly, occupational environment, the nature of the work, and job security help to regulate health behavior and well-being.

The health care system, finally, represents another context crucial to health behavior. Populations most in need of health care are among those least knowledgeable of it. Once people decide to seek either preventive or ameliorative care, where do they seek help? With current changes both in the organization of the medical care system and in its funding, knowledge of where to turn is likely to be unequally distributed. Moreover, as the studies of such researchers as David Mechanic (1972) show, once people are directed to care, they do not necessarily have equal access to it. And, among people who gain access, the benefits of the case can be unequal. To illustrate, a physician might be more effective with informed patients whose compliance to medical regimens does not require detailed explanation or monitoring than with those needing closer guidance and supervision. How the health care system recruits, channels, and affects its clientele are issues awaiting further study.

### *Uses of Basic Knowledge to Formulate Intervention Strategies*

As we learn more about mechanisms linking behavior and health, promising and specific interventions based on this knowledge can be implemented and studied. Clinical research in behavioral medicine involves the utilization of empirically derived procedures designed to modify health and disease. The treatment procedures may attempt to modify a physiological response directly, as in relaxation, biofeedback, or classical conditioning, or the treatment may target a physiological response indirectly, such as using changes in eating and exercise behaviors to produce weight loss and a decrease in blood pressure. In either case, the treatments used should be based on current research suggesting the control-

ling variables that influence the development and maintenance of the disease, and research on learning theory and behavior change in relation to health-damaging and health-enhancing behavior.

Clinical research should develop treatment strategies based on contemporary behavioral or biological theory. For example, one common consequence of cancer chemotherapy is nausea that occurs in anticipation of the chemotherapy. Since this response may be classically conditioned to the chemotherapy situation, or even to thoughts about the situation, the response may also be susceptible to extinction. Research has shown that systematic desensitization, a method of deconditioning, has been successful in reducing conditioned nausea, using systematic desensitization.

One common observation in most clinical research is that short-term effects of treatment are often not maintained. The majority of persons who lose weight regain their weight, the majority of persons who stop smoking resume their habit, and the majority of persons who begin an exercise program will resume a sedentary lifestyle. For this reason, researchers have become increasingly interested in developing methods designed to promote maintenance of behavior change, using such methods as relapse prevention, cognitive retraining, and behavioral contracting.

Sophisticated clinical research ideally should use powerful interventions that target specific clinically relevant behaviors, which produce treatment effects that are maintained over time. For example, childhood obesity has strong familial effects, suggesting that treatment must be directed at both parent and child. Based on this model, Epstein and colleagues developed a treatment program that targeted behavior change of both parent and child; the child alone; or a nonspecific target. Results showed that all groups lost weight over the first eight months of treatment, but after five years significant weight control was shown only for the parent and child group, while the other two groups showed a return to preintervention levels.

### **Essential Resources and Technological Advances**

This report has placed emphasis on the interdisciplinary nature of research in health and behavior, which involves integrations of psychosocial, behavioral, and biological knowledge at many levels. In addition, there is a need for lifespan and longitudinal perspectives in order to understand antecedents and consequences of health behaviors. Accord-

ingly, our discussion of essential resources reflects the emphasis placed on these issues. In addition, our discussion of resources includes several recommendations for research and training priorities.

### ***Longitudinal Research Designs***

Researchers and funding agencies must appreciate the necessity of using longitudinal research designs in order to study age-related changes in behavior-health relationships and behavioral predictors of good and poor health at different stages of the lifespan. The most adequate research must go beyond merely describing associations that exist between health and behavior to examine psychosocial and/or biobehavioral mechanisms. This requires the use of appropriate analysis and design techniques to examine time-related phenomena. The emphasis of such research should be on behavioral and biological processes, not only on associations between behavior and disease.

### ***Physiological Measurement and Endocrine Assay Techniques***

Biobehavioral research on health and behavior has progressed beyond the domain of pure social science, on occasion requiring laboratory and equipment support for biological science facilities. Recent improvements in physiological measurement technology and the development of biochemical and endocrinological assay techniques now make it possible to examine behavioral and biological (e.g., endocrine, cardiovascular) responses simultaneously and to assess physiological measures in naturalistic settings such as home or workplace. For example, the development of commercially available, portable units for the ambulatory measurement of blood pressure and also for drawing integrated blood samples over time have allowed the measurement of responses to periods of stress and exercise, and investigation of the role of cardiovascular responses to stress in cardiovascular disorders (Krantz and Manuck, 1984). In addition, biochemical assessments provide a means for objective validation of psychological states and also enable the direct investigation of endocrine mechanisms whereby behavioral factors may affect health of the body (Baum et al., 1982). For example, improvements in the measurement of endocrine responses by assay of urine have been employed in recent studies of occupational stress and of the psychological effects of technological disasters (e.g., Baum et al., 1983; Frankenhaeuser, 1983).

In the past few years, there have been dramatic developments in the commercial availability of biochemical assay kits. Individual investigators



with access to university or commercial biochemical laboratory facilities can therefore obtain the necessary reagents to examine endocrine measures such as catecholamines, cortisol, and insulin. However, further training is necessary to inform behavioral scientists of the meaning of such endocrine and biochemical variables and of the theoretical basis for incorporating such measures in biobehavioral research. Training of a smaller group of investigators to secure hands-on experience with the assay procedures themselves in a standard biomedical laboratory would appear to be important in enhancing the quality of research.

### *Postdoctoral Training*

Researchers in health and behavior must be broadly trained in the behavioral sciences and in elements of the biomedical sciences relevant to the phenomena they study. Knowledge of public health and of the health care system may also be required for some types of investigations. To facilitate this type of interdisciplinary training for investigators already trained in one discipline, better mechanisms than those currently in existence need to be implemented. Scientific agencies should increase support for training programs for established or mid-career investigators with the specific purpose of fostering cross-disciplinary research skills. Current Research Career Development Awards place too heavy an emphasis on proposal writing that presumes that the applicant is already conversant with the area of study to be pursued.

One existing postdoctoral training opportunity of this type is the National Institute on Aging Special Emphasis Research Career Award (SERCA) in behavioral geriatrics. This award offers an opportunity for established social and behavioral scientists to acquire supplementary biomedical and interdisciplinary research training. Other government agencies as well as private foundations such as the MacArthur Foundation, through its research networks, may be particularly well situated to implement such training opportunities.

Another important vehicle for training is the intensive summer institute, whose purpose would be to provide behavioral scientists with beginning and advanced training in relevant biomedical science skills and vice versa. Several years ago, the Social Science Research Council sponsored very successful summer institutes for social scientists in the areas of psychophysiology and neurobiology. These institutes, almost universally well-received, provided concentrated training opportunities that could not otherwise be obtained. We believe that intensive institutes in both biological and methodological areas are an effective training vehicle, and that mechanisms for such training institutes ought to provide

incentives in the form of equipment or overhead costs for sponsoring institutions.

## Recommendations

### *Special Problems of Support for Interdisciplinary Research*

As this report has asserted, research on health and behavior involves links between the social, behavioral, and biological sciences. Certain hazards must be overcome if such research is to flourish. Specifically, new areas of interdisciplinary research encounter a lack of understanding and support by the various individual disciplines involved (see Miller, 1982, for discussion). Particularly in times of limited budgets, individual disciplines or funding agencies with missions organized around particular disciplines tend to be reluctant to make commitments to nontraditional approaches. The problem is not usually solved by assembling an ad hoc group of experts from two or more fields involved. Each individual—behavioral and biomedical scientist alike—is likely to believe that, no matter how ingenious or interesting the work, it is not quite central in that it is not advancing the fundamental knowledge of the reviewer's own specific discipline. Furthermore, the technical specialists in each field are likely to look askance at the compromises that may be required to adapt procedures evolved in one field to the difficulties inherent in working on the border of some other field.

Therefore, we must strongly emphasize that for peer review of an interdisciplinary project, the only appropriate peer is another interdisciplinary reviewer (Miller, 1982). The Behavioral Medicine study section at the National Institutes of Health (NIH) is an improvement over the previous single-discipline review panel structure, but it chronically suffers from too few members who themselves are truly interdisciplinary researchers or who at least have a critical appreciation of the multidisciplinary perspective.

This problem is illustrated by but by no means confined to the area of psychoneuroimmunology which requires integration of both conceptualization and methods at an interdisciplinary level. Granting agencies such as NIH are not administratively organized for the appropriate review of grant proposals in this area. The National Institute of Allergy and Infectious Diseases (NIAID) may be interested in and qualified to review the immune aspects of proposals concerned with psychoneuroimmunology, but it lacks the expertise in dealing with the neuroscience and behavioral aspects of relevant studies. Use of ad hoc consultants to review these components is often not adequate because of the lack of

familiarity with immunology. As a result, research proposals in the area are reviewed by ad hoc committees or combined study sections. The integrative and interdisciplinary nature of the research is penalized by this type of review.

Although the foregoing discussion has focused on psychoneuroimmunology as an example, similar problems are shared by other research areas. All too often, proponents of biological approaches to psychiatry give lesser priority to the important and necessary behavioral and psychosocial components of mental disorder. Research has shown that psychosocial concomitants of social class (e.g., postnatal care, family cohesion) can reverse many adverse behavioral consequences of low birth weight (cf. Masten and Garmezy, 1985).

A possible solution to this general problem (which has the effect of discouraging research in the area) would be a unified policy from several NIH institutes to support research in the area of psychoneuroimmunology as well as other promising areas that cut across institute missions (e.g., smoking and health, compliance in health care, risk and protective factors). Study sections could be developed with representatives from the various disciplines (or in consultation with interdisciplinary societies such as the Academy of Behavioral Medicine Research) who have expertise and interest in the further investigation of particular fields. Such an approach would reduce the current random assignment and review of application in these fields and would provide the necessary administrative and scientific recognition of the need for research in such promising areas.

There are additional problems for health and behavior research that derive from the current organization of the Behavioral and Neural Sciences branch at the National Science Foundation (NSF). The programs in social and developmental psychology, cognitive sciences, and psychobiology primarily have the expertise to review proposals in the areas of the social sciences or of biology. Integrative biopsychosocial proposals, therefore, meet with the barriers described at the outset of this section. Furthermore, the basic science mission of NSF is often narrowly interpreted as *precluding* the consideration of research proposals that use clinical populations as subjects or that in any way involve clinical treatment. However, it must be realized that some of the most significant "basic" research on health and behavior falls into these categories, and the decision of relevance to the NSF mission should not be based on such arbitrary criteria, lest some of the high-quality proposals be discouraged under the current system.

Innovative mechanisms for research can do much to further the field of health and behavior. For example, the MacArthur Foundation, through its research networks on Health and Behavior, Risk and Protec-

tive Factors in Psychopathology, and the Psychobiology of Depression, has brought geographically diverse behavioral and biomedical researchers together with the aim of fostering research collaborations and sharing ideas. These network efforts are still in experimental stages, and it remains to be seen what research advances such efforts will promote.

### *Need for Center Grant and "Program Project" Grant Mechanisms*

Many areas of research on health and behavior are relevant to a variety of health problems and are not specific to a particular disease. The problems of compliance in health care and cigarette smoking have wide-ranging health consequences, and may be targeted for research by NIH institutes only insofar as they have an impact on the specific disorders (e.g., cardiovascular disease, cancers, diabetes) within that institute's purview. Thus, the National Heart, Lung, & Blood Institute may foster studies of smoking cessation in cardiac patients or compliance with antihypertensive medications, but has a lesser interest in the basic behavioral issues as they affect a range of health problems. Such general bi-behavioral areas are either left for The National Institute of Mental Health or National Institute of Drug Abuse to promote (even though the study may deal with major risk factors for *physical* disease), or are eligible for support by NSF only if they meet restrictive guidelines regarding nonuse of clinical populations or avoiding issues of treatment.

We feel that a selected number of well-conceived institutional or "center grant" programs organized around generic health-and-behavior mechanisms will foster interdisciplinary collaboration and focus greater research attention on these generic nondisease-specific research issues. Within the NIH, such initiatives would warrant cross-institute support, with ensuring the continuity of resulting research being an important goal.

Private foundations, particularly the MacArthur Foundation, through its collaborative networks, have made a commitment to the linkage of several well-developed research nodes in areas relevant to health and behavior. These MacArthur experiments with the network process have the specific aim of fostering collaborative research, but the foundation also makes substantial commitments to established research groups investigating areas identified as particularly promising. Included in the networks is an opportunity for mid-career scientists who wish to expand their training in a more health-related direction by working with several established investigators.

Collaborations between smaller numbers of investigators could be fostered by a greater number of so-called program project grants focused

on biobehavioral issues. This research mechanism involves several individuals at the same institution working on problem areas around centrally related research themes. Several NIII institutes provide for this type of mechanism, which fosters the kind of collaborations and pooling of expertise needed to further scientific progress in health and behavior. The peer evaluation of such proposals would be facilitated by the creation of review committees composed of professionals with expertise in a variety of disciplines.

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## Social Interaction

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The study of social interaction is a central concern of the behavioral and social sciences (Allport, 1968; Jones, 1985). Social interaction is commonly explored by social and personality psychologists, who place particular emphasis on individual cognitive, emotional, motivational, and behavioral processes. Sociologists investigate many of the same phenomena: their emphasis on the influence of social structures, roles, and institutions external to the individual complements the psychological emphasis on internal factors. In a very real sense, the study of social interaction is an interstitial discipline, linking fields such as sociology, anthropology, economics, and political science, which analyze larger social structures, with fields such as psychology, linguistics, biology, and artificial intelligence, which are concerned with processes that occur within the individual. Although social interaction typically refers to the relations between two or more humans, the continuity between species is represented by links to ethology and behavioral biology. In short, the study of social interaction links the psychological and biological concern with structures and processes located within the individual with the anthropological, sociological, and economic concern with structures and processes located outside the individual in the dyad, group, society, and culture.

Traditionally, the disciplines of psychology and sociology have taken a special interest in the study of social interaction, and it is within these fields that important and exciting theoretical and empirical advances have occurred in the last decade or so. These advances represent a shift away from the simple identification and listing of the probable causes implicated in certain *products* of social interaction—a happy or unhappy marriage, a productive or unproductive work group, a successful or unsuccessful negotiation—toward an understanding of the *process* by which several causal factors meet in a particular social setting, mix with each other, and feed back upon themselves over time to produce the phenomena of interest (Darley and Fazio, 1980; Kelley et al., 1983).

To illustrate: a good deal of traditional research indicates that dissimilarity in attitudes is an important factor in producing feelings of enmity and disaffection between two people. This fact has been documented in numerous studies using the conventional experimental paradigm to

study social influence. That paradigm focuses upon one-step causal sequences between two strangers, one of whom provides the independent variable (an attitude, revealed as a questionnaire response) and the other of whom provides the dependent variable (liking or disliking, also expressed as a questionnaire response). Other possible causes (e.g., physical attractiveness) are eliminated from consideration, and actual interaction between the parties is severely restricted if it is permitted at all.

Of course, the traditional design has been expanded to include multiple causal factors and their interactions, as well as multiple dependent variables. In keeping with the precepts of traditional experimental design, however, the causal elements have not been allowed to vary freely, and therefore interact as they might in the real world. Most important, traditional experimental designs—however complex they may be in their consideration of the interactions among independent variables—do not encourage investigators to explore events occurring beyond the one-step, independent variable–dependent variable sequence, to examine the manner in which an initial effect alters the subsequent relations among the independent variables and affects other events occurring later in the causal sequence.

In contrast, the emerging social interaction paradigm attempts to discover the processes by which a known causal variable interacts with other known causal variables to produce an initial effect on some aspect of behavior in a realistic interpersonal interaction (whether between strangers, acquaintances, or intimate friends). Equally important, it goes on to study the manner in which these initial effects are amplified or dampened over time. Thus, perceived attitude dissimilarity, in the presence of strong demands for courtesy and an annoying physical environment, may produce a subtly derisive remark by one party, followed by a sharp retort by the other, leading to a spiral of hostility that can produce many different kinds of untoward effects, including physical violence.

To summarize, the shift in emphasis characteristic of the emerging social interaction paradigm involves the simultaneous study of many elements, freely varying in naturalistic settings, assessed over time, in a manner that is open to discovering reciprocal causal relations. This manifests itself concretely in the following tendencies among researchers:

1. They confront the fact that pertinent causal factors producing any interaction phenomenon are located not only within the individuals but also in the social environment in which they are embedded, as well as in emergent properties of the interaction itself (Bowers, 1973; Magnusson and Endler, 1977).

2. They recognize that these causal factors often interact with each other over time in a nonlinear and reciprocal fashion (Bandura, 1978).
3. They study larger temporal units of interaction, and conduct analyses of sequences of events within an interaction setting.
4. They perform multimodal assessment of interaction processes using self reports, observer ratings, behavioral codings, and psychophysiological measures.
5. They study interaction in naturalistic settings where the participants have both a history and an anticipated future together.

It should be emphasized that behavioral and social scientists have *always* construed the study of social interaction as the playing out, over time, of complex interactions and reciprocal causal relations. What has happened is that research in the discipline has come to reflect more closely the reciprocal, cyclical nature of social interaction, and its theories have become increasingly precise and mathematical in their expression. This evolutionary trend has been made possible by a number of historical events:

1. Technological advances including high-fidelity videotape, psychophysiological recording, and high-powered microcomputers.
2. Advances in statistics, including the invention of techniques for time-series analysis, curve-finding, and path analysis, as well as refinements in multiple regression and analysis of variance (Kenny and LaVoie, 1984; Thomas and Malone, 1979).
3. Advances in theory formulation, including computer simulation as a medium for writing formal quantitative theories of social interaction processes (Abelson, 1968).

As a result of these advances, the field now has in hand, or at least clearly in sight, the paradigms and analytical tools that will enable it to advance beyond current formulations. What has been accomplished so far has been done with extremely modest levels of funding. The increase in understanding that will come with widespread adoption of the new approaches carries a higher price, in terms of new hardware, personnel, and institutional structures. Yet the higher price remains modest compared to other sciences, even those in the behavioral and social realm. The benefits to be had, in terms of our increased understanding of ourselves and our place in the social world, and our ability to improve social relations, would appear to be well worth the expense.

## Conceptual and Methodological Advances

Although the interactive, reciprocal nature of social interaction has often been assumed at the conceptual level, these features have not always survived translation of theories and hypotheses into empirical tests. Many studies of social interaction focused on only one element at a time—analyzing the structure of attitudes, for example, or the features of social roles. Other studies encompassed two elements, examining the influence of attitudes or social norms on behavior, or the effect of environmental variables on emotional or motivational states. Rarely was analysis extended to encompass three or more elements, and even more rarely was there any attempt to analyze reciprocal causation. Much was learned from such two-element, one-step studies, and much still can be learned from them. At the same time, advances in laboratory technology and statistical methods now permit investigators to analyze multiple causation in extended sequences and to examine many variables and the dynamic relations among them observed over time.

### *Analysis of Interaction over Time*

The increasing focus on social interactions as they unfold over time can be observed in expansions of the traditional two-element design to designs that cover relations among three or more elements. Good examples of this kind of research may be found in recent attempts to reveal the processes underlying the self-fulfilling prophecy, attempts that permit at least one full interaction sequence to take place (Darley and Fazio, 1980; Merton, 1948).

In one experiment, young men were asked to converse over the telephone with young women who were strangers (Snyder et al., 1977). Prior to making the call, the subjects were led to believe that their partners were either attractive or plain. The women did not know about this assignment, and in fact both men and women were assigned to the two conditions of the experiment at random, irrespective of appearance and personality. One group of judges formed impressions of the men's personalities after listening to their side of the conversation. Another group, who listened only to the female partners, rated the women. Men who thought that their partners were attractive behaved in a more outgoing, sociable, and warm manner than those who thought that their partners were plain. Further, those women thought by their partners to be attractive behaved in a more poised, sociable, gregarious, and self-confident manner than those who were thought to be plain. The men's beliefs concerning their partners led them to act in a manner congruent with these expectations. Their behavior, in turn, elicited behavioral re-

sponses *from the women* that were also congruent with those expectations. This phenomenon is known as the behavioral confirmation effect. Other research shows that even when the target's behavioral responses are relatively neutral, the actor may still *interpret* his or her partner's behavior as congruent with expectations—a phenomenon known as perceptual confirmation.

If such interactions are allowed to continue over several exchanges, it is possible to observe amplification of the initial effects (Snyder and Swann, 1978). In one recent investigation a number of distressed couples, unhappy with their marriages and seeking counseling, were videotaped as they tried to resolve a marital problem (Gottman et al., 1977). These couples were likely, even early in the process, to get into a cross-complaining "loop" in which each person's statement of a problem was met with the partner's statement of a second problem, quickly degenerating into an exchange of negative affect. In contrast, nondistressed couples began with a validation sequence in which one partner's statement of a problem was met with the other's acknowledgment and agreement that the problem exists. Other studies have shown that the distressed partners' negative views of each other and pessimism about the future of their relationship contrast with the positive views and optimism of normal couples. Given the kinds of behavioral and perceptual confirmation effects just described, it is plausible to conclude that distressed and nondistressed couples alike exhibit the operation of self-fulfilling prophecies.

In another example, the interactions of aggressive boys and their mothers were recorded before, during, and after a course of family therapy (Patterson, 1974, 1977). Before treatment, noxious behaviors on the part of the child elicited noxious behaviors from his mother, and so on in a sort of vicious cycle. Family therapy broke this cycle, so that the mother showed less tendency to respond in kind. The result was a diminution in noxious behavior on the part of the boys during treatment, a status that was maintained upon follow-up. While these examples involve negative behaviors, other studies document amplification effects with positive behaviors.

Another perspective on interaction processes is provided by research on the early stages of children's friendships (Corsaro, 1985). In a recent observational study, children's interactions in school and on the playground were sampled systematically, recorded on videotape, and analyzed in considerable detail. The research highlighted a set of "access rituals" used by children to insinuate themselves into ongoing play activities from which they have been excluded. For example, a child might initially feign indifference to the activity, but gradually move closer,

strategically retreating when challenged, until either firmly rebuffed or eventually incorporated into the group.

Research has also explored interaction processes in small groups involved in work and decision making. For several decades researchers studying group behavior in these contexts have focused on the products of group process, and on the relation between the distributions of individual initial opinions and dispositions (preferences) and the ultimate decision rendered by the group (equilibrium). Now there is an increasing flow of research on the actual interactions among individuals and factions within the group, the nature of the communications and events that affect individual positions in discussion, and the expression of individual differences. There has also been a clear shift from stochastic or game-theory models that predict the outcome of interactions to computer simulations that predict individual behavior during them. This historical situation is somewhat analogous to the use of individual introspective reports in cognitive psychology. Until theories developed to the point where detailed data representing complex mental events was relevant, the status of introspective reports was uncertain and they played only a peripheral role in the research enterprise. For similar reasons, rich records of group discussion were collected for many decades but underutilized until, as at the present time, theories of social interaction became advanced enough to guide data analysis and motivate even more extensive collection of such data.

Studies in this tradition often examine the effects of various factors on deliberation by mock juries (Hastie et al., 1983). The jurors (sometimes drawn from actual jury pools rather than student populations) view a complete reenactment of an actual trial. Often the charge to the jury involves choosing between manslaughter or murder, or simple as opposed to aggravated assault, so that complex questions arise concerning the motives and states of the defendant and victim. Rather than focusing on the relations between such variables as the defendant characteristics or case quality and verdict, the newer research analyzes the quality of the deliberation itself, as recorded on videotape and coded by elaborate computerized schemes. This research shows clearly that the quality of deliberation (as opposed to mere outcome) is affected by both group size (six or twelve persons) and decision rule (unanimity or majority). With a majority rule, members of small factions contribute less to discussion, and larger factions attract new members more quickly. Other research explores the expression of individual differences on group interaction (Cowan et al., 1984). In capital cases, for example, juries selected to eliminate members who are opposed to the death penalty are less robust and discerning in their deliberations. In other contexts it has been

shown that the outcome of a group discussion is determined by those members who frequently shift their opinions, rather than by those with more extreme initial positions.

Such findings focus attention on the dynamic interactions among group members rather than on dependencies between pairs of variables measured at only two points in time. Something similar may be expected to occur at the dyadic level as well. These findings have led to the development of more complete theoretical accounts of the dynamics of interaction in groups. They have also been cited by legal policymakers concerned with constitutional guarantees of due process and the selection of representative juries. In this way, research on group decision making exemplifies the theoretical and practical contributions to be made by the emerging style of research on social interaction.

Research on small group processes directly addresses a theoretical issue that is important throughout the social sciences: how to combine psychological principles describing the thoughts and behavior of individuals with social principles describing group dynamics in order to predict both the aggregate behavior of groups and the contributions of their individual members. Economic theory accomplishes this generalization by using mathematical techniques involving linear equations, based on a model of the individual as a rational maximizer of outcomes. From a psychological or sociological point of view, however, such models fail to recognize and deal with a number of important problems. For example, individual values are labile and highly dependent on the manner in which the decision is framed; capacity limitations on the human information-processing system prevent thorough consideration of all the data available to the individual; and predictions and judgments follow shortcut strategies rather than the normative laws of probability. Several realistic psychological models of the individual have been developed, but their extension to aggregate group behavior has proved mathematically intractable. Recently, computer simulation has been used to realistically model individual behavior, social constraints, and interaction processes, yielding predictions that can be tested in formal experiments (Hastie et al., 1983; Latane and Wolf, 1981; Tanford and Penrod, 1984). These techniques, when further developed, may radically alter existing theories of economic and political behavior, permitting them to take explicit account of principles discovered by psychological, sociological, and anthropological research.

### *Development of Relationships*

As the course of interaction is studied over longer periods of time, we observe the processes involved in relationship development (Kelley et



al., 1983). The nature of the changes in the intrapersonal and interpersonal processes that accompany the emergence of an enduring relationship are presaged in laboratory studies of interaction between strangers who expect that their initial encounters will be followed by more lengthy ones. Previous research, in situations where interactions between subjects were casual and without long-term consequences, showed a tendency for people to rely on stereotypes and first impressions. But even the *possibility* of future interaction has dramatic effects on individual behavior before the first contact takes place, and as the dyadic interaction begins (Berscheid et al., 1976; Darley and Berscheid, 1967; Erber and Fiske, 1984; Gruder, 1971; Marlowe et al., 1966; Srull and Brand, 1985).

A major effect of anticipating future interactions appears to be a heightened attention to one's partner and the development of a more individuated impression of that person. For example, a recent study shows that a person anticipating future interaction with someone else pays more heed to information inconsistent with his or her preconceptions about that other and tends to process that information for its implications about the other's traits and attitudes. In contrast to tendencies to discount such information, as in the intrapersonal processes supporting behavioral confirmation effects, the person who anticipates a future relationship frequently relies on surprising information to revise his or her initial impressions.

The attitudes and other characteristics of one's partner are exceedingly important in long-term relationships. In contrast to brief encounters, close relationships occasion much cognitive and affective activity, and they magnify the effects of the positive or negative events that take place during an interaction. The motivational effects of anticipated future interaction are generally positive, with heightened liking of, commitment to, and cooperation with future partners, in contrast to the more neutral or even self-serving orientation found in short-term relationships. However, this effect is sharply reversed if the future partner is understood to be self-centered or exploitative. Both findings seem to reflect the self-fulfilling and amplification processes discussed earlier.

One important feature of the development of a relationship is that the motivation for the relationship is transformed (Clark and Mills, 1979; Kelley and Thibaut, 1978). In the laboratory, the possibility of a future with the partner changes the format of interaction regarded as most appropriate from one of simple "social exchange," in which a benefit is given in response to receiving a benefit from the partner, to one of "social responsiveness," in which the benefits given take into account the perceived needs of the partner. The details of the interaction process under these two circumstances have not yet been fully explored,

but it seems clear that people distinguish between the two kinds of relationships and know how to express their responsiveness to each other's needs. It also appears that those who treat their personal relationships strictly in terms of exchange place them at risk.

The shifts in motivation with relationship development are captured in recent theories that systematically analyze the initial patterns of incentives, spelling out the nature of the interdependence, and the formal transformations that may be performed on both. Such theories combine elementary exchange principles adapted from economic analysis with more complex responsiveness concepts that reflect human beings' abilities to regulate their behavior out of consideration of its effects on other persons, and on interactions in even the distant future.

The simple social exchange concepts have been successful in generating principles found to apply across various types of relationships, such as couples, roommates, and coworkers. In both longitudinal and retrospective studies, the relative dependence of two persons has been found to predict such interaction phenomena as jealousy, influence, abandonment, and stability (Hill et al., 1976; Peplau, 1979). Thus, we now know how certain gross structural properties of relationships are related to general features of the interaction. However, information is lacking about the detailed processes and significant episodes that, on the one hand, underlie these aggregate effects and, on the other hand, account for the many exceptions to the general trends.

A good example of the transformation of motivation is the recalibration of outcomes one derives from interaction through comparison with those derived by others. Theory based on this comparison process has led to a distinction between satisfaction with a relationship and whether one remains in it or leaves (Russbult, 1980). There is good empirical support for this distinction, and evidence is accumulating about its associated intrapersonal and interpersonal processes—for example, about the contrasting scenarios of interaction when group members are both satisfied and stable within the group versus when they are dissatisfied but unable or unwilling to leave it. On this point, some of the most informative recent process analyses are those, mentioned above, that have compared distressed and nondistressed couples.

A full understanding of evolving relationships in the real world must consider the effects on interpersonal processes of the extended time frame in which the relationship is embedded. Some hints of the changing nature and quality of social interactions over long periods of time is provided by research on shifts in levels of satisfaction over the course of an enduring marriage or career. For example, marital satisfaction is high early in marriage, decreases with the birth of the first child, reaches its

nadir as the children enter adolescence, and increases as children leave the household. Similarly, work satisfaction increases until approximately age 40, levels off through the mid-50s, and rises again thereafter. While there are a number of competing explanations for these statistical trends, the empirical data indicate that investigation of spousal, parent-child, coworker, and worker-supervisor relations at different phases in the lifecycle is essential to our understanding of social interaction processes as they evolve over long periods of time.

### *The Individual in an Interaction Context*

The principles governing social interaction are not independent of the personal attributes of the individual participants. Thus, questions of personality and other internal structures and processes, both stable and transient, are highly relevant to any effort to reveal the general principles of social interaction. For example, it is generally agreed that social interactions are cognitively mediated, guided by the person's perception of the current situation, memories of relevant past interactions, judgments of causal relations, and other inferences (Fiske and Taylor, 1984; Hastie, 1983; Hastie et al., 1985; Schneider et al., 1979; Wyer and Srull, 1985). Other, less cognitive internal factors, such as emotion and motivation, are also of crucial importance (Clark and Fiske, 1982; Leventhal, 1984; Showers and Cantor, 1985). For example, happiness and other positive moods facilitate the processing of positive information about a person, and the remembering of positive events from the past, and increase the likelihood of engaging in cooperative or altruistic behavior.

Research on social cognitive processes has kept pace with developments in research and theory in the areas of nonsocial perception, memory, judgment, and inference. Similarly, research on affective processes draws heavily on current psychophysiological concepts and methods. In both domains, it is now possible to examine directly the impact of cognitive and affective processes on aspects of social interaction—as well as the influence of social factors on cognitive and affective states. The influence of interaction goals and other motivational states—whether people are oriented toward competition or cooperation, exchange or responsiveness, etc.—are also relevant and are receiving increasing attention in the literature. In this way, the study of social interaction retains its historical role of promoting the integration of the trilogy of mind—cognition, emotion, and motivation—into a comprehensive view of individual and social behavior.

The importance of intraindividual structures and processes is also seen in research on the effect of individual differences in personality on

social interaction (Snyder and Ickes, 1985). Although some investigators continue to employ traditional trait concepts, more recent work has introduced new individual difference dimensions that are derived directly from the analysis of social interaction, and of the cognitive structures and processes that mediate it (Cantor and Kihlstrom, 1985; Mischel, 1983). For example, a person's impression of him- or herself is an important determinant of social interaction, and a great deal of recent research has been devoted to analyzing the structure and function of the self-concept (Suls, 1982; Suls and Greenwald, 1983). Many personal decisions appear to be guided by the match between the actor's self-concept and the concept he or she possesses of other people who have chosen a particular option (Niedenthal et al., 1985). Moreover, people who have formed strong impressions of themselves on some personality dimension are highly resistant to contrary feedback from the social environment. They may also initiate social interactions, or modify ongoing ones, in order to reestablish a satisfactory self-presentation. Just as people differ in terms of those aspects of their personalities that they view as particularly important, so they also differ in terms of the attributes emphasized when forming impressions of other people (Bargh, 1983; Markus and Sentis, 1983). In this way, individuals given identical sets of information about someone can form quite different impressions of his or her personality; these impressions, in turn, can lead the individuals into quite different social interactions with that person.

Work on the self-fulfilling prophecy shows the manner in which features of personality, at least in terms of how the person appears to outside observers, develops and changes over time through social interaction. Rather than being preordained and static, personality is, to a considerable degree, constructed and reconstructed through relations with others (Cantor and Kihlstrom, 1985). This phenomenon is clearly shown in recent studies of aggressive boys (Dodge, 1980; Dodge and Frame, 1983). This work identifies a process in which these youngsters expect aggression from their peers, and accordingly interpret even ambiguous provocations in terms of hostile intention rather than as accidental occurrences. This leads them to retaliate aggressively. This reaction both confirms their social reputations for aggressiveness and elicits peer counterreactions that further strengthen the boys' beliefs about the hostility of their peers.

Somewhat different processes have been identified as generating and sustaining boys' hostile behavior within their families. In one recurrent scenario, the child's demanding and hostile reactions result in compliant responses from other family members which, in turn, cause the child to desist. It appears that this coercive cycle derives its strength from the

fact that both actors are rewarded for their respective roles in the interaction, the boy benefiting from the family's compliance and they from his momentary cessation of demanding, hostile behavior. However, this research does not yet allow us to describe how these cycles come into being in the first place. An important advance along these lines would be longitudinal research extending over a considerable time span, permitting the separation of intrapersonal and interpersonal factors, and analysis of amplification effects.

However, the individual's preexisting self-concept sets limits on interaction. For example, actors may not behave in a manner corresponding to their intentions, if those behaviors are not in their repertoire or are inconsistent with their own self-concepts. Moreover, targets do not always behave in a manner that is congruent with the way in which they have been labeled by actors (Swann, 1983). When the self-fulfilling prophecy is strongly incongruent with the target's self-concept, the initiatives of the actor may elicit a reaction from the target that attempts to shape the actor's impressions—what is known as strategic self-presentation (Jones and Pittman, 1982). Through a sequence known as self-verification, the target can lead the actor to behave in a manner that is consistent with the target's self-concept, rather than the actor's preconceptions. Finally, the targets' beliefs about their own behavior are important. If they believe that their behavior is the product of factors unique to the current situation, they are unlikely to display it in another, more neutral, setting. On the other hand, those who believe that their behavior reflects their personality may revise their self-concepts and be more likely to display the new behavior. In this way, the person may acquire a new, stable feature of personality.

It is a truism that the motives and preferences of individuals are shaped to a great degree by their interactions with others. We are learning much more about the conditions under which the internalization of such motives and preferences takes place. For example, compliance under conditions of external reward may be simply a response to available incentives. However, it has been shown in a remarkable series of experiments that that provision of rewards for performing an initially attractive activity seems to rob the activity of some of its intrinsic interest (Lepper et al., 1973). This was clearly demonstrated in a classic experiment in which children showed a decline in their tendency to play with magic markers in a free-play period that took place some time after they had been rewarded for playing with them. Subsequent experiments have shown that this decline in intrinsic motivation does not occur if the reward is given as a sign of competence or excellent performance; in fact, intrinsic motivation can be enhanced under these circumstances

(Harackiewicz et al., 1985). The effects of reward structure on intrinsic motivation are also mediated by individual differences in achievement and competence motivation.

A particularly important feature of human social interaction is that the participants intuitively understand many of the principles governing the process and, as a consequence, are capable of manipulating it for their private purposes. Research of the last two decades has revealed the skills that naive persons show in presenting their personalities to other people (Jones and Pittman, 1982). Whereas some forms of self-presentation are directed toward the maintenance of socially delivered outcomes, others have more to do with the individual's attempt to construct an identity or inculcate an accurate impression of him- or herself in the mind of another person. A number of experiments have shown that people can play an active role in shaping the information about themselves that can be derived from their social interactions. For example, if their self-image as competent people is threatened, they will try to protect it by handicapping themselves in ways made available by the environment (Berglass and Jones, 1978). These methods include drug usage, avoiding opportunities to prepare, or withholding effort. In this way, they have an excuse for poor performance if it occurs, and gain credit from good performance if that occurs. In the case of strategic self-presentation, in which the personality presented to others is not in line with the person's self-concept, the attempt at social deception sometimes can be betrayed by subtle features of facial expression. A major line of research in social interaction focuses on the microprocesses through which information is communicated nonverbally, and sometimes unconsciously, about one's personality (Ekman and Friesen, 1983).

### *Social Structure and Social Interaction*

Just as the character of a social interaction is determined in part by the properties of the individual participants in it and by their internal cognitive, motivational, and affective processes, so is it shaped by a context provided by institutional, societal, and cultural structures and processes (Smelser and Smelser, 1970). Personality and socioeconomic factors initially serve to constrain the development of social relationships and the course of interactions within those relationships by bringing together or keeping apart particular kinds of people, and by providing differential resources to them. Moreover, institutional, societal, and cultural structures condition the maintenance and change of interactions as well as their origins.

One insight provided by the study of social interaction is that social structures organize interactions in a manner that serves to reproduce those structures (Berger et al., 1980). This process may be illustrated by experiments that bring together strangers who visibly differ with respect to status characteristics—attributes such as race, age, sex, social class, and language use—which are differentially valued in society at large. These individuals are then given a problem to solve, a task to which these status characteristics are objectively irrelevant. One finding is that, in the absence of information about one another with respect to task-relevant skills and knowledge, persons with high status characteristics are *assumed* by all participants to have more of the required skills than low-status persons. As a consequence, they are allowed to initiate more interactions; they are deferred to more often in the course of the interaction; and their solutions are more readily accepted by the group. In short, a status ordering is produced within the group that reproduces the ordering that exists in the external social order, somewhat along the lines of the self-fulfilling prophecy.

Whereas it has long been clear that the broader context of social structure affects the course and nature of social interactions in a variety of ways, it is less clear how the various norms and rules of social life develop out of social interactions and help to stabilize them. A fascinating insight into this process was provided by an experiment with pairs of schoolchildren (Thibaut and Faucheux, 1965). The children participated in a series of bargaining games in which one subject had more power over the other but the low-power member had an attractive alternative to remaining in the bargaining relationship. Under these particular conditions, where the high-power person is interested in the loyalty of his or her partner and the partner is interested in equity, the pair members tended to form binding contracts so that the game could be played subsequently under conditions agreeable to both. Contractual activity was much less intense when the power was more equal and the outside alternative was less attractive. This experiment suggests how norms will develop in natural groups when members can exploit each other in various ways. For example, we might expect contracts and other norms protecting both the employer and employee to develop under the combined circumstances of attractive outside offers and high company earnings. Each should be willing to accede to the normative constraints that prevent exploitation by the other.

This research again highlights the contribution that studies of social interaction can make to the development of theory in other areas of social science. For example, economic models of social decision making

assume that individuals attempt to maximize their gains and minimize their losses. Although these models have had some success in predicting aggregate behavior, they are limited by the fact that concepts of "fairness," "norms," and "implied contract" determine what will be considered an acceptable outcome or bargain. While economic theory is not equipped to handle such concepts, they are at the heart of social psychological analyses of bargaining. Research by psychologists and sociologists has identified important rules of the "fairness game," including principles relating to both distributional (who gets what outcomes) and procedural (how allocation is determined) concepts of justice, their development in the individual, and the settings in which the various concepts are applied (Thibaut and Walker, 1975). For example, it has been shown that perceived equity predicts the individual levels of satisfaction and duration in an intimate relationship. As another example, in Western culture adversarial procedures are more acceptable than inquisitorial ones in resolving legal disputes—regardless of whether the actual outcome favors the individual. In a related vein, it appears that people are more likely to base their judgments of fairness on a set of heuristic principles rather than on a rational calculation of gains and losses. For example, people perceive it as less fair to impose losses on people than to eliminate their gains, even when the net outcome is held constant.

The theoretical importance of these findings is that they cannot be derived from rational, economic analyses. Better models of negotiation and bargaining behavior, phenomena of central importance to a democratically organized society, are likely to result from a combination of economic and psychosocial principles. Already these empirical and theoretical principles are being borrowed frequently by researchers and commentators in fields as diverse as economics, law, organizational behavior, labor relations, and marital counseling.

### **Resource Recommendations**

The conceptual and methodological advances outlined above have set the stage for further progress in understanding social interaction processes.

#### ***Priority 1: Expanded Support for Individual Investigators***

We believe that the priority need in this area is for expanded funding of individual research projects. As elsewhere in social science, most of the major advances in our understanding of social interaction processes have



stemmed from the work of individual investigators. There seems no reason to expect that this situation will change in the foreseeable future. We recognize that the natural sciences have developed many centralized institutes for research and development and have focused on the collaborative use of large instruments. However, this does not seem to be the foremost need in the study of social interaction at the present time. In a time of declining federal support for science or of steady-state support in an era of escalating costs, we should not seek to build structures that we cannot maintain, especially at the expense of support for structures on a smaller scale that are still the loci of most important discoveries.

There are at present many individual investigators productively engaged in research on social interaction at a variety of levels, laying the empirical foundations for the important theoretical advances of the future. Some of this research is abstract—relating, for example, to the structure of cognitive representations of social entities, causal attribution, and other judgments, decision making in a social context, and the development and presentation of the self. Other research, pertaining to traditional topics in social psychology and personality such as aggression, altruism, attraction, outgroup prejudice, and other aspects of group dynamics, applies these general principles to specific content domains. Both kinds of research are absolutely essential if the field is to continue to make progress: the abstract work supplies potential reformulations of theories, while the content areas test their generalizability. In this way, we build an empirical basis for further theoretical advance. At current levels of support many promising lines of research are going unfunded.

### *Priority 2: Modernization of Research Facilities*

Continued or increased support for individual research projects will not be enough. It is also necessary that we engage in a modernization of the laboratories and other environments in which research on social interaction takes place. The conceptual and methodological advances illustrated here require a different kind of research than has been common in the past. A great deal of traditional research on interpersonal processes takes place in cubicles that prevent the participants from actually interacting with each other, and checkmarks on questionnaires and rating scales often substitute for actual social behavior. In the past, there were pragmatic reasons for this state of affairs. The available statistics emphasized the identification of simple, unidirectional causal relations, coding of complex interactions was unreliable and cumbersome, and computing power effectively limited the number of cases and variables that could be subjected to analysis. This is no longer true. Innovative

statistical techniques can examine both directions of causation between multiple elements in social interaction; advances in video recording technology permit actual social interactions to be recorded cheaply and preserved indefinitely; random-access audio- and videodisk equipment permits more elegant research designs than were previously possible when stimuli had to be constructed and presented by hand or on film or tape; and the revolution in microelectronics that has put mainframe computing capacity into desktop machines facilitates the application of detailed coding schemes to ongoing social interactions. Most departmental laboratory facilities for the study of social interaction were developed in the 1960s and 1970s, before the advent of the video and computer technology described above. They were not built to support research applying the new concepts and technologies. Therefore, a major need is for funds to support modernization of laboratory facilities, perhaps along the lines of the NSF multiuser computer grants.

Another major need is related to the way in which social interaction theory is coming to be written. The classic theories in personality, social psychology, and sociology were primarily verbal and somewhat nebulous. Beginning in the 1960s, there has been a trend for many social scientists to write their theories in the form of mathematical models or computer simulations. These have the advantage, from the scientific point of view, that they force the theorist to be specific about structural features and procedural details. An operating computer program, representing some aspect of social interaction, is an important research tool because it can derive theoretical implications that will escape the human theorist. However, few social scientists possess the programming expertise that will enable them to implement these simulations themselves. Thus, there is a need for expert programmers who can translate verbal theories into operating computer simulations. Even without increases in the number of research grants awarded, funding levels must increase to accommodate the increased needs for advanced computer hardware and skilled supporting personnel.

### *Priority 3: Support of Collaborative Arrangements*

Although it is difficult to gainsay calls for more research, progress in the field does not depend solely on the funding of individual investigative projects. This is because the sort of research currently emerging in the field, and envisioned in this outlook report, is too complex, and too expensive, for an individual investigator to conduct. The same developments that have outpaced available laboratory facilities have also put

strains on the competencies of individual investigators. In principle, understanding even a mundane social interaction requires knowledge of role relationships, social-cognitive structures and processes, attitude-behavior relations, person-by-situation transactions, group processes, motivational and emotional processes, and the like—in short, work for an entire institute on social relations. Even our best programs rarely produce individuals who have expertise in all of these areas. Sophisticated analysis of social interaction will increasingly demand that investigators collaborate with each other.

Much of this collaboration can take place within the context of established departments of psychology and sociology, or among members of these departments, and the upgrading of multiuser laboratory facilities is expressly intended to foster this activity. However, other kinds of collaboration, crossing campus boundaries, are also necessary. For example, it is desirable to bring individual investigators together to focus on particular theoretical problems, both before research is initiated and after the data have been collected. A few such centers exist at present, but these are primarily geared to sabbaticals that take investigators away from their research sites for extended periods of time. Moreover, they rarely provide continuity for the group members through the life of the problem being studied. An alternative model would permit groups of researchers who share a commitment to a particular problem area (though holding different theoretical or methodological positions) to exchange views on a regular basis. Ideally, these groups would travel a circuit, and have a schedule for rotating membership that will permit the constant infusion of new ideas. Given recent advances in telecommunications and computer technology, it also would be possible to link large numbers of related investigators permanently together to foster the continuous exchange of information.

Just as it is difficult for an individual to keep up with trends in his or her discipline, it is almost impossible for even moderate-sized departments and universities to provide their students with a complete repertoire of investigative tools. Because these techniques evolve so quickly, even recent graduates are likely to be left ignorant of some developments. Without these analytic resources, even the most creative investigator will not be able to translate ideas into results (in fact, this was the situation in social psychology until recently, as conceptualization outstripped methodology). Therefore, there is an acute and growing need for centers devoted to disseminating technical information and analytic skills to both students and established researchers. Again, we envision both permanent facilities, to which students could travel for work-

shops and training institutes, Chautauqua-type traveling courses, as well as workshops sponsored by professional organizations at annual disciplinary meetings.

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## Gender

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The concept of gender is a basic organizing principle for every human culture: "There appears to be no other dichotomy in human experience with as many entities assimilated to it as the distinction between male and female" (Bem, 1981, p. 354). Yet its centrality in human thought and behavior has until recently not been widely recognized, or it has been recognized on a superficial level only. While gender is perhaps the most potent organizing principle of all known human societies, conceptualizations of gender differ from society to society as well as across time within a society. While by gender here we refer primarily to the social distinctions made between female and male, feminine and masculine, it is not so easy to define, as the physiological and social aspects are often intertwined. In gender studies, gender is what is to be studied and understood, hence ultimately defined.

Since gender has been such an important concept to our own as well as to all societies, it has been hard to establish the critical distance necessary for competent research on gender. We have often tended in the past to think that we know all we need to know about the concept of gender, or that what anyone needs to know is obvious. The single most important advance in gender studies is that the existence of gender has been rendered problematic: gender can no longer be treated as a simple "natural" fact. The apparent naturalness of gender is derived from at least two sources: its previously unexamined identity with (anatomical) sexual differences and the existence of social and intellectual conditions that are no longer predominant. The existence of gender studies is in part rooted in the rapid social changes of the last twenty-five years. In turn, gender studies seek, among other things, to explain these changes so that social policy can be more effective in the future.

The fundamental goal of gender studies is to analyze gender and its consequences: how gender is constituted and experienced and how we think about or, equally important, do not think about it; how its existence as a social construct affects the political, social, and emotional lives of both women and men. The study of gender includes but is not limited to what are often considered distinctively feminist issues: the situation of women and the analysis of gender-based inequalities. Although gender has long been seen as a natural attribute of individuals, the newer

scholarship is also concerned with gender as a social construct. Earlier research related to gender tended to look at observed "sex differences" and offered "sex role" explanations for their existence. Contemporary gender researchers and theorists seek to understand not only the particular causes and effects of gender patterns within their respective domains but also the impact of a gender focus on the methods and theories of their disciplines, and indeed on social theory as a whole.

One of the major advances of gender studies in the past ten years has been a rapid increase in the sophistication of the questions asked and in the methods used in research. We can now see how advances in gender studies are dependent on a constant interplay between empirical and theoretical work and between researchers in virtually every field (and not only in the social sciences). Among researchers in gender studies, there is agreement on many of the most important questions, although not necessarily on the answers to them.

These questions would include: What is gender? How is it related to anatomical differences? How is gender constituted and sustained, in one person's lifetime and more generally as a social experience over time? How does gender interact with other key factors affecting social relations such as race or class? Does gender have a history? What causes gender systems to change over time? What are the relations between gender, sexuality, and a sense of identity? Are there only two genders? What are the relations between forms of male dominance and gender? What are the important determinants of differences among men and among women as well as between men and women? To what extent are our current concepts of "objectivity" and science gender-bound or -biased? Are there now and/or could there be gender-neutral methods of research in the social and behavioral sciences? What would these be? Would gender "wither away" in truly egalitarian societies? Is there anything distinctively male or female in modes of thought and social relations? If there is, are these distinctions innate and/or socially constituted? Are gender distinctions socially useful and/or necessary? If so, what are the consequences for the social goal of attaining "gender justice"?

In order to further advance the field of gender studies, our definitions of gender and the methods for studying it must be refined. There is a pressing need for the researchers who do theoretical work and those who do empirical work in this field to be able to discuss and integrate their research. As we have seen in the past, our refined questions about gender and definitions of it have come from considering the results of empirical research. Yet empirical research on gender has shown the limitations of methods and assumptions within existing disciplines. These

limitations restrict our understanding not only of gender but of human (and animal) life as a whole; and they point to the need for more extensive theoretical and methodological work in gender studies, which can then further empirical research in all the social and behavioral sciences.

Out of the dozens of research questions and the many provocative lines of research currently being pursued, we have selected three as examples of research that (1) has had profound consequences for the interpretation of the phenomena under study; and (2) is in a particularly "yeasty" period of development, ready to give tremendous payoff, given the right nourishment. These three fields are primate behavior studies; politics; and work and the family.

### **Primate Behavior Studies**

Recent investigations focusing on females in primate behavior research are striking down some old beliefs in this field and stimulating exciting theoretical developments. The first field studies of nonhuman primates, in the 1960s, underwrote an account of human origins, and hence the supposed genetic bases of human conduct and social arrangements, which can best be understood, in retrospect, as a projection of present-day Western cultural assumptions about gender differences.

#### ***The Baboon Model***

This early account adopted baboon society as the model for that of the first hominids, since hominids, like baboons, were supposed to have undergone the transition from forest to savannah life. Like baboons, it was argued, hominids adapted to the savannah by developing hierarchical social groups in which males cooperated with one another to protect the group and ruled over females and young as part of their aggressive protector role. Although they cooperated in defense against predators, males of the group competed among themselves for sexual access to females, the most dominant being the most successful. Because of their innate aggressiveness as well as their innate propensity to cooperate with one another, early human males were presumed to monopolize hunting when that adaptation was added to the subsistence repertoire. Females, then, because they did not hunt but bore and reared late-maturing dependent children, required male economic support as well as protection; constant female sexual receptivity was thought to be an evolutionary adaptation enabling females to keep provider-mates.

### *The Baboon Model—New Evidence*

Subsequent writing and research, beginning with a now classic paper by Slocum in 1975, have questioned almost every assumption underlying the baboon-hominid comparison (Fedigan, 1982). For example, writers have called into question the justification for singling out one primate species among many as a model for early humans and for choosing this species over others, particularly the chimpanzee, which has been shown to be a close evolutionary relative of humans. The assumption that the habitat in which baboons are found today is that in which they evolved is also attacked. The theory that present-day human behavior carries a genetic inheritance from some baboonlike hominid ancestor requires the further assumption that the rigid, hierarchical, and male-dominated structure supposed to be characteristic of those savannah-dwellers is wholly genetic. However, there is continuing controversy as to how much of the behavior patterns of baboons and other primates is learned rather than genetic, and hence may be variable across ecological niches.

Another assumption, that bipedalism, increased intelligence, language, food sharing, and other distinctively human traits were adaptations to hunting carried out by aggressive males has been countered with speculation about the adaptive consequences of the gathering, child care, and child socialization required of early human females (Tanner, 1981). Some researchers have suggested that hominids did not hunt at all but scavenged instead, a scenario raising the possibility that females could have traveled alone or with their dependent offspring, maintaining themselves and these dependents on gathered and scavenged food without male assistance (Shipman, 1985).

The baboon model was not the only one applied to early humans; other primate species were also assumed to exhibit rigid hierarchies of males competing for sexual access with passive females, whose roles were limited to bearing young. "It was," observed Hrdy (quoted in *New York Times*, 1984), "as if scientists had projected onto primates a mirror image of the social structure of an American corporation or university." The past ten years of fieldwork investigating primate behavior in the wild has documented their tremendous variability across species, as well as their learned variability within species inhabiting different ecologies.

This research, carried out over much longer time spans than the earliest studies and with much more systematic methods, has dispelled a number of assumptions about primate social life that were part of the composite model of primate behavior. Even baboons have proven to fit the model badly, not being nearly so hierarchical, male-focused, and

aggressive as early research suggested (Altmann, 1980). In the early model, sexual access to females was seen as the prerogative of high-ranking males<sup>1</sup>; but among baboons and other species, the accumulated evidence is inconclusive as to whether dominance in a male hierarchy predicts an individual's reproductive success (Dunbar and Dunbar, 1977).

In many species, female hierarchies, far from being weak or nonexistent, have proven more stable than those of males; the position of an individual in the male hierarchy may even depend upon the hierarchical position occupied by its mother (Koford, 1963). While earlier models cast competing males as the sexual aggressors, and females in a passive, receptive role, a number of more recent investigations have demonstrated that females exercise sexual choice and influence mating patterns (Dunbar and Dunbar, 1977; Tutin, 1976, 1979; Wolfe, 1984).

### *The Female Role: Sociobiology Revisited*

This new focus on female actors has had a revisionary effect not only on the description of primate mating patterns but also on their theoretical interpretation (Hrdy and Williams, 1983; Wrangham, 1980). The theoretical account that emerged in the 1970s, drawn from sociobiology, revolved around the processes and outcomes of kin selection for males, treating female traits as by-products of selection pressure on males, or at best, as limited and only partially successful responses to competition with male strategies.<sup>2</sup> But the descriptions of female-female competition and alliances, female hierarchies, female solicitation of males, and definite female preferences for particular males, which are emerging from the newer field studies, have occasioned a renewed consideration of selection pressure on females.

Furthermore, new field studies are casting doubt on the central sociobiological assumption of differential parental investment by primate males and females. This assumption says that because females make a large initial investment and have fewer possible offspring, they will rear carefully those offspring they conceive, choosing their mates selectively with regard to both physical fitness and likelihood of parental contribution. Males, on the other hand, are assumed to invest less and compete

<sup>1</sup>As Hrdy (1984) has commented, "a scenario perhaps not unfamiliar to graduate students then and now, but not a particularly apt model for the matrilineal societies baboons live in."

<sup>2</sup>In this respect the sociobiological account was no less biased than the earlier version, which explained human intelligence as a product of selection pressure on males alone.

with other males for reproductive success, and therefore act so as to produce as many offspring as possible, moving on to fertilize other females. Females thus should be sexually cautious, in contrast to males who actively seek additional copulations, and while females should invest heavily in each offspring, males should only do so when in monogamous breeding systems with a high certainty of paternity (Trivers, 1972). As Hrdy (1984) points out, this prediction is hard to square with growing observations such as those of male baboons and macaques, who, in spite of female mating with multiple partners, protected and carried about selected infants, many but not all of whom were probably their own (Altmann, 1980; Packer, 1980; Busse and Hamilton, 1981).

### *Rethinking Parental Investment*

Many other observations anomalous or contradictory to this account of parental investment, for example, of extreme female sexual aggressiveness (Wolfe, 1984) and lack of selectivity in mating in some species (e.g., Lancaster, 1984), are now finding their way into the literature. In this climate, Fedigan (1982) has called for a rethinking of the assumptions behind the model of parental investment itself, questioning, for example, whether investment ought to include courtship costs rather than being calculated only from the moment of conception, and whether primate males really have less limited possibilities to reproduce in their lifetimes than do females.

The more systematic methodology and longitudinal design of recent studies, including systematic observation of females, has led to a rethinking of the application of evolutionary theory to primate behavior. The picture is much more complex than previously supposed. Selection pressures on both males and females must be considered in any explanation of phenomena such as female feeding priority, now reported for some primate species (Pollock, 1979; Jolly, 1984), and the nearly exclusive male parenting found in others (Wright, 1984). Evidence of patterns such as these and other findings, such as the failure to substantiate a breeding advantage for dominant males, have led behavioral primatologists to be more open to the possibility of different levels of selection—individual, kin, and group—operating in interaction (Hrdy, 1984; Fedigan, 1982). This major theoretical qualification of kin selection theory as it applies to nonhuman primates, coupled with the reconsideration of assumptions behind one of its key constructs—parental investment, as discussed above—renders premature and extravagant the initial claims of some sociobiologists and social scientists (notably Wilson, 1975; Van den Berghe, 1979) that the new paradigm from biology will wholly re-

write the sciences of human behavior and human society. Thus, in a striking way, the study of primate behavior illustrates how new research, self-consciously addressing the possibility of male bias and dedicating itself to a more balanced investigation of the sexes, may promote thorough-going theoretical revision in an entire scientific field.

## Politics

### *Women and Politics in American History*

Recent scholarship on the political activities of nineteenth-century American women has provided us with new insight into the cultural determinants of the American political process.

#### *Economic Versus Cultural Determinants*

Prior to the emergence of the field of U.S. women's history, scholars had assumed that women's political activities were primarily shaped by economic considerations. Following the lead of Dexter (1924), they assumed that a rough equality between men and women prevailed in the public domain of the colonial era, as measured by their common law rights and their status in public professions, such as innkeeping, printing, and school teaching.

Sustained by the methods of the new social history and by the questions posed by the reemergence of feminism, and in contrast to earlier assumptions about the primacy of economic factors shaping women's political participation, recent scholars have emphasized the importance of cultural considerations, such as women's increasing literacy, their active voluntary associations, and the growing ideological import of women within family life.

Thus scholars of the Revolutionary era found that the most important change for women lay not in their greater command of economic affairs but in the ideological shift wrought by republican values, which emphasized the importance of a virtuous citizenry and thereby elevated the significance of moral motherhood. Norton (1980) and Kerber (1980) found that changing expectations regarding women's literacy and education, along with the elevation of their status as the mothers of future citizens, were far more important than the temporary economic responsibilities women assumed during the Revolutionary war. These changes established a solid base for the entry of women into the political domain of the early nineteenth century.



### *Women's Organizations and Political Reform*

That entry has been studied from a variety of perspectives in works of biography, collective biography, and community studies, which have demonstrated the significance of women's organizations in effecting political change at the local and national levels long before women could vote (Cott, 1977; Hewitt, 1984; Lerner, 1967; Ryan, 1981; and Sklar, 1973). Lerner (1979), for example, analyzed the correlation between female antislavery societies and the centrality of women with the petitions sent to the 25th Congress, 1837–1838. These petitions precipitated the “gag rule” and escalated the antislavery movement into a struggle for free speech as well.

Bordin (1981) studied the largest voluntary organization in American history, the Women's Christian Temperance Union, showing how it organized on the local level to affect both local and national policies. Lerner (1979) showed that minority women also organized to advance the interests of their communities. In the Progressive era women became even more active in the development of public policy, as can be seen in Sicherman's (1984) study of Alice Hamilton, the person most responsible for the development of industrial medicine. Ware (1981) analyzed the important network of women reformers who sustained and advanced the new public policies of the 1930s. Hall (1979) documented the progress made against lynching by the Association of Southern Women for the Prevention of Lynching in the 1920s and 1930s.

### *Women's Rights—In the Mainstream*

Complementing these studies of women's activities on behalf of social reform are a wide variety of works analyzing feminism and the women's rights movement in the nineteenth and twentieth centuries. Biographies such as those by Griffith (1984) and Moynihan (1983), social histories such as those by DuBois (1978) and Lemons (1973), and intellectual histories such as that by Rosenberg (1982) have given us a far more complex view of feminist political activity, locating it within the mainstream of American social and intellectual life, and demonstrating how women's rights have historically been linked with other types of reform.

### *The Interaction of Race and Class*

The works mentioned above constitute the tip of a very large iceberg of published research on the history of the political activism of American women. Collectively they represent a substantial contribution to our understanding of our democratic heritage. They also point to a new paradigm for analyzing the participation of women within political processes—a paradigm that needs to be developed in future research.

Work done in the past fifteen years suggests that there is a pattern in the ways that gender-related issues connect with other political issues related to race and class. The relative weakness of class-related issues in the American body politic has fostered an environment in which gender frequently serves as a surrogate for those issues, as was the case in the Progressive era when protective labor legislation was initially enacted solely for women workers and later was applied to men as well. On the other hand, the relative strength of race-related issues in American politics has created a climate in which basic human rights have frequently been articulated in terms that can easily be applied to gender-specific rights, as when discrimination on the basis of sex was added to Title VII of the 1964 Civil Rights Act. Gender studies research has illuminated large and new areas of American political history. The field deserves further research that will develop its potential for even greater discoveries about the interaction of gender, class, and race in the American political domain.

### *Women and Politics in American Political Science*

Political scientists in the United States tend to focus on institutionalized forms of political action, especially on the official structures and activities of national governments and on groups that have the most influence on these structures. Because of the empiricist bias in political science methodologies, what gets studied is most often what can be directly observed and counted (e.g., voting) rather than absences or more subtle determinants of power relations. Since women are largely absent as powerholders within the institutions political scientists traditionally study, women and their activities tend to be absent from the work of political scientists.

### *Early Research on Women's Political Behavior*

The initial research on "women and politics" focused on influences on women's political behavior and the possible determinants of this behavior. Scholars argued about the relative importance of socialization, roles, and situational facts in determining this behavior and pointed out the gender bias in existing literature on political behavior (Githens and Prestage, 1977; Jaquette, 1976). New research was also undertaken on women as decision makers in official governmental institutions (Diamond, 1977; Kirkpatrick, 1974). As in other fields, early gender study researchers tended to utilize the existing methodologies and assumptions of mainstream political science. They tried to extend preexisting concepts and methodologies to the study of women's political behavior.

### *The Limitations of the Public/Private Distinction*

As in other disciplines, it soon became evident that these mainstream concepts and methodologies were themselves gender-bound and gender-biased, and unable to account for much of what is significant in the political world as a whole. Scholars in gender studies began to reconsider the existing definitions of politics and the presumption in political science that there exist two separate, independent domains: the public and the private. Okin (1979), for example, demonstrated how this presumptive but false dichotomy has structured the entire history of Western scholarship on politics, removing much of women's activities from scrutiny and limiting the consideration of political activity.

Much of the most exciting recent research in gender studies has focused on reconceptualizing definitions of politics and the relationship between the public and the private (Hartsock, 1983; Pitkin, 1981; Sapiro, 1984). What is beginning to emerge is a more complex understanding of the ways in which gender identities, family roles, and anxieties about these enter into and form part of the substance of "public behavior."

Luker (1984) has shown how women's feelings about gender enter into their decision to participate in pro- or antiabortion pressure groups. Klein (1984) has analyzed the more subtle effects of gender on voting behavior and why and when gender emerges as an overt factor in political life as a whole. Ferguson (1983) has begun to explore the interconnections between typically masculine senses of power and the structure of modern bureaucracies and their policymaking. Flax (1983) has drawn connections between changes in family structure, gender relations, and the emergence of "the family" as a political issue. Possible connections between gender relations and the pervasiveness of violence in politics have also begun to be explored (Hartsock, 1983; Pitkin, 1984).

The most important and promising areas of further research include:

- Cross-cultural studies of the determinants of women's status and the salience of gender to political life. Especially important here are how anxieties about gender relations affect public policy formation in different societies and how access to different resources affects women's chances to exert power.
- Women and development. Not enough research has been done on who benefits from "modernization," whether concepts of and practices related to development are gender-bound or -biased and whether concepts of sociopolitical equality or progress are culturally and gender-biased. There is some evidence that failure to pay attention to gender relations may help to account for the failure of

development efforts in parts of the Third World and the continuing impoverishment of women throughout the world (Beneria and Sen, 1982).

- What "counts" as politics and why and possible gender bias in how political scientists count. The failure of mainstream political science to predict or explain why feminism reemerged as a political movement in the late 1960s suggests that better and less biased methodologies would lead to more accurate knowledge about the political world.
- The complex interactions of race, class, and gender as determinants in the distribution of political access, power, and rewards are an especially important, underresearched area of investigation. The persistence of continuing asymmetries in the distribution of power and rewards within the United States and cross-culturally has not been adequately researched or accounted for.

### **Work and the Family**

One of the least understood phenomena in all of the social sciences is the differential pattern of men's and women's participation in the work forces of Western (and Westernized) countries. The enigma revolves around the oft-observed gap between the wages earned by the two and the related fact of women's segregation from men into different, lower-paying occupations and jobs within occupations. Despite the obvious social and economic implications of this persistent pattern, and the considerable theoretical challenge it poses to traditional social scientific models of the labor market, existing explanations for it neither find general acceptance in the research community nor adequately account for the available data. The problem and the research needs it engenders have undergone redefinition within the broad enterprise of gender studies.

To gauge the research problem one must appreciate the extent, stability, and interrelatedness of the phenomenon. Since data have been available for the United States, women's average earnings have remained remarkably constant at about three-fifths of men's for full-time, year-round workers. That the gender wage gap persists must be attributed largely to the sexual segregation of labor. The issue of comparable worth, increasingly disputed in the courts and at the bargaining table, is one response to the growing perception of how closely linked are

women's lower wages to their work force segregation. And like the wage gap itself, sex segregation across occupations has been a remarkably stable phenomenon; it has not changed much since at least the turn of the century, even though particular occupations have come and gone and changed in their composition from predominantly male to predominantly female and vice versa (Reskin and Hartmann, 1986). This can be seen even from the gross occupational categories of 1980 census data, which reveal that out of 503 occupational categories, 275 are composed of at least 80 percent female or male workers; and about half of all women and men workers are employed in them.

The gender wage gap takes on new visibility at a time when white women's sharply higher entrance into the labor force has increased the numbers of women workers to 49 million, or 43 percent of the total work force in 1983. The pattern of women's labor force participation is converging with that of men. Women spend many more years in the labor force than before, many more women occupy full-time jobs, and many more women work uninterrupted, in spite of motherhood, throughout their employment careers, at the same time that the labor force participation of men has fallen slightly (Reskin and Hartmann, 1986). A steady rise in the number of female-headed households and recent demonstrations that women and their child dependents compose the vast proportion of those living in poverty heighten the meaning of this conjunction of wage gap and social reality (Kamerman, 1984; Zinn and Sarri, 1984).

### *Human Capital Theory*

Existing explanations for the wage gap and job segregation are notable for being framed entirely in terms of characteristics of women, said to justify their entrance into lower-paying occupations (Reskin and Hartmann, 1986). One such model from economics, known as human capital theory, posits that wages reflect differences in the productivity of workers, and that productivity can be estimated from differences in workers' stock of "human capital"—their education and training, work experience, continuity of work history, effort or commitment, health, and so forth. Since women bring or are thought to bring less of these resources than men to the workplace, they are argued to invest less human capital in their work (Treiman and Hartmann, 1981). The most detailed version of this theory, formulated by Mincer and Polachek (1974), hypothesizes that women, in the anticipation that they will interrupt their employment to bring up a family, make different occupational choices than men, selecting jobs they can leave and reenter easily.

### *Human Capital Theory—New Evidence*

This hypothetical scenario, heavily laden with cultural assumptions about women's preferences, had little empirical support at the outset; recent research has produced a mounting critique, replete with counter-evidence (see Treiman and Hartmann, 1981; and Reskin and Hartmann, 1986, for extended reviews). Following are some of the major objections:

- That women should seek jobs requiring less training, as human capital theory predicts, does not explain why they cluster in female-dominated jobs, since many male-dominated jobs also require little training (Blau and Jusenius, 1976).
- That women are found in occupations with low wage growth may result, not from their own choice, as Mincer and Polachek (1974) assume, but from their exclusion from other occupations (Marini and Brinton, 1984).
- Studies of women and their work careers find no greater tendency for women in discontinuous careers to be concentrated in heavily female jobs (Corcoran et al., 1984), suggesting that it is not career interruption that causes this concentration.
- A study that examined job mobility of both men and women from jobs atypical of their gender to jobs typical of their gender (and, less commonly, the other way) found that family responsibilities had no effect on the pattern of such movement (Rosenfeld, 1985).
- Women's earnings do not suffer because of intermittence in male-dominated occupations, as human capital theory would predict. The rates at which the earnings of women in predominantly female occupations appreciate with experience have been shown not to differ from those for women in less segregated occupations (England, 1981, 1982). Moreover, the negative effect of time out of the labor force on earnings appears to be no different for male-dominated occupations than for those less male-dominated, and counter to what Mincer and Polachek (1974) assume, women in male-dominated occupations have higher lifetime earnings than women in female-dominated ones, suggesting that it is always economically rational for women, whatever their plans, to stay out of women's work (England, 1981).

### *Socialization Theory*

An alternative explanation for the sex segregation of jobs and the lower wages paid for women's work also seeks to understand this pattern in

terms of choices women themselves make. By this second approach, sex role socialization leads women to prefer certain occupations and the training and education that entrance into these occupations requires. Those objecting to this account of how women come to occupy segregated, lower-paying jobs point out, first of all, that both men and women display considerable movement within the labor force, and a moderate amount of this mobility occurs across sex-typed occupations (Reskin and Hartmann, 1986). More specifically, it is objected, women *have* proved responsive to newly available job opportunities and increased wage rates: for example, in the flood of women into previously male jobs during World War II, the dramatic increase of women clerical workers relative to men from 1880 to 1900, the rapid movement of black women out of domestic service and into clerical work over an equally brief period in this century, and the more recent sharp increases in the proportions of women doctors, lawyers, and coal miners (Reskin and Hartmann, 1986). A different critique points out that women's occupational aspirations are themselves shaped by their expectations about the kinds of occupations accessible to them (Burlin, 1976; Marini and Brinton, 1984; Reskin and Hartmann, 1986; Smith, 1984).

If women's occupational choices and preferences do play a role in explaining their segregation into lower-paying occupations, one comprehensive review of the evidence concludes, it is a limited role (Reskin and Hartmann, 1986). Both the human capital approach and that of sex role socialization can be criticized for "blaming the victim" (Sokoloff, 1980): both overlook the degree to which others influence women's assignment to women's work. One line of accumulating research has investigated the role of socialization agents who make information about various occupations and their entrance requirements differentially available to male and female students, from preschool to vocational training programs (Reskin and Hartmann, 1986). Other research focuses on the behavior of employers who for various reasons refuse to hire women in "men's" jobs and vice versa, or steer prospective applicants into openings for sex-typed jobs. Male-dominated unions similarly have a history of excluding women or, at the very least, ignoring demands for pregnancy leave and other benefits women want, and supporting seniority and other demands that give men the advantage in obtaining and holding jobs in unionized plants and industries (e.g., Berch, 1982; Hartmann, 1976).

Another factor helping to explain women's disadvantage in the labor market may be public policy, whose makers, influenced by familial ideology, decide not to fund daycare centers or other institutional support for working mothers on the grounds that women should stay home with

their young children—even when to do so is not an economic option for these women. And last but not least, analytic attention has focused on husbands, who, by exerting a variety of subtle pressures on their wives, discourage them from job training or employment that would interfere with their regular home duties or persuade them to leave their present jobs in order to move to a new geographic area when these husbands themselves are relocated (e.g., Ferree, 1984).

Of course, these effects of others are reinforced by the institutions that such attitudes have helped to create: the differential job ladders and training programs which firms may have instituted for men's and women's positions; the firm-wide job evaluation systems which may underestimate the training and conditions of women's jobs; the institutionalized requirements of many men's jobs that they work overtime or relocate at the employer's bidding; or the present shortage of institutional child care. Not least among these is the wage gap itself, which may perpetuate a pattern of household decision making in which a husband's occupational opportunities and choices, because they are more critical for household income, come first (Sokoloff, 1980). These structural factors, too, have been the subject of research and debate in gender studies.

The most general point to emerge from this varied and interdisciplinary scholarship on the pattern of women's participation in the work force is that no one model of any single process will do; the causes of the phenomenon are interconnected. Gender studies, joining as they do researchers and scholars from multiple disciplines, have been particularly receptive to multifactor explanations of the kind that often elude the traditional fields of social science. What has especially come into focus is the complex interrelationship between what goes on in the "public" place of work and the "private" place of the family.

### *Interplay of Work and Family*

Historical studies of women's labor force participation have found a close relationship between women's work outside and inside the home. This was dramatically the case in colonial America, when women's entry into professions such as printing, innkeeping, and shopkeeping was facilitated primarily by their activities assisting husbands or fathers, and their continuing in these activities after the death of their male relatives. Other professions, such as schoolteaching and nursing, were extensions of women's domestic responsibilities (Dexter, 1924).

Changes wrought by industrialization and democratic ideals in the first half of the nineteenth century altered women's work inside and



outside the home, but it did not change the close relationship between the two domains. Studies of the feminization of the American teaching profession, which took place during the vast expansion of the occupation with the spread of common schools and the settlement of the West, have found that the values fostering the increasing number of women teachers included the domestic values associated with moral mothering as well as the economic values associated with hiring more teachers for lower salaries.

The close relationship between family and work was dramatically revealed in the history of southern black women after the Civil War. Scholars have noted the dramatic decline in productivity among former slaves, but not until gender was investigated as a category of historical analysis did scholars discover that one important reason for this decline was the fact that black women withdrew from the agricultural labor force in order to put their labor to domestic purposes, including gardening to raise better food for their family's consumption (Gutman, 1976; Jones, 1985).

During the years of heavy industrialization, 1880-1940, women constituted less than one-fifth of the total labor force, but they contributed to their family economics by taking in boarders and lodgers and by performing work in the home. Only 11 percent of married women worked outside the home, but a much higher proportion of young, unmarried women (about 30 percent) participated in the paid labor force. With the rapid expansion of the service and white collar sectors of the economy after 1945, the proportion of married women in the paid labor force has steadily increased until in the mid-1980s women constituted nearly half the paid labor force, most of them being employed in service or clerical jobs. The clerical labor force was feminized between 1870 and 1930, as the number of office jobs rapidly expanded and became more specialized (Davies, 1982). Before 1950, however, minority women were excluded from clerical work, imposing an additional hardship on Afro-American and Mexican-American women, whose employment was often necessary to their family's support (Blackwelder, 1984). This occupational shift to clerical work was accompanied by shifts in the ages of working women, as more married women entered the work force in their mid-thirties and remained there until their mid-fifties (Weiner, 1985).

The recognition of this close relationship between work and the family for women has demanded new social science investigation and analysis. In particular, the newer scholarship on the question of women and work has challenged the traditional social scientific division of labor between those who study the workplace and those who study marriage and the family, a division that parallels the public/private distinction that left

women out of the study of political behavior. This new scholarship argues that the dynamics of neither can be understood without examining the other (e.g., Sokoloff, 1980). Thus, both the human capital approach and the sex role socialization explanation are criticized for taking as given the relationship of domestic arrangements to the occupational choices women are said to make—leaving unanalyzed the cultural assumption that housework and child care naturally fall to women. In fact, the smaller paycheck women bring home may be used to justify the fairness of an arrangement by which they assume most of the housework and child care.

In stressing the interaction between home and workplace factors, gender scholarship has also revealed the interplay of interests that maintain the pattern of women's work force participation, such as the interests of the employers who benefit from paying women less and the husbands who benefit from their wives' household services.

Current research in gender studies has begun to replace a number of myths about the family in relation to work with a series of more accurate and historically well-founded claims. For example, rather than the myth "that families are 'natural,' that is, biological, units whose form can be understood in terms of blood ties and in relative isolation from social forces and institutions" (Rapp et al., 1979, p. 183), the family is a social unit, "always decomposing and recomposing, in continuous interaction with larger domains" (p. 175). Furthermore, "the family" is not a static, monofunctional unit; rather, families are simultaneously engaged in productive, reproductive, and psychodynamic forms of activity, all of which may vary enormously across time, culture, race, class, and gender.

Productive activities refer to the ways in which families acquire, pool, and consume resources necessary to sustain the lives of their members (Treiman and Hartmann, 1981). These activities immediately link the family and the economy. Families may produce goods for sale, engage in subsistence farming, and/or send some of their members out to work for wages. Consumption also requires work and links to the economy (Kreps and Leaper, 1976). In contemporary developed societies this work primarily takes the form of purchasing goods and services outside the home and making them available in usable form to family members. In turn, the resources of the family members (for example, their ability to command wages at higher or lower levels) will have an enormous effect on the family's "private" life, for example, the amount and kinds of goods and services available to it, including food, medical care, schooling and housing.

Counter to the myth "that families are the only areas in which significant emotional contact takes place" (Rapp et al., 1979, p. 183), there has been great variation over time and culture in the expectations about

and nature of affective relations among both family and nonfamily members. The family's reproductive activities again link it with the "outer" social world, particularly in its socialization responsibilities. In carrying out these activities, families enter into complex and historically variable relations with other agents of cultural reproduction, including schools, religious organizations, workplaces, and state institutions. Again, the resources other institutions make available to families to support their reproductive activities and the pressures and expectations brought to bear on families by these reproductive institutions vary historically as well as by race, class, and gender (Kamerman, 1979).

In opposition to the myth that the sexes and generations experience families in the same way, with identical needs and interests, gender studies have emphasized that the family has its own internal politics, in which the concerns of children and wives "are often uncritically absorbed into those of the male 'head of household'" (Rapp et al., 1979, p. 183). Families may be the locus of bonding for survival and support and the locus of relations of dominance, inequality, and violence; members of the same family may even have different standards of living (Ferber, 1982; Oren, 1973). The inequalities in family life may both reflect and contribute to inequalities in the rest of the social world. The availability of work outside the home and the existence of social services such as child care that make labor force participation more possible affect the power relations within the family and the ability of its members to change them (Leader, 1983; Sapiro, 1984; Simms, 1983).

### *Future Directions*

Further research into the relations of gender, families, and work can continue to provide better understandings of the family. More effective social policies may also emerge from more accurate information and theories about these topics.

In an area in which analysis has run ahead of research, literature reviews have also stressed the wide range of questions that research has not yet answered (Sokoloff, 1980; Reskin and Hartmann, 1986). One of the most difficult of these centers on the motivational force and historical persistence of beliefs about gender, as these beliefs apply to both the work world and the domestic world. These cultural beliefs—for instance, that women's place is in the home; that women are best at work, such as nursing and teaching, which draws upon their natural capabilities as wives and mothers; that men are better equipped for work that requires rational thought or the exercise of authority (Reskin and Hartmann, 1986)—have various histories and have undergone different transformations as they have been adapted to the perceived realities of

work and home life. Subject to much analysis and debate in the feminist literature and in some historical documentation (e.g., Easton, 1976; Jordanova, 1980), these beliefs about gender, work, and family have received less attention from researchers who traditionally study attitudes, stereotypes, or belief systems. The research approaches that might most appropriately be brought to bear on beliefs about gender—from cognitive psychology and cognitive anthropology, for example—have not produced a satisfactory theoretical formulation of the link between cognition and motivation that would explain why such a constellation of beliefs engenders goals and behavior (Quinn and Holland, 1987). Thus no theoretical approach is currently adequate to explain the directive force and historical persistence of these beliefs about gender, work, and family. We hope that recent promising work on gender stereotypes within psychology (e.g., Deaux and Lewis, 1984) will be broadened to meet the cross-cultural demands for understanding these links. It is precisely the existence of the field of gender studies that makes such multidisciplinary contributions possible.

### **Important Areas of Inquiry Not Covered in This Report**

We cannot neglect to point out that gender studies constitute a field in which there are numerous areas in ripe stages of development, a situation not uncommon in relatively new fields. In addition to the three "hot" areas described above, the following areas should be mentioned as ones in which interesting empirical findings and/or theory development have combined to make lively subfields of inquiry:

*Human development.* Issues of the mental and social life of infants; gender socialization; new theories of family dynamics (e.g., Chodorow, 1978; Dinnerstein, 1976) and of moral development (Gilligan, 1982), which demand further investigation.

*Cognitive and physiological functioning.* Social and physiological factors in the menstrual cycle and in fertility; social and developmental factors in relation to cognitive abilities—visual-spatial/mathematical and verbal; relation between cognitive abilities, abstract thinking, social activities, and social networks.

*Belief systems and human interaction.* Attitudes, stereotypes, the self system, and gender schemas; the social construction of gender; sex bias, gender differentiation, and power in language and nonverbal communication; links between macro and micro social structures.

*State formation—women and the state.* Broad changes in the condition of women, initiated by the rise of states, the introduction of colonial rule, and modern economic development; women in the military, women and militarism, women and institutions of force (e.g., can women be integrated into any society without wielding a traditional source of power such as army or police force?)

## Research Resources for the Next Decade

### *Major Resources Needed*

The need for "think tanks" in gender studies is the most sorely felt. The multidisciplinary nature of the field makes contacts with scholars of other disciplines both an inspiration and a necessity. Add to this the fact that many gender studies scholars are at teaching institutions and, with writcup funds short in social sciences, have less opportunity to write their research and theory. Having debated many aspects of the need for advanced institutes as well as support for existing ones, we propose the establishment of:

- A *National Institute for Studies of Gender*, to be located in Washington, D.C., where it will have proximity to an international community, government resources such as the Department of Labor Women's Bureau and Library of Congress, other institutes for policy studies, and organizations of minority and Third World women.

Another major need is for a funding unit directed specifically at the field. An example of the burgeoning of research interest is the flood of applicants in 1987 for Rockefeller Foundation grants on "Changing Gender Roles": over 500 proposals received for 13 funded grants. Coupled with the important advances of gender studies research and with recent reports of a serious decline in federal funding for such research in federal agencies (National Council for Research on Women, 1985), this information highlights the need for major research resources in the years ahead. As some agencies have divisions for specific funding needs, we propose the establishment of:

- A *Gender Research Division*, which will review proposals across disciplines and distribute funds for research grants, conferences, etcetera.

A third need is for development (transformation, increased access) of existing large-scale data bases and for some new large-scale data collection. Advances in the research described in political science, economics, sociology, and history in particular necessitate the availability of appropriate data bases. The existing large-scale data bases do not necessarily have gender-related items, and where they do, the items may be unsophisticated in construction. There are a few collections with a specific interest in gender (e.g., the Murray Center at Radcliffe, the collection at the University of North Carolina, Chapel Hill); however, they are often underdeveloped compared to other data base collections. We therefore propose:

- *Development of large data bases and accessibility to them.*

First, existing data bases need development. The needs here are to (1) transform (reexamine and recode) existing data bases to highlight gender issues; (2) computerize data, both quantitative and qualitative (qualitative data will need to be coded and to have raw data put on computer by means of document readers); (3) put data on-line (rather than just on tape) to increase access nationally; (4) increase access through travel grants and visiting scholar programs until all data are readily accessible by computer, and through support for information and access managers at data base centers.

Second, new large-scale data base centers should be established with a specific focus on gender, either within existing centers or at new sites, for example, at the new supercomputers being placed at selected sites around the country.

Third, there needs to be collection of new large-scale data bases that ask the questions that we now know to ask. Data on neglected groups, such as minority women, are most sorely needed.

### *Needed Resources in All Gender Studies Subfields*

Other needed resources are:

- support to existing centers for research on women
- support to existing advanced institutes (e.g., Stanford's Institute for Advanced Behavioral Studies) to foster gender studies research and theorizing, to build a "critical mass" within these institutes
- in-depth working conferences of from ten to twenty scholars for two weeks around a specific question
- summer institutes

- funding to individuals for both data collection and analysis of existing data; special attention should be given to support of in-depth ethnographic studies, collection of discursive data, and other types of rich data gathering that have been neglected in the pursuit of more easily gathered and quantifiable information
- pre- and postdoctoral training grants, especially cross- and multidisciplinary support

### *Resource Needs Specific to the Three "Hot" Areas*

Some of the needs listed above are of special value in particular areas; the resource needs specific to the areas developed in depth in this report are listed below.

#### *Primate Behavior Studies*

One reason this field of study has taken off has been the effective pattern of support it has had in the past. Longitudinal studies are very important in primate study: studies begun ten and fifteen years ago are now paying off in vital new information. To keep up with the burgeoning of theory, this funding pattern needs to be continued and expanded, in the form of:

- sustained funding support for longitudinal studies
- in-depth working conferences bringing together scholars from different disciplines
- multidisciplinary research institutes

#### *Politics*

In this field we are on the verge of disentangling many of the issues involving race, class, and gender. Special resources that will enable us to follow up in these promising analyses include:

- in-depth working conferences
- collection of new data, especially on minority women and politics (here, for example, data must be rescued from being lost as county courthouses crumble and records become less and less accessible)
- comparative studies of political participation and movements

#### *Work and the Family*

Advanced analysis has highlighted many unanswered questions in this field, thus we are at a point to know what the questions are, and simply await the resources to research them. Areas of special need are:

- postdoctoral training in a variety of fields—economics, political science, psychology, anthropology, history, sociology—to prepare researchers for data gathering, analysis, and interpretation, as well as conceptualization and theory construction
- funds for further analysis of existing data bases, particularly for expanded access, as described earlier, to yield more information on women in the work force and the relationships within the family
- collection of additional data bases where needed, especially on minority women
- predoctoral training for comparative and international studies
- funding of collaborative studies between cognitive psychologists, sociologists, and anthropologists

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## Crime and Violence

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## Problem and Scope

The problem of crime has remained at the forefront of national attention for at least the past two decades. Surveys have consistently ranked it among the nation's most serious domestic problems. This has been especially true for the kinds of serious crime that frighten people the most, the kind the Federal Bureau of Investigation (FBI) tabulates at the top of its index of crime (murder, forcible rape, aggravated assault, robbery, and burglary). Few demands the public makes of the social sciences are more insistent than the call for knowledge that can lead to more effective interventions to reduce the rate of these kinds of crime.

These calls have become more focused in recent years with the growing recognition that even though participation in minor crime or juvenile delinquency is fairly common, a small number of "career criminals" or "violent predators" contribute in a grossly disproportionate way to the total crime problem. Research on criminal careers is beginning to identify these individuals, but the developmental processes leading to their criminal involvement are still poorly understood.

Perhaps even more fundamental from a research perspective is the fact that behavior socially defined as criminal continues to provide insights into the content and boundaries of law-abiding behavior. Indeed, since "normal" and "deviant" are at least implicitly defined in contrast to each other, the study of illegal activity is as much a study of why many people do *not* commit crimes.

Because protection against crime is such a fundamental societal process, the study of crime and society's response to crime provides an important opportunity to pursue key issues in social, legal, and political organization—ranging from the way in which families and communities exercise informal social control over their deviant members to the way in which legal institutions and government agencies devise laws, policies, and practices for dealing with a wide variety of criminal activities.

With so broad a scope to consider, and with so difficult and important a problem to pursue, we had to make some difficult choices. Rather than try to assemble a large variety of possibly valuable projects, we chose to identify a single project—a major longitudinal study of individual criminal careers—that could move the field forward in a significant way and within which many other more narrowly focused studies could be



embedded. Aside from the coherence inherent in such a presentation, we believe that the field is ready for such an advance. Such an effort should generate an array of causal hypotheses that are more strongly supported than most that are currently extant; many of these will be expressed as potential interventions intended to diminish individual criminal activity. The validity of these insights or the effectiveness of the interventions they stimulate should then be pursued through the experimental approach because of the scientific strength of the findings that would emerge.

In making these choices, we tried to capitalize on the most promising opportunities offered by recent developments, but we also realized that many other attractive choices would have to be played down as a result. Thus, many important areas that we believe warrant continuing research have been addressed less fully here than we would have liked. For example, family violence is an issue of important social and theoretical concern. We address it here as a potential crime-causation factor during a child's developmental period, but we do not address it adequately as an issue of concern in itself, partly because we believe the causal mechanisms may well be different from those involved in crime between strangers.

A second example is represented by the complex set of institutions charged with dealing with crime—the criminal justice system. There are important theoretical questions surrounding the ways in which its rules, its behavior, and its decisions are developed, and especially the ways in which these change over time in response to changes in public attitudes about crime and punishment, and in response to shifts in resource availability. We have not pursued these opportunities in detail, even though we do point to the crime-reduction role of the criminal justice system through incapacitation, rehabilitation, and general deterrence.

As a third illustration, we have focused on the kind of crime of greatest public concern, for which the richest body of research has already been accumulated. Many other kinds of crime, and particularly white-collar crimes (such as securities fraud, for example), have important theoretical interest because they can illuminate related questions of compliance, especially groups from social classes different from those who are more typically involved in "street crime."

## Background

Crime control has been an area of public policy where positions are frequently influenced more by ideology than by understanding. In part, this results from the strong value content inherent in any views about

crime and punishment. It is also attributable to the dearth of public knowledge regarding the salient factors underlying criminal behavior and the effects of various policy actions—either as prevention or as sanction—intended to diminish criminal activity. Insights just now emerging increase the policy usefulness of the evolving knowledge base for policy purposes.

### *The Problems of Research*

These knowledge problems derive from the fact that the traditional scientific methods of systematic observation and experiment are extremely difficult to use in the study of criminal behavior. Direct systematic observation is rarely possible because of the inherently stealthy nature of most crime, and randomized experiments are frequently precluded because of legal, ethical, and operational problems.

Thus, until recently, even though there had been considerable theorizing about the causes of crime, there had been little well-controlled empirical testing of the theories, partly because of the difficulty of such testing, partly because the constructs within the theories were frequently too broad or too ambiguous to operationalize for testing. Most of the empirical research in the field had involved examination of the correlation (and often only a bivariate correlation) between aggregate crime rates and potential causal explanatory variables. Such correlations were generally high for any of a wide range of indicators of social disadvantage, but since these indicators were all so highly intercorrelated themselves, distinguishing among them was virtually impossible. Also, much better partitioning of aggregate variables was needed to isolate the influence of any causal factor on the different aspects of crime and criminality.

### *Some Recent Developments*

Developments over the past ten to twenty years have led to important advances in measurement and in conceptualization of the research issues.

In the area of measurement, until the early 1970s, virtually the only regular data series on crime was the Uniform Crime Reports, based on reports by police departments to the FBI. The potential and actual sources of error in these statistics generated vigorous challenges to any results based on those data. Since the early 1970s two additional measuring instruments have been developed and refined: self-report surveys of individuals, to learn about their criminal activity; and victimization

surveys, to learn about victims' experiences. Thus, we now have three primary, independent observation methods for observing the elusive phenomenon of crime as viewed by the police (the Uniform Crime Reports), offenders (individual self-report studies), and victims (the National Crime Survey). These three are thus available for mutual validation and calibration, and have generally been found to reflect mutually consistent or reconcilable observations (see, for example, Hindelang et al., 1978). Indeed, a major contribution of the multiple data sources has been to validate many of the inferences drawn from information in the Uniform Crime Reports.

In conceptualization, there has been a shift away from aggregate measures of crime rate and toward an increased focus on individual offenders and their criminal "careers"—the longitudinal sequence of offenses each commits.\* This perspective reflects the inherently longitudinal nature of patterns of offending. It recognizes that an individual's pattern will change in response to his personal life experiences (including treatment he receives from the criminal justice system), to the broader environment in which he finds himself (including his perception of the sanctions imposed by the criminal justice system), and to interactions between these.

Initial estimates of the key dimensions of the criminal career have only recently begun to emerge:

- Lifetime prevalence of arrest among U.S. males is surprisingly high—about 50 percent are arrested for a nontraffic offense sometime in their lives (Belkin et al., 1973); about 25 percent of urban males are arrested for an "index" offense (e.g., Blumstein and Graddy, 1982). Figures for convictions in Great Britain are comparable (e.g., Farrington, 1981). The figures are higher for black males in the United States and even extend to victimization risk: homicide is the single largest cause of death for young black males, whose lifetime homicide risk is about 5 percent in some cities (Barnett et al., 1980).
- Measures of the individual crime frequency of active offenders (in terms of the number of crimes each commits) average on the order of ten index crimes per year (e.g., Peterson and Braiker, 1981; Blumstein and Cohen, 1979), but with extremely broad variation across offenders—most commit only a few, but a small number

\*The literature is summarized in the report of the National Research Council Panel on Research on Criminal Careers, *Criminal Careers and Career Criminals*, Blumstein et al. (1986).

commit crimes at a very high rate (e.g., Chaiken and Chaiken, 1982). Differences between blacks and whites are much smaller in individual crime frequency than in prevalence.

- The average duration of active criminal careers is quite limited, on the order of five to fifteen years, and varies considerably with crime type; mean *residual* career lengths (the average time remaining in a criminal career) appears to increase with age in the twenties (as the marginal offenders drop out) but then decreases with age beginning in the forties (e.g., Blumstein, Cohen, and Hsieh, 1982).

These measurements have been important not only because they have provided some basic factual building blocks for the development of theory but also because they have highlighted the different dimensions of criminal careers (and especially to distinguish prevalence from characteristics of criminal careers of active offenders) and the potential for any causal factor to influence only one of these without necessarily being important to the others.

The next major step in the research must thus focus on the factors that influence these dimensions of the criminal career. In particular, we still lack adequate understanding of the developmental processes that distinguish participants from nonparticipants, those who start early from those who do not begin until they are adults, high-rate offenders from low-rate offenders, and those with long careers from those with short ones. We know too little about interventions that would prevent criminal careers from starting, reduce their intensity, or encourage their early termination.

In recent years, there has also emerged a significant research community prepared to address these issues. That community has a shared consensus regarding the central basic-research questions, has the methodological skills to pursue these questions, and has a clear commitment to emerge with a cumulative knowledge base that is not directed toward one or another ideological policy position. That community is multi- and interdisciplinary. In addition to the sociologists, historians, psychologists, and political scientists who have traditionally been involved in the field, it has benefited from the new perspectives brought by economists (e.g., by a strong emphasis on modeling behavior and by the introduction of concepts of "supply" and "demand" for crime) and by methodologists (statisticians, mathematicians, and operations researchers, with their sophistication in data analysis and in the formulation of stochastic models of crime-committing processes). In all this, the field has developed increasing quantitative sophistication and so has been able to make good use of a large archive of data sets on crime and on individual crim-

inal histories derived from various sources. This has fed a growing scientific orientation of the various disciplinary aspects of criminology. The field is thus ready for a major new thrust to remedy some of the important limitations of current research materials.

### **Need for New Longitudinal Data**

The new data must focus on individuals and the development of their offending patterns over time. Its perspective must be longitudinal to trace the course of development and to isolate the sequences involved in the evolution of a criminal career. Such a research program should provide opportunities for controlled experimental intervention in those careers both to test hypothesized causes and to explore the possibility of various forms of intervention.

The inherently longitudinal and dynamic characteristics of criminal careers have led over the past number of years to a number of longitudinal data collections (e.g., Wolfgang et al., 1972; Elliott et al., 1983; Farrington, 1983; Shannon, 1982). Each of these has been valuable in its own way, but each has been limited, primarily because of the small samples involved and the few instances of serious offending of the type that is of major policy as well as research concern. These earlier studies have provided important theoretical insights into the development of criminal careers and methodological insights into measurement of the phenomena, and so have prepared the way for the next important step—a major effort to pursue a significant new longitudinal study of offending.

Even though the fundamental criminal career phenomenon is inherently longitudinal, its pursuit as a longitudinal study rather than as a sequence of cross-sectional samples is a design choice rather than mandatory. First, drawing and following a single longitudinal sample may well be less expensive than assembling the necessary records on a comparable sequence of multiple cross-sectional panels. Also, the single longitudinal panel avoids the variation that individual heterogeneity introduces into either a single cross-sectional sample or a sequence of cross-sectional waves. Thus, the longitudinal design is probably more efficient in terms of cost per unit reduction of error.

More important, however, are the scientific issues. First, the recovery of life history events for an individual is subject both to considerable memory decay and to errors in the time-sequencing of events (Sudman and Bradburn, 1974). To describe and understand criminal careers, it is essential to recover as many law violations and other life events as possible and to order them precisely in time. Repeated cross-sections with

different individuals must recover information over too long a time for any individual to attain the necessary level of accuracy. Repeated interviews for the same individuals are needed to achieve this level of precision, or even to be feasible.

Second, information on life events and behavioral measures for the same individuals would be obtained from teachers, peers, employers, juvenile and adult justice officers, and others. The difficulties involved here mandate that the effort be pursued for the same individuals at rather frequent intervals.

Third, for at least one of the sites, information about the individuals' group involvements should be studied. Reconstruction of the sociometric networks retrospectively is extremely difficult. The prospective longitudinal design more readily permits the recovery of information on those relationships (Sarnecki, 1982).

Thus, we have converged to a primary strategy of pursuing a prospective longitudinal study with a number of cohorts. That study would offer an arena in which to pursue a broad range of issues, all related to individual patterns of crime and deviance. That range covers the developmental experiences engendering compliance behavior, the influence on subsequent behavior of various forms of experience with the juvenile and criminal justice systems, and the behavioral precursors of subsequent delinquency and criminality. Generally, the project would seek to make detailed measurements of the initiation and termination of individual criminal careers, including a focus on the distinction in those patterns between different kinds of crime, especially between the more and the less serious crimes. With those observations, as well as information on important crime events, the sequential ordering of crimes and various relevant life events (e.g., family conflict, divorce, school failure or dropout, drug initiation, arrest, incarceration, employment, marriage) will begin to suggest directions of respective influence.

Within this large array of potential issues, we want to highlight three that follow from emerging research and warrant special attention:

1. Measurement of the factors influencing the key dimensions of criminal careers, and especially of individual crime rate ( $\lambda$ ).
2. Measurement of the effects of various important life events, and especially including the imposition of sanctions by the criminal justice system, on key milestones in criminal careers, especially career termination.
3. Measurement of the effects of characteristics of the ecological environment—and especially family structure—on the dimensions of individual criminal careers.

### *Influences on Key Criminal Career Dimensions, and Especially the Individual Crime Frequency ( $\lambda$ )*

Research on criminal careers has highlighted the importance of knowing more about individuals' rates of offending (designated as  $\lambda$ ), particularly because of the great skewness in its distribution across offenders (see Chaiken and Chaiken, 1982). Two separate studies (Wish and Johnson, 1985, and Nurco et al., 1984) have found that serious drug users, during the period when they are actively using narcotic drugs, commit nondrug crimes at a rate on the order of one per day, whereas their rate shrinks to about 10 percent of that when they are not actively using drugs. No other comparably strong factor has been identified that accounts for time variation within particular individuals or for variation across individuals during their periods of active offending, or that applies to offenders who are not serious drug abusers.

### *Factors in Terminating Criminal Careers*

Research on criminal careers has highlighted the very high rate of participation in criminal activity by teenage males. Although the participation rate drops as the scope of offenses is narrowed to the most serious, about 15 percent of U.S. urban males are arrested for an index crime by age eighteen (Tracy et al., 1985; Blumstein and Graddy, 1982). This fraction is far greater than is commonly recognized, and suggests that when one adds the individuals participating in crime who are never arrested, offending by teenagers may be behaviorally more common than "deviant."

The majority of these participants discontinue this activity during the late teenage years, and only a few are still actively involved in their mid-twenties (Wolfgang, 1977; Polk et al., 1981; Shannon, 1982). We still know very little about the dynamics of termination, particularly about the relative influence on termination of the variety of life events marking the transition from adolescence to adulthood, such as marriage, employment, joining the military, leaving the neighborhood, and finding new friends. We do not yet know whether one or more of these key events are necessary to bring about the termination, or in what ways societal intervention can stimulate termination directly through punishment or indirectly through its effects on the various forms of transition. Drug treatment, for example, seems to lower the individual's rate of offending appreciably, but it has not been shown to be effective in terminating the career. These issues are important topics to be explored in a longitudinal study and then to be tested experimentally.

### *Effect of Social Environment, Especially Family Structure*

The family is potentially an important instrument of control through socializing young children and through social control of teenagers (when prevalence of offending reaches its peak), and even for mutual social control of the spouses. The respective impact of these various influences is still poorly known, however. In cross-sectional studies, aggregate rates of divorce or single-parent households have been found to be disruptive as ecological variables (perhaps reflecting a greater prevalence in the peer environment of children with diminished social control), but it is not necessarily the case that children in a single-parent household are themselves more likely to be involved in offending. Marital separation seems to be less of an issue than the conflict that generally precedes divorce (McCord, 1982; Rutter, 1981). The clarification of these issues requires individual-level data from a longitudinal study. This is particularly important in view of the growing rates of divorce and out-of-wedlock births.

### *Other Issues*

In addition to these illustrative issues, a large number of findings to date suggest other questions that are best explored through such a design. Examples include the following:

1. We know there is a strong nexus between drugs and crime (see Wish and Johnson, 1986; Gandossy et al., 1980; Chaiken and Chaiken, 1982), but we need to learn to what extent the involvement in drugs precedes or follows the involvement in other crimes.
2. We know that some behavioral precursors in preschool years and early childhood (e.g., lying, stealing) are strongly indicative of involvement in serious crime as a juvenile (Loeber, 1982; Loeber and Dishion, 1983; Rutter and Giller, 1984; Olweus, 1980), but because most children engage in these behaviors at least occasionally we need to know more about whether specific patterns are distinctively associated with a high risk of later offending, and error rates if they are used as predictors.
3. The longitudinal study should examine the nature of the initiation process (who starts at what age and who does not start), the role of groups as a recruiting device, and especially the role of an individual within a group as a predictor of his future involvement.



4. We know that most adult offenders have a record of juvenile offending (e.g., Wolfgang, 1977; Farrington, 1983), but we still need better indicators to identify those juveniles most likely to continue offending into adult years. Factors that have already been found indicative include early onset of the criminal career (Chaiken and Chaiken, 1982), early use of drugs or alcohol (Robins and Wish, 1977), and sibling or parental criminality (Robins et al., 1975; Farrington, 1983).
5. We know that most index offenders engage in an array of crimes (e.g., Wolfgang et al., 1972), but we cannot yet identify the extent to which serious violent criminality is separately predictable and at what point in chronological and career development.
6. We need to determine the effects of different actions by the justice system and how they compare to the effects of nonpenal events (changes in the peer group, marriage, unemployment) on criminal careers.
7. Perhaps most distressing, the differential rates of involvement between the races in serious crimes range from factors of three to as much as ten (for homicide, for example). While race is an important observable variable, it is not theoretically relevant and so it is most important for matters of both theory development and operationally useful public policy that we begin to disentangle the various factors—unemployment experience, family structure, educational levels, involvement in use of drugs—that might reasonably be contributing to these large differences.

### **Considerations in the Design of the Longitudinal Study**

Our main attention focuses on the development of criminal careers from birth through the mid-twenties. This period covers early developmental stages, the onset of offending in the years of middle childhood, the peak age of participation or prevalence in the mid- to late teens, and the termination of criminal activity by a majority of those ever active. In designing longitudinal surveys, a number of key choices have to be made.

The first problem is whether to follow one cohort or several. Since the interest is in the period from birth to age twenty-five, an obvious way to study this would be to follow one cohort from birth. There are important drawbacks to a twenty-five-year study that lead us not to rec-

commend this approach. The most important is that it would take a generation to complete. Thus knowledge from the project would be substantially delayed. It would be at least twelve years before the larger research community could begin to build on what was being learned. Moreover, the original cohort would shrink through attrition and mobility, and aging and period effects within the one cohort would be fully confounded. Finally, as a practical matter, it would be very difficult to secure a funding commitment for such a long term.

A good compromise is to follow a few cohorts for a few years. One possibility would be to follow each of four nonoverlapping cohorts for six years: from birth to age six, age six to age twelve, age twelve to age eighteen, and age eighteen to age twenty-four. Alternatively, it might be more fruitful to allow overlap and to select cohorts on more theoretical grounds. For example, one might go from birth through age eight, when criminal careers begin in measurable numbers; a second from seven through fifteen, the age at which initiation of criminal careers peaks; a third from twelve through twenty, covering most of the juvenile career as well as the transition from the juvenile to the adult justice system; and a fourth from eighteen through twenty-six, by which time most adult careers terminate. These periods would be short enough to be feasible, and long enough to study continuity and developmental change, to ascertain long-term effects of interventions, and to synthesize a full picture of development from birth to age twenty-six. The appropriate length of follow-up for any cohort would depend on the rate of relevant developmental change at any age within the cohort and the expected continuity of relevant behavior. If resources permitted, some of the original cohorts could be followed up further, and some new cohorts could be followed up between the same ages, in order to disentangle aging and period effects.

In deciding what kinds of people to study, the major choice is among (1) a representative sample from the general population; (2) a "high-risk" sample; and (3) a sample of arrested offenders. The main advantage of following up a general population sample is that the results could be generalized widely. However, for the purpose of studying crime and violence, this advantage is outweighed by the fact that representative samples contain too low a "yield" of the types of individuals in whom we are especially interested—the frequent, serious, and violent offenders. Also, because of the importance of illuminating theoretically relevant factors that lie behind the large race differences in prevalence of offending, samples should be stratified to assure full representation of minorities. At younger ages, one way of providing a greater yield of the

persons of interest is to oversample from subgroups of the general population who are most at risk of later becoming offenders.

Because the crime problem is most acute in cities, the project should focus initially on urban areas. Even if criminal behavior patterns in cities are not representative of other places, the greater yield of frequent, serious, and violent offenders justifies whatever bias may be entailed.

It is important that preparatory exploration precede selection of the target cities. One would have to identify cities where cooperation by relevant agencies such as schools is likely, and where the official-record systems are most adequate and available to researchers. Also, since environmental variables in the city can themselves be important influences on criminal careers, it would be desirable to choose cities in which an appropriate range of variation in a few key variables could be covered, such as punitiveness (as measured, say, by sanctions imposed per offense), social class (as measured, say, by education level and population below the poverty line), and social disorganization (as measured, say, by the divorce rate).

Within each of the cities selected, attention should be directed at several local areas. Extensive information should be collected on the characteristics of these areas, community and institutional resources, local schools, and police and court practices. Key persons in the areas, such as police chiefs and other criminal justice personnel, school administrators, social agency representatives, and community leaders, should be interviewed. This will allow the behavior of the subjects to be placed in its relevant social context. Moreover, a locally based project should be able to link up individuals to study patterns of offending by groups.

Data on individual offending and on hypothesized causal events should be collected from a variety of sources. The success of the National Youth Survey (Elliott et al., 1983) in collecting such data from over 1,700 youth in a national cluster sample suggests that it is reasonable to expect cooperation with such a study. Repeated face-to-face interviews are vital, although it may be possible to collect some information by mail questionnaires and by telephone. Data should also be collected from available records and from key informants such as parents, teachers, peers, employers, and criminal justice personnel. Information should be obtained from and about each subject at intervals of not more than one year, although the desirable interval would be shortened during periods when developmental change is particularly rapid. In addition to regular, periodic data collection, special interviews could be triggered by special events such as arrest, marriage, or a large-scale

social change that affects an entire community, such as the closing of a factory.

In addition to data on individual subjects, extensive information will have to be collected from organizations with institutional responsibilities for processing and recording information about the subjects, such as schools, criminal justice agencies, and work organizations.

### **Considerations in the Choice and Implementation of Experimental Interventions**

#### *Possible Experimental Interventions*

The value of a longitudinal study can be enhanced considerably by testing the emerging insights through experimental interventions or quasi-experimental opportunities offered by natural variation. Thus, as results from the longitudinal study reveal the developmental sequences leading to greater or lesser involvement in crime, experiments may be conducted to test in a careful and rigorous way various strategies for affecting those sequences and their consequences. For example, there may be evidence that certain kinds of parental styles foster law-abiding behavior, which in turn may encourage an intervention of "parent training" (e.g., Patterson et al., 1982). A local school system within one of the existing study sites may be willing to work with its students' parents (or with its students who are already parents) to offer parent training, and to do so in an experimental design. Alternatively, some police responses to minor delinquencies might be found to foster future law-abiding behavior. If so, one might, with the cooperation of a local police department, systematically study various police responses to eight- and nine-year-old pranksters and misdemeanants.

It would be desirable to delineate a full schedule of critical intervention experiments, but we cannot yet do that with full confidence. Some potentially attractive interventions can be indicated on the basis of current research. More are certain to emerge from the analysis of the longitudinal study as critical hypotheses are formulated and tested in a preliminary way. The important aspect is that the opportunity be provided for carrying them out as the knowledge develops.

The issues to be explored through experimental interventions might include the following:

1. A variety of programs have been formulated for the teaching of parenting skills, but we must experiment to learn whether and to what extent this can lead to a reduction in subsequent delinquency.
2. Some crime-reduction effects seem to be associated with Head Start programs (Schweinhart and Weikart, 1980), but we still need some careful experiments to study more directly the mechanisms by which preschool enrichment programs reduce subsequent involvement in crime, and to confirm the general finding.
3. We know that incarceration serves to diminish criminal activity through incapacitation (Blumstein, Cohen, and Nagin, 1978), but we need experiments to determine whether the incarceration experience lengthens the criminal career, and thereby simply postpones the criminal activity until release; it may be that the effects reduce crime for some and increase it for others—if so, we have to learn what kinds of individuals fall into each group.
4. We need a broader range of experiments to test the impact of intensive residential programs such as youth service, military service, job corps, and conservation corps on the later criminality of high-risk adolescents.
5. A variety of valuable research opportunities will offer themselves through changes in law. If those occur in jurisdictions containing experiments or longitudinal surveys, then the opportunity to test the effects of the legal change on compliance by the subjects as well as response by the criminal justice system should be anticipated. Recent law changes have included new gun control laws and drunk-driving laws. Court decisions such as *Gault* (1967) or *Chapman* (1981) could well have significant effects on processing by the criminal justice system.

An important emphasis should be placed on relatively early interventions (for example, before the age of ten) that could prevent antisocial behavior well before the child faces serious sanctions by the criminal justice system. Effective day care provided by caring and nurturing staff might help compensate for stressful home environments. Voluntary parental participation offers an additional opportunity to work with the family as a unit and to provide parenting training and other familial resources.

Experiments could be designed based on information about interventions tried in other settings. One might begin by systematically canvassing sites and agencies outside of the study areas for recent innovations

purporting to affect crime. Descriptions about such efforts could then be screened for the most promising prospects. Finally, available quasi-experimental data could be analyzed to obtain some initial indications about program impact. An end result of such a winnowing process might be the identification of several promising programs, which might then be tested more carefully. Some possibly promising results were found in programs originally implemented in shelters for battered women, designed to reach victims' children. It might be possible to obtain evidence that these programs reduced the likelihood that children exposed to violence in the home would in turn become violent as adults. Such positive results would in turn further focus the ongoing longitudinal study and might suggest still further experimental interventions.

It should also be possible to find innovative programs in the cities in which the longitudinal study is being carried out. If the administrators of these programs were prepared to employ strong experimental designs, experiments could then be mounted in their locales, especially if the interventions promised insight into relationships uncovered from analysis of the longitudinal data. There is some preliminary evidence that programs such as Head Start reduce later involvement in crime (Berrueta-Clement et al., 1984). If a community undertakes such a program, it should participate actively in evaluating the program's effectiveness.

There will also be opportunities to "piggyback" onto field experiments designed to test the impact of one or more innovations on outcomes not related to crime. Researchers experimenting with the impact of job training on employment of high school dropouts might well agree to add self-report measures of illegal activity to any follow-up questionnaire (presumably emphasizing posttraining labor market experiences).

### **Organization and Resources**

Our primary proposal is to undertake a major longitudinal study of the evolution of individual criminal careers with the possibility of experimental interventions to clarify critical questions concerning causality and countermeasures. The panel design presents that rare opportunity to test with the same individuals competing explanatory theories and models, thereby simplifying some of the methodological problems that would otherwise arise, and reducing costs in doing so. Such a study is larger in scope and in its demand for diverse disciplinary perspectives than can reasonably or appropriately be expected from a single research investigator. Rather, it is more appropriate that the effort be carried out

by a consortium of individuals from a diversity of disciplines, with varied theoretical perspectives, and with the range of methodological skills necessary to design such an enterprise and analyze the resulting data. The task of data collection and coding would probably be performed by an organization specializing in that activity. This section addresses some of the organizational arrangements necessary to carry out such an endeavor.

### *Research Team*

An effective longitudinal research program requires considerable financial support over perhaps a decade or longer. Such expenditures can be justified when they serve both the collective and the distributive interests of a scientific community. A longitudinal design requires intellectual as well as financial resources that lie beyond the capacities of individual investigators. This pooling of intellectual, financial, and data resources requires collective organization of scientists to plan the longitudinal design. More, however, is required than agreeing upon a design and methods of data collection. Inevitably, individual interests will exceed the capacity of any design to accommodate them. The many tasks of implementing the design (e.g., negotiating with officials at the selected research sites), organizing and staffing data collection, and deciding on timing and form of data release all require the setting of policies and division of labor. These activities are best left to a consortium of knowledgeable and involved researchers. The team must also be responsible for securing funding on behalf of a wider community of scientists.

The research team planning and directing the project must be interdisciplinary in the sense of including all those whose perspectives may contribute to competing explanations of criminality and its causes, and to identifying means of intervention. This requires theorists from all the relevant social sciences. Specialists in design must include experts in modeling and measurement and in data collection techniques. The structure of the consortium overseeing the U.S. Dept. of Education's High School and Beyond project, which is tracking the high school classes of 1972 and 1980, may represent a reasonable model.

The scientific team should include younger as well as more senior investigators. A carefully planned longitudinal inquiry, even if funded initially for a relatively short period of time, can span the residual lifetime of some of its planners and initiators. More fundamentally, a continuing flow of fresh insights and novel research perspectives is essential in any such endeavor.

The research consortium should address the interests of practitioners

as well as scientists. The practitioner community exercises control of the "laboratory," and continually generates policies and practices that must be evaluated and tested to determine their scientific merit and their practical consequences. Effective collaboration between scientists and practitioners is essential to the development of cumulative knowledge. The research team must find ways to learn of the rapidly changing interests of practitioners and ways to accommodate them, at least in the design and testing of interventions.

Oversight of the project must be a continuing responsibility of the consortium. The interventions to be chosen will be based on both interim causal analyses and on the emergence of intervention technologies. The consortium will have to seek out innovations and select for testing those that seem most promising.

### ***Methodological Research***

Methodologies for design and analysis of longitudinal data have grown considerably in recent years. Moreover, any large-scale longitudinal study confronts the problem that social and individual changes will modify the initial concepts and measures. Thus, initial efforts at developing measurement and analytical techniques will be required, and should be continued throughout the project. Various methods for dealing with missing data will have to be developed and tested. Various forms of interviewing (e.g., matching or not matching on various subject characteristics, telephone or face-to-face) can be assessed in terms of efficiency and richness of recall. Various approaches will be pursued for stimulating proper recall or for estimation of the frequency of high-rate events. Such efforts will be important to the study and to the improvement of survey methods generally.

Data that result from the study should be available to all who responsibly seek to utilize them, and so the data tapes should be released at the same time to all who wish to undertake analysis using the data base. In doing so, great care will have to be exercised to protect the privacy of the individuals involved. This has been accomplished with the Department of Education's High School and Beyond longitudinal survey, which is broadly available to the research community.

### **Some Next Steps**

The basic strategy of a longitudinal study with experimental interventions outlined here can make important contributions to basic research on the problems of crime and violence. In doing so, it will offer many



broader opportunities for research on the factors that influence individual behavior and on the relationships between law, individual behavior, and society.

In order to progress toward that objective, we would urge that some next steps be initiated. Appropriate auspices should be found (perhaps the National Academy of Sciences or the National Science Foundation) to convene a planning committee representing a range of interests and disciplinary perspectives to outline and develop a proposal for the conduct of the research program identified here. That planning committee will then be in a good position to address the many detailed issues that this working group necessarily left open.

We believe that such an effort will represent an important opportunity for social science to capitalize on some of the significant developments of the past decade, and to make a major new contribution to alleviate one of society's most pressing problems.

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PART

**III**

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**CHOICE AND  
ALLOCATION**



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# 10

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## Collective Choice Institutions

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During the last forty years a succession of breakthroughs in theory has revealed enormous possibilities of new research on collective choice, with, potentially, far-reaching intellectual and practical effects. Consequently, the opportunity is great for significant discoveries in the 1980s and 1990s.

By collective choice we mean decisions arrived at by the interaction of several persons. Their values, preferences, and beliefs are brought together through organizing institutions (governments, firms, international systems, committees, etcetera) by means of aggregating institutions (such as voting, auctions, markets, wars, and even "the sense of the meeting"). The choice arrived at by aggregation is usually regarded as effective for and binding on the relevant participants in the organizing institution.

The subject matter of the study of collective choice is, thus, behavior within institutions. While the outcome of aggregation in each instance depends in part on the preferences aggregated, we know—from a fundamental theorem, Arrow's possibility theorem—that the outcome also depends in part on the institutions by and through which aggregation is carried out. The intent of the study of collective choice is to generalize about just how much difference these institutions make. We assume, of course, that preferences make a difference. That is, holding institutions constant, different sets of preferences will give different social choices. But what about the other direction? Holding preferences constant, do different institutions make a different social choice? If so, how much and what kind of difference? These are the questions that the study of collective choice is intended to answer.

Collective decisions are impossible without organizing and aggregating institutions. Furthermore, the effectiveness and efficiency of society depend on the nature of its institutions. In an ideal world, therefore, people would adjust their institutions for the best social results. Ours is not an ideal world, and typically institutions with defects are not revised until some notable failure occurs: debilitating inefficiency, stalemate, collapse, revolution.

The explanation of the discrepancy between the ideal and the actual is, of course, that our understanding of institutions is just beginning to



rest on scientific principles. It is true that from classical times right up to the present scholars have described institutions, sometimes in elaborate detail. Unfortunately, until recently, scholars have concentrated on the unique features of particular institutions and have not developed a theory about the operating characteristics of institutions in general. Consequently, we have at best understood institutions as they formerly were, not as they might be. Occasionally, perhaps, we have understood a particular institution well enough to see its defects accurately, but we have not then been able to predict how reforms might work and have hesitated to make adjustments because they might be counterproductive or because we have disagreed about the potential consequences of revision. So while scholars have described at leisure, participants have reformed (with varying results) only in revolutionary crises of confidence.

To repair institutions before they come to disaster, we need to know general principles from which we can infer how newly designed or revised institutions will work. General principles in turn depend on a theory of institutions. The current scientific opportunity is that we seem to be on the verge of developing just such a theory and a method of testing it out.

The confluence of a number of theoretical developments in the 1940s is the basis of that opportunity: (1) Arrow's theorem (1953), which, as already noted, revealed the significance of institutions; (2) Black's (1958) geometric representation of preferences, which in turn led to theorems about the methods and processes of aggregation, for example, the median voter theorem; (3) game theory, which emphasized social interaction, that is, the effect of individuals' anticipation of others' choices on the social outcome; and (4) Samuelson's (1954) notion of public goods, which allowed an interpretation of the failure of an institution—the market—in terms of the objects of individuals' preferences. All these theories shared assumptions about rational individual behavior, namely, that participants choose in a coherent, purposive, maximizing, and sometimes far-sighted fashion, and all involved the expectation of some non-optimal social outcomes in the face of optimal individual choice.

The first flowering, during the 1960s and 1970s, of the confluence of these ideas was social choice theory, which was mainly concerned with (1) elaborating Arrow-like impossibility theorems with new settings and assumptions; (2) searching for new game-theoretic solution concepts for situations of aggregation (e.g., the notions of the core and bargaining sets and of Nash equilibrium); and (3) developing an experimental methodology.

Part of the success of social choice theory came from the suppression of relevant but distracting considerations, treating as exogenous matters that, with deeper analysis, were revealed to be endogenous. The next step in the development of theory is the folding in of the suppressed considerations, some of which are surveyed in the next few paragraphs.

### Strategic Choice

Much of the early work on social choice theory took individual preferences as given, that is, as parameters of a situation. Thus, individuals were taken to be nonstrategic in their behavior; their preferences were taken as fixed, given, and knowable (or honestly revealed by them); and the main task was seen as devising institutions that would "fairly" and accurately combine these individual preferences into a social or collective choice.

An occasional reference to the possibility that individuals might strategically disguise their preferences can be found in the literature of the 1950s. And the early work in economics on the theory of public goods (Samuelson) alluded to the free-rider problem, according to which an individual would understate his true demand for a public good in order to reduce his personal tax burden. But it was not until the publication of Farquharson's *Theory of Voting* (1969) that a systematic inquiry into the phenomenon of *strategic preference revelation* was undertaken. Farquharson distinguished between "sincere" and "sophisticated" preference revelation and, for a class of voting situations, demonstrated that the two forms of revelation need not coincide. That is, in many circumstances, an individual has an incentive to disguise his true feelings. The significance of this discovery resides in the problems it poses for the design of institutions.

Lest it be thought that Farquharson's discovery was idiosyncratic, the 1970s produced a general theorem, independently discovered by the philosopher Allan Gibbard (1973) and the economist Mark Satterthwaite (1975), which established that, in a broad class of social choice situations, it is almost always in somebody's interest to misrepresent his preferences (or, as it is colloquially put, to "manipulate" the social choice process).

The opening of preferences and preference revelation as matters to be theorized about provides an intellectual bridge between the theory of social choice and game theory. This, in turn, permits a considerably deeper understanding of how institutions operate in practice in arenas in which rational behavior occurs. Seasoned participants in markets,

bureaucracies, legislatures, and other social settings are surely not naive and unsophisticated. This new theoretical synthesis, involving strategy and aggregation, is therefore an important development. Begun in the 1970s, it is sufficiently productive that it will likely remain a central theoretical thrust for the rest of the century.

### Incentives

Accepting the pervasiveness of strategic behavior, one response has been to treat it as a constraint on the theory and on design of institutions for group choices. We evaluate institutions on the premise that individuals follow their self-interest, and we seek to design institutions to guide self-interested individual choice into socially desired outcomes. That is, we seek to impose institutional constraints that are compatible with individual incentives. This view integrates principal agent problems, information aggregation issues, and preference revelation. Rather than providing impossibility results, this research has opened up major possibilities for the informed design of institutions. An early example is the discovery by Groves and Ledyard (1977) that although, in general, individuals may have an incentive to understate their valuations of public goods (the conventional wisdom initiated by Samuelson and believed until 1975), it is possible to design the rules (including property rights) such that this incentive disappears and such that socially optimal decisions can be arrived at. The fact is that institutions affect outcomes by changing the incentives of individuals to provide information. Therefore, the design of institutional rules determines both the information on which group decisions will be made and, ultimately, the final social choice. We are just beginning to understand the subtleties of these processes. For example, it is not well understood how individuals behave when confronted with different mechanisms, but it is very important. Game theorists have one view, economists another, and psychologists yet another. Concerted theoretical and experimental research is needed to achieve better models.

### Decision Rules

Another response to the pervasiveness of strategic behavior is to think of social choice as a game of strategy. Since strategic behavior is context-dependent, a fuller explication of and attention to that context is required. Not only is rational strategic behavior dependent upon the stra-

tegic moves of others in the setting but it also depends on a variety of rules governing the social decision process. A master of institutional politics (such as Lyndon Johnson in the U.S. Senate of the 1950s) knows not only what makes other people "tick," he also knows how to exploit the rules of procedure.

The earliest social choice theories were rather sparse in their depiction of institutional decision rules. Typically such theories were characterized by (1) a set of choosers; (2) a set of alternatives from which a choice must be made; and (3) a rule that described how the choices of individuals from this latter set would be "added up" to produce a social choice. Left implicit (if not wholly exogenous) were such issues as

- Who gets to make choices?
- Where did the set of alternatives come from?
- How may the choosers compare and rearrange the alternatives once given?

That is, most real-world institutions possess an organizational structure (not merely an undifferentiated set of choosers but rather a system of committees, institutional leaders, and so on), an agenda-formation process (in which certain privileged individuals, e.g., members of a relevant committee, are empowered to propose alternatives for consideration by the entire group of choosers), and a set of formal procedures by which alternatives, once an agenda is given, are composed. Legislatures, for example, are structurally complex, are characterized by a division-and-specialization-of-labor that provides mechanisms by which alternatives are placed on the institutional agenda, and are governed by complex decision rules governing what motions are in order, who may move them, and so on.

In short, rational strategic behavior is determined by the structure and procedure that characterize a game. The early social choice theorists only dimly perceived this as they sought to understand the rudiments of rational behavior. Their own work provides a firm foundation on which to erect an institutionally richer theory.

As we incorporate into our theory and consideration such basic features as strategies, incentives, and rules of decision, we are becoming able to apply the theory in a variety of socially important and intellectually significant areas. Some of the matters now under investigation are: the nature of cooperation, institutions (e.g., agendas, parliamentary structures, electoral rules), information, and the effect of cognition on

preferences. In the following paragraphs we comment briefly on these matters.

### The Study of Cooperation

The problem of how and why people cooperate has had a long and troubled history, but with the tools of collective choice theory scholars are now beginning to understand it in a deeper and more useful way. Traditional "solutions" have asserted that cooperation is natural (Aristotle), is induced by social structure (Marx) or by norms (Durkheim), or is individually advantageous in the long run (Hobbes, Smith, and other contract theorists). Such explanations fail (1) because they are simply tautological (as in Aristotle's assertion that people cooperate because they are cooperative); (2) because they rely on some exogenous—hence, unexplained—factors (technological change, as in Marx; the internalization of norms, as in Durkheim), or because they explain too much (why isn't, one asks the contract theorists, noncooperation also individually advantageous?). As a result, these traditional explanations tell us nothing about how to predict cooperation or to design institutions that encourage it.

Application of the choice-theoretic perspective led to the important discovery that often self-interested choice leads to an inefficient equilibrium in that all participants are worse off than they would have been had they made cooperative choices. Contrary to the beliefs of Mandeville and Smith, the invisible hand does not always guide self-interested choice to socially optimal results. Since both cooperative and noncooperative behavior occurs, it is necessary to explain how self-interested agents sometimes manage to avoid attaining inefficient social outcomes.

Most of these explanations emphasize that individual choosers, recognizing the private advantages of social cooperation, make contracts to cooperate (as was suggested by the social contract theorists). Although the contract metaphor is a convenient form of discourse, contracts are governed by legal formalities and mechanisms of enforcement; recognition of this is what distinguishes present-day approaches from the previous ones.

The simplest enforcement mechanism is, of course, the police power of the state. Among recent theorists, Olson (1965) has explored this avenue well. But considering the state's police power as an exogenous force is ultimately unsatisfactory because it immediately leads to two critical questions. Where does this power come from? And, who enforces the enforcers? It turns out that there are several endogenous

mechanisms to encourage cooperation and discourage shirking, cheating, and free-riding, and these are the currently promising subjects of investigation.

One subtle feature of cooperation is revealed in situations of *repeated interaction*. Suppose the same persons interact repeatedly in similar situations (e.g., games) where the single-choice equilibrium among self-interested choosers is inefficient. Then, in the supergame composed of repeated choices, the rational choice may well be different from the choice for a single play. Assume that each chooser faces two different opportunities: (1) a one-time-only windfall from noncooperation; and (2) a discounted future payoff from repeated cooperation with its attendant advantages. If the benefits of (2) are estimated to exceed those of (1)—as is typically the case when the single-play equilibrium is inefficient—then these conditions provide a built-in incentive for cooperation (Friedman, 1977; Taylor, 1976; Axelrod, 1984). This incentive underlies observance of the norm of reciprocity, an understanding that one person's initial act of cooperation will encourage others to respond in kind.

In addition to reciprocity, another force for cooperation is *reputation*. Having a reputation for cooperation and for prompt retaliation for noncooperation encourages others to cooperate as well. Repeated interaction allows reputations to enter into individuals' decision calculus about cooperation (Selten, 1978; Kreps and Wilson, 1982a; Kreps et al., 1982).

Still a third force toward cooperation is *uncertainty about retribution* or sanctions. Recalling the eighteenth-century "hedonistic" philosophers Beccaria and Bentham, it may be that variations in the certainty and severity of sanctions have clear effects on cooperation. If there is even a small possibility that retribution will be severe and swift, cooperation may be maintained—the so-called trembling hand equilibrium (Selten, 1978; Kreps and Wilson, 1982b; Milgrom and Roberts, 1982).

If reciprocity, reputation, and uncertainty about sanctions can all lead to cooperative behavior in the absence of policing, why is cooperation so problematic? For one thing, all of these mechanisms depend upon highly stringent assumptions about information. For example, if noncooperative behavior is hard to detect (and thus carries a low risk of retaliation), then the rational agent's optimal strategy may be defection so that, consequently, the resulting equilibrium will be inefficient. A variety of structural and institutional features—ranging all the way from group size to the actual physical arrangements under which interaction takes place—probably affect the spread of information. The investigation of these features will help us to understand how apparently spontaneous cooperation may in fact be the consequence of particular institutions that, formally or informally, provide incentive compatibility.

Hence, although the attainment of cooperation has been a recurrent problem in social thought since antiquity, in the 1970s social scientists have, for the first time, begun to attack it empirically and in careful detail. One consequence may well be an increased efficacy in designing institutions for both large- and small-scale processes of governance.

### Institutions—Agendas

A major theoretical discovery of the 1950s was Black's and Arrow's observation that the outcome of a majoritarian process might depend upon the sequence or path in which options were considered. This is a mathematical property of majority rule and has nothing at all to do with individual psychology. Many researchers attempted to determine the patterns of preference that led to this phenomenon and the degree of arbitrariness that characterized path-dependent outcomes. With respect to patterns of preference, it was shown that, when unconstrained by institutions or special preferences, outcomes were almost always path-dependent. As for the degree of arbitrariness, it was shown that, under plausible assumptions about individual preferences, if the sequence of motions can have any effect at all, then it dominates all other effects completely (McKelvey, 1979). That is, an appropriately chosen sequence of motions can lead a group from any option to any other option, including those options that everyone would recognize as disasters. The logical structure of majority rule is that "anything can happen" if the agenda sequence happens to be appropriate.

The significance of McKelvey's discovery is twofold. First, from a technical point of view, his formal apparatus makes it possible to formulate and answer precise questions about voting models. Since these models are extremely complex, the development of tools to analyze their logical properties constitutes a significant advance. Second, McKelvey's theorem and many related results imply that a knowledge of the voting rule itself is not sufficient to generate an understanding of the behavior of voting bodies. The agenda and related considerations are also critical.

Consequently, the treatment of the agenda must be part of any explanation of social decisions. Since the sources of power in organizations rest on the ability to affect agendas, both constitutions and strategic posturing of "opponent" groups (i.e., rhetorical presentation of issues) must be analyzed in terms of agenda processes. Ultimately the design of procedures must incorporate our knowledge of agendas, and efficient procedures must generate agendas that avoid disasters.

Basic research on agendas has been of three types. First is the much

simplified case where agenda items arise in a known order and the preferences of all participants are known with certainty by everyone. This polar case permits prediction about the operation of voting processes when participants behave strategically without error (see, e.g., Kramer, 1972; Farquharson, 1969). Once that extreme case is understood, it is possible to study cases closer to the real world, cases in which the preferences of others are not completely known and the principles of strategic behavior are not perfectly reliable (Plott and Levine, 1978; Fiorina and Plott, 1978; McKelvey, 1981). These simplified cases provide ideal types against which outcomes in complicated and realistic events can be measured.

The second type of research involves the choice of an agenda (Banks, 1985; Shepsle, 1979; Shepsle and Weingast, 1984; Miller, 1981; McKelvey, 1986). In many social situations, the construction of the agenda is itself part of the decision process, and the research goal is to isolate the principles that govern the evolution of the agenda. Which agenda will be used? Results from the first type of research are important here because the probable consequences of agendas surely affect the attitude of people choosing among agendas. A good example of this second type of research is the case in which a subcommittee uses the terms of its report to affect the agenda used by the whole committee.

The third type of research on agendas is the product of newly developed experimental methods. Thus recent experimental work overwhelmingly demonstrates that an agenda of a particular form, implemented and rigidly followed in situations where individuals are unaware of others' preferences, can be used to control a group decision (Plott and Levine, 1976). Similar experiments suggest that agendas influence the strategic behavior of participants (McKelvey and Ordeshook, 1984). It may also be that agendas can—as a kind of rhetorical stance—affect beliefs, in which case agenda effects may be magnified manyfold.

### **Institutions—Parliamentary Structures**

The study of preference revelation, strategic behavior, and social decision rules has advanced the farthest in the study of parliaments and legislatures. Part of the reason for this undoubtedly is the rich body of empirical detail about legislatures, developed over the last century, against which the spartan structure of social choice theory may be compared. The wide chasm between theory and well-documented reality has served as a spur both to theory development and to empirical testing.



Particular questions that currently may be addressed theoretically include:

- What are the consequences of decentralizing a legislative institution by positioning its members into committees? (Shepsle, 1979; Shepsle and Weingast, 1984)
- How might policy be divided up into jurisdictions, and what effect is to be expected by delimiting jurisdictions in different ways? (Krehbiel, 1984)
- What effects are produced by imposing different rules by which a motion might be amended?
- What consequences flow from dividing a legislature into two bodies (bicameralism)?
- How are legislative rules chosen in the first places, and how are they altered?

The real advantage of fashioning a strategic theory of social choice for legislatures is, first, that legislatures are well-defined institutions with long lives. Second, the empirical world has sufficient diversity to permit statistical (if not quasi-experimental) possibilities for actually testing theoretically inspired hypotheses. Third, the empirical-theoretical link is strengthened by the fact that the theoretical assumptions of rational maximizing behavior and strategic calculations are plausible to empirical scholars. Fourth, legislatures are sufficiently complex, both structurally and procedurally, to require theoretical guidance; a theory is necessary to cut through a very cluttered empirical thicket.

In short, parliaments and legislatures provide an almost ideal setting for theoretical enrichment of social choice ideas; theoretical synthesis of organizational, institutional, and behavioral approaches; and serious empirical testing. Here, as perhaps in no other area, there is the high-probability prospect of theoretical development and cumulative knowledge about the principles of behavior and outcomes in a well-defined setting.

### **Institutions—Electoral Rules**

After a hiatus following the seminal work of Rae (1971), the study of macropolitical electoral institutions has been flourishing, with more than a dozen monographs and edited volumes appearing between 1980 and 1985. While much of this work is still largely descriptive and the analytic

techniques used are rudimentary, an impressive start has been made toward bridging the gap between formal models of social choice and the more traditional analyses of the comparative politics literature. Among the most exciting research is that on the link between the type of electoral system, political support (as measured in votes received by a party's candidates), and legislative seat share. For example, an important early hypothesis, the so-called cube law of seats-votes relationships, has been reformulated in a very general fashion, incorporating factors such as average number of seats being contested per district and the "effective number" of political parties contesting the election (Grofman and Lijphart, 1984). This model permits far more accurate predictions of the probable consequences of changes in election procedures (as in France's shift from and to plurality to a special form of proportional representation), but a number of key issues (e.g., changes in candidate and voter behavior in response to the changes in election rules) have not yet been incorporated into the model.

### **Information**

One of the main reasons for institutions is that the information about preferences, endowments, etcetera, needed for group decisions is initially dispersed among many different individuals. However, in accepting the principle of incentive compatibility one cannot expect individuals to provide that information if in doing so the social choice will leave them worse off than if they provide false information (Austen-Smith and Riker, 1987). For example, when asked how much one needs to be compensated for environmental damage, and knowing the actual compensation is a fraction of the stated amount, it should be expected that overstatements will occur. One of the very deepest, but potentially most rewarding areas of research involves finding out how to provide incentives to asymmetrically informed individuals so that they will provide the (correct) information needed for social decisions. Examples of work in this area include the role of markets in aggregating information (rational expectations, efficient markets, and the role of insiders) and the role of polling and campaigns in providing the appropriate information needed for informed elections. We are just beginning to understand these mechanisms, and the forces they generate, in equilibrium. We do not know very much at all about situations involving sequential processes, learning, and reputations. A major research effort should be undertaken in this area.

## Cognitive Effects

Recent research in cognitive psychology (Kahneman and Tversky, 1979; Tversky and Kahneman, 1981; Nisbett and Ross, 1980) has revealed that choices are sometimes affected by the framing of options and by other judgmental biases, in a manner that is quite independent of their substantive content. To take one instance, people tend to be risk-averse when it comes to gains. Imagine that someone is given a choice between two options: (1) a sure gain of \$80; and (2) a risky prospect that offers an 85 percent chance of winning \$100 and a 15 percent chance of winning nothing. Most people who are presented with this choice prefer the certain gain to the gamble, in spite of the fact that the gamble has a higher monetary expectation than the certain outcome. This line of research raises the possibility that the framing of a collective decision—for example, whether it is presented to participants in terms that suggest a choice between losses or between gains—may in and of itself affect the resulting outcome. Just as agendas structure participants' choices in collective decisions, do they also help determine outcomes by framing and other kinds of cognitive mechanisms?

As a result of the foregoing work on institutions and choice, scientists are now able to think seriously about practical applications in the design of institutions. Such design has, of course, gone on from the time of the first legislation or constitution, but now it can be done somewhat scientifically. While it is not feasible to describe all this work in detail, a list of more or less typical kinds of current work indicates some of the wide range of possibilities.

One important area is the custom design of institutions to solve social choice problems where traditional market or election devices are either unsuitable or are likely to produce less than optimal outcomes. In particular, we have attempted pricing in nonmarket situations. Thus, transfer pricing (between different units of the same organization) is being studied in the light of our recent discoveries on incentives. The choice of which programs to produce for the Public Broadcasting System (a public good selection problem for station managers) is accomplished each year through the use of a mechanism (institution) designed solely for that purpose and of the discoveries about incentive-compatible revelation of preferences. The National Aeronautics and Space Agency is considering alternative mechanisms for pricing and using the Space Station, a situation in which the traditional device of historical data to predict the future is unavailable.

In general, an institution may be considered to be *incentive-compatible* if its rules give an incentive to individuals to act in support of collectively preferred outcomes. Whereas economists have typically studied the incentive compatibility in the allocation of resources, the concept is also relevant to other kinds of problems.

Choice-theoretic work on crime appears under the rubric of *deterrence theory* (Becker, 1976), which holds that punishment deters crime through two distinct mechanisms: (1) by making the punished individual less likely to engage in subsequent criminal behavior; and (2) by apprising potential lawbreakers of the costs of such behavior. Most research has dealt with the latter question, attempting to test whether greater celerity, certainty, and severity of punishment would lower the crime rate (Gibbs, 1981). While the scientific status of deterrence theory is unclear, the contribution of collective choice theory renders it at least promising.

One problem of institutional design important for contemporary society is electoral systems. One goal is to render such systems fair to both individuals and significant groups. In the United States, Britain, Australia, and Canada, unlike continental Europe and most other areas of the world, the most common election procedure is plurality. While single-member district (SMD) plurality elections predominate at the national and state levels in the United States, multimember district (MMD) elections using plurality are common at the municipal or county level. Plurality MMDs have come under increasing attack in federal courts as dilutive of the voting strength of racial and linguistic groups (see, e.g., *Rogers v. Lodge*, 458 U.S. 613 [1982]), and have also been challenged as political gerrymandering. Testimony based on social choice theory has played a critical role in these challenges to MMDs. Similarly, when some local legislatures in New York sought to use weighted voting systems, testimony involving tools derived from game theory were used to judge the fairness of such systems in terms of "one person, one vote." Provisions for majority run-off elections (as used in some southern states) have also come under challenge as racially discriminatory in their effects. A body of research completed during the past decade sheds light on the properties of run-off systems. If this analytic theory can be fused with a synthesis of analytic and substantive approaches, social scientists may be able to offer definitive testimony on these effects.

And for a final example, since this report has often mentioned progress in the study of collective choice in legislatures, this list of direct applications appropriately concludes with research on the effect of dif-

ferent kinds of legislative rules. One goal of legislative reform is the construction of rules that lead to particular results in budget making. It is now possible to calculate a priori the effect of different rules for making budgets on increases and decreases in budget size (Ferejohn and Krehbiel, 1985).

Thus collective choice theory has many practical applications in designing and judging institutions. It also has many applications in understanding and interpreting existing institutions. Again, a list of some of these applications indicates the range of possibilities. For one thing, a deeper understanding of the role of property rights is emerging. For a long time, philosophers, such as Locke, propounded a moral basis for property. This view continues to survive among contemporary philosophers such as Rawls and Nozick but provides little that is useful or realistic for the analysis of social institutions. The new interpretation of property rights views them as a device to encourage and enforce incentive-compatible arrangements. Coase (1960), Demsetz (1967), and others have shown how property rights encourage socially desirable environmental protection and the preservation of assets. Further work is needed on the effects of varied property rights assignments (e.g., who is liable in the event of an oil spill), especially when participants are differentially informed about the facts. The simple statement "the assignment of rights does not matter" is correct only if agents are identically informed. Legal contracts and judicial decisions will be deeply affected by this research.

No other democracy makes greater use of the vote as a decision-making mechanism than the United States. We have by far the highest ratio of elected officials to citizens of any country in the world. We have a complex federal system whose multitiered structure generates layers in our political parties. Voting is rarely simple majority decision making. Special majorities are required for certain kinds of actions; concurrence of more than one voting body is usually required (bicameralism); veto powers of various sorts govern the relationships between legislature and executive; etcetera. One agency may have the power to propose, another the power to block.

Understanding the properties of such complex and layered institutional arrangements is no easy task but it is one on which a great deal of progress has been made in the past two decades. In particular, game theory models have permitted us to reexpress many complex institutional arrangements (e.g., the "big-five" veto powers in the U.N. Security Council, the U.S. Electoral College, voting rules in the European Economic Community) in terms of a common framework. When so

stated, it is possible to discover whether minor changes in procedures may have major effects that we would not otherwise have been able to anticipate.

Going beyond matters of decision making to the interpretation of the entire institution of democracy, one obvious problem concerns the relation between voting and the democratic ideals of popular participation in and control over government. In the conventional explication of democracy, it is required that voting produce coherent results so that participation may be sensible and control be effective. Yet Arrow's theorem and its elaborations reveal that the incoherent results from voting are always possible. So the conventional notion of democracy is based on unrealistic expectations. Responding to this discovery, one might either abandon democracy or significantly reverse the expectations. Riker (1982) has drawn a distinction between voting as a means to embody the popular will in law (populism) and, more restrictively, voting as a means of changing officials (liberalism). The populist interpretation requires that the results of voting be coherent while the liberal does not. Given this distinction, then, liberal democracy is consistent with the discoveries of collective choice, while populist democracy is not. This reinterpretation of voting is just the first set of a general reinterpretation of democratic institutions (e.g., representation, accountability) necessitated by developments in social choice theory.

Going beyond institutions to deal with very general kinds of social behavior, consider rhetoric or the art of persuasion, which is directed at bringing about collective decisions advantageous to the practitioner and is thus clearly an applied study of collective choice. It is an ancient subject, continuously studied for at least 2,500 years. Indeed, it is the earliest known approach to the study of collective choice and political events and it was probably the first subject of higher education to attract full-time, professional teachers. Yet despite this long and intriguing history, it has not been systematically studied in the sense of formulating theory about the process and then testing out inferences from theory. Rather, teachers of rhetoric have, in an unrationalized and ad hoc way, categorized rhetorical devices and stances and collected examples to illustrate each category. Unfortunately, this method could not tell and has not, in fact, told us much about how persuasion works. In the last generation some students of advertising, propaganda, and political campaigns have begun some empirical studies of the process of persuasion, but these were not usually generalizable because they lacked a theory of choice. With the advent of social choice theory, however, it is possible to focus research on the way that persuasion leads to decision, on the role of agendas in focusing persuasion, and so on (Riker, 1986). It

seems possible that in the next decade or two, therefore, we may begin to make some progress on a subject that has not hitherto been thought of as one in which knowledge could be built up.

There seems to be no doubt that it is now possible to make great progress in the study of collective choice. Some of this will occur simply because of the intellectual momentum now established. To maintain and increase this momentum we believe that it would help to foster networks of scientists and encourage kinds of empirical research not ordinarily undertaken by economists and political scientists.

One of the most powerful devices for encouraging deep and rapid social scientific research has been the fostering, on a continuing basis, of networks of individuals working on closely related issues. The research at the Cowles Commission at the University of Chicago in the 1950s was an example of the major breakthroughs that can occur if like-minded scientists interact on a regular basis. Much of the recent work on incentives and information comes from initial breakthroughs made by a group of researchers meeting regularly (twice a year) under the auspices of the NSF conferences on mathematical economics. It is clear to us that the area of collective choice would be dramatically propelled forward if there were such devices for the scientists in this area. It is important that a small group of appropriate scientists from economics, political science, psychology, and sociology be provided with the resources to be able to interact on a regular basis. One or two one-week conferences during the academic year, with a two-four-week summer workshop would be ideal. Coupled with resources for graduate students or postdoctoral fellows, this structure could encourage rapid breakthroughs in this area.

Although economists and political scientists have done little experimental research, some such work has been done in collective choice (Fiorina and Plott, 1978; Kormendi and Plott, 1982; Eavey and Miller, 1984).

Experimental methods were developed from a need to test theories when there existed no other systematically gathered data. Almost all theories are a priori reasonable, but historical circumstances seldom presented researchers with the opportunity to test competing general theories against each other. Only very special circumstances would allow the separation of theories. By using substantial amounts of money as incentives, researchers have been able to accomplish the necessary separation and determine the types of models that most accurately capture behavior.

The impact of the new data source on theory has been dramatic. Al-

most all principles have come under close examination. The Von Neuman-Morgenstern solution that commanded the attention of mathematics for decades has been essentially discarded. Bargaining sets, the notion of the core, values of games, and even the concept of a characteristic function have been the subject of critical empirical analysis. The response of advocates of theories and approaches has been in the form of new theories and changes in old ones. As a result, the advance in understanding about the mathematical foundations of group choice and game theory has been dramatic.

Application of experimental methods requires, however, a long-term investment in basic research methodology. Applied projects are unlikely to have immediate payoffs unless the groundwork necessary for applications has been laid. Experimental methods require three types of support:

1. Support is needed for the development of laboratories, that is, equipment, space, communication devices such as interactive computers, and, most of all, professional staff who can develop software and recruit and train subjects.

Research now suffers from the lack of a broad subject pool and the need for many observations. Current experimental technology requires the principal investigator to recruit, instruct, and manage subjects, as well as to deal with software problems. In short, the principal investigator (or intellectual equivalent) must be present for almost all experiments. The time demands are substantial.

2. Theorists from a variety of disciplines must be able to participate in the design of experiments. The theoretical issues are so detailed that face-to-face communication over a long period of time is necessary for the design of appropriate experiments. Communication across disciplines is not good. Yet, the phenomenon clearly demands input from several disciplinary sources so that the emerging set of principles can find use in applications. No source of funding provides for this type of research effort. Travel, released time, and even visiting professorships are needed.
3. The heart of the experimental method is the replication of results by different researchers. Replication necessitates standardization of procedures and methods. Such standardization has occurred in the natural sciences through decades of teaching of laboratory methods in high school and college. Access to the basic methodology is not difficult.

Experimental methods in collective choice have not benefited from such large-scale background support. Financing should be



available for those wishing to spend some time at one of the universities where experimentation is being conducted. Support should also be available for those wishing to replicate the results of others and thereby advance scientific understanding while developing their own skills.

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## Information and Decision Making

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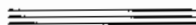
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The field of information and decision theory has changed dramatically over the past ten years in ways that will profoundly influence the direction of research for the next ten. A decade ago, although the "expected utility" approach provided the model of individual choice under uncertainty on which virtually all of decision theory, game theory, and the economics of uncertainty were based, researchers in these areas had undertaken almost no empirical testing of the underlying assumptions of that model. While psychological models of choice under uncertainty (which typically took a more cognitive, information-processing approach) were regularly subjected to testing, there was little communication between psychologists and economists. Today there is a much greater degree of interaction between the two disciplines. Psychologists have identified several behavioral aspects of decision making that challenge some of the very foundations of classical decision theory, and economists are beginning to develop, test, and analyze the economic implications of choice models that depart from the expected utility approach as well as incorporate some well-established psychological aspects of decision making.

The last decade has also seen much activity in the areas of multiperson bargaining and the theory of games. We now possess a better and more formal understanding of the phenomena of "disagreement" and "reputation" in situations of bargaining or strategic interaction. Another important development has been the explicit incorporation of the structure of information among agents and the recognition of its effect on the outcomes of single play or repeated games. Models of imperfect and/or asymmetric information are now being applied to, and yielding new insight into, such problems as the Prisoner's Dilemma, predatory pricing, auctions, product quality, and other economic and strategic issues.

Finally, the area of information and decision making has come to place a much greater emphasis on both laboratory and field testing. Economists in particular have increasingly come to recognize the importance of experimentation and field observation in both the testing and generation of theories of individual choice and market behavior, and this trend is likely to continue at an increasing rate in the future.

At the expense of omitting other important developments, as well as possibly drawing some misleading boundaries, we have identified five areas in the field of information and decision making that have seen the most dramatic advances in research methods or theories, and that in our view hold the greatest promise for continued contributions over the next decade: the development and application of non-expected utility models of choice under uncertainty; the integration of "framing," "heuristics," and other psychological phenomena into decision-making models; theoretical and empirical studies of multiperson bargaining; the theory of games of incomplete information; and game-theoretic field studies. For more complete overviews of these (as well as related) areas, the reader is referred to MacCrimmon and Larsson (1979) and Machina (1983a, 1983b) (non-expected utility models); Einhorn and Hogarth (1978, 1981), Slovic, Fischhoff and Lichtenstein (1977), and Kahneman, Slovic and Tversky (1982) (psychological decision theory); Roth (1979, 1983, 1985b) (bargaining); Milgrom and Roberts (1982a) and Wilson (1985) (games of incomplete information); and Graham and Marshall (1987) and Roth (1984) (game-theoretic field studies). Our report concludes with some brief remarks on the interaction of these different branches of inquiry as well as some general remarks regarding funding.

### **The Development of Non-Expected Utility Models of Decision Making**

From the late 1940s to the late 1970s, the "expected utility" approach proposed by Bernoulli (1738) and axiomatized by von Neumann and Morgenstern (1947) provided the framework for virtually all theoretical and applied work in decision theory, game theory, and the economics of uncertainty (e.g., gambling and insurance decisions, portfolio choice, job search). Essentially, this model postulates that individuals assign (or act as if they assign) a utility value  $U(x)$  to each possible monetary or nonmonetary outcome  $x$ , and when faced with a set of alternative risky prospects, choose the one that yields the highest mathematical expectation of utility. Adopted primarily because of the normative appeal of its axioms, the elegance and tractability of its characterizations of behavior, and the large number of theoretical results it has produced, the expected utility model never received the type of careful empirical testing by economists that they typically accord their other theories or that had been routinely carried out by psychologists (e.g., Edwards, 1953, 1954, 1955). Throughout this period, practically the only empirical matter to

receive any attention was a single example due to Allais (1953) that has come to be known as the "Allais Paradox." This example consists of asking a person to choose a risky prospect out of each of the following pairs (where \$1M = \$1,000,000):

$$a_1: \{1.00 \text{ chance of } \$1\text{M} \quad \text{versus} \quad a_2: \begin{cases} .10 \text{ chance of } \$5\text{M} \\ .89 \text{ chance of } \$1\text{M} \\ .01 \text{ chance of } \$0, \end{cases}$$

and

$$a_3: \begin{cases} .10 \text{ chance of } \$5\text{M} \\ .90 \text{ chance of } \$0 \end{cases} \quad \text{versus} \quad a_4: \begin{cases} .11 \text{ chance of } \$1\text{M} \\ .89 \text{ chance of } \$0. \end{cases}$$

While the expected utility model explicitly predicts that an individual would either make the respective choices of  $a_1$  and  $a_4$  (if the term  $[-.10U(5M) + .11U(1M) - .01U(0)]$  were positive) or else  $a_2$  and  $a_3$  (if it were negative), experimenters such as Allais (1953), Morrison (1967), Moskowitz (1974), Raiffa (1968), Slovic and Tversky (1974), and others have found that the modal if not majority choice of subjects has been for  $a_1$  in the first pair and  $a_3$  in the second. Although this finding might have been expected to stimulate further empirical inquiry, it was with few exceptions (e.g., MacCrimmon, 1968) treated largely as an isolated example, and such preferences were universally considered to be "mistakes" on the part of the subjects who expressed them.

The last ten years have seen a blossoming of economists' interest in the empirical validity of the expected utility hypothesis and the development of alternative models of individual choice under uncertainty. This phenomenon, and its growing acceptance in a field traditionally wedded to the expected utility model, is due to two parallel developments. The first is the recognition of the systematic nature of observed departures from expected utility maximization, in particular, of violations of the key "independence axiom" of the theory. Experimenters have now identified four types of violations of the independence axiom: the "common consequence effect" (e.g., MacCrimmon, 1968; MacCrimmon and Larsson, 1979); the "common ratio" or "certainty" effect (Hagen, 1979; Kahneman and Tversky, 1979; MacCrimmon and Larsson, 1979); "oversensitivity to small probabilities" (Edwards, 1955; Ali, 1977; Yaari, 1965); and the "utility evaluation effect" (Karmarkar, 1974; McCord and de Neufville, 1983, 1984). Each of these has been independently observed, and sometimes independently discovered, by researchers in various fields (economics, psychology, operations research). More significantly, each of these four "effects" has been shown to follow from the same qualitative form of departure from expected utility preferences (Machina, 1983a, 1983b). This particular type of departure from ex-



pected utility preferences has also been observed in laboratory rats choosing over alternative "lotteries" (random reinforcement schemes for food) (Battalio, Kagel and MacDonald, 1985).

The second advance in the last decade has been the development and analysis of non-expected utility models of choice under uncertainty—for the most part, generalizations of the expected utility model (Chew, 1983; Chew and MacCrimmon, 1979a, 1979b; Fishburn, 1983b; Hagen, 1979; Machina, 1982a; Quiggin, 1982; Selden, 1978; Yaari, 1987). These models preserve the standard economic framework of maximization, and allow for the study of the behavioral properties of monotonicity (first order stochastic dominance preference), risk aversion, comparative risk aversion, asset and insurance demand, etc., in a more general framework, giving us a much better idea of which aspects of the expected utility model (and the tremendous body of economic and game-theoretic analysis based on it) are robust to violations of the expected utility hypothesis and which are not. In addition, most of these models have the ability to incorporate the systematic nature of preferences revealed by the above-mentioned departures from expected utility, allowing for a more structured (e.g., parametric) testing and measurement of such effects.

Economists have now begun to take seriously observed violations of another of the expected utility axioms, namely, transitivity. In what was at first a skeptical reaction to findings by psychologists that pairwise preferences over certain types of lotteries are intransitive (Lichtenstein and Slovic, 1971, 1973), economists set out to test this phenomenon for themselves, and indeed found it (Grether and Plott, 1979; Pommerrehne, Schneider, and Zweifel 1982; Reilly, 1982). (This effect is discussed further below.) Since transitivity also constitutes one of the cornerstones of the economic theory of choice under *certainty*, the implications of this finding (and more important, of economists' acceptance of it) for economic research are great indeed.

Motivated in large part by the above empirical results, another class of new theoretical models (Bell, 1982; Fishburn, 1983a; Loomes and Sugden, 1982) have dropped the property of transitivity and offer consistent explanations of a large variety of other phenomena as well as formal characterizations of the concept of "regret" in economic choice.

Given the existing developments in this area, we feel that the two most promising avenues for future work are the application of non-expected utility models to economic issues and empirical inquiry of a more structured and directed nature. Regarding the former, researchers have begun to apply non-expected utility models to the analysis of important economic questions, including some that could not be satisfac-

torily answered by the traditional expected utility framework (Kreps and Porteus, 1978, 1979; Machina, 1982b, 1984; Rossman and Selden, 1979; Selden 1979a, 1979b), and such work can be expected to lead to more satisfactory models of asset and insurance markets, optimal lottery design, etc. These newer models also provide a more flexible framework for the elicitation of preferences in decision analysis contexts, and, to the extent that they better represent the attitudes of decision makers, they possess prescriptive value. Finally, the common nature of observed departures from expected utility preferences allows for the construction of more empirically grounded policy prescriptions (Machina, 1983a).

We feel that those studies that incorporate systematically varying parameters (e.g., MacCrimmon and Larsson, 1979) as well as realistic economic incentives (e.g., Lichtenstein and Slovic, 1973; Grether and Plott, 1979; MacCrimmon and Wehrung, 1985) hold the greatest promise for a better qualitative and quantitative understanding of the actual nature of individual preferences over lotteries. A natural and important next step would be the design and implementation of field studies involving the interaction of agents in "real world" markets and institutions.

### **The Integration of "Framing," "Heuristics," and Other Psychological Phenomena into Decision-Making Models**

In addition to the above rather sharply defined departures from the traditional economic model of choice under uncertainty, researchers over the past decade have uncovered several aspects of decision making that challenge the very foundations of classical decision theory.

The principal finding in this area has been the sensitivity of individuals' responses to the manner in which probabilistically or actuarially identical problem situations (decisions, choices, or judgments) are presented or "framed" (Slovic, 1969; Payne and Braunstein, 1971; Moskowitz, 1974; Kahneman and Tversky, 1979; Schoemaker and Kunreuther, 1979; Hershey and Schoemaker, 1980; Tversky and Kahneman, 1981; Hershey, Kunreuther and Schoemaker, 1982; Slovic, Fischhoff and Lichtenstein, 1982).

A dramatic illustration of such an effect is given by McNeil, Pauker, Sox and Tversky (1982), who asked people to imagine that they had lung cancer and had to choose between two therapies: surgery or radiation.

Each therapy was described in some detail. Then, some subjects were presented with the cumulative probabilities of surviving for varying lengths of time after each type of treatment. Other subjects received the same cumulative probabilities expressed in terms of dying rather than surviving (e.g., instead of being told that 68 percent of those having surgery will have survived after one year, they were told that 32 percent will have died). Framing the statistics in terms of dying dropped the overall percentage of subjects choosing radiation therapy over surgery from 44 percent to 18 percent. In fact, of the three groups of subjects used in the study—patients, students, and physicians—this effect was found to be the strongest among physicians. Numerous other examples of this type may be found in the above cited studies.

A related group of findings are what may be termed “response mode effects.” One distinction in experimental work, for example, is whether a task is presented as one of judgment or evaluation of individual options, or of choice among two or more options. Although classical decision theory views these processes as equivalent, studies have found that the information-processing strategies used in making choices are quite different from those employed in valuing individual options. As a result, choices and evaluative judgments over the same sets of options often differ, sometimes dramatically.

An early demonstration of this effect was conducted by Lichtenstein and Slovic (1971), who offered subjects two types of gambles: one type featuring a high probability of winning a modest sum of money (the “P bet”), the other type featuring a low probability of winning a larger sum of money (the “\$ bet”). They found that people often chose the P bet but assigned a greater monetary value to the \$ bet. Similar “preference reversals” have subsequently been observed by Lichtenstein and Slovic (1973), Grether and Plott (1979), Pommerehne, Schneider, and Zweifel (1982), and Reilly (1982). (See also Slovic and Lichtenstein, 1983.) This phenomenon is of particular interest to economists since it implies that preferences over the two bets and their respective certain monetary equivalents form an intransitive cycle.

Another class of phenomena is the use of “heuristics” in judgment under uncertainty. While such “rules of thumb” typically serve to lower the informational and cognitive costs of decisions, they can be shown to lead to incorrect conclusions in certain situations. Among those systematic tendencies that researchers have uncovered are “availability” (where the estimated frequencies or probabilities of events are unduly influenced by the ease with which examples can be brought to mind), “representativeness” (where judgment on the basis of similarity can lead to insufficient incorporation of prior probabilities and systematic violations

of Bayes' Law), and "anchoring" (where estimates or judgments are insufficiently adjusted from initially computed or considered values) (e.g., Kahneman, Slovic and Tversky, 1982). Indeed, it was the finding that subjects would anchor on the prize levels in generating monetary values and on the success probabilities in outright choice (Slovic and Lichtenstein, 1968) that originally led these researchers to predict the existence of the "preference reversal phenomenon" of the previous paragraph.

Needless to say, framing and related phenomena possess important potential implications for public policy. One recent example is the lobbying effort by the credit card industry to have differentials between cash and credit prices labeled "cash discounts;" rather than "credit surcharges" (Thaler, 1980). Similarly, there is evidence that the way unit prices are presented in supermarkets can have significant effects on consumers' choices (Russo, 1977). In general, if individuals' preferences over alternative social and political choices depend on how these issues are framed, it is not clear how best to promulgate social policies such as cigarette warning labels, truth-in-lending laws, or informed consent laws. If, as we have seen, reporting survival rates results in different choices than reporting mortality rates, what is the "right" thing to do? The possibility that there may be no "neutral" way to frame a decision problem or to present information, coupled with the ease with which preferences can be manipulated, has important ethical and political implications that need to be examined. In the extreme case, such issues become critical in a democracy if, for example, votes are significantly affected by the way in which issues are phrased on ballots.

We feel that the most productive future work in this area will proceed along two lines. First, although several major psychological phenomena have been identified and numerous explanations for these effects have been proposed, we continue to lack any sort of unifying explanatory or predictive principle behind such effects comparable to the essential unity of the observed departures from expected utility maximization discussed above. Such a unified understanding of these effects (and the conditions under which they are most likely to arise) must come from a more holistic approach to the process of choice, linking framing and response mode effects to the mechanisms whereby stimulus information is perceived, encoded, and processed, and the resulting response is expressed. In particular, approaches that view choice or evaluation behavior as being generated by a multistage process that involves the encoding (e.g., framing) of information, its evaluation, and the expression of choice or valuation seem to hold the greatest promise for a unified understanding of these diverse yet well-established phenomena.

A second area of future research concerns the incorporation of these and other psychological phenomena (unified or otherwise) into economic models, both at the individual and at the market level. The growing acceptance of experimental data and of alternatives to the expected utility model reported above reflects a level of receptiveness among economists unknown ten years ago. Indeed, most (though by no means all) of the pioneering efforts along these lines have been conducted by economists themselves.

At the individual level, Thaler and Shefrin (1981) have formalized the behavioral concept of "self-control" and incorporated it into a model of intertemporal choice capable of explaining institutions such as Christmas Clubs and whole life insurance, both of which offer extremely low rates of return and cannot be explained by standard economic theories. This model can also explain the observed (and heretofore puzzling) tendency for increases in mandatory pension contributions to be less than fully offset by decreases in other forms of savings, a finding that has implications for both national saving and national health insurance policy (see also Schelling, 1984, and Winston, 1980). Similarly, Akerlof and Dickens (1982) present a formal model of cognitive dissonance in economic behavior, with applications to job safety and social security policy, and Hogarth and Kunreuther (1985, 1989) develop a formal model of the concept of "ambiguity" and its implications for the demand for insurance.

At the market level, the existence (prevalence?) of agents who are "quasi-rational" (i.e., who systematically violate the expected utility axioms and Bayes' rule but are consistent in other respects) raises many questions for economic theory and practice. Theoretical studies by Akerlof and Yellen (1985), Haltiwanger and Waldman (1985), Kreps, Milgrom, Roberts and Wilson (1982), Russell and Thaler (1985), and others have addressed such issues as the robustness of standard market efficiency properties to the number of such agents and to the magnitude of their departures from standard economic rationality. It is also possible to determine those characteristics of markets (e.g., the possibility of short sales) that affect their relative sensitivity to quasi-rational behavior.

Empirically, researchers are now beginning to investigate whether actual markets do or do not show evidence of quasi-rational behavior and whether it affects the performance of these markets. Dreman (1982), for example, has shown how the prevalence of the representativeness heuristic and consequent departures from Bayes' rule can explain the well-documented above-normal returns that can be obtained from investing in stocks with low price/earnings ratios. De Bondt and Thaler

(1985) have used these same principles to predict the existence of a new anomaly, namely, that contrary to the maxims of the standard "efficient markets/random walk" theory of financial markets, stocks that have done poorly for several years later yield excess returns (this phenomenon was in fact subsequently observed).

### Theoretical and Empirical Studies of Bargaining

In addition to the relatively impersonal forces of the marketplace, many aspects of both the national and the international economy (e.g., wage bargaining, terms of access to international markets) are influenced by the outcome of negotiations. Bargaining, of course, also plays a key role in many if not most noneconomic (e.g., social or political) interactions such as the legislative process and arms control negotiations.

Because pure bargaining is the economic situation in which the personal attributes of individual agents have the most potential to influence the outcome, standard modes of economic analysis have traditionally classified bargaining problems as fundamentally "indeterminate" (Edgeworth, 1881; Hicks, 1935), while modern attempts to resolve this indeterminacy have depended on more detailed models of individuals, either in terms of their risk preferences (Nash, 1950; Roth, 1979), their time preferences (Rubinstein, 1982), their information (Fudenberg, Levine, and Tirole, 1985), or some combination of these. Recent progress in the strategic approach to the theory of bargaining has been due in large part to two developments in the general theory of noncooperative games. One of these developments, originating in the work of Harsanyi (1967, 1968a, 1968b), extends the theory to include games of "incomplete information," which allow more realistic modeling of bargaining situations in which a bargainer holds private information. The other development, originating in the work of Selten (1973, 1975) on "perfect equilibria," offers a technique for reducing the multiplicity of possible equilibria found in many noncooperative games by considering the credibility of the implicit or explicit "threats" involved. An important reformulation of some of these ideas on credible equilibria, which makes explicit how behavior may depend on agents' beliefs about one another's reactions, has been given by Kreps and Wilson (1982a).

As mentioned above, one of the fundamental recent advances in this area is an improved understanding of the nature and causes of disagreement in negotiations, even when both sides recognize that there might

be gains from mutual cooperation. Whereas earlier models interpreted disagreements as resulting entirely from mistakes or misperceptions on the part of the bargainers, current models frequently predict a positive probability of disagreement in equilibrium. Basically, what has been captured in these models is the idea that if you bargain in such a way that you always reach an agreement whenever a mutually profitable agreement is possible you are probably not getting as good terms as you could have (e.g., Myerson and Satterthwaite, 1983; Chatterjee, 1985). The price of holding out for better terms is that you will have to forgo some worthwhile agreements.

The nature and role of precedents and historical relationships in determining the outcome of bargaining and strategic interaction has also come to be much better understood. Recent work demonstrates, for example, that the role of reputation is different in repeated bargaining involving the same parties than in bargaining between parties whose past histories involved other bargaining relationships. One example is the influence of the Falklands War on England's respective relationships with Argentina and, say, Spain (see Wilson, 1985, and Rosenthal, 1985, for surveys of this subject).

Another theoretical advance has been the systematic analysis of the effect of a bargainer's risk aversion on his or her predicted bargaining success (Roth, 1979, 1985a; Kihlstrom, Roth, and Schmeidler, 1981; and Roth and Rothblum, 1982). Apart from its implications for bargaining theory itself, this work is important in that it serves to connect the theory with the substantial literature on choice under uncertainty. One potentially fruitful line of further work in this area is the application of the nonexpected utility models to bargaining theory (e.g., Crawford, 1984).

These theoretical developments have been matched by a corresponding advance in experimentation. Paradoxically, the designs of early experiments were such that they could only be interpreted as tests of bargaining models under the assumption that all parties had identical risk-neutral preferences, even though the theories they sought to test implied that differences in the bargainers' success were due to differences in their risk posture. However, starting with Roth and Malouf (1979), techniques have been developed that allow experimenters to control for differences in risk posture and thus to test these theories properly. These techniques have recently been extended by Daley, Dickhaut, and O'Brien (1985). Although experimental tests have falsified some of the major predictions of bargaining models (Roth, Malouf and Murnighan, 1981; Roth and Murnighan, 1982; Roth and Schoumaker, 1983), they have demonstrated that there are a number of observable

regularities in bargaining concerning, for example, the establishment of "focal points," the criteria that determine "credible" bargaining positions, and the roles of bargainers' expectations of one another and the structure of their shared information. Finally, although not experimental, Raiffa (1982) has applied bargaining theory to the analysis of such important political issues as the Panama Canal negotiations, the Camp David Accords, and the Law of the Sea Conference.

Besides applied studies of the type discussed below, perhaps the most promising avenue of future research lies in the application of computer technology, which offers the opportunity of more thorough interactive experiments. The possibilities of more control in multiple-person situations are obvious. The development of standardized software and operating systems would greatly lower the cost of performing experiments and go far toward generating a large body of empirical data free from the effects of differing experimental design.

### **Game Theory: Games of Incomplete Information**

The last decade has seen a remarkable burst of activity in game theory and its applications, mostly in economics and related fields but also in political science and biology. This work has provided both striking new theorems of general applicability and explanations for specific observed patterns of behavior that could not be understood in terms of earlier analyses.

Two factors have been central in this success. The first is explicit attention to the timing of opportunities for action and to the information available to decision makers when they act. This is done by working with games expressed in their more detailed or "extensive" form (Luce and Raiffa, 1957) where these aspects are made explicit, and by considering games with repeated opportunities for play. The second factor has been the recognition that differences in the information available to the players is a key determinant of their strategic behavior (Harsanyi, 1967, 1968a, 1968b).

A simple example involving three different card games can illustrate the importance of the structure of information. In Game 1, each player is dealt two cards face up. As many rounds of betting as are desired follow, then the best hand wins. This (trivial) game involves perfect public information, which historically has been the framework of much economic analysis. Game 2 is similar except each player gets one card face up and one face down and cannot look at the latter until the betting is



completed. This game, which involves uncertainty but no private information, is not trivial, but would not generate any really interesting behavior. In Game 3, each person gets one card face up and one face down and can look at his or her "hole card." Here there is private information, and one can imagine that actual play would involve a variety of phenomena that would not have arisen before, such as bluffing, reputation formation, and attempts to infer others' private information from their behavior.

A game that has received much attention is the well-known "Prisoner's Dilemma" (e.g., Luce and Raiffa, 1957). Here, each player has two options: C ("cooperate") or S ("sell out"), and they must make their choices simultaneously and independently. As seen from the example

		PLAYER II	
		C	S
PLAYER I	C	(3,3)	(0,5)
	S	(5,0)	(1,1)

payoff matrix (which specifies the respective payoffs of the players depending upon their choices), each player can do better by playing S rather than C no matter what action his or her opponent takes. Yet the payoffs obtained when both play S are strictly less than when both play C: making the "rational" choices yields a socially inefficient outcome. This class of games has been widely studied and applied in economics, ethical theory, political science, sociology, and biology. It has been used to model such disparate problems as industrial competition on prices, advertising, and new product introduction; the provision of collective benefits such as pollution control; the formulation of alliances; arms races and nuclear deterrence; altruistic behavior within species; and competition between species (Axelrod, 1984; Maynard Smith, 1982; Kreps, Milgrom, Roberts and Wilson, 1982; Scherer, 1980).

Many of these applications involve repeated play of this game, with the payoffs aggregated over the rounds. A pattern that has been widely observed in practice, and almost universally in the laboratory, is that (except possibly in the last rounds of play) cooperative behavior is adopted (Rapoport and Chammah, 1965; Worchel, 1969). This has been seen as a fundamental problem, since playing S at each state can be proven to be the unique stable pattern of action that would arise from individually self-interested behavior.

Recently, it has been shown that introducing a "small amount" of the "right kind" of incomplete information can generate equilibrium behavior that exhibits the patterns of cooperation observed empirically (Kreps, Milgrom, Roberts and Wilson, 1982). Specifically, it is assumed that while each player is "rational" in the classical sense, neither is completely certain that his or her opponent is. For example, each player

might allow some slight possibility that the other enjoys cooperating, or that he or she follows a conditionally cooperative behavioral rule such as tit-for-tat. In such cases the unique equilibrium behavior will be to choose C early in the interaction in the hope that this cooperation will be reciprocated. Any failure to cooperate destroys the possibility of future cooperation, because the uncertainty disappears and one is back in the full-information situation. If there are enough opportunities for future mutual gain, the short-run advantages to playing S are overwhelmed by the loss of these opportunities, and cooperation results. This is important because it reconciles the hypothesis of rational self-interested behavior (which has elsewhere proven so powerful) with the empirical observations of cooperation, and it does so in a manner that offers promise of wider applicability.

When such informational asymmetries are present, it becomes necessary to infer others' private information in order to be able to forecast their behavior. This provides the basis for reputations, which are essentially estimates of motivation and future behavior based on inferences from past observations (Kreps and Wilson, 1982b; Milgrom and Roberts, 1982b; Wilson, 1985). Essentially, reputations can be expected to arise whenever there are these types of informational asymmetry plus the opportunities for repeated dealing and for observing past behavior. The possibility of developing a reputation for aggressive responses to competition that would deter challenges from competitors provides an explanation for predatory pricing. The possibility of obtaining future credit at better interest rates if one has a reputation for prompt repayment provides an important motivation for paying on time, even when it is painful to do so. Similarly, reputational concerns provide incentives for the maintenance of product quality and for living up to agreements. Moreover, although formal analyses have yet to be undertaken, similar arguments would seem to provide insight into the variety of phenomena in political science and international relations known as the "rationality of appearing irrational," which suggests that apparently extreme overreactions to slight provocations can be useful in deterring more serious challenges.

This result—that informational asymmetries can generate rich, complex patterns of strategic behavior that would not otherwise arise—is the key to understanding a wide variety of phenomena. Formal work in this area has begun to identify certain recurrent phenomena: "moral hazard" (the problem of unobservability of actions), "adverse selection" (the problem of unobservability of privately known information), "incentive compatibility" (the problem of designing contracts, offers, or insti-

tutions so as to induce individuals to truthfully reveal their information or preferences through their choices or actions), and "signaling" (costly and possibly unproductive actions taken to aid others in inferring one's private information) (e.g., Akerlof, 1970; Arrow, 1968; Groves and Ledyard, 1977; Pauly, 1968; Spence, 1974). Current and potential applications include optimal regulation of industries under imperfect information; improved voting procedures; optimal bidding mechanisms for government contracts, oil-leases, or Treasury bills; optimal design of employee-management schemes; and stability and detection in cartels or collusive groups (e.g., Baron and Myerson, 1982; Brams and Fishburn, 1983; Hamburger, 1979; Milgrom and Weber, 1982).

This area is developing so rapidly that it is difficult to identify the most fruitful lines of research, even over the next few years. It is safe to say, however, that it will continue to be characterized by important theoretical advances along the lines drawn above.

### **Game Theory: Field Studies**

In addition to its use as a theoretical tool in economics and political science, game theory has recently begun to emerge as a tool in applied economics, yielding surprising new insights into "real world" phenomena. Roth (1984, 1986) studied the labor market facing graduating medical students seeking internships and residencies at U.S. hospitals. From 1900 to 1951, this market was in considerable disarray, and by the 1940s the rules by which it was organized were being changed every few years in an effort to avoid massive market failure. In 1951 a new procedure was adopted which immediately stabilized the market, and it remains in use to this day. However, since the mid 1970s the market has once again begun to exhibit problems reminiscent of those prior to 1951. Working from minutes of the meetings of medical organizations attempting to deal with these problems, Roth was able to reconstruct the various rules by which this market had been organized over time, and to demonstrate that the market failure prior to 1951 was due to the fact that the market did not produce an allocation in the "core" (the set of strategically stable arrangements), that the procedure adopted in 1951 did produce a core allocation, and that changes in the demographics of the market that became significant in the mid-1970s prevented the procedure from continuing to do so. Thus a standard set of game-theoretic tools was able to explain the operation of this market since its inception at the turn of the century, and to diagnose its current ills (in fact, several other

contemporary problems the market faced were also illuminated by this analysis).

A similar analysis was conducted by Graham and Marshall (1987), who examined the institution of bidder "rings" at machine-tool auctions, obtaining detailed information on the organization and operation of these rings from informants among dealers who are regularly involved in such arrangements. Other such studies of real-world institutions and practices include Wolfson (1985) on the lease-versus-buy question and Staten and Umbeck (1982) on disability compensation for air traffic controllers.

Once again, the nature of this topic is such that it is almost impossible to predict particular areas in which the most useful future progress will be made. We feel, however, that serious proposals of this kind that show evidence of both theoretical sophistication and careful attention to the quantitative and qualitative aspects of institutions are likely to lead to an improved understanding of many real-world phenomena.

### Conclusion

It would be unrealistic to pretend that there is no tension between the above branches of inquiry, or that they will not continue to develop in at least some isolation from each other. Economists studying the market implications of non-expected utility models are likely to ignore at least some of the psychological phenomena discussed above, and bargaining and game theorists will continue to use the expected utility framework for much of their work. Accordingly, it would be naive to expect that most topics worthy of funding over the next decade (such as the ones we have suggested above) will not continue to reflect these divisions.

Nonetheless, researchers in these areas are in closer contact and are experiencing more cross-stimulation than ever, and we feel that there are at least some specific opportunities to further this trend. At a very basic level, proposals should be judged in part on whether their authors are at least cognizant of the above-mentioned psychological phenomena as well as the potential economic implications of their findings. Though not always possible or appropriate, another potentially fruitful type of approach might be joint experimental/field studies of a particular aspect or phenomenon. Besides yielding methodological insights that would undoubtedly carry over to the study of other phenomena, the funding of this type of work may well encourage researchers from different traditions to join forces in the design and execution of such combined endeavors.

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# 12

## Market Efficiency

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Economic efficiency is a universal concern. Failure to utilize scarce physical and human resources to productive advantage is a waste that none can afford. Enhancing efficiency is valued everywhere—in advanced capitalist economies in Western Europe and the United States, in the industrialized nations of the Warsaw Pact, and in developing capitalist and socialist economies.

Practicality is not the only consideration, however. The study of economic efficiency is intrinsically interesting. It has been and will remain a central issue on the economics research agenda for purely intellectual reasons. It is nonetheless gratifying when intellectual and practical interests are joined. That is plainly the case here.

Thus it is of both intellectual and practical importance to ascertain which modes of economic organization have superior productive properties under what circumstances and why. The study of economic incentives and of economic institutions is central to such an undertaking. A discriminating assessment of the structural and incentive features of the full range of organizational alternatives—including private companies, government agencies, and public or nonprofit suppliers—is needed. What are their respective strengths and weaknesses under what circumstances and why?

New and more general ways of assessing economic organization in the private sector and of evaluating government instruments regarding taxation, price regulation, the provision of public services, insurance, and the like have witnessed progressive development in the past twenty-five years. This has been attended by a considerable deepening of interest in and understanding of economic institutions and individual decision-making processes. Thus whereas it was once believed that the details of economic organization were relatively unimportant—in which event the study of economics could proceed abstractly in terms of an “institution-free core”—it has since become clear, in both theory and practice, that economic organization is important and that the details of economic institutions and the behavior of decision makers matter. Consider the following:

Whereas the tools of economic research and public policy analysis once favored the view that

1. technology was largely determinative of economic organization
2. public policy intervention was justified upon a showing of "market failure"
3. the incentive effects of various tax, subsidy, and compensation schemes can be inferred from relatively simple or crude models of individual choice and corporate behavior
4. economic organization had a bimodal distribution—activity being organized preponderantly either in classical markets or in centralized firms

The ambit has since been expanded by the progressive development of economic theory and its applications to acknowledge that

- 1'. a variety of organizational structures can usually support any given technology, whence the choice among the feasible alternatives must be broadened to include other efficiency criteria
- 2'. all forms of organization—including regulation and public ownership—are imperfect, whence the assessment of all forms of economic organization must proceed comparatively
- 3'. much more detailed knowledge of incentive structures and of individual and corporate decision processes is needed to infer actual effects of tax, subsidy, and compensation schemes
- 4'. economic organization is distributed along a continuum, within which economic activity in the middle range (joint ventures, franchising, complex long-term contracting) is much more important than had hitherto been realized—and the internal organization of the corporation is both variable and matters

Whereas the tools of economic research and public policy analysis once favored the view that

The ambit has since been expanded by the progressive development of economic theory and its applications to acknowledge that

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| <p>5. long-run price and output were the relevant data for assessing economic performance</p> <p>6. static models of competition and monopoly provided the relevant bifocal lens with which to assess business behavior</p> <p>7. the courts were a low-cost and efficacious contract enforcement mechanism</p> <p>8. economics was a largely nonexperimental science.</p> | <p>5'. short-run adaptive features and organizational competencies are also germane and are sometimes critical</p> <p>6'. some of the more important issues of rivalry entail complex intertemporal moves and replies, which is to say that strategic behavior can be crucially important</p> <p>7'. the courts play more of a background role, and the immediate parties to a contract (and close observers thereof) exercise considerable influence over contract enforcement through their own efforts to craft local contractual safeguards and perfect reputation effect mechanisms</p> <p>8'. careful experimental design and execution yield extraordinary insight into the microanalytics of economic processes and corroborate the proposition that the details of economic institutions matter.</p> |
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The upshot is that the market efficiency research agenda has been greatly expanded in the past twenty-five years. A combined study of



economic institutions and economic efficiency has emerged. Although the earlier tools of analysis are important and remain serviceable, new conceptual and empirical apparatus has been added. More is in prospect in what is an intellectually exciting and important area for economics research over the next decade.

### **Recent and Prospective Research Advances**

Studies of economic incentives, economic regulation, competition policy, and experimental economics are among the more important areas where recent research advances have been made and where more is in prospect.

#### *Assessing Economic Incentives*

The matter in which incentives (reward and penalty structures) interact with governance structures (the institutional matrix within which transactions are located and their integrity is decided) is an important part of the market efficiency research agenda. It is not, for example, true that the use of transfer pricing within firms will replicate the performance of similar pricing used for the procurement of identical goods or services within markets. It is likewise untrue that cost-plus procurement within firms and between firms (across markets) will yield identical outcomes. Incentives thus need to be evaluated and utilized in conjunction with governance structures in a discriminating way. This is true in health care and in a variety of other contexts.

The rising cost of health care in the United States has been a major public policy concern for some time, and an aging population makes it even more important. The rapidly rising costs of health care (now over 10 percent of GNP) have raised questions about the incentive effects of public and private health insurance programs and of health expense reimbursement rules and arrangements. Medicaid and Medicare reimbursement policies are currently under extensive review. Major changes have recently been made and other changes will be made in the near future. These changes are slowly being adopted by private insurers as well. Current reimbursement rules of the government health insurance plans encourage institutionalized care over care at home, even though the cost of home care may in many instances be much less expensive than institutionalized care. In many states, nursing home care is covered by medical insurance only after the private resources of the elderly have

been exhausted. Thus there is a continuing need for analysis of the incentive effects of public health insurance coverage and ways in which quality care could be provided at lower cost.

Recent empirical research in economics has begun to investigate the incentive effects of tax systems in a considerably more realistic manner than previous research had done. The importance of the research area is evident: (1) Is individual work effort affected by the tax system? (2) Are individual savings decisions affected by the tax system and by the special features of IRAs, Keogh plans, and various 401-type plans? (3) Is investment in startup ventures and other venture capital projects affected by the capital gains tax? The answer to each of these questions is almost surely yes. However, while economic theory and introspection both lead to this answer, the magnitude of the effect in each question requires the construction and estimation of econometric models that permit systematic evaluation of individual responses to the tax system. Given the estimates of the models, various tax reform proposals can be evaluated both to assess their effect on government revenue and, equally important, to assess their effect on individuals' standards of living and economic welfare.

Recent research on the effects of marginal tax rates indicates that a decrease in marginal tax rates would have an important work incentive effect, especially among women in the labor force. At the same time, economic welfare could be significantly increased by decreasing the distortions created in individual behavior by high marginal tax rates.

Individual retirement accounts (IRA) and Keogh plans were initiated to increase private savings for retirement, and thus to enhance the welfare of the elderly, and more generally to increase the national savings rate. A specific purpose of the plans was to provide tax-deferred savings for individuals without private pension plans. Economic analysis has already revealed that such savings mechanisms are not a substitute for private pension plans, and that persons without private plans are also unlikely to contribute to IRA plans. The context thus evidently matters, but additional study of these issues is sorely needed.

The analysis of these and other questions has been greatly aided by recent major advances in econometric analysis that allow evaluation of the effect of policy and institutional constraints on individual behavior. Possibly the most far-reaching econometric advance has been in the area of qualitative choice. New techniques have evolved in conjunction with efforts to analyze the incentive effects of particular public policies. For example, the analysis of income maintenance proposals (as defined in the series of recent income maintenance experiments) and housing allowance subsidies (as defined in the housing allowance demand experi-

ment) has led to the development of econometric methods to take appropriate account of discontinuous and nonlinear budget constraints. Related econometric advances in sample selection have also evolved in good part from analysis of these experiments.

The importance of the techniques is that they allow analysis of the incentive effects of programs like those described above. In short, not only does economic analysis allow a structuring of the issues in a way that highlights the incentive effects of the programs, but also recent advances in econometric analysis allow more precise quantification of these incentive effects. Thus the ability of economists to analyze the effect of further policy and institutional constraints on individual behavior is central to what economists do, and their ability to do it well has been substantially enhanced over the past decade and a half.

### *Regulation, Deregulation, and Regulatory Reform*

The study of regulation in a comparative institutional context has made considerable headway in the past twenty years. Public policy toward a new technology, cable television, is an example. An important issue in the 1970s was the extent to which extensive development of cable television would reduce the viability of "free," over-the-air broadcasters, especially independent stations and stations using UHF channels. Substantial work by economists addressed the question of the underlying cost structure of cable television, the demand for subscriptions to it, and viewing patterns of subscribers to ascertain who would be the winners and losers under various scenarios for cable development. This research indicated that the primary effect of cable television was to increase the likelihood of effective competition with the three national networks, in part by enhancing the market position of UHF independent stations. These results influenced subsequent decisions by the Federal Communications Commission and by the relevant congressional oversight committees to relax regulatory constraints on cable television.

In similar fashion, substantial research on costs, demand, and the nature of competition in the airline industry had a bearing on policy questions concerning the effects of deregulation of the airlines on fares, service, and quality and on the availability of service to smaller communities. This research played an important role in convincing Congress that airline deregulation would not lead to a substantial reduction in service but would cause substantially lower fares.

Both of the preceding examples deal with the relevance of research in the context of decisions about whether to regulate or deregulate, but research has also played a significant role in addressing questions related

to the consequences of changes in methods of regulating. For example, substantial increases in energy costs during the 1970s caused most regulatory officials and business executives in the electric utility industry to examine new methods for pricing electricity. For business and residential consumers, the central issue was whether and how to introduce "peak-load" pricing, that is, pricing that varies through time to reflect differences in cost and demand conditions. An examination of the details of cost and demand conditions in the electricity industry was needed so that the consequences of alternative pricing methods could be estimated. Eventually the federal government financed several field experiments with peak-load pricing, designed and evaluated with the tools of economics and statistics. The key questions were, first, which form of pricing produced the greatest efficiency gains (from one extreme of prices that vary literally from moment to moment, to the other of prices that change only according to the season of the year), and second, for which classes of customers did a switch of pricing methods yield a gain that exceeded the implementation costs.

These empirical and institutional studies of peak-load pricing could not have been done well, if perhaps at all, but for earlier theoretical work that had been done on these matters. The lesson here is this: basic research on economic incentives and alternative modes of economic organization are absolutely essential if the study of regulation in applied contexts is to move forward in a well-considered manner. A recent illustration is the theory on the cost revelation effects of different price-setting rules. The object is to design rules that give regulated firms the incentive to keep cost records of the most appropriate kind for rate-making purposes. This has a bearing on current concerns in the electric and telephone industries, where efficient regulation requires that accurate costs be secured both by specific services and by classes of customers.

Another active research area with practical import is "technology forcing" regulation: the development of a sequence of performance standards through time that balances the benefits of greater performance against the costs that better-performing but higher-cost technologies will be adopted more slowly and hence cause longer lives for capital assets that perform less well. Still another example is research that focuses on the opportunities for deregulating certain segments of the electric power industry. While the retail distribution of electricity appears to be a natural monopoly, opportunities exist for competitive market forces to play a role in allocating resources in wholesale electricity markets, that is, purchases and sales between utilities. A microanalytic assessment of alternative modes of contracting was needed to help fashion discriminat-

ing policy on these matters. The Federal Energy Regulatory Commission is conducting an experiment in the southwestern part of the United States to develop further information on this question, and recently opened an inquiry to evaluate its wholesale electricity pricing regulations.

### *Competition Policy*

The study of economic organization and of public policy toward business has been reshaped by the progressive development of transaction cost economics and by new apparatus for assessing strategic behavior. The first of these took exception with the prevalent view of the business firm as a production function whose boundaries are largely defined by technology (whence the firm was treated as a "natural" unit, the size, composition, and organization of which were unproblematic). Instead, the firm was reconceptualized as a governance structure whose boundaries need to be defined. The systematic and comparative study of transaction costs—the costs of planning, adapting, and monitoring task completion under alternative governance structures—was brought to the fore.

A crucial problem on which transaction cost economics and orthodoxy divide is that of vertical integration. The latter treats vertical integration—the decision to join successive stages of production under unified ownership—as determined by technology and relative prices. Transaction cost economics, by contrast, begins with a more primitive unit of analysis, the transaction, and examines the efficacy of a variety of organizational forms (or governance structures) within which different transactions might feasibly be located. Transactions, which differ in their attributes, are thus assigned to governance structures, which differ in their costs and organizational competencies, in a discriminating way.

This comparative institutional approach to economic organization involves a more microanalytic approach to economic organization than was characteristic of prevailing approaches. The study of institutions takes on added importance. The same strategy for recasting vertical integration in transaction cost terms turns out to have application to a wide range of organizational and institutional issues. Vertical market restrictions, franchising, labor market organization, rate of return regulation, corporate governance, the organizational structure of modern corporations, and the study of credible commitments more generally can be reconceptualized as variations on the same underlying contractual scheme. Public policy toward business—both competition policy, especially in the area of mergers, and regulation—has been reshaped to reflect these developments.

Empirical studies of the contracting process and of economic organization have disclosed that the main implications of the transaction cost economics approach are borne out by the data. These studies have required that data be collected that correspond with the more microanalytic level of detail at which transaction cost economics operates. This is a very time-intensive undertaking, but it is an unavoidable price to pay if the core efficiency issues of comparative institutional analysis are to be reached.

The second area in which competition policy has been vastly reshaped in the past fifteen years is in the study of strategic behavior. To be sure, economists had long maintained that the study of business conduct was a matter of economic importance. Express or tacit collusion, for example, was universally conceded to be a problem. A new line of analysis, however, held that barriers to entry were central to the study of business behavior.

The barriers-to-entry approach to economic organization flourished during the decade of the 1960s. It nevertheless had two serious limitations. For one thing, the definition of entry barriers was often vague, and applications were sometimes overreaching as a result. Second, the phenomena of interest often were dynamic and were beset by uncertainty while the underlying models were static and deterministic.

Both of these defects have been greatly relieved, if not fully remedied, by recent research. The issues of entry and its deterrence have been recast in the context of "credible threats." And problems of interfirm rivalry have been recast in a way that develops their intertemporal features and takes account of uncertainty and imperfect information.

Whereas a static treatment of predatory pricing and other forms of predatory behavior disclosed that such practices are "irrational," it is now clear that strategic interfirm behavior is a much more complicated matter. Studies of pricing, investment, R&D, and international competition have been recast to make allowance for sunk costs, timing, learning by doing, and devices designed to "raise rivals' costs." The upshot is that strategic behavior is a fundamental feature of the competitive scene with which competition policy must come to terms.

Developing an informed public policy toward strategic behavior poses formidable difficulties, however. There is a serious danger of prohibiting legitimate rivalry, thereby protecting competitors rather than competition. But the earlier claim that strategic behavior is the product of economic imagination and is not borne out by economic behavior in practice is plainly wrong in both conceptual and empirical respects. Although a sound conceptual foundation has been laid, further theoretical and related empirical research on these matters is sorely needed.

A particularly important line of research coming out of recent work on problems of strategic behavior is in the area of international competition. This has come to be an area of increasing concern in terms of both cooperative and noncooperative behavior. In some of these issues transaction cost economics and strategic behavior are usefully joined. In others the interaction between international trade theory and strategic behavior is apt to be productive.

The study of cooperation within and between corporations requires additional theoretical and empirical work in several areas. These include joint ventures, global sourcing, licensing of technology, and international mergers and conglomeration. The manner in which trade policy interacts with R&D policy to determine the dynamic evolution of technology-intensive industries is a matter of critical importance. Excesses of protection or of free trade are both possible unless these issues can be sorted out more definitively and discriminating approaches defined.

Both economies of scale and scope are in need of refinement in the context of international competition. The ramifications of economies of scale that are internal to firms only, internal to nations but not to firms, and internal to global industries but not to firms or nations, as well as the trade-policy implications of globalization based on economies of scope have yet to be worked out.

### *Experimental Economics*

It is useful to distinguish two broad categories of allocation institutions that are represented in the development of institution-specific theory. The first is markets, which are characterized by considerable structure and organization that define the language, process, and allocation rules that apply to the transacting participants. Examples include the English and Dutch oral auctions, discriminative and competitive sealed-bid auctions, posted bid and offer pricing, double oral auctions, competitive sealed bid-offer auctions, and so on. The second category is contracting (bilateral or multilateral), which defines not only the language, process, and allocation rules but also the relationship of the parties over time, the elements that require continuing consideration for agreement, and the contingencies that permit reopening of the agreement. Whereas there are a very large number of different kinds of markets, the number of different contractual arrangements is mind-boggling. Examples include power-pooling agreements; long-term bilateral contracts for commodity, service, or power delivery; construction contracts; special warranties; and so on.

Recent experimental research in economics has been expressly motivated by recent institution-specific theory. The theoretical basis for earlier experimental research, by contrast, was the traditional monopoly, competitive equilibrium (static or intertemporal), oligopoly, core, and related theories. Since these traditional theories predicted market allocations, they could be tested with the data derived from laboratory experiments—provided that the original theory was extended and operationalized to give it a specific institutional interpretation. Had it turned out that the typical theory's predictive power was the same under all its institutional interpretations, then the justification for an institution-free economics would have been more secure. But the recalcitrant facts often showed otherwise: alternative auctioning and bidding institutions were neither equivalent nor benign, at least when numbers were "small."

Experimental work on price-posting institutions led to studies that have had an effect on policy. Studies showing the deleterious effects of price posting have had a direct effect on decisions regarding regulatory policies in the barge industry and the air freight forwarding industry. More recent research has been used in antitrust litigation regarding market organization similar to price posting. Indirectly, the posted-price research has led to the development of a body of experimental data about market organizations that have the potential of replacing traditional modes of controlling monopolies. Even these experiments evolved in unexpected ways to the recommendation of a new method of awarding international airline routes that are monopolies by virtue of agreements with other countries.

The research described above was guided by theories and policies that involved well-defined commodities and highly institutionalized markets. The major function of such organizations is the determination of price and allocation. Literally hundreds of different types of allocative institutions exist. Research efforts on understanding their differences have barely touched the surface. Experimental technique and design will be greatly facilitated as the theory is fully articulated through an extensive form institution.

In addition to the study of theories about existing market institutional arrangements, experiments have been used to assess the design of systems that are altogether new. Experiments are an inexpensive source of data to ascertain the performance of processes that have never before existed. Airport congestion and electric utility deregulation are two examples.

Decentralized bidding processes for airport use rights have been designed to fit the specific technological demands of this allocation problem and its behavioral properties compared with standard auction and



after-market trading institutions. This is an example of institutional designs based on institution-specific theory pretested in the experimental laboratory prior to field application.

Recent experimental work is investigating a decentralized market for the network exchange of electric power within the framework of long-term contractual agreements concerning transmission use rights and the level of power reserves for emergency assistance. This work is motivated by the question of whether existing institutions can be reorganized and adapted to create private market alternatives to the rate-of-return regulation of electric utilities.

The use of experimental economics to help assess the ramifications of complex economic institutions has plainly crossed a threshold: it is feasible, intellectually challenging, and operationally instructive. But it has only scratched the surface of potential applications. Theoretical and experimental refinements are sorely needed.

### Resource Needs

Although there has been growing appreciation for the importance of research on economic institutions, much of the research on these institutions has taken place outside of the mainstream. Indeed, most of this research has been done by individual scholars in a piecemeal way. Follow-up research, especially of an empirical kind, will require broader and more systematic research support.

The study of economic institutions often poses issues of an interdisciplinary kind. The advantages of addressing many of these issues from the separate vantage of each of the relevant disciplines notwithstanding, there are also benefits from a combined perspective. The latter, however, is very difficult to orchestrate. It will be done better if an educational mission is frankly acknowledged and specific interdisciplinary research exercises are set up in which established and junior scholars are brought together for a month or so to share competencies and craft a combined perspective.

#### *Computer Support for Empirical Studies of Incentive Issues*

Research on incentive issues of the kinds discussed above is both data- and computer-intensive. The data requirements are large because of the requirement of the models to be based on individual, rather than aggregate, data. To achieve statistical estimates of sufficient preciseness to make them useful requires survey data and sample sizes in the thou-

sands. Detailed data on these individuals must be collected, verified, and put into conveniently accessible form. This task is beyond the resources of any individual econometrician. Data sets used to date have been collected and created by specialized research organizations. The support of these organizations or of government agencies is necessary to gather additional data so that the effect of the 1981 and 1986 tax reforms can be ascertained. This research will permit further development and validation of the econometric models created to date.

The research also requires substantial computer resources. The interaction of the nonlinearities of the budget set and the econometric requirements to model differing preferences in the population lead to quite complicated models. While much of the research to date has been done on large mainframe or super minicomputers, more realistic models will likely require the use of supercomputers. For instance, one current model takes approximately eleven hours to be estimated on the most advanced VAX computer and about one hour on the Cray supercomputer.

### *Regulation, Deregulation, and Regulatory Reform*

An important element of work in regulatory policy, in order eventually to be of use to policymakers, is a substantial element of empirical work. In recent years, the relative emphasis has tended to favor the theoretical over the empirical, in large measure because of limited financial support for empirical research. Because empirical work is more expensive than theoretical, there has been a tendency to keep as many researchers as active as possible by giving them small grants to support less expensive work, usually of a theoretical character. The cause of the problem is a substantial reduction of support for economic research, especially in operating agencies (such as the Department of Energy, the Department of Labor, and the Department of Health and Human Services) but also in the National Science Foundation. In 1985 the source of funds most directly relevant to this area, the Regulation and Policy Analysis Program at NSF, was eliminated.

Research on regulatory reform and deregulation in the electric power area would benefit from the collection and analysis of data on the behavior and performance of electric utilities. Several state regulatory agencies have introduced "incentive regulation" provisions in an effort to provide regulated firms with better incentives to minimize costs. Neither the diverse efforts nor the results have been subject to any systematic analysis. Similarly, while the debate on opportunities to replace or supplement regulation with competition in wholesale electricity markets

goes on, market forces are coming to play an increasingly important role in allocating coordination services. Extensive interconnections and bilateral coordination contracts have led to informal markets for power in most regions of the country. How efficiently are resources being allocated by market forces? What are the unexamined alternatives? In lieu of careful analysis of actual transaction and market behavior we must rely on theoretical and impressionistic evaluations.

### *Competition Policy*

The study of competition policy is one that can benefit from an interdisciplinary perspective. Competition policy also has distinctive empirical needs. The reconceptualization of vertical integration described above, for example, was possible only upon making express allowance for the behavioral propensities of human actors as these have a bearing on the contracting process. Organization theory and economics were thus joined. Public policy applications required that knowledge of the law and of its institutions be added. A combined law, economics, and organization perspective thus emerged.

A pressing problem for future research is to place the study of "bureaucratic" failure on a parity with the study of market failure. This is a comparative institutional undertaking to which economists, sociologists, anthropologists, and social psychologists can all contribute. Organizing a series of summer seminars to meet for, say, a month in each of five successive years around this topic should help promote a deeper understanding and an eventual unpacking of these complex issues.

Empirical research on competition policy will benefit from microanalytic studies of two kinds. One involves focused case or industry studies, the other contract analysis. Successful studies of both kinds have been done in the past decade. However, these are more in the nature of pilot studies. Much more work of both kinds is needed.

The key to successful industry studies and contractual research is that this be done in a *focused* way. Our conceptual understanding of the key issues of efficient contracting, of the techniques for doing comparative institutional research, and of strategic interfirm behavior has progressed sufficiently to permit empirical assessments. Much of the relevant data are not, however, available in government, trade, or financial statistics but must be collected from primary sources with the needs of the new theory specifically in mind. This is enormously time-intensive, but pilot studies on these issues suggest that the benefits more than repay the costs. Support for Ph.D. dissertations in which microanalytic empirical research is actively favored also helps to promote work in this area.

### *Experimental Economics*

Experimental economics will also benefit from an interdisciplinary treatment of the issues. We suggest the implementation of a program to assess organizational innovations of both historical and proposed kinds. Experimental replication under a variety of test conditions is vital to our understanding of the issues.

Research would be designed to compare institutions with similar functions over a wide range of times and places in an attempt (1) to examine the relationship between the form of the institution and the resulting market outcomes; and (2) to determine if there are systematic relations between the economic (level of competition or the extent of the market, for example) and noneconomic (cultural, geographic, or temporal) environments and the structure of the institution of choice. A substantial effort should be directed toward dysfunction in institutions or toward those institutions subject to unexpected shocks since such examples make the analytical task somewhat easier.

Much of the market institution "data" fall outside of the range of items collected by a typical government agency on a regular basis; and, even when they fall within that range, the appropriate data will often involve the efforts of several agencies and will seldom be published on a regular basis, if at all. A substantial archival commitment to collect and store experimental data is needed. There appear to be potential economies of scale in making those data available to other scholars, if arrangements are made to assure easy access and the maintenance, documentation, and integrity of the materials collected.

Scholars interested in learning about the use of experimental methods should have the opportunity to locate briefly at one of the universities in which experimental methods are being used. Procedures for dealing with subjects, subtle aspects of markets, methods of dealing with problems, etcetera, are all becoming standardized and reflect a substantial accumulation about how the methods should and should not be used. The material is so complicated that it can be learned only through experience and direct observation. This transfer of technology is critical for the replication and comparison of results across experimenters and subject pools.

Equipment and supporting technical personnel are needed. As the phenomenon studied by experiments increases in complexity, the need for equipment and technicians grows. Such laboratories as currently exist are poorly funded and for the most part use equipment that was intended for other purposes. Communicating computer systems are needed. Video and audio equipment are needed for training subjects and for communicating results among researchers.

The connection between theory and experiments has always been close in economics. As the complexity of the phenomena becomes more complicated, specialization between experimental methods and theory becomes necessary. In order for theories to remain central to the experimental designs, provisions must be made to have the theorists present as critical experimental decisions are made. This need is even more pressing as the research moves to consider organizational factors that can span several disciplines. Theorists from the respective disciplines such as psychology, sociology, and political science must participate in the experimental design process. The complex issues can be resolved only by face-to-face interactions among researchers over a sustained period.

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# 13

## Markets and Organizations

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## Introduction

Research on markets and organizations has been important in several social and behavioral sciences, especially since the late 1960s. For several hundred years a central problem in economics has been to understand how a society organizes or should organize its economic activity. Until recently, the main objective has been to understand how a system of markets does its job. But there are also other mechanisms for coordinating economic activity, many of which play important parts in the existing economies of the world. One of the most important developments in economics of the past two decades or so has been the introduction of rigorous methods for the study of this broader class of mechanisms. A main objective of this effort is to provide a theory of organization and a related body of empirical knowledge and experimental results that can serve as a basis for the design of efficient organizations to carry out specified objectives of performance, taking into account the underlying constraints that apply to organizations, including those arising from limited capacity to process information and from divergence of goals among economic agents.

Study of organizations has been an important field of sociology. In sociological analysis organizations play a pivotal role because they link broad institutional change to individual action and outcomes. Individuals shape social structure mainly by participating in organizations; organizations filter the impacts of broad-scale changes for individuals. Understanding these links requires theories of (1) the effects of broad social, political, and economic changes on organizations; and (2) the effects of organizational structures on the life chances of individuals. Here, we concentrate on the first problem. Study of organizations has also been an important part of anthropology, organization behavior, political science, and social psychology. Since these disciplines were not represented in our group, we have limited ourselves to noting briefly points at which we feel those disciplines touch on the questions we address.

The objectives of research on organizations may be put in the form of questions the field needs to answer:

1. What laws or constraints govern the (design of) internal processes of organizations, including markets? These internal processes in-

- clude decision processes and processes that determine the internal objectives of organizations.
2. What relationships exist between internal processes and performance? How does performance relate to centralization or decentralization? to hierarchical or parallel structures? How should different organizations be compared?
  3. What criteria can be used to evaluate organizations, including markets and other institutions? Such criteria may be externally generated or may come from internally generated organizational goals. In economics, the criterion of Pareto optimality is widely used, and others, such as fairness, have also been studied.
  4. What laws or constraints govern the endogenous adaptation of organizations to changing environments, especially when environmental changes are unpredictable?

### *Useful Knowledge*

The acquisition of fundamental knowledge about organization is important as a scientific enterprise, but it is also important from the standpoint of usefulness to society. It has been asserted that the superior competitive performance of the Japanese in the economic sphere is due to special characteristics of their organization of economic life, in the context of Japanese culture and society. The knowledge needed to decide whether this assertion is true would come from a deeper understanding of the principles governing economic organization, of the factors and relationships conditioning the possibilities of organizational design, of the way in which performance depends on the organizational design, and of the role of historical and social factors in organizational design.

We can cite a few examples of important problems in areas to which knowledge about organization could be usefully applied. These include defense contracting, or government contracting more generally (McAfee and McMillan, 1988). (Here, the relevant scientific models are those of mechanism theory, including the so-called principal-agent models discussed below.) The granting of rights to explore or drill for oil at offshore locations is another example. (Here, the recently developed theory of auctions provides important insights (McAfee and McMillan, 1987; Wilson, 1987.) Another example is that of unemployment. The impact of economic changes on employment depends in part on the internal processes of employing organizations. The impact of an economic change, say, a recession, makes itself felt on employment as the result of a chain

of effects working its way through the structure of positions in those organizations (Akerlof and Yellen, 1986).

We are now in the middle of a profound change in the technology of information processing. In the light of the importance of information processing in organizations, this change constitutes a profound change in the technology of organization itself. Therefore, relationships that may have prevailed in the past between organizational characteristics and processes on the one hand, and performance on the other, may no longer be valid in the future. In that case, experience alone, or empirical regularities that may have been observed in the past, cannot be a reliable guide to the design of efficient organizations in the future. If, as is often asserted, organization and management are important determinants of economic competitiveness and productivity, more fundamental knowledge of the relationships governing organizations will be a valuable, perhaps indispensable, basis for policy.

We may also expect that the accumulation of knowledge in this field will eventually affect the education of managers in professional schools. While this is not a direct objective of scientific research, the patterns established in the past show that fundamental scientific progress in a field eventually influences how professional practitioners are trained. The effect of progress in microbiology on medicine is an example. The effect of the development of mathematical models of allocation, for instance, linear programming, on industrial practices in a wide range of industries—and on the training of managers—is another.

### *Formal Models*

Market systems, governmental organizations, private firms, and various other economic institutions have been viewed as instances of a general concept of resource allocation process (Hurwicz, 1960, 1972), or mechanism (Mount and Reiter, 1974). Because the concept of a mechanism has been given formal mathematical expression, it is possible to analyze mathematically important properties of different mechanisms, or organizations. This facilitates comparison of organizations and makes possible an efficiency analysis.

The explicit representation of organizations makes possible a formal analysis in which the organization is a variable of the problem; it plays the role of an "unknown" to be solved for. Important properties and features of organizations can be expressed as mathematical properties of these formal entities, and so can be used to define subsets of the set of formalized organizations. Hence they provide a basis for normative analysis via constrained optimization models.

Two types of properties have been stressed so far in the analysis of economic organization: (1) information processing; and (2) incentives. In the area of information, major emphasis has been on "bounded rationality"; on communication requirements entailed by specified performance; on the informational requirements of stability; on the informational requirements of Pareto optimality in both static and dynamic models, including an overlapping generations model; and on rational expectations. The relationships between information processing problems and methods in economics and in computer science are being examined (Mount and Reiter, 1980, 1983). Finite state automata expressing the boundedness of memory are being used to study the consequences of bounded rationality of players in repeated games (Neyman, 1985; Kalai, 1987; Kalai and Stanford, 1988; Rubinstein, 1986). Relationships between decentralized resource allocation processes and distributed computing are also under study, as is the potential impact of the availability of supercomputers.

In the area of incentives, major emphasis has been on the use of (noncooperative) game theory to study the interactions of individuals in response to their own incentives in single-period and repeated games. Many important aspects of divergence of goals among economic agents, and of dispersion of information, have been analyzed with the aid of game theoretic models.

The two strands of research come together in a number of important topics, such as principal-agent models, and the incentive aspects of economic situations involving asymmetric or incomplete information.

### *Effect on Applied Work*

These models and the results obtained from them have had a revolutionary effect on several important applied fields. Topics or fields to which such models have been applied include, among others, industrial organization, the internal economy of large firms, the study of economic regulation and of law and economics, and the study of financial markets and labor markets.

### *Experimental Work*

The past decade or so has also seen the development of controlled experimental methods in economics (Plott, 1982; Smith, 1982). (There is, of course, a long history of sophisticated experimentation with human subjects in psychology.) The ability to study interactions among human agents in economic institutions has been greatly facilitated by modern computing equipment. Many experimental studies of markets, and more

recently of other allocation mechanisms, have been carried out. It appears to be the case that the formal representation of allocation processes or mechanisms referred to above is particularly well suited to experimental investigation, because of the explicit formulation of the processes involved. Experimental work has helped to bring the models of abstract theory to useful application.

While (nonexperimental) empirical research on organizations in economics has tended to rely on econometric methods, research in sociology has used surveys and direct observation of internal processes of organization. Instruments such as questionnaires are frequently used in sociological research. The theoretical formulations used in sociology are rather less formal and mathematical than those seen in economics. The now standard approach to organizational research in sociology is *contingency theory* (Galbraith, 1973; Lawrence and Lorsch, 1967; Thompson, 1969). This stresses the influence of uncertainty and change in the environment on organizational forms. The importance of social values and norms in shaping individual behavior, and therefore organizational performance (Di Maggio and Powell, 1983; Meyer and Rowan, 1977; Meyer and Scott, 1983; Parsons, 1956), has been stressed, as has the influence of networks of social relations on the behavior of individuals (Baker, 1984; Faulkner, 1983; Granovetter, 1985; White, 1983; White, Breiger, and Boorman, 1976). In more current lines of work, attention is being paid to the effect of internal political conflict on resource allocation and on organizational performance (Pfeffer, 1981; Pfeffer and Salancik, 1978). The notion of efficient organization is important in sociology, as it is in economics (Eccles, 1981).

### *Other Approaches*

Another approach, used in both economics and sociology, views organizational structure as the result of an evolutionary process (Carroll, 1985; Hannan and Freeman, 1977, 1984, 1986, forthcoming; Nelson and Winter, 1985). Competition among and survival of organizations are modeled as an evolutionary process analogous to natural selection. Models of population ecology are also being used in sociology to study populations of organizations (Carroll and Delacroix, 1982; Delacroix and Carroll, 1983; Freeman and Hannan, 1983, Hannan and Freeman, 1987).

Economic historians have studied the appearance of and changes in economic institutions in different places and times, and have sought to account for the development of specific institutions in historical and economic terms.

Some agricultural economists, interested in institutional change, have also studied innovation of institutions. Like some economic historians,



they view institutional changes as endogenously generated. In one view, institutional change is a response to a demand derived from demand for improvement in production.

We have mentioned the existence of mathematical models in which the concept of organization has an explicit formalization, and in which various important properties of organizations can be expressed mathematically. This opens the way to relating different investigations and contributions to one another. This is essential, for it permits investigations to build on the basis of preceding ones, and so for knowledge to accumulate in a coherent structure. The existence of a rigorous general model allows each investigation to be justified by its relationships to elements of the general structure. This permits an investigation to contribute one brick to the building under construction. We may therefore hope to escape from a situation described by the sociologist David Zeaman, "One of the differences between the natural and the social sciences . . . [is that] in the natural sciences [we are told] each succeeding generation stands on the shoulders of those that have gone before, while in the social sciences, each generation steps on the face of its predecessors."

The broad picture presented by the research referred to above is one of great activity and high promise for future research. The same underlying problem—to elucidate the laws governing the processes and performance of economic organizations—is being approached from several points of view in different fields.

The research developments sketched above form a solid base of models, techniques, and results for research on organizations and markets in the next decade. A substantial development of these models is needed to extend and enrich theory by incorporating phenomena important to understanding organization, and to derive and test new results.

### **Selected Directions of Recent Research Mechanisms**

We present here a more detailed account of certain important lines of research on organizations, including markets. We begin with existing formal models of allocation processes in economics. These have the following elements:

1. A relation between the set of environments regarded as possible and the set of possible outcomes or actions. This relation expresses desired performance.

2. A space of messages, signals, or strategies, expressing the behavior open to individual agents in the process.
3. A relation between complexes of messages or strategies, and outcomes, determining which outcome results from any given complex of message or strategy choices of the agents.
4. An initial distribution of information about the environment among the agents.

The elements listed above are used to define a *game form*, a model used to analyze incentives (Gibbard, 1973; Hurwicz, 1986). In that structure the solution concept employed induces a relation between environments and strategies, which expresses the strategic behavior of the agents; for example, if the solution concept is Nash equilibrium, then the relation induced is the correspondence that associates to each environment the set of Nash equilibrium strategies of the players for that environment.

The set of admissible relations between environments and strategic choices, or messages, can be restricted, thus expressing restrictions on behavior of agents. Where the focus of investigation is on informational issues, the relation between environments and messages is, in some cases, specified in advance. This may be regarded as a limiting case in which the domain of admissible relations is reduced to a single element.

The elements of this model together constitute a formal representation of a system for organizing economic action, that is, an organization (Hurwicz, 1981, 1986; Radner, 1987a).

One basic formulation of this type of model in economics is dynamic. The process of interchange of messages among the agents is represented by a set of difference or differential equations.

### *An Example*

It may be helpful to have before us a concrete example to which we can refer the concepts and theorems of these models before continuing a discussion of them in more general terms.

The renegotiation of defense contracts provides a transparent illustration of the way in which this type of mechanism model captures essential features of the situation and of the relevance of analytical results obtained in those models. Early in World War II, a concerted effort was made to convert industrial production to defense. At the same time, there was concern that there should be equity of sacrifice, in particular that "profiteering," so prominent in previous wartime periods, should

be prevented. Cost-plus contracts were used to induce firms to convert to defense production, and the renegotiation of these contracts to recover "excess profits" was to be the instrument to prevent profiteering. Congress passed legislation providing for the renegotiation of government contracts. The act specified five factors to be taken into account in determining whether a given profit was excessive. These included such items as *unusual efficiency*, *special value to the defense effort*, a sufficient *degree of innovation*, and the like. A federal agency, the Renegotiation Board, was set up to administer the act.

Connections between this example and the models referred to above may be seen as follows:

1. Specification of the goals or desired outcomes. This is expressed in the legislation by defining (implicitly) allowable profits.
2. The information processing tasks entailed by the goals in the context of the conditions that prevail outside the control of the Congress or the administrators of the act; for example, technical conditions of production. In this case the task of gathering data on costs, on comparative efficiency of production in different firms, on technical innovation, and on the relative values of different products or acts of production to the defense effort, is implied by the information needed to realize the goals expressed in the act.
3. The incentive problems implicit in the situation. Incentive issues appear in several forms. Cost-plus contracts are used expressly to induce firms to change over to defense production. Having done so, firms have an incentive to inflate costs and, in providing information to the renegotiators, to rationalize as normal as much as they can of the actual profit made.

Because of the informational and incentive aspects of the problem, it is likely that the performance achieved by the mechanism actually set in place bears only a very rough approximation to the performance prescribed by the Congress, especially in view of the fact that the resources given to the Board to administer the act bear little or no relation to the informational requirements imposed by the goals. Was this the best, or even an acceptable, way to achieve the desired goals? The formal structures of mechanism theory provide an analytical framework in which such considerations can be formalized and in which such questions can be addressed.

We may now add a few words in more general terms, explaining further the four elements listed above and giving a sample of some results on the informational side:

1. A relation between the set of environments regarded as possible and the set of possible outcomes or actions. (An environment includes the economic data, such as preference, technological relations, resources, and the like, relevant to the desired outcome but not subject to the control of the economic agents or the designer of the process.) This relation expresses the goals or performance desired of the economic mechanism. Modeling of the environment may be timeless or involve intertemporal relations, such as production, consumption, and accumulation of capital over time.
2. A space of messages, signals, or strategies. This expresses the behavior, whether strategic or simply communicative, open to individual agents in the process.
3. A relation between complexes of messages or strategies and outcomes, determining which outcome results from any given complex of message or strategy choices of the agents. This relation, usually called the outcome function, formalizes institutional arrangements, for example, in the context of public goods economies, the tax laws, and rules for determining the production of public goods. This relation expresses the way in which the institutions translate final messages or strategy complexes (i.e., the behavior of agents) into outcomes.
4. An initial distribution of information about the environment among the agents. Usually, what one agent knows directly about the environment is private information, either not observable by others or, in some formulations, partially observable at a cost.

### *Information*

As noted above, the basic formulation of this type of model is dynamic, modeling a process of interchange of messages among the internal parts of the organization. As the renegotiation example makes clear, the communication requirements imposed by a specified performance are essential dimensions of the problem of mechanism design. The communication requirements of various kinds of stability have been studied in the context of dynamical systems that achieve the specified performance (Jordan, 1987; Mount and Reiter, 1987). Analytical difficulties with dynamic mechanisms make it easier in some cases to study associated static mechanisms. The equilibria of the dynamic system are used to define a (static) equilibrium relation between environments and equilibrium messages. This formulation has been used to study the communication requirements of a given desired performance (Hurwicz, 1972, 1977; Mount and Reiter, 1974; Sato, 1981; Chander, 1983).

Results of these investigations are in the form of lower bounds on the communications required by any organization in the set of organizations defined by the model to achieve the specified performance. These theorems tell us that for any organization to be capable of the specified performance, it must be provided with the capacity to communicate among its members the values of at least the number of variables given by the applicable lower bound theorem.

In some cases, the lower bound has been shown to be infinite, e.g., when increasing returns to scale prevail in production. Such a result may be interpreted as telling us that we must settle for second-best performance. It is notable that this trade-off between performance and informational costs follows from communication constraints alone.

Analogous informational theorems have been obtained for models where the focus is on incentive issues. The communication requirements of implementing a specified performance relation by a game form in dominant strategy equilibria, for example, have been analyzed, and a theorem giving a sharp lower bound on the number of strategic variables each agent must have has been obtained. Similar results exist for some important cases of implementation in Nash equilibrium and Bayes equilibrium.

In addition there are results giving methods for constructing mechanisms with minimal communication requirements that can achieve a specified performance relation (in cases that meet certain technical regularity requirements).

Constraints that apply to organizational design and performance include those related to the complexity of the required information processing, a major subject of investigation in mathematical and computer science. This has also been studied in mechanism theory. It has been shown, in the context of one formal model of computation and complexity, that trade-offs can exist between the degree of complexity and the number of variables communicated among agents in an organization achieving a specified performance. It has also been shown, by modeling each player in a two-person repeated game, such as the finitely repeated Prisoners' Dilemma, as a finite state automaton, that the bounded memory capacity of each player can be used by the players to reach a cooperative equilibrium (Radner, 1986a).

Results of this kind are highly relevant for understanding the performance of organizations and for guiding the design of organizations to attain a specified performance.

It should be noted that the development of statistical decision theory following World War II contributed to a wide variety of economic models by providing a foundation for rational decision making under uncer-

tainty. The theory of teams, which is rooted in statistical decision theory, is an important contribution to the study of organization in economics (Marschak and Radner, 1972; Marschak, 1986).

### *Principal-Agent Models*

On the side of incentives, the large body of work on principal-agent models and models with asymmetric information has already had profound effects on the way in which institutional and organizational arrangements are viewed (Mirrlees, 1974; Holmstrom, 1979; Grossman and Hart, 1983; Arrow, 1986). The fields of industrial organization, labor economics, and managerial accounting, among others, have been fundamentally changed by these models. A similar development is taking place in the field of law and economics. The legal framework of social and economic life includes, of course, important institutions of society. Many of the important areas of law, such as contracts, are being analyzed at a level of fine detail, using these models and the techniques for working with them (Hart and Holmstrom, 1987). Sociologists are interested in these models and some are relating them to their own research. Indeed, the past decade has seen an explosion of work on organizations that falls into the category of principal-agent models. The neoclassical model of the firm or other organizations in economics did not explicitly model the information that various agents in the organization had, or were able to observe. At the heart of the principal-agent model is the careful modeling of precisely what information each of the agents in an organization has at the outset, and what information he is able to observe during the operation of the organization. This careful modeling is particularly important for problems in which there is asymmetry of information, that is, where one party knows more about some aspects of the environment in which an organization is operating than others. One of the first applications of this manner of modeling organizational problems was to the so-called sharecropper problem (Cheung, 1969; Stiglitz, 1974). This problem is of interest because it contains in a simple setting phenomena of general importance. The problem involves the nature of the contract, commonly observed in both the United States and other countries, between a landowner and a tenant farmer, by which the parties share the output in some fixed proportions. This contract was somewhat difficult to explain given the previously generally accepted models of the economics of organization, because it resulted in the landowner and the farmer bearing the same amount of risk as to their total compensation from this activity. The accepted models in economics suggest that the risk should be borne asymmetrically, with the landowner bear-

ing more of the risk than the tenant farmer, since the possibility of a bad crop (and hence a small share to each party) would be much more serious for the relatively low income tenant farmer than for the landowner. Conventional models in economics would have suggested that the landowner, who is much more able to absorb such risk, would in fact bear most of the risk.

What seems to be anomalous from the point of view of previous models of organization is much easier to understand when viewed in the light of the principal-agent model. The emphasis on the possible asymmetries of information in monitoring difficulties led people to focus on the problems that would be raised by a contract between the tenant farmer and the landowner in which the landowner bore most of the risk. Such a contract would leave the tenant farmer with a share of the output that did not vary much as the total output varied. Such a contract would cause incentive problems as the tenant farmer would be likely to exert less effort in farming activity if his share was not likely to be affected significantly by such exertion. The tenant farmer will obviously know the amount of effort exerted in farming activity, but the landowner typically would only be able to observe such activity at very high cost. Having carefully modeled the information and the monitoring difficulties, researchers then asked what kinds of contracts would lead to the largest net output (output minus the cost and effort of producing this output). When done carefully, the model will show that the kinds of contracts and organizational structure that we observe are in fact consistent with these models of organization.

The principal-agent paradigm has been extremely important in many areas of organizational behavior in rationalizing the kinds of organizational structures and interactions that we observe. There are many questions of public policy that presuppose some notion of organizational behavior. The entire question of what kinds of contracts and transactions firms should be allowed to engage in can be analyzed only with some hypothesis in mind as to why firms would want to utilize such structures or transactions. A specific question of this sort addressed in recent years has been the question of retail price maintenance. Retail price maintenance is the practice by which a firm will sell goods to an intermediate firm but restricts the price at which the goods can be resold to the final customer. This practice has at various times and places in recent years been both legal and illegal. The discussion of whether it is in the ultimate customer's interest that such practices be prohibited cannot be answered without some explanation as to why the original firm wants to restrict the price at which the goods can be resold. Consistent explanations as to why a firm would want to do so have been lacking until

recently. The principal-agent model has provided explanations that many people find plausible. This model facilitates careful consideration of the conditions under which such practices would be in the best interest of the ultimate consumer. In a similar matter, the question of whether it is in the general interest to encourage or discourage mergers between large firms can be discussed sensibly only if there is some understanding of the purpose that the merger might serve. In the case of a vertical combination, that is, a merger between a firm and one of its suppliers, it has long been a question as to what advantages a merger would serve that could not be served equally well by some sophisticated contract between the two firms (Williamson, 1975; Grossman and Hart, 1986). A focus on the difficulty of monitoring contracts between separate firms, as opposed to internal processes in a single (merged) firm, has led to a better understanding of the purposes served by a vertical combination (Alchain and Demsetz, 1972; Hart and Holmstrom, 1987; Holmstrom and Tirole, 1988). This better understanding provides the groundwork for a more informed policy regarding such business practices.

The above examples focus on the need for models that better explain the existing structure of organizations. Questions involving the benefits of changing organizations can be answered only if we understand why organizations are structured as they are in the first place. Besides giving us a range of models to better understand existing organizational structures, the principal-agent model has also helped in the design of alternative organizational structures. Once we have carefully specified the kinds of information and incentives that agents within an organization have, we can begin to ask what organizational structures and reward schemes will induce the outcomes that we would like. We mentioned the problem of the tenant farmer above as an example in which the principal-agent paradigm was helpful in understanding why a particular type of organization evolved. For the same problem we could use these techniques of modeling to ask whether there were alternative organizational structures. Since people have had centuries to experiment with alternative organizational structures it is not surprising that the structure that we see now is efficient relative to the alternatives. There are many problems of organization for which we have not had centuries of experimentation. The careful analysis of such organizational problems, using the principal-agent paradigm, allows us to identify, at least in crude form, the kinds of organizational structures that are optimal for the problems we face. The principal-agent paradigm has been extremely successful in several areas; among the most successful is managerial accounting. Managerial accounting deals primarily with the question of how firms use accounting information for internal operations and man-



agement. This is opposed to financial accounting, which has to do with the use of a firm's accounting data by people outside the firm. The internal use of information by a firm has to do precisely with the question of monitoring the activities of the various parts of a firm and the use of that information to reward the employees of the firm. The principal-agent model has revolutionized the way in which managerial accounting is thought about and is taught in business schools. To a large extent, the principal-agent model has *become* managerial accounting. While some of this may be faddish, such an extensive change in such a short time in the way this field is viewed by its practitioners must be taken as evidence of the usefulness of the principal-agent model for analyzing organizations.

While the contribution that the principal-agent paradigm has made to our understanding and analysis of organizations is substantial, the possibilities for extending and applying the paradigm over the next decade are equally exciting. It has begun to be used extensively in international trade problems to model the relationship between industries in various countries and the governments that must set the rules for competition, with only partial information as to the internal structure of the industries. Many of the techniques of the principal-agent model are now being applied to bargaining and arbitration problems (Chatterjee and Samuelson, 1983; Myerson and Satterthwaite, 1983; Roth, 1985). The application of bargaining problems in which an arbitrator is only partially aware of the costs and benefits of various alternatives to the parties involved is difficult, but some progress has been made. Last, there have been initial attempts to study the evolution of organizations over time in an attempt to understand what characteristics of organizations best allow them to have the flexibility to respond to changes in the environment.

A forerunner of the literature on principal-agent theory was Simon (1953). Early contributions to the analysis of static principal-agent models were made by Ross (1973), Stiglitz (1974), Mirlees (1975, 1976), Hurwicz and Shapiro (1978), Shavell (1979), and Holmstrom (1979). For further analysis and references see Grossman and Hart (1983) and Stiglitz (1983). For results and references about repeated principal-agent relationships see Radner (1986b).

### *The Internal Economy of Large Firms*

The large size of modern firms forces the de facto decentralization of information and decision making, whatever the structure of formal organization. Even the smaller firms are forced to decentralize information

to some extent, given the very limited capacities of decision makers to observe, process, and communicate information. This is even true of decision makers aided by large computer systems.

Table 12.1 gives some idea of the size of the largest U.S. firms, as reported by *Fortune* magazine in its report on "The Fortune 500" for 1983. In terms of employees, the largest is General Motors, with approximately 700,000 employees. This is roughly the same as the number of persons employed in manufacturing in all of the Netherlands in the same year. Most of the Fortune 500 are, of course, much smaller; the mean is 28,100 employees, and the median is 13,100. Nevertheless, it is clear from the table that the internal economies of these firms are very large indeed.

The private information of a decision maker gives him some measure of private power to pursue his own goals, which may sometimes be in conflict with corporate goals. The private power conferred by informational decentralization and the conflicts of interest among the members of the firm will in general give rise to inefficiencies, whatever the structure of formal organization, compared to what would be ideally attainable with the given distribution of information among the decision makers.

Although uncertainty obviously makes decision making more complex, the picture of the firm as headed by a rational, profit-maximizing entrepreneur persists in the mainstream economic model of the firm, although not without dissent. The mainstream model was fortified by the burst of activity in statistical decision theory and by operations research during and after World War II. This activity not only showed that very complex decision problems could be solved but also led to fundamental reconsiderations of the foundations of the theory of probability and uncertainty.

One current of dissent stressed the importance of "bounded rationality" in the decision making of individuals and groups. Another current questioned the mainstream picture of the "unitary" direction of the firm, especially arising out of the separation of management from ownership. This separation of owners and managers gives rise to a loss of control of the managers by the owners. The discretion of the managers to do things other than maximize the profits of the firm is enhanced by any monopoly power of the firm, the actual degree of which is better known to the managers than to the owners. This partial loss of control extends to the relationship between the management and the workers (or between different levels of management). It is possible for the owners to improve their monitoring of the information and actions of managers and

**Table 12.1 Some Large U.S. Firms, 1983**

Sales Rank	Company	Sales \$ Bill.	Assets \$ Bill.	Net Income \$ Bill.	Number of Employees in Thousands	Number of Employees (Rank)
1	Exxon	88.6	63.0	4.98	156	(9)
2	General Motors	74.6	45.7	3.73	691	(1)
250	Monfort	1.32	.199	.016	3.5	(472)
308	Ex-Cello	.955	.690	.050	13.1	(250)
500	Tandem	.418	.415	.031	4.4	(454)
	Mean	3.37	2.71	.138	28.1	

Source: "The 500," *Fortune*, April 30, 1984, pp. 274-322.

workers, but only with the expenditure of resources, and so the net benefit to the owners of such increased monitoring may not be unambiguously positive. Formal models of this loss of control were introduced in a more general setting (theory of incentives in teams). The post-World War II development of the theory of games provided the methodology needed for the formal analysis of the conflicts of interest within the firm, and it is the methodology of game theory that underlies most of the recent developments.

We should emphasize three points. First, there are many "players" in the firm's internal economy: shareholders, directors, managers, and workers (and sometimes creditors). These different players typically have at least partially divergent interests, hence, the difficulty of imputing to the firm a single objective. Among the objectives of the several players in the firm are profits, growth, monetary compensation for managers and workers, quality of work, perquisites, and status. In addition, in the pursuit of these objectives, the different players may have different attitudes toward risk.

Second, the constituent parts of a firm may have technologies that do not conform to the "neoclassical" hypotheses. The internal economy of the firm will usually be full of instances of increasing returns to a scale (up to a point), indivisibilities, and externalities. These technological features may also be important at the level of the entire firm as well, and the neoclassical hypotheses about the technology of the firm are no doubt inadequate for the analysis of many questions about the interactions of firms in markets. For a theory of the internal economy of the firm to be successful, it must broaden the neoclassical assumptions for the firm's constituent parts.

Third, a study of the internal economy of the firm should pay attention to the structure of information processing within the firm (observation, communication, computation), and also to the structure of authority, which limits the things that the players may and may not do (Dalton, 1959).

The rules for the transfer of goods and services from one part of the firm to the other, and for the pricing of such transfers, are not given a priori but can be designed by the firm itself, as can the rules for compensating the managers of different units and at different levels. Indeed, the very division of the activities of the firm into separate units can be determined by the headquarters.

The following summary lists the main conclusions for the internal economy of firms from study of the existing models of decentralized decision making:

1. Equilibria of decentralized organizations are typically inefficient, relative to what could theoretically be attained with the given distribution of information among the several decision makers.
2. The inefficiency of equilibria is caused by various incentive problems that arise from conflicts of interest among the decision makers and/or between the decision makers and the organizer. These incentive problems can be classified under three headings:
  - a. misrepresentation
  - b. moral hazard
  - c. free-riding
3. Misrepresentation can be remedied by suitably designed compensation mechanisms, provided "budget balancing" is not required (Groves-Clarke-Vickrey mechanisms).
4. The inefficiency due to moral hazard alone may be remedied in long-term equilibria to the extent that the players are not too short-sighted, that is, to the extent that they do not discount future utility very much (principal-agent games).
5. Free-riding and moral hazard together may be only partly remedied in long-term equilibria (of repeated games) with far-sighted players, unless there are individual measures of performance that are publicly observable (partnership games).
6. Efficiency can be improved in long-term equilibria with far-sighted players if they are not too "greedy" (approximate equilibria).
7. Long-term games (repeated games) typically have an infinite number of equilibria.

The fact that long-term games typically have an infinite number of equilibria poses a problem for the theory, because it makes the predictions of the theory relatively imprecise. The situation is even more extreme than is implied by the statement that the set of equilibria is infinite; equilibria of a long-term game are typically not isolated, so that an equilibrium will have other equilibria arbitrarily close to it. The situation is analogous to that of a ball on a slightly sticky horizontal surface; the ball will stay put wherever you place it, and small shocks to the ball will result in correspondingly small displacements.

This state of affairs is probably not a comfortable one for contemporary economic theory, which tends to explain observed economic states as locally stable equilibria in a system with a finite set of equilibria. On the other hand, a theory that leads to a continuum of equilibria is open

to completion by adding considerations of history, the social and cultural environment, and an explanation of the particular equilibrium that is currently observed. This could lead the economic theory of organization back toward the older institutional and historical approaches from which it rebelled not so many decades ago (Hess, 1983; Radner, 1986c; White 1983).

### *Labor*

We have referred in the Introduction to new approaches to analyzing the effect on employment of changes in economic activity. There is a big difference between labor employed in small units highly permeable to market influences and labor in large organizations with their own internal labor markets (Granovetter, 1984). Both economics and sociology have incorporated this distinction in recent research. In economics, the realization that many workers are employed in large organizations with internal processes in allocating labor has spurred interest in the allocation efficiency of those internal processes (sometimes referred to as internal markets) and in the allocative efficiency of the large organizations themselves. Here, the impact of specific human capital and transaction costs of living and training are important elements. The focus on larger organizations has encouraged the development of formal models to analyze the long-term relationships that develop. This has led to the literature on implicit contracts, deferred compensation, and mandatory retirement. Conflicts of interest among those making work assignments and those executing them have been studied with the aid of principal-agent models.

In sociology the field of labor is also a "hot area." There was already a substantial literature in "industrial sociology" dealing with the day-to-day operations of large organizations by ethnographic research methods (Abegglen, 1958; Blau, 1955; Buraway, 1979; Burns and Stalker, 1961; Crozier, 1964; Dalton, 1959; Gouldner, 1954; Selznick, 1949; Whyte, 1955). This literature had not been integrated into more systematic work on the overall structure of labor markets. Recent work has corrected this imbalance (Baron and Bielby, 1984). Research on contact networks in labor markets has studied how such networks, based on both professional and social contacts, allocate workers to positions in the status hierarchy of society as well as in its organization of productive labor (Boorman, 1975; Granovetter, 1974, 1981). Many recent sociological monographs have traced the impact of the labor market position of an individual, increasingly identified by the characteristics of the employing organization, on his status and life chances (Sorensen and Tuma,

1981). The extent to which personal unemployment results from the details of one's career history is also discussed in this literature, which incorporates analyses in economics of heterogeneity versus state dependence as causes of labor market position (Granovetter, 1986; Sorenson and Tuma, 1981).

Work in sociology beginning in 1970 has opened up another line of research (White, 1970). When new positions are created or old ones vacated by death or retirement, a cascade of vacancies flows through a system of organizations, as each position is filled by someone who leaves a vacancy in his previous position (White, 1970; Stewman and Konda, 1983). Mathematical models of such cascades make better predictions of system turnover than models oriented to characteristics of individuals, and cascade models focus attention on exogenous demographic constraints on mobility: rates of retirement and of entrance of new recruits, and macroeconomic parameters such as rates of creation and abolition of jobs (Jacobs, 1981; Pfeffer, 1983; Rosenbaum, 1978; 1979a and b; 1981). Research stemming from these ideas has shed important new light on the demography of promotion and the structure of internal labor markets (Granovetter, 1974, 1981; Rosenbaum, 1978, 1979a and b; 1981; Stewman and Konda, 1983).

Though the two disciplines of sociology and economics have independently made rapid progress in understanding labor markets, the most exciting prospect lies in a surge of interdisciplinary work in the coming years. The general theoretical frameworks of sociology and economics have converged somewhat, and each has important insights to offer to the other. Sociological work will benefit from the discipline of formal models and the emphasis on efficiency and on the optimal structuring of incentives. Economic argument will become more sophisticated when it takes account of the structure of social relations, the impact of noneconomic motives and the constraints of demographic stocks and flows (Granovetter, 1985; Pfeffer, 1983; White, 1983). Interdisciplinary work will enable these two bodies of already booming research to complement one another in ways that should yield genuine breakthroughs.

### *Experimental Economics*

One of the important developments in economics over the past two decades or so is the emergence of controlled experimentation. A substantial number of experimental studies has been conducted on markets and more recently on other types of organizations for allocating resources (Smith, 1982). Experimental work mirrors theory. Experimental methods require specification of the detailed structure of the processes op-

erating in the market or organization under study. The models of mechanism theory described above appear to be particularly well suited to the needs of experimental research, because these models contain explicit formulations of essential processes. This connection between mechanism theory and experimentation has already led to fruitful interaction in which theoretical investigations provide models for experimenters and experimental results provide guidance for new theoretical formulations. An illustration of this sort of interaction is given by a recent experiment involving a mechanism (the Walker mechanism) for allocating a public good. This mechanism has been shown to yield certain optimal allocations (Lindahl allocations) at its Nash equilibria, but it does not guarantee individual feasibility, that is, individuals can experience bankruptcy (negative payoffs) as a result of the actions of others in the experiment (Walker, 1981). This possibility is not acceptable in the experimental setting. The experimenter introduced a variant mechanism to prevent bankruptcy, but this opened the possibility that the strategic structure of the game was distorted. Stimulated by this situation, a graduate student at Minnesota came up with a modification of the Walker mechanism that preserves its essential strategic structure but guarantees individual feasibility. There has also been deeper analysis of the variant mechanism by another theorist. What might appear to be a mere theoretical nicety turned out to be a crucial point for experimental investigation; in turn, the experimental need gave rise to improvements of theory.

Experimental work has confirmed some theoretical contributions. It has supplemented theoretical models with the detailed arrangements needed to put them into practice, producing important organizational and institutional design achievements. It has also opened up new issues for theory as the limits of applicability of certain theoretical models are found. These matters are discussed below in more detail.

The pattern of results from experience demonstrates that the game-theoretic approach to understanding auctions has much power of explanation. In first-price auctions and in second-price auctions the Nash equilibrium model is remarkably accurate for most parameter values. It is as though individuals are capable of calculating incredibly complicated game-theoretic solutions. Hundreds of observations document the phenomenon. The importance of the discovery is derived not only from its practical usefulness in understanding auctions but also from the applicability of the deep insights derived from mechanism theory, should the Nash equilibrium prove to be behaviorally reliable.

The Nash equilibrium concept was clearly not the end of the story. As research moved from auctions in which no information processing is



involved to auctions that involve a heavy information component, paradoxes began to emerge. For example, a winner's curse can be observed in common-value auctions. When estimating the value of an object, some participants will draw information that suggests the object has a value much higher than it actually does. It is a purely statistical phenomenon that should be discounted when tendering a bid. If it is not discounted, the winning bid will actually be higher than the value of the object—the winner's curse. Why the participants fail to anticipate the strategic interactions under this institution is a challenge to theorists and to experimentalists. If the phenomenon persists under close examination, theorists will be forced to search for a modification of basic principles that are now widely applied. In addition, a search will be initiated for institutions that prevent what can be perceived as a problem. On the basis of current theory, the phenomenon should not exist. Data from related experiments also suggest that it should not exist. The lone voice that has claimed otherwise has been that of oil companies involved in bids for leases; they, of course, were believed by no one. Existing data suggest they should be taken seriously.

The mechanism literature has added a new type of question to those traditionally asked by economists. Is it possible to design a set of institutions that solve a problem and use only a limited, clearly stated set of behavior principles? the Groves/Ledyard process was an answer to such a question regarding the provision of public goods. The response of experimentalists has been along two lines. First, an effort was made to explore the reliability of the Groves/Ledyard process. The positive results encouraged experimentation with closely related processes. Based upon the foundation of this work, a series of experiments was performed for the Public Broadcasting System on ways to choose programs and allocate the costs among member stations (Forejohn et al., 1979). The most recent application was a check to see if one of the proposed processes was sufficiently reliable to allocate the common costs that would exist on the space station now being designed by NASA. Initial experiments indicated that the process, while a great advance over other processes, was nevertheless not sufficiently reliable to use as a foundation for the cost-recovery problem. The experiment provided an inexpensive means of testing the process. A field test could have cost hundreds of thousands of dollars in mistakes and delays. Experimental debugging of new ideas is much less expensive.

The design motivation from mechanism theory can be clearly identified in the most recent attempts to solve the congestion problem at airports. Litigants at Westchester County, New York, recently achieved an out-of-court settlement on a method of allocating rights to use the ter-

minal facility. The rights are to be distributed by lottery with an after-market—a process first proposed for Washington National Airport as the product of experimental analysis (Grether et al., 1981). Parties at Westchester had agreed to an auction process in which a new method of tendering bids would define those parts of the facility a carrier would use. The Westchester process was constructed upon a previously studied decentralized auction mechanism for allocating landing rights. The Westchester process was “near” demand-revealing in the mechanism sense, and was therefore “near” perfectly efficient. Like its predecessor, it was designed with the aid of mechanism theory and debugged with experiments. The auction mechanism would have been used except for intervention by the Federal Aviation Administration. Similar auction mechanisms are under review by the New York Port Authority for potential application at the major airports. In all cases, the processes being used and those under consideration have evolved from the benefits of both mechanism theory and experimental analysis.

A third area of investigation is the phenomenon of asymmetric information in markets. The topic was originally explored in the context of markets that involve expert advice such as mechanics, insurance, consultants, and physicians. With the basic framework established, the investigation has expanded to include insider warranties and auditing contracts. Many experimental studies are now underway to explore the nature of principal-agent relationships.

For formal treatments and overviews of the literature on market equilibrium under uncertainty, including theories of rational expectations and “symmetric” information, see Radner (1981) and Jordan and Radner (1982). For a less technical summary see Radner (1987b). On overlapping-generations models and “sunspot equilibria,” see Shell (1987).

### *Organizational and Market Uncertainty*

Businessmen are responsible for making economic decisions in the face of uncertainty. Some of this uncertainty is the result of uncertainty about economic fundamentals—tastes, endowments, and production possibilities. The weather, for example, affects the economy through its influence on crop yields and on the final demand for umbrellas and swimsuits. This type of uncertainty, transmitted to the economy through uncertainty about the fundamental parameters of the economy, is successfully modeled by conventional general-economic-equilibrium theory.

Businessmen face another, significant source of uncertainty. This is market uncertainty, generated within the economy itself, about the eco-

conomic outcomes (such as prices) given the fundamental parameters. Businessmen wonder whether the economy will be healthy or unhealthy, whether consumers' confidence will be high or low, whether credit will be loose or tight, whether prices for their factors will be low or high, and so forth. Most businessmen quite properly worry more about the uncertainty of the outcome of the market process than they do about the uncertainty of the fundamental parameters.

The formal modeling of market uncertainty until recently has lagged far behind the modeling of uncertainty about its fundamental parameters. The recent work in economics game theory on correlated equilibrium (Aumann, 1974, 1987) and so-called sunspot equilibrium (Cass and Mitra, 1987; Cass and Shell, 1983; Peek and Shell, 1987; Shell, 1977) represents substantial advances in our understanding of market uncertainty. We now have serious rational-expectations models in which the equilibrium allocation of resources can be probabilistically distributed even if the economic fundamentals are immune from random disturbances. We can show how economic actors can be uncertain as to whether markets will be "thick" (with many trades taking place) or "thin" (with few trades taking place), or even whether these markets will be open or closed. In attempting an assessment of market thickness, an economic actor must judge the confidence of other economic factors. A wide range of such beliefs is rational (i.e., confirmed by subsequent events). If, for example, demand in a particular market is weak, then in response supply is weak, which in turn justifies (establishes the rationality of) the weak demand.

Market uncertainty—or, more generally, organizational uncertainty—is a topic of obvious social importance. Macroeconomics, for example, asks what policies the government should adopt to counteract disturbances generated by the economy itself. When we design organizations—or choose mechanisms—we shall want to know how uncertainty is created and transmitted by these new forms.

This is one of the most promising areas of research for the next decade. The problem is there. A start has been made, but much more work remains. Since the economic models of market uncertainty (or organizational uncertainty) are not determinate, there are serious grounds here for collaboration with social scientists from other disciplines.

### *Dynamics*

Allocation processes have been studied in a dynamic context not only in the sense that the internal processes of communication have been modeled in time but also in the broader sense that action unfolds in time.

Some results on the informational requirements of Pareto optimal allocation have been obtained for an infinite horizon overlapping generations model (Balasko and Shell, 1980, 1981a, b; Majumdar, 1987; Samuelson, 1958; Shell, 1971). In addition, there is an extensive, sophisticated literature devoted to analysis of markets in the context of capital theory, where intertemporal phenomena are of the essence. Overlapping generations models have helped explain the role of money, a problem previous analyses could not handle. They have also been used to study the role of expectations in macroeconomics.

Another important area of intertemporal phenomena is that of exhaustible resources (Cass and Mitra, 1987). Dynamic models have been used to analyze optimal utilization of exhaustible resources, such as petroleum reserves. The area of economic dynamics, broadly understood, has had a strong development in the past two decades, and has a solid base of achievement in methods and results important for the study of organization, including markets, in the coming decade. The connections with nonlinear dynamics, currently undergoing rapid development in mathematics and being applied in the physical and biological sciences, opens up interesting possibilities for use in studying organizations, particularly in light of the increased availability of supercomputers. Particularly interesting is the possibility of obtaining fluctuations that come from the structure of the mechanism itself, without a stochastic source, as is the case with chaotic dynamics.

### *Research on Organizations in Sociology*

The past twenty years have seen considerable progress in theory and research on the determinants of organizational structure. The first phase in this program of research developed what has come to be known as contingency theory (Thompson, 1967). According to this perspective, optimal organizational design buffers the technical core (the actual processes of production) from environmental shocks by using peripheral structures specializing in deflecting or absorbing environmental turbulence. The optimal design depends on the detailed needs of the production system and the nature of environmental variations (Stinchcombe, 1959, 1965; Thompson, 1967; Woodward, 1965). This approach, now the standard textbook theory in sociology, has proved useful in sociological analysis of a broad range of market and nonmarket organizations in diverse cultural contexts (Scott, 1975).

Research in the past decade has added two complications with far-reaching implications: organizational politics and institutional constraints. Resource allocation within organizations is subject to intense political contest among subgroups and individuals in the organization

(Baker, 1983, 1984; Cyert and March, 1963; Dalton, 1959; March and Simon, 1958; Mintz and Schwartz, 1985; Perrow, 1979). Allocation (and thus ultimately structure and strategy) depend at least in part on processes of coalition formation and contest within organizations, especially when the costs and benefits of alternative allocations are difficult to measure and forecast (Pfeffer and Salancik, 1978). It might be argued that these processes simply produce random noise in an otherwise well-behaved allocation process. However, recent research shows otherwise (Cohen and March, 1974; Cohen, March, and Olson, 1972; March and Olsen, 1976).

A second line of active research has shown that organizations face strong institutional constraints (DiMaggio and Powell, 1983; Granovetter, 1985; Macauley, 1963; Meyer, 1977; Meyer and Rowan, 1977; Meyer and Scott, 1983; Mintz and Schwartz, 1985). Organizational designs are constructed and evaluated in a sociocultural context. Some designs have extensive social backing, for instance, by professional associations and schools or by government agencies (DiMaggio and Powell, 1983). Designs also stand as markers of difficult-to-observe competences such as managerial acumen, and are therefore used strategically to signal such competences (Meyer and Scott, 1983). Finally, seemingly neutral arrangements tend to become infused with moral value by members of organizations, turning means into ends (Selznick, 1948; Zucker, 1977).

Analysis of organizational politics and institutional processes has shown that the choice of organizational design reflects more than technical demands and environmental threat. Designs may proliferate even when they make little or no contributions to productive efficiency but serve political or institutional purposes (Carroll and Huo, 1986; Meyer and Scott, 1983; Tolbert and Zucker, 1983).

Research on organizational politics and institutional processes has also made clear that organizations face strong inertial pressures (Hannan and Freeman, 1984; Stinchcombe, 1965). Attempts at radical redesign, especially in large, established organizations, spark political opposition and activate institutional resistance. If such opposition delays organization in order to take advantage of changing opportunities (or to respond to changing threats), they make organizations relatively inert. Unfortunately, little is known about the relative rates of the various processes. A core problem in explaining the spread of organizational forms is to learn how structural arrangements affect the speed and flexibility of response of large organizations (Brittain and Freeman, 1980; Carroll, 1984; Fligstein, 1985).

When change in environments is rapid, it also becomes important to learn how new forms of organization arise and spread (Fligstein, 1985). This involves linking processes of entrepreneurial activity with organi-

zational dynamics. Since most entrepreneurs come from existing organizations, the dynamics of populations or organizations undoubtedly affect the rates at which entrepreneurs are spun off and the likelihood that new forms will establish footholds in competitive environments (Brittain and Freeman, 1980; Freeman, 1982).

Recent lines of theory and research have cast these issues in evolutionary terms. These theories disagree about key mechanisms: some emphasize adaptive learning and coping (Nelson and Winter, 1982); others emphasize competitive selection (Aldrich, 1979; Hannan and Freeman, 1977; McKelvey, 1982; McPherson, 1983; Singh, House, and Tucker, 1986). However, they agree that the processes of change are highly path-dependent. For example, the sequence in which organizational forms enter a system determines what other forms can proliferate (DiMaggio and Powell, 1983; Nelson and Winter, 1985).

Theory and research on organizational dynamics have developed considerable momentum. In particular, appropriate dynamic models are now in use for studying life histories of organizations and populations of organizations (Coleman, 1981; Tuma and Hannan, 1984). Promising starts have been made in modeling organizational learning and copying. However, much remains to be done. Convergences with other lines of social and economic dynamics have become clear but have not yet been developed. It seems highly likely that progress in understanding the issues here will be speeded by building bridges to research at the frontier of dynamic modeling.

### **Research on Organizations in Economic History and Agricultural Economics**

Historians of economics have studied the development of economic organization, including markets and other institutions. The point of view underlying much of this research is to see an institution as a solution to some functional problem or economic need. In this view, an institution can be seen, in a somewhat idealized way, as a solution to an optimization problem. This fits well with the formal approach, described above, in which the institution or organization is a variable whose value is to be determined. Beyond this, however, historians are interested in the development of institutions over time, and some study the endogenous aspects of historical phenomena. They examine the intertemporal and cross-cultural aspects of economic institutions and enrich abstract economic analysis by illuminating ways in which the performance of economic institutions is shaped by noneconomic factors. The phenomenon

of religious views about interest, and their effects on economic life over centuries, is an example.

A similar point of view has been developed in agricultural economics, where analyses have been made of innovation and institutional change as endogenous responses to a demand for institutional knowledge derived from demand for technical change in commodity production. The establishment and contributions of agricultural experiment stations in the United States has been studied from this point of view.

### *Data Problems in Empirical Research*

Empirical research on markets and organizations is being carried out in the various applied fields of economics and in sociology. Empirical research in this area faces a major difficulty. A large constraint on empirical studies of organizations is the absence of systematic data on representative samples of organizations over time. Most published research on organizations in economics and sociology suffers from one of a pair of resulting defects: (1) aggregation problems and (2) unrepresentativeness. Because the Census Bureau and other agencies aggregate data on establishments (and firms) to preserve confidentiality, most analyses of published data use information on aggregates to test propositions stated at the level of organizations. When researchers collect original data to avoid such problems, they are hampered by the existence of an adequate sampling frame for organizations. This problem, along with problems of nonresponse, makes these potentially more valuable studies subject to distortion due to unrepresentativeness. This is particularly problematic when data are used to evaluate the consequences of policy interventions.

The problem is not that the appropriate data do not exist. Rather, constraints due to confidentiality prevent the appropriate use of a wealth of data in the files of such government agencies as the Census Bureau, the Internal Revenue Service, and the Labor Department. Given this situation, it is worth exploring arrangements that would provide access to disaggregated data on firms and other kinds of organizations without impairing confidentiality unacceptably.

### *A Problem of Empirical Research*

The models underlying empirical studies of organizations are formulated with different degrees of precision and formality, different objectives, and different basic assumptions. This makes it difficult to interpret empirical results and to relate one study to another and to theoretical mod-

els. We need to build bridges between the formulations that guide empirical research and those that are used in theoretical investigations. This would be especially significant across disciplinary boundaries.

### The Next Decade

Research directions and opportunities for the next ten years can be viewed in two ways: first, in terms of the objectives of research, and second, in terms of the predictability of research topics on the basis of existing research.

The general objectives of research on organizations are:

1. To improve and extend the analytical basis for the evaluation of alternative methods of resource allocation, including markets and other forms of organization, and to bring to bear experimental and empirical studies and results.
2. To build bridges between the formulations of organization theory in the different social sciences, particularly between the models and the methods of different disciplines.
3. On the basis of (1) and (2), to construct an analytical basis for the optimal design of institutions.
4. To provide a basis for understanding the endogenous modification of the structure of ongoing markets and other institutions over time. This task involves theoretical, experimental, and empirical investigations. It appears to have important links to analogous problems in computer science, cognitive science, and biology.

Thus, research in the next decade will seek to model the internal processes of organization, to develop criteria for evaluating the performance of organizations, and to expose by experiment or systematic empirical investigation the relationships between the structure and internal processes of organizations and the resulting performance.

In terms of predictability, research directions and opportunities can be grouped into three categories: (1) those that can with reasonable clarity be seen to emerge from the base of existing research; (2) those that are further removed from current research and therefore of a more speculative character; (3) a residual category of the unexpected, provided to recognize explicitly that though we cannot foresee what new questions, ideas, or methods will appear, we can be quite confident that such things will appear.



### *Information*

In the first category of opportunities and directions is the extension and deepening of the analysis of information processing and incentives, and of the relationship between informational properties of organizations, their incentive properties, and their performance. We need to develop further models that give effect to the restrictions imposed by the technology of information processing, including the limitations of human beings as information processors, on the design and performance of organizations. Similarly, we need to continue the ongoing exploration of the effects of divergence of goals and dispersion of information on the design and performance of organizations.

### *Contracts over Time*

An important research topic in this area now in early stages of exploration deals with the endogenous revision of contracts over time. One of the principal conclusions of the theory of general equilibrium under uncertainty is that risk can be spread effectively if contracts can be written so as to be responsive to the realization of different events. For this to be operational, all of the parties to these contracts must be able to verify that each of the distinct set of circumstances specified in the contract has in fact occurred. When such verification is not possible, either control over the contract must rest with those who have the information necessary to administer it or the contract must specify the same outcome across several events. In the former instance, the propriety of this information creates incentives to use it for personal rather than collective ends. In the latter, the advantageous risk-spreading properties of contingent contracts are lost.

Much of the theory of incentives concerns the balancing of these two forces. In this context, the following problem often arises. Given an ongoing contract, and given that new information changes the beliefs of the parties to this contract, how should the contract be rewritten? The difficulty here is that even if a new contract were superior to the old one, it might not be possible for those with the new information to convince the other individuals that it is in fact in their interest to abandon the old contract and switch to a new one (Hart and Moore, 1988). Considerable progress has been made in this area, but there are many important unresolved questions, especially when new information is given simultaneously to more than one individual. Much is to be gained from further analysis of these problems. The implications go far beyond economics to the study of relationships within organizations and social networks, as well as to the fundamental structure of our legal institutions.

### *Normative Analysis*

We also need to develop criteria to be applied in a normative analysis of organizations. Current work in economics relies mainly on the Pareto criterion, but there are clearly other considerations to be taken into account, for example, fairness.

Along this line, but perhaps in the second category, is the problem of criteria for evaluating organizations when participants have preferences for processes and procedures as well as outcomes. This raises issues similar to those raised by endogeneity of preferences, a topic discussed below.

### *Stability*

Also in this first category of predictability is the problem of stability of dynamically modeled organizational processes, including the important study of structural stability (stability under variation of the environment).

A sophisticated body of research exists in the area of dynamic economics, ranging from the analysis of local stability of adjustment processes to analysis of infinite horizon overlapping generations models incorporating expectations and money.

### *Adaptation and the Larger Dynamics*

Also in the second category is a large variety of problems and questions related to the flexibility and adaptability of organizations, to their ability to adjust to changing environments. These can be viewed as problems of structural stability. But the nature of the problem is critically dependent on what is assumed to be known about the laws of motion of the environment. Phenomena of learning and adaptation have been studied in a number of disciplines, including economics. Learning models of the formation of expectations have been studied in the literature on rational expectations. But this area is still in an early stage of development, especially when viewed in the context of a richly dynamic setting.

One approach to the study of organization in a dynamic context views the process of organizational change as endogenous. While some models approach the problem in an optimization or rational decision framework, others do so with evolutionary concepts. Organizations are regarded as competing with one another in populations. Some selection process distinguishes between organizations by implicitly defining *survival value*, a concept that reflects the underlying processes and constraining elements

that constitute the "technological laws" governing adaptation, and, in the evolutionary context, survival.

### *Endogenous Uncertainty*

Organizations as well as individuals confront change, the nature of which is unforeseen and perhaps unforeseeable. The possibility of unforeseeable change gives rise to the need for a rigorous formulation of rational behavior in the face of this possibility. Existing models of rational behavior under uncertainty do not address this issue, because in one way or another they assume that the possible states of the world are known, and that uncertainty about them is expressed by probability distributions over the known set of states. The critical feature of unforeseeable change is precisely that the set of states is now known, and is perhaps not even well defined.

This is an important and exciting problem whose relevance extends far beyond the area of research on organizations. It seems likely that rational behavior in this broad context will involve adaptation, learning, complexity, and similar phenomena. These are topics in the second category, and must be regarded as speculative. Yet it is worth noting that the same kinds of problems and ideas—involving nonlinear dynamics, adaptation, learning, and complexity—are to be found now in a number of very different scientific fields. A conference held at the Center for Non-Linear Analysis at Los Alamos in May 1985, whose title was "Evolution, Games, and Learning," included papers in physics, evolutionary biology, paleontology, ecology, biochemistry, computer science, complexity and artificial intelligence, machine learning, neuroscience, and economics. Many of the same ideas and questions were addressed in different languages and different contexts in these papers. The simultaneous emergence of these ideas as research topics in so many fields suggests that the time is ripe and the means are at hand to explore them.

### *Cross-Disciplinary Aspects of Research*

We have noted that the study of organization crosses disciplinary boundaries. This naturally gives rise to the idea of research at and across disciplinary boundaries, and to the possibility of support to facilitate and foster such research. Experience with such research in the behavioral and social sciences is mixed. It is therefore important to consider more closely what makes for successful research across disciplinary boundaries. Economic history is an example of a successful "interdiscipline"

between economics and history now so long and well established that it is not usually regarded as interdisciplinary but as a subfield in its own right. This parallels the situation in the natural sciences, where investigators approach the study of a common phenomenon from separate disciplines and, by combining their theories and techniques, develop a common paradigm. The eventual result is to establish a new subspecialty with its own methods and models. Physical chemistry and biophysics are examples. The essential feature of these cases seems to be the existence of a common model more or less formally defined that permits the research of different investigators to be related in a precise and systematic way.

An example in the social sciences where this can be seen clearly is that of the new field of "public choice," which brings together political science and economics. In recent years, the field of public choice has brought together researchers in voting theory and voting behavior from political science with social choice theorists from economics. Social choice theory provided the seed around which this field crystallized, by providing the initial formal framework. It is now well along in the process of becoming an independent subfield.

Another mode of cross-disciplinary interaction is a type of cross-fertilization of disciplines that takes place when one discipline incorporates material from another into its own models. For example, both economists and sociologists have pointed out that certain game-theoretic models cannot be satisfactorily closed within game theory itself. Solution concepts that reflect only the strategic structure of the game situation frequently result in a multiplicity of nonequivalent solutions. This is frequently the case with Nash equilibria of the game forms that arise in studying incentive problems. Sociologists have long pointed out the importance of values and norms as influencing behavior in such situations, and economists have come to recognize that such elements may have to be incorporated in their models to produce satisfactory results. There does not appear to be any serious obstacle to doing this, once the relevant values and norms are made explicit. There are no doubt several ways in which they could be incorporated, one of which would be to impose values and norms as constraints on the set from which a player may choose his strategy. This, if carried out, would be an example of one field (economics) using concepts (values and norms) from another (sociology) by incorporating them into a formal model.

A second example is that of endogenous preferences. Typically, in economic models preferences of economic agents are taken as unchang-

ing, given from outside the model, or if changing over time, doing so in a way determined outside the model. In short, preferences are assumed to be exogenous. Other behavioral and social scientists have pointed out that preferences of individuals are influenced by actions and events, by their experiences, some of which are typically endogenous variables of the model (Hirschman, 1982). Thus, the history of preferences over time becomes endogenous. The processes that describe the development of preferences over time may be central objects of study in sociology or social psychology, with the result that endogeneity of preferences may be a common feature of models in those fields.

It is doubtful that any person, whether or not an economist, would seriously dispute the proposition that education, experience, and other social influences affect people's preferences. Furthermore, it seems entirely possible to formulate dynamic models in which preferences are endogenously developed over time. Models, similar to those of capital theory, in which preference at any moment depends on past preference and on the events that influence preferences to change, can be formulated, and could perhaps incorporate knowledge of influences on preferences acquired from sociology or social psychology. But, in the context of research on economic organization, including markets, this has not been done. The difficulty is as follows: an important objective of research on organization is to be able to compare different organizations and to assess their efficiency. For that, it is necessary to have some criterion of efficiency. In economic models the criterion of efficiency usually depends on individual preferences, as is the case with the Pareto criterion, as well as various proposed notions of fairness. If preferences are endogenous, the logical basis for a normative analysis is unclear. In this case, the possibility of finding a formulation that provides a common ground for research between a discipline interested in modeling the endogenous evolution of preferences and one whose problem is to analyze the efficiency of mechanisms depends on being able to find a satisfactory efficiency criterion when preferences are endogenous.

Although the scope of productive interdisciplinary interaction is unclear at this time, there appears to be some convergence of interests, ideas, and perhaps models, between economics and sociology. The fact that there is now a larger number of sociologists who have mathematical and econometric interests and of economists interested in sociological models and results is helpful for communication.

It does seem clear that an essential condition for fruitful work on organization across disciplines is the ability to relate research in different fields in a precise, detailed, and rigorous fashion.

## Research Report

This report indicates that research on organizations is already an important area of research. New organizational designs and practices are likely to be needed as a result of new developments in computing and communication technologies. This increases the need for deeper scientific understanding of organization, and will offer new opportunities for analysis.

We strongly hold the view that this area of research will fare best if it is carried out via projects that grow out of the interests and ideas of individual investigators, or spontaneous collaborations among them. Such projects should compete for support in the review processes of institutions that support research, such as the National Science Foundation (NSF) and private foundations.

Because research on organizations can cross disciplinary boundaries it is important that proposals, especially unconventional proposals, or proposals that may not appear to be in the mainstream of any one discipline, not fall in the cracks during the evaluation process. Institutions interested in supporting research in the field of organization should be sensitive to the risk of too narrow a perspective.

### *Colloquia*

We think that it would be very productive to have a series of regular meetings, or colloquia, patterned to some extent on summer conferences in biological sciences. These meetings would take place at fixed and known times every year and last from one to three weeks. The group invited to participate, about thirty people, would have varying memberships, depending on the topic. The site for each meeting would be chosen at least a year in advance, allowing the organizers of each meeting a full year to plan the program. The meetings would be a mixture of formal sessions, sessions devoted to expository presentations, cross-disciplinary contacts, and time for informal work.

The Conference-Seminar on Decentralization, one of a number of conference-seminars supported by the National Science Foundation via a grant administered by the National Bureau of Economic Research, has been meeting since 1971, once or twice a year (depending on availability of funds) at different universities. These meetings have had an important effect on the development of research in the field of decentralized resource allocation, as was the case with their predecessor, the Purdue Symposium. Research on organization would benefit from an improved version of a program of regular conferences along the lines sketched above.

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## Jobs and Inequality

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Important research questions in the areas of jobs and inequality range from the effects of entrepreneurship and reorganization of firms on employment and wages to the changing distribution of wealth as influenced by international trade and immigration. Our approach to these subjects is multidisciplinary. Both economists and sociologists often agree on what are the major topics, but we have not attempted to formulate an interdisciplinary approach. Rather, we believe that most of the subjects outlined below merit the attention of researchers from several social sciences and can benefit from different methodologies within each social science.

A major consensus was the likely payoff to data on organizations and individuals within organizations in contrast to the much more widely available data on individuals and individuals within families. We tentatively concluded that, with extensive planning, such a data base could be developed but that it was beyond the scope of our mission to formulate an outline or sample design or to have an extensive discussion of other implementation issues. To illustrate, the issue of gaining cooperation by the sample organizations is a major design issue which requires extensive planning and methodological study.

A basic theme is that a wide range of social changes are having a major influence on wealth and well-being. There are major changes in macroeconomic demand conditions, accentuated by rapid internationalization of economies, technology, and culture. This changed environment is influencing mobility and lifetime earnings. As the market for unskilled labor becomes a world market through trade, will wealth in the United States be defined in terms of high skill levels or in terms of ownership of housing, land, and raw materials? To illustrate, in Silicon Valley a major wealth form is land ownership while wages of medium- to low-skilled workers are generally only at or below the national average. The national average real wage of unskilled workers eroded during the 1970s when the United States experienced slow economic growth, and its current improvement may be cyclical rather than long-term.

Family structures have been changing rapidly and female-headed (single-parent) households account for a rising share of households in poverty. Female-headed households have increased the number of



workers in certain medium- to low-skill sectors of the labor market, notably the medium- to low-skill service sector. To date this has been a technologically stable sector. Yet recent advances in computer-aided design and manufacturing may well spread to the service sector, accelerating organizational and labor market changes.

In textiles, as illustrative of the production sector, joint technology projects by unions, textile firms, and their suppliers (e.g., Du Pont) have given promise to technological breakthroughs that can lower production costs to match foreign competition. If this technology is introduced, it will have wide-ranging effects on the industry. Employment and wages are both likely to experience downward adjustment. These adjustments will be less traumatic than if production costs are not lowered to meet world competition, which could lead to industry collapse. A major side effect may be technology transfer to the service industries, since some of the technology could be used in office automation.

To study this change process one can use aggregate industry data, but the consensus of our working group was that many of the decision processes and effects are potentially more observable at the level of organizations. By having data on individuals within those same organizations for which we would have an extensive set of variables, such as investment plans, inventory policy, hiring policy, and career ladders, one could link organizational and individual change.

In designing studies of jobs and inequality it is important to analyze different communities as well as different organizations. Los Angeles and Milwaukee are very different in terms of community variables. Immigration, climate, and industry structure all differ, and may very likely influence the labor market policies of the organizations. Extending the idea of different communities, our working group saw strong potential in international comparative data on organizations. Here the organizations and individuals would be operating in different macroeconomic environments and under dramatically different tax laws, educational policies, and reliance on the market or government for economic signals. A unified data base on firms spanning countries and communities could have an enormous research potential.

## Specific Research Areas

### *Nonmonetary Costs and Benefits of Work*

In trying to explain why some people earn more than others, one classic line of argument, repeated in most introductory economics texts, is that

some jobs are more agreeable than others. For most analytic purposes, we would like to have measures that combine the monetary and non-monetary rewards of work into a single composite. Sociologists have tried to approximate this by focusing on occupational status, which takes account of nonmonetary costs and benefits but has the serious drawback of ignoring intraoccupational variance in wages and working conditions. Psychologists have tried to solve the problem by focusing on job satisfaction, which has its own set of difficulties because different workers evaluate the same job differently. Economists have generally focused on wages, and while some have tried to estimate the monetary value of nonmonetary job characteristics, they have made no systematic effort to assess the significance of nonmonetary job characteristics as a whole, or to take them into account when examining such things as return to schooling, race and sex discrimination, or the overall degree of inequality between jobs.

In order to solve this problem we need two kinds of data: a representative sample of employers to obtain data on the characteristics of a large representative sample of jobs, and data on how workers of various kinds evaluate jobs with varying characteristics. One option is to get workers to evaluate their own jobs. Averaging across many jobs, this allows one to say how good the average worker in a job with specified characteristics thinks that job is, and hence to compare the impact of wages to many other job characteristics. The other option is to look at revealed preferences by examining the patterns of (voluntary) movements between jobs. The first option requires a sample of jobs and job holders on the order of a Current Population Survey (CPS) to get detailed information on how workers appraise jobs. The second requires analogous data on job holders at two or more points in time.

A wide array of research questions on nonmonetary benefits from work can be addressed by a sample of firms. In such a sample, objective measures of work environment can be measured by observers. These variables would apply to many of the workers but are probably difficult for workers to report directly. Such variables include price and inventory policy, extent of technological change, hiring policy, and extent of international competition.

### *Discrimination*

We have learned a lot over the past twenty years about the extent to which race, sex, and age influence workers' occupations and their earnings. We know, for example, that wage differences between blacks and whites are only partially explicable in terms of differences in workers'

measured characteristics: experience, education, academic achievement or IQ, region, community size, residential location, and so on. We also know that the unexplained differences between black and white workers' wages are greatest, at least in percentage terms and perhaps in absolute terms as well, among less educated workers—a reversal of the historical pattern, in which black-white wage differences were greatest among the highly educated. What we do not know is why unexplained differences persist, or why their pattern has changed. The "null hypothesis" among economists has been that unexplained wage differences represent differences in "school quality," "ability," "motivation," or other unobserved sources of variation in individual productivity. But only a handful of studies have tried to measure productivity directly (Medoff and Abraham, 1980), usually using supervisors' ratings, and these studies have not to our knowledge addressed the issue of race or sex differences in earnings, promotion patterns, and the like.

While direct measures of productivity would not resolve the uncertainties surrounding these wage differentials entirely—productivity itself is often endogenous—such data would certainly allow us to make major advances on our present understanding. If, for example, differences between male and female or black and white workers' trajectories within a firm were explicable in terms of differences in supervisory ratings, we would at least have pinpointed the locus of discrimination, if any. We still would not know if supervisors' ratings were accurate or objective, but we would know why wages ended up being different, and we would know what to do next to pursue the question of whether discrimination occurred within firms. Hence the case for data on individual careers within firms, as well as between firm movements.

### *International Competition and Inequality*

Virtually all formal analysis of the labor market treats the United States as a self-contained labor market. This is demonstrably wrong, and increasingly so for recent years. There is some evidence that the dispersion of expected annual earnings for specific jobs has grown more unequal over the past twenty years. It seems possible that some of the change reflects increased exposure of American workers to competition from workers in other countries. If Third World workers are increasingly able to produce the same manufactured goods that American workers produce, the wage differential between American and Third World workers is almost certain to fall, according to the hypothesis of factor price equalization. If the same logic does not apply to American managers and professionals, the wage differential between occupations is

likely to grow. Thus, international competition may contribute to domestic inequality because the threat that jobs will move elsewhere keeps blue-collar wages down and because the availability of immigrant labor within the U.S. labor market does the same thing.

Testing this line of argument requires fairly detailed information on what kinds of jobs have experienced high and low wage growth over time. A fair amount could be done with occupation-by-industry data from the CPS or census, but more detailed information about job characteristics would be useful, and data from a sample of employers would facilitate such research.

### *Division of Labor, the Production Function, and Technology*

Previous theory and research have sharpened the lines of disagreement between several very different views of technology, work, and opportunity (Spencer, 1983). One area of controversy concerns how organizational and technological changes are affecting workers and jobs: are they increasing or diminishing skills, responsibilities, and rewards on the job? A second area of debate concerns the key factors that shape the organization of work. Social scientists have often simply assumed that firms adopt the work arrangements that best satisfy constraints of administrative and technical efficiency. However, a growing body of work questions that view. Some recent studies of technological change document considerable discretion in how specific jobs are organized (Stafford, 1988), arguing that imperatives other than technical efficiency shape work design. For example, some authors have argued that detailed distinctions among jobs serve to reduce workers' skill and control on the job and to separate various subgroups of the labor force. There is evidence that powerful actors in organizations often define work roles to suit their idiosyncratic interests and abilities, but not necessarily the firm's (e.g., Miner, 1984).

A better understanding of how and why organizations structure jobs is essential to refining our knowledge about socioeconomic inequality. For instance, understanding the link between schooling and career success requires studying how and why employers define jobs in particular ways and use educational credentials to decide whether individuals satisfy those job requirements. It also requires understanding the opposite causal link: how organizations define jobs and career paths around workers' educational attainments. Current knowledge is limited principally because there are so few detailed data on work arrangements within firms and how they are changing over time. We require longitudinal data for large and representative samples of firms, jobs, and workers.

These data would allow researchers to study how forces outside organizations, including technological changes, affect the way in which jobs and careers are organized. These data would also allow investigators to test various hypotheses that have been proposed by economists and sociologists about the factors inside organizations, including the structure of the firm and the composition of the work force, that shape job design and career outcomes.

### *Interorganizational Mobility*

Although recent interest in "internal labor markets" has focused attention on career movement within organizations, there has also been increased interest in movement between firms. Some organizational theorists claim that personnel exchanges are one mechanism firms use to manage their environments by gaining information about relevant constituencies on whom they depend (see Pfeffer and Salancik, 1978). Indeed, some theorists regard interorganizational mobility as a primary means by which organizational innovations diffuse, as has happened in Silicon Valley, where individuals move frequently within a particular interorganizational "network."

From the standpoint of individuals, there may be very different determinants and consequences of career mobility between versus within firms (e.g., Felmler, 1982). Women, racial and ethnic minorities, the young, and the old are claimed to move from firm to firm within "secondary" labor markets, where skills are general and there are few career advantages obtained by switching firms. However, the empirical evidence is conflicting on this point, largely because social scientists lack comprehensive life history data for individuals that describe the organizations in which individuals have worked throughout their careers.

Such data would be invaluable to students of labor markets as well as complex organizations. They would allow labor market analysts to test hypotheses about the segmentation of industries, firms, and labor markets; differences in the income returns to general versus firm-specific skills; the boundaries of local and regional labor markets; and the causes and consequences of turnover. They would allow organizational researchers to examine whether networks of firms exchange personnel to manage their interdependencies, obtain strategic information on competitors, or adapt to changing environments by mimicking the innovations of other firms. To date, the absence of representative and detailed data describing career moves across organizations has restricted hypothesis testing and theoretical refinement in both areas.

***New Data Resources:******Constructing Machine-Readable Data Files from Historical Archives***

As researchers have become interested in how organizations mold career attainment, their perspective has, inevitably, become increasingly historical. Contemporary changes in the labor force, in firms, and in the nature of work are obviously having profound effects on the distribution of career opportunities and outcomes. Consequently, social scientists have become interested in studying the effects of similar changes in the past, when the composition of the labor force changed radically due to immigration and when the nature of work was transformed by changing technologies and organizational structures in earlier periods.

Longitudinal and comparative data on work arrangements in the past are hard to come by. Recently, however, researchers have identified several large-scale sources of data that could revolutionize historical research on work and careers. During the 1930s, the U.S. Employment Service began gathering data on the staffing patterns of establishments as well as their promotion ladders and job requirements. Until the program was eliminated several years ago, these data were collected throughout the nation (and, for some organizations, at multiple time points) to prepare the *Dictionary of Occupational Titles* and other government publications. Microfilm and/or original documents exist in Washington, D.C., and in the program's central repository in Raleigh, North Carolina. Producing machine-readable versions of these data would provide researchers with invaluable longitudinal and comparative information on the organization of work and opportunity in American industry over the last half-century.

Other comprehensive historical data on organizational careers have not yet been made accessible to researchers for detailed quantitative study. Several large corporations, including Ford Motor Corporation and the Pullman Company, have detailed data describing employees' job histories over many decades. These records have been converted to "inactive" status by these companies, for whom they are no longer of any practical value.<sup>1</sup> Yet they represent a marvelously rich source of data for researchers, allowing them to assess how changing technologies, organizational structures, and labor market conditions affected job design and workers' career outcomes in an earlier era. From these sources, we

1. This endeavor would be aided considerably by the fact that researchers have already converted a very small subset of this archive (California enterprises analyzed since 1959) into machine readable format, developing coding procedures that could be used in the larger-scale effort (see Baron and Bielby, 1980).

would gain a better understanding of how contemporary changes are likely to influence organizations and workers.

### ***Wages, Tenure, and Productivity***

In the past ten years the theory of implicit contracts has been developed and used (Lazear, 1981) to justify a positive wage-tenure profile that is independent of productivity. Simultaneously, some empirical research (Medoff and Abraham, 1980) has been unable to find a positive tenure-productivity relationship, though the small subsequent empirical literature partly conflicts on this point. The conflict between standard human-capital theory and the contracting theory needs to be resolved by careful empirical work, for the resolution has implications for such important issues as the efficiency effects of tenure-related employment restrictions, antilayoff policies, and others.

While we have seen the development of convincing theories, available data have not permitted satisfactory tests to be made on them. The availability of a longitudinal data set linking firms and workers would enable this problem to be resolved. With information on establishments and on workers' progression in the establishments, productivity measures could be developed for broad classes of workers who differ in their characteristics, as could measures of their wages and seniority.

## **Implicit Contracts and Long-Term Employment**

The theory of implicit contracts developed in the 1970s by Baily (1974), Azariadis (1975), and others provides a strong theoretical basis for the failure of labor markets to behave like spot markets. Also, the empirical work of Hall (1982) has demonstrated that, contrary to past impressions of a work force that is highly mobile among jobs, most workers settle into employment fairly quickly and hold very few different jobs after age thirty. The theory and the related facts have changed people's views of how the labor market works. They are important for understanding how business cycles can create unemployment and how the long-term nature of the employment relation will affect both micro- and macroeconomic wage adjustments.

What the theoretical and empirical findings do not tell us is how the long-term relationships change with the characteristics of the workers as *these characteristics interact* with those of their employers. Surely workers' attachment to a firm will differ depending on the nature of the

job and workplace. With knowledge of how these differences arise and what they are we could infer how the interactions of both sides of implicit labor markets affect wage and employment adjustments. Clearly, given that these issues rest on the relationships between workers and their jobs, information must be available on both together. Also, these data must be longitudinal so that the interaction can be followed as the worker's career within the firm progresses.

### *Lags in Labor Market Adjustment*

In the past twenty years theories of labor market adjustment have developed models that include costs of adjustment in production functions (Holt et al., 1960) and, more recently, models of staggered wage contracts and emulation (Taylor, 1980; Hamermesh, 1975; Akerlof, 1980). Most of the testing of these theories has been on very crude data, usually covering the entire economy or, at best, highly aggregated industries. Empirical work has not been able to distinguish among these theories, which is unfortunate, as we have not been able to understand what produces the sluggish behavior that characterizes labor markets.

Research is now required that will integrate existing theories of adjustment and consider the sources of lags. Accompanying that must be the empirical basis upon which new and existing theories can be tested and that can inform modifications of them. While there are sets of data on firms, they provide very sparse information, are not longitudinal in nature, and provide no link to detailed characteristics of workers. A panel study of firms that is linked to panels of their workers will enable researchers to formulate and test alternative hypotheses about the simultaneous adjustment of behavior on both sides of the labor market, and about how one employment relation is affected by adjustment elsewhere in the economy. Only with such a source of data can we go beyond the broad-brush testing that advocates of existing theories have used to provide impressionistic evidence in support of their hypotheses. And, since slow labor market adjustment is the fundamental fact that produces the painful side effects of product-market cycles, such data should enable researchers to obtain a greater understanding of the labor market effects of macro fluctuations.

### *Macro Effects on Micro Relations: Cyclical and Spatial*

The effects of long periods of high unemployment on careers has been cited as one possible long-run impact of the 1970s experience. We know now (Ellwood, 1982) that the major effect is due to the lost labor market experience, not to any long-run change in labor market skills of the un-



employed workers. Similarly, the sharp changes in industrial structure of the past decade have shifted the location of employment within metropolitan areas and among regions. There is some indication that the changing location of jobs has slowed the equalization of outcomes among racial and ethnic groups in the labor market (see Kain, 1968).

We do not know how the migration and/or demise of firms over time affects workers' careers, or how aggregate fluctuations in demand affect firms' abilities to advance the interests of the workers who are tied to them. Given the importance of long-term employment relationships, this lack of knowledge is unfortunate. With a longitudinal employer-based data set such knowledge can be obtained. Similarly, it should give researchers the ability to track firms' movements by location relative to their workers' locations and thus assess the importance of spatial relations in labor markets.

### Conclusion

One of the unifying themes of these identified research topics is that the theory of organizational functioning needs to be developed to the same extent that we have developed theoretical and empirical models of individuals and families. In this development there could be an important role for historical, comparative, and even case study data. From these studies and the research questions we have identified, one could proceed to think of a multidisciplinary data set focused on the relation between organizational and individual well-being.

Such an overall project with independent analysis by many scholars would lead to a much better understanding of the distribution of earnings and working conditions. We would have a better understanding of how organizations influence individuals and how they are themselves shaped by individuals. This could lead to a far better understanding of such structural processes as entrepreneurship and the starting of new firms, technological change, discrimination, productivity growth, and patterns of organization response to permanent market shifts as well as cyclical fluctuations.

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## Macroeconomic Policy Research

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Macroeconomic policy research is both internally and externally driven. The internal drive comes from the dynamics of the field itself and from theoretical and empirical developments in other branches of economics. The external pressures come from the macroeconomic problems and events of the day. That mix of factors determined research priorities and progress in the 1970s and early 1980s, and determines prospects for the next ten years.

## Recent Research Developments

### *The Rational Expectations Approach*

The 1960s saw the culmination of the Keynesian revolution in U.S. macroeconomic research, with the development of large-scale econometric models of the economy. These models, some with over 100 stochastic behavioral equations, embodied an extended mainstream Keynesian model driven largely though not entirely by aggregate demand. The models were, and continue to be, used for both forecasting and policy analysis. For instance, the Wharton and DRI models are commercially successful, and the Federal Reserve-MIT-Penn model is used for policy simulations in the Federal Reserve System.

Expectations in these models were generally assumed to be formed adaptively, based on past behavior of the variable about which expectations were formed. It was also assumed that some markets, notably the labor market, might be in disequilibrium over substantial periods of time.

Expectations and equilibrium are at the heart of the rational expectations approach to macroeconomics developed in the early 1970s by Robert Lucas, Thomas Sargent, and others (Lucas and Sargent, 1981). The rational expectations approach combined Muth's (1961) theory of rational expectations with equilibrium models of the business cycle. Many of the important innovations in macroeconomic theory and econo-

metric modeling since then are a result of either the development of the rational expectations approach or reactions to it.

The rational expectations hypothesis in its weak form is that private agents form expectations by optimally using all the information available to them. Expectations should accordingly have the properties of optimal statistical forecasts. These properties have played a crucial role in major empirical analyses of the last decade.

### *The Consumption Function*

The life cycle and permanent income theories of consumption of Modigliani and Brumberg (1954) and Friedman (1957) imply that individuals' consumption is determined by their lifetime income and wealth. The current level of consumption thus depends on consumers' estimates of the present value of future income.

Assuming rationality of expectations and under certain other assumptions, Hall (1978) showed that the life cycle hypothesis implies that consumption follows a random walk. This result depends on the fact that forecast errors from statistically optimal forecasts are serially uncorrelated. Hall showed that the time series of per capita real consumption comes remarkably close to satisfying the random walk property.

Subsequent research has attempted to characterize and account for the divergences between actual and implied random walk behavior of consumption. A consistent finding is that of excess sensitivity of consumption to fluctuations in income (Flavin, 1981): consumption is more closely related to current income than life cycle and permanent income theories imply. This finding, while providing some support for earlier Keynesian theories of the consumption function, has in turn led to both theoretical and empirical investigation of the effects on consumption of liquidity or borrowing constraints that make it impossible for individuals to borrow against future income. The empirical work (e.g., Hall and Mishkin, 1982; Zeldes, 1984) has used panel data to examine the sensitivity of individual consumption to income, and has found some evidence of excess sensitivity.

This research on the consumption function bears directly on the important policy question of the effects of government budget deficits on real interest rates. The Ricardian hypothesis, given a modern treatment and impetus by Barro (1974), is that the private sector internalizes the budget constraint of the government. Thus, when the government borrows to finance a deficit, the private sector realizes that it will have to pay taxes in future to retire the debt; given that the present value of the taxes is equal to the amount of debt sold by the government, the argu-

ment is that government borrowing is equivalent to future taxation, and thus that explicit taxation and deficit financing have the same effects on real interest rates.<sup>1</sup>

The Ricardian hypothesis, important as it is, has proven difficult to test using aggregate consumption data. Direct tests of the effects of deficits on real interest rates have been inconclusive.<sup>2</sup> If research succeeds in showing that liquidity constraints affect consumption, then it will be possible to conclude that real interest rates are affected by the method by which the government finances its spending.

### *Stock Market Efficiency*

Observers of the stock market have long argued that the market fluctuates "too much" relative to fluctuations in the underlying determinants of stock value. Shiller (1981) and Leroy and Porter (1981) tested this hypothesis by using an implication of rational expectations that the forecast of a variable generated by a stationary time series should fluctuate less than the variable itself. Shiller's results, based on the time series behavior of aggregate stock price indexes, suggested that the market indeed fluctuates too much. Related results, rejecting the simple expectations theory of the term structure of interest rates, were obtained by Shiller (1979) and Singleton (1980).

One of the hallmarks of rational expectations econometrics has been the imposition of cross-equation constraints on time series. These restrictions arise either because optimization implies relationships among endogenous variables (e.g., portfolio pricing relationships) or because particular variables enter relationships only through their influence on expectations. These restrictions have been extensively employed in testing, for example, models of asset market equilibrium (Hansen and Singleton, 1983). Typical findings in these areas are that asset returns behavior is not consistent with the implications of basic models such as the consumption beta model of Breeden (1979).

Advances in the study of asset pricing in the last decade are a result of the development of improved models of asset pricing with clear empirical implications, of the development of econometric methods for dealing with expectations based on the rational expectations hypothesis, and of the availability of excellent data on asset returns. These data come from the Center for Research in Security Prices of the University

<sup>1</sup>A variety of other assumptions is needed to derive this result, including the assumption that all taxes are lump sum.

<sup>2</sup>Plosser (1982) has used data on the response of interest rates to announcements about the deficit to test the Ricardian hypothesis.

of Chicago Business School, and sometimes from the commercial CompuStat tapes.

There have been two reactions to the failures of basic models found in recent empirical analysis. First, there has been an intensive examination of the underlying statistical assumptions (e.g., Marsh and Merton, 1984, 1985), and second, the underlying theory has been reexamined and extended.

Most significant has been the development of the theory of speculative bubbles. A speculative bubble exists when an asset's price diverges from the price determined by its "fundamentals." For instance, a stock's price may derive in part from the possibility of reselling it to someone else, who in turn expects to resell it at a higher price not related to the fundamental determinant of value, the stock's dividend stream. The key question is whether such bubbles are possible when expectations are rational. An early analysis suggesting a positive answer was by Blanchard (1979); Azariadis and Guesnerie (1984) and Tirole (1985) settled the issue by showing that bubbles are indeed possible in equilibrium, and by deriving the conditions for their existence. Empirical tests for the existence of bubbles have been presented by Flood and Garber (1980) and Blanchard and Watson (1982) among others.

An alternative approach is to forego the assumption of rational expectations and to develop models of markets with elements of irrationality (Shiller, 1984). Behavioral studies show persistent biases in decision-making under uncertainty (Kahneman, Slovic, and Tversky, 1982), suggesting the potential fruitfulness of the approach in economics.

The underlying issue in studies of asset price determination is that of the economic efficiency of stock and other asset pricing mechanisms. Early studies of this question posed the issue in terms of whether it was possible to "beat the market" by use of mechanical trading rules (e.g., Alexander, 1964). But the issue goes well beyond that of whether smart people can make easy money in the stock market. Because asset prices guide the investment process (a company whose stock is priced high can raise capital cheaply through the sale of equity), stock market inefficiency means also that the process by which the economy allocates investment is inefficient.

#### *Exchange Rate Dynamics*

During the period of pegged exchange rates from 1945 to 1973, influential analyses of the behavior of a floating rate system suggested that speculation would stabilize exchange rates, implying that floating rates would not fluctuate much (Friedman, 1953). In the period since 1973, exchange rates have fluctuated far more than was expected. Because ex-

change rates affect the volume of imports and exports, swings in exchange rates imply shifts of real resources.

Dornbusch (1976) proposed a model in which the exchange rate is driven primarily by actual or potential capital flows, which equalize the rates of return portfolio holders expect to earn from holding domestic and foreign assets. Domestic monetary contraction raises the domestic interest rate, leading to a capital inflow that proceeds to the point that the rationally expected depreciation of the domestic currency offsets the excess of the domestic over the foreign interest rate. For the domestic currency to be expected to depreciate, it has to appreciate to above the level expected for the future.

The exchange rate thus fluctuates relative to its long-term value. An essential assumption in producing this "overshooting" result is that changes in the stock of money affect interest rates. This result requires stickiness of the domestic price level, so that a reduction in the nominal quantity of money translates into a reduction in the real quantity of money. The price stickiness issue, which we discuss further below, is at the heart of several major controversies in macroeconomics.

The empirical evidence on exchange rate behavior is not consistent with the view that expected rates of return on domestic and foreign assets are either equalized or differ by a constant amount (Cumby and Obstfeld, 1984). Rather, premia or differences in expected returns among assets vary over time. Much recent research has attempted to uncover the determinants of these premia (e.g., Frankel, 1982); here the research on exchange rates overlaps the research on domestic asset price determination. There has been little support for the view, implied by most theories, that national interest rates differ by amounts that depend on the outstanding stocks of the different assets.

Research in this area is ongoing. Some of the issues are of almost immediate policy relevance. A key question for the United States is the extent to which it can draw on foreign sources of savings to finance its budget and trade deficits without substantially raising U.S. real interest rates relative to foreign rates. The greater the influence of outstanding stocks of assets on relative rates of return, the more U.S. interest rates will rise relative to foreign rates as the current account deficit continues.

### *The Equilibrium Approach*

The statistical implications of rational expectations have played a key role in significant research in each of the preceding three areas. They have also been used in research on inventories (e.g., Blanchard, 1983, Eichenbaum, 1983), on labor demand (Sargent, 1978), and indeed in virtually every area of macroeconomics. The equilibrium approach to



macroeconomics associated with the rational expectations revolution has also played a prominent, logically separate, and less widely accepted role in macroeconomics in the past decade.

The equilibrium approach to business cycles, well described in Lucas (1977) and by Sargent (1982), attempts to ground macroeconomics in the basics of tastes and technology, hoping to endogenize market and price formation arrangements. This rigor is an ideal to which most macroeconomists would aspire.

Given the distance to go until successful macroeconomic models based on tastes and technology and including monetary phenomena exist, however, the approach tends to inhibit discussion of macroeconomic policy issues. Fortunately, consistency has not been so demanding as to prevent those attempting to carry out this research agenda from speaking to policy issues.

Perhaps the best known implication of the earliest equilibrium type business cycle model (Lucas, 1973) was that anticipated changes in the money supply affected only prices and not output. This conclusion was based on the so-called Lucas supply function in which suppliers are imperfectly informed about the causes of changes in the prices in their markets. Knowing only the price of their own output, not the aggregate price level, they respond positively—though partially—to all increases in the price of their own output. Aggregate output thus increases when all suppliers find their prices rising, that is, when the aggregate price level rises.

The Lucas supply function is one explanation of the Phillips curve relationship that has played a key role in macroeconomics since the early 1960s (Phillips, 1958). The Phillips curve, which was a finding that the rate of change of *nominal* wages is inversely related to the unemployment rate, was the key to the determination of the inflation rate in most econometric and analytic models. Friedman (1968) and Phelps (1968) made the significant modification that the rate of wage change was affected also by the expected rate of inflation. The Lucas supply function can be viewed as providing a more sophisticated basis for the Friedman-Phelps formulation.

Barro (1977) concluded that indeed only unanticipated changes in the money stock had real effects, seeming to provide strong support for the Lucas formulation. However, further research by Barro (1978) cast doubt on the Lucas supply function as the source of this result; and Mishkin (1983) was able to show that the Barro conclusions were not robust.

These results leave the equilibrium approach without an adequate explanation of the apparent causal relationship between money and real

output. Given this difficulty, and results by Sims (1980) casting doubt on whether money causes output changes, some recent research in the equilibrium framework has attempted to explain business cycles as purely real phenomena, with the behavior of money as endogenous.

Kydland and Prescott (1982) have presented a complete equilibrium real business cycle model that starts from intertemporal optimization by households and firms, and embodies many of the business cycle propagation mechanisms studied in the last decade. Among these are fixed capital and inventory accumulation, intertemporal labor substitution, serially correlated productivity shocks, and the slow diffusion of information. Kydland and Prescott attempt, with mixed success, to fit the serial correlation and cross-correlation properties of the business cycle.

A major difficulty in this model, and in most intertemporal optimizing market clearing models, is inconsistency of the behavior of consumption and labor supply. Under simple and usual restrictions on utility functions, consumption and labor supply should move in opposite directions over the cycle. Yet labor input is higher in booms, when consumption is higher. This characteristic is largely responsible for the striking rejection by Mankiw, Rotemberg, and Summers (1985) of a simple intertemporal optimizing model; this result is based on time series data. Further research on this issue will likely use panel data.

### *Aggregate Supply and Price Inertia*

The theory of aggregate supply developed during the 1960s centered on the Phillips curve and Okun's Law. Okun's Law describes the relationship between the growth rate of real GNP and changes in unemployment. Sophisticated modeling of these links involving firms' hiring responses to changes in demand were included in the major macroeconomic models developed in the second half of the decade (for instance, the Federal Reserve-MIT-Penn model).

As an empirical matter, Phillips curves augmented to include expectations of price increase as determinants of wage change, or including other adjustments of wage change to recent and ongoing inflation, fit the facts reasonably well. A Phillips curve with some price inertia implies that even anticipated monetary disturbances have real effects. The difficulty is that there is as yet no satisfactory theoretical basis for such inertia.

Recent research has focused on the details of contract structure and lags and staggering of price adjustments as determinants of aggregate price dynamics and output supply (Azariadis, 1975; Fischer, 1977; Taylor, 1980; Blanchard, 1983; Azariadis and Cooper, 1985). Some of these

papers take the contract structure of the economy as given, and can then show that anticipated monetary policy has real effects. Others pursue in considerable detail the price dynamics that emerge from what seem to be innocuous short lags in nominal price adjustment. It has also been shown in recent work that contracts with wage and price stickiness are possible equilibria in well-defined problems. The latter results do not, however, imply that output responds to anticipated monetary shocks, and the former essentially posit rather than derive that result.

Some progress has been made by positing that it is costly to change prices (Rotemberg, 1982; Mankiw, 1985; Akerlof and Yellen, 1985; Blanchard, 1985). The key result is that output may respond substantially to shifts in nominal demand with even small costs of changing prices. The aim of this research has been to explain the facts of price inertia and the apparent sensitivity of output to nominal disturbances. For that reason there has been little empirical research examining contract terms and the details of price setting at the firm and industry level (Cechetti, 1983, is an interesting example.) Such research would be likely to produce progress both in understanding price dynamics and in discriminating among theories. However, neither contract data nor price setting information is readily or at all available. The contract data exist in the Bureau of Labor Statistics and could be made available; microeconomic information on price setting is probably not available at present.

#### *Unemployment Dynamics*

Considerable progress has been made in analyzing the nature and dynamics of unemployment. During the 1970s, Hall (1975b), Feldstein (1975), and others developed a view, based on the examination of Current Population Survey (CPS) and Bureau of Labor Statistics (BLS) data on turnover rates in manufacturing, that unemployment was an essentially transitory state. The data show that the average spell of unemployment is short and that turnover is high. These results were taken as supporting the view that most unemployment is accounted for by workers who had chosen to become unemployed to search for a new job, or who are on temporary layoff.

Clark and Summers (1979) showed that despite the shortness of the average spell of unemployment, much of unemployment is accounted for by individuals who are unemployed for long periods each year. This result, based on the CPS data, has been supported by subsequent work (e.g., the papers in Freeman and Wise, 1982); it has substantially changed the understanding of the nature of unemployment, and has changed the theoretical focus away from search and layoff theories.

### *The Macroeconomics of the Supply Side*

The theory of growth developed in the 1950s and 1960s was largely a theory of capital accumulation (Solow, 1970). Important empirical research by Solow (1957) and others showed that about half of long-term growth in the United States was a result of technological progress. This was early supply-side economics.

In the second half of the 1970s, supply-side factors became more prominent and more integrated into macroeconomics. Two very different determinants of supply have been studied. The major oil price shocks of 1974 and 1979 have led to extensive research on the role of energy in determining the level of economic activity, and in the response of economies to sharp increases in the price of imported inputs. At the same time, empirical research has focused extensively on the impact of government tax and transfer programs on individual incentives to work, save, and invest. The incentive as well as the demand effects of tax and transfer policies are now regarded as of first-order importance.

In addition to these developments, researchers have studied the productivity slowdown that has plagued the industrialized world since at least the early 1970s and perhaps since the mid-1960s. In a sense, the productivity slowdown is the most important of all macroeconomic problems. Had productivity growth continued at historical levels from 1967 until the present, real U.S. GNP would be more than 20 percent greater today. In contrast, during the 1982 recession real GNP declined by less than 5 percent.

### *Supply Shocks and Macroeconomic Policy*

Naive classical models predict that relative shocks should have no effect on the overall price level. With a constant stock of money, relative prices should change but there is no reason for the overall price level to rise. The evidence of almost every industrialized country following the two oil price shocks falsified this prediction. Thus the supply shock experience has underscored the need for theories in which wages and prices are not fully flexible. The very different responses of the European economies and the American and Japanese economies to the oil shock have attracted attention. Since 1973 the United States has created more than 20 million new jobs, while employment has been completely stagnant in Europe. Research by Bruno and Sachs (1985) provides at least a partial account of this phenomenon. In Europe, it appears that real wages are rigid so that reductions in the standard of living due to oil shocks cannot be transmitted to workers. When wages rise relative to productivity, unemployment increases. In the United States and perhaps Japan, it is nominal rather than real wages that are rigid. Increases

in the price level permit reductions in real wages, which in turn make possible the maintenance of high levels of employment.

The deep question concerns the reasons for these differences in the apparent behavior of wages around the world. The reasons are not well understood. But a start has been made by Gordon (1982) in his careful comparative study of wage and price behavior in a number of nations.

### *Tax Policies*

Empirical research in the last few years has been associated with a substantial change in economists' perception of the effect of tax policies on economic behavior. The traditional view expressed, for example, in Musgrave's (1959) textbook was that taxes had relatively small effects on economic behavior. In many cases this was because income effects and substitution effects were thought to operate in offsetting directions. The view that taxes had only very modest effects on economic behavior was until recent years supported by most empirical research.

Recent research has led to significant revisions in this thinking. Perhaps the clearest example comes in the research on the effects of taxes on labor supply. At the theoretical level, it is now widely understood that it is the compensated effects of tax policies that are of crucial normative importance. More formally, excess burdens are calculated by integration under compensated rather than uncompensated demand curves. Thus theoretical work has made it clear that the crucial empirical issue is the compensated elasticity of labor supply.

Investigating the effects of taxes on labor supply poses important methodological problems because marginal tax rates are to an extent endogenous variables. Persons who work harder earn more income and are pushed into higher tax brackets. In an important contribution, Hausman (1981a, 1981b), building on earlier work by Hall (1975a), showed how the labor supply decision could be modeled in the face of a nonlinear budget constraint such as that induced by taxation. His results suggested that taxes on labor supply have significant effects. An important and policy-relevant finding is that shifting from a progressive to a flat-rate tax could lead to substantial improvements in economic welfare.

Hausman's work focused on the static labor supply decision. The recent development of the Euler equation technique by McCurdy (1981) and others will make it possible to refine these estimates taking account of dynamic considerations. (These methods have been used extensively also in modeling consumption, investment, inventory, and factor demand decisions.)

Research has also focused on the effect of taxation on capital formation. Given the very strong international correlation of domestic savings and investment rates and rates of economic growth, this is an issue of

substantial long-term importance. On the savings side, some recent research recognizing the existence of "human wealth effects" and the relevance of compensated tax changes, argues that economic theory provides the presumption that the rate of savings should respond positively to increases in the rate of return (Summers, 1981). Some supporting empirical evidence has been presented, but at this point the issue is unsettled.

Of greater significance, research has emphasized the important effects of taxation and of inflation working through the tax system on investment. In a series of papers, Feldstein (1983) has presented analyses suggesting that the interaction of inflation and the tax system contributed to the substantial reduction in net capital formation and in the real value of the stock market that the United States suffered during the late 1970s. This research had an important impact, contributing to the enactment of major business tax incentives in 1981. More recent research (e.g., King and Fullerton, 1984) has emphasized the importance not just of the overall level of investment but of its composition. It has been argued that the pattern of investment in the United States is substantially distorted by the wide variation in effective tax rates across different types of capital assets.

Events have also forced a reassessment of the economic effects of social insurance programs. The possible effects of social security on the level of national savings has been a major subject of research. Following the early work of Feldstein (1974), a variety of conflicting empirical estimates have been presented. Research has also shown that unemployment insurance has important effects on the level of unemployment. These operate through a number of channels. Most obviously, unemployment insurance increases the duration of unemployment. More subtly, it increases turnover because employers do not bear any liability for the costs they impose by laying off workers. These issues are surveyed in Clark and Summers (1983).

#### *The Productivity Slowdown*

The productivity slowdown has attracted a considerable amount of research attention. Unfortunately, to date economists have not been very successful in explaining, as opposed to characterizing, the dimensions of the productivity slowdown. Research by Denison, Kendrick, Baily, and others has outlined the dimensions of the problem. Productivity growth began to decline sometime in the late 1960s in the United States and since then has been about 2 percent a year less rapid than it had been during the early part of the postwar period. Particularly large declines in productivity are noticeable in the immediate aftermaths of the 1974

and 1979 oil shocks. The productivity slowdown is a worldwide phenomenon, though. Europe and Japan have witnessed productivity slowdowns at least as large as that in the United States, though starting from much higher levels of productivity growth. Productivity deterioration has been most serious in service industries.

Unfortunately, it has proved very difficult to isolate the causes of these productivity declines. Standard growth-accounting calculations suggest that capital formation does not play a large role. Nor can the productivity slowdown be easily traced to regulatory interventions or to the effects of higher energy prices at least as treated in simple ways. A provocative finding that may point in the direction of future research explaining the productivity slowdown is Baily's (1981) observation that the productivity is quite well predicted if capital is valued at its market value rather than its replacement cost. His argument is that energy-induced obsolescence has led to a substantial overstatement of the value of the capital stock. With capital input properly measured, much of the productivity slowdown may be explicable. This conjecture is being studied using disaggregated data.

Other work has emphasized social dimensions of productivity. A series of studies by Richard Freeman and James Medoff and their students surveyed in Freeman and Medoff (1984) suggest, for example, that in many settings unionized establishments are more productive. This in turn is the result of their lower quit rates, reduced absenteeism, and perhaps better treatment of grievances. Additional evidence supporting the importance of social factors in determining productivity comes from international comparisons of comparable plants. These studies, surveyed briefly in Nelson (1981), suggest that equivalent plants operate in Britain, the United States, and Japan at very different levels of productivity. The further exploration of these social hypotheses regarding the productivity slowdown is likely to involve fruitful collaboration between macroeconomists and economists oriented toward the microeconomic behavior of firms.

### *Policy Modeling*

The standard approach to the modeling of economic policy equips the government with an objective function, a set of instruments, and a model of the economy, and then poses the question of how best to use these instruments to achieve the objectives. This type of problem has been extensively studied in engineering. Engineering-type solutions have provided considerable insights for economics, for instance, on the issue of whether the Federal Reserve should use interest rate or money

stock targets (Poole, 1970), and on how lags in and uncertainty about the effects of policy affect policy decisions (Brainard, 1967).

Significant recent progress in the understanding of policy has come from two sources: the analysis of the response of the private sector to policy changes, and the application of game theory. Analysis of the "failure" of the 1968 tax increase to reduce consumption pointed to the need to distinguish permanent from transitory tax changes. This suggestion was generalized by Lucas (1976), who claimed that existing econometric models were ill-suited for policy analysis because of their simplistic modeling of private sector decisions. Lucas argued that many of the types of policy changes being studied in existing models would change the form of "structural" equations in those models, and thus that the models could not be used to analyze the effects of the changes.

Kydland and Prescott (1977) brought to macroeconomics the problem of the dynamic consistency of government decisions by showing that a government optimizing over several periods might well not want to carry through on previously announced plans. The problem is likely to arise whenever expectations of government policy are important for current decisions. Kydland and Prescott showed that a government doing what was best in the short run might well produce worse outcomes for the economy than could be obtained if it could precommit its policy.

Problems of precommitment and the associated issue of reputation were being studied in game theory, and have now come fully into macroeconomics.<sup>3</sup> Barro and Gordon (1983) showed how a government unable to precommit itself could produce a higher inflation rate than was achievable with precommitment. Such analyses are highly suggestive but have not yet been used in careful empirical analyses of international differences in inflation behavior. The role of the reputation of the government in determining the inflation rate has been examined by Backus and Driffill (1985).

Progress in this area will come not only from empirical analyses but also from the modeling of incentive mechanisms and institutions. Issues of dynamic consistency will play an important role. Topics to be analyzed here would range from the type of incentives that might be provided to policymakers to produce desirable behavior on their part to the issue of rules versus discretion in policymaking.<sup>4</sup>

<sup>3</sup>Pindyck (1977) is an early application of game theory to policy. Pindyck views the government as consisting of two agents, the Treasury and the Fed, whose interactions determine the inflation and unemployment rates.

<sup>4</sup>This principal-agent problem has been extensively studied in other contexts.



## Research Priorities

Research in macroeconomics is typically small-scale, not susceptible to central organization, and with the profession providing incentives for researchers to move into the interesting and potentially productive areas. Our discussion of research priorities has two aims: first, to identify public-good-type resources that would raise the quality of research in the field but that is not likely to be provided within the existing research structure, and second, to highlight the areas in which we believe significant advances are likely in the next decade.

### *Data Needs*

#### *Information About Current Data*

Macroeconomists currently do most of their empirical research with time series data, generated by the government. The standard data are available in data banks from Citibank, D.R.I., and a few other commercial concerns. The data are revised frequently, and there are difficulties, known to some researchers, with several of the series. The data are available in the major economics departments, and at the National Bureau of Economic Research, but not easily to the profession at large. Other time series, for instance, for the cost of capital, or the cyclically adjusted budget deficit, may be created by individual researchers. Descriptions of the data are frequently sketchy at best.

All this has led to a situation where it is often extremely difficult to replicate the results of other researchers. Empirical research would be both improved and made easier if a center were set up, within a university, to maintain the published data, to receive and keep all created time series data that appear in published papers, and to maintain a bibliography of discussion and documentation of data.

The model we have in mind is the Center for Research in Security Prices at the University of Chicago Business School, which puts out data (the CRSP tapes) at a low price. These data have been extensively used in the highly successful empirical research that has been undertaken in finance in the last fifteen years. Such a project would have a high payoff in encouraging and improving the quality of macroeconomic research.

The *Journal of Money, Credit and Banking* has made progress in the direction of data documentation by requiring authors of accepted empirical papers to deposit the data for the paper with the *Journal*. The experiment has not been running long enough to judge its usefulness.

*International Data*

International comparisons provide a potentially fruitful way of testing theories. For instance, more is likely to be learned about savings behavior by comparing the United States, Japan, and Germany than by continuing to mine the existing U.S. time series data. Understanding of wage behavior has been improved by international comparisons that suggest that Europe suffers from real-wage inertia while the United States has nominal-wage inertia (Bruno and Sachs, 1985).

The quality of foreign data has been improving rapidly. The OECD will shortly be putting out a comprehensive set of data on disks on their members, who are all the noncommunist industrialized economies. Some data are available through DRI to those able to tap that system, some are available on the IMF tapes, but only a small subset of the profession has ready access to such data.

The profession would benefit greatly from ready access to these data. It would be sensible, though not essential, that the data be centralized in the same institution as existing U.S. time series data. However, the task might be too large for one center to handle, in which case a separate center for international data should be set up.

*Panel Data*

Some of the most successful recent research in macroeconomics has used cross-sectional, particularly panel, data. These data have been used to examine consumption and savings behavior (e.g., Hall and Mishkin, 1982), labor supply and retirement decisions (Diamond and Hausman, 1984), and individual portfolio behavior (Friend and Blume, 1975). Panel data on labor force participation and unemployment have significantly changed our understanding of unemployment.

But data problems here are far more serious than with time series. The data are not readily available, and because each researcher tends to select a sample for her own purpose, results are extremely costly or impossible to replicate. Most important of all, existing panel data on assets and consumption are seriously inadequate.

High priority should be given both to the construction of two major new panels and to centralization of information about existing panel data. Given recent developments in theory, in which heterogeneity among economic agents often plays a significant role, in econometrics, in which techniques for handling cross-sectional data have been improved, and in computer speed, and given recent successes using panel data, it is likely that major advances will come from the development and study of panel data.

Improved panel data on consumers, including asset, income, consumption, and labor supply data would be of great value. A second panel, on firms, would be of possibly even greater value, for there are few data on firms' hiring and firing decisions, on the nature of the contracts they have, and on their pricing decisions. It would be useful in designing such a panel to coordinate with researchers in industrial organization.

### *Research Areas*

The discussion under Recent Research Developments, above, identified several research areas in which ongoing work is likely to produce significant advances. Here we point to other areas in which progress is to be expected.

#### *The Microeconomic Foundations of Money*

Basic models of money and monetary economies accepted since the 1960s are being reexamined and new models introduced. The new models are precise in the way in which money enters the model, and lead to strong predictions about the behavior of prices, including exchange rates (e.g., Helpman and Razin, 1982). Improvements in the basic theory here should ultimately lead to improved understanding of price dynamics domestically, and exchange rate dynamics. The payoff is not close, but it is important.

#### *The Open Economy*

On the more applied front, the openness of the economy and the role of the exchange rate in affecting the domestic economy raise many research issues. The effects of exchange rate changes on domestic prices and inflation are large. The precise mechanism remains to be described. The fiscal-monetary policy mix appears, under flexible exchange rates, to have a major impact on the trade-off between inflation and unemployment.

The mix also affects real interest rates and international capital flows. In all these areas, applied research isolating the channels through which exchange rate changes work will assist in understanding key aspects of domestic as well as international economic performance. The important work of Kravis and Lipsey (1983) on the failure of purchasing power parity to hold will likely be a basic building block of such research.

### *Economic Growth*

Economic turbulence since the late 1960s has shifted attention away from the determinants of long-term growth. The work on productivity reported above has not at any point been center stage, despite the crucial importance of productivity for long-term growth in the economy.

There are major mysteries about long-term growth, some of them mentioned earlier. In particular, the role of investment is unclear. High-investment countries are also high-growth countries, but estimates of the effect of capital accumulation on growth come nowhere close to accounting for the strength of the cross-sectional relationship.

Because differences among national savings rates are closely related to differences in national investment rates, one hypothesis is that saving is high when the growth rate is high. Another is that embodied technical progress can account for the cross-sectional relationship. International comparisons should be of major assistance in uncovering causal relations at work here. Improved access to international data will provide major benefits in studying growth and other macroeconomic issues.

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PART

**IV**

EVOLVING INSTITUTIONS



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# 16

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## Origins of Culture

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Glynn Isaac drafted this chapter on short notice in the midst of a hectic summer schedule of overseas field work and teaching. He intended to review, polish, and properly reference it upon returning from Africa and Asia, but he became ill and did not return alive. Rather than yield his synthesis to reshaping by other hands, we choose to publish it as he left it, including a memo, written by one of his colleagues, which Glynn appended with the note that "Unfortunately as coverage worked out its material receives only brief mention—in spite of great importance—however your committee should have a copy of the document—."

The Editors

### The Beginnings of Human Behavior

The diversity of the social sciences springs from the unusual mental and behavioral abilities of the human animal. In particular, our species has developed the capacity to exchange and store information. The exchange is predominantly accomplished through language, to which other kinds of symbol systems have been added, such as musical and mathematical notations. Initially, storage took the form of oral tradition, joined by writing some 5,000 years ago, now augmented by electronic and other means. Other animals, including mammals, birds, and social insects, learn from experience and transmit a fraction of that learning across contemporaneous social networks and also down to succeeding generations. However, no other species handles anything like the volume and diversity of communications that all modern human individuals and human societies do. The differences have not been quantified but probably involve a factor of the order of a million or more times, with the gap widening increasingly rapidly in historic and modern times.

The information dealt with is immensely varied but includes patterns of cognition and symbolism, categorization of people and other items and phenomena, rules of conduct, procedures for making things and for doing tasks, stories, songs, dances, music and rituals, etc. A vast corpus of information of this kind is learned by every human child in growing up, and along with language, it comprises the intricate, structured phenomenon that anthropologists call *culture*. The scale and scope of cul-

ture in turn is clearly associated with that most distinctive human organ, the brain.

The human animal has another crucial, distinctive set of characteristics that put the social sciences in business. We are a species with an economy. That is to say, humans exchange not only information, but also goods and services. Human survival and reproduction universally depend on participation in networks of cooperation and interchange. Until comparatively recent times, individuals were generally embedded in networks involving only a few hundred other individuals spread over an area no more than a hundred or so miles in diameter. Now, the networks are vast, many-tiered, and at the largest scale, global.

Exchange of goods and services is not unique to humans; beavers do it, birds and bees and various other social animals do it. What is hyper-developed in humans is the geographic scale of exchanges and the linkage with information interchange.

This kind of perspective on the human condition reflects awareness that our species is derived through the processes of evolution and social-cultural development from antecedent animal forms that lacked the distinctive abilities which now set humans apart. Careful comparisons of humans and other species elucidate the varied results of evolutionary differentiation. However, crucial though comparative studies are, they cannot answer many historical questions. How did humans get to be how they now are? What effects does the trajectory of past configurations have on the characteristics of extant systems? Seeking evidence for an understanding of prehuman and early human adaptive systems is a task undertaken by archaeologists and biological anthropologists working in close conjunction with human ecologists, comparative physiologists, and primatologists, together with a variety of other social and natural scientists. This chapter concerns prospects for these interdisciplinary enquiries over the next decade.

During the century since Darwin, scientists concerned with human evolution concentrated their attention first on securing evidence for the narrative of changes (see Table 16.1). Although gaps and ambiguities remain, as a result of these endeavors, we have a fair outline of the sequence of anatomical changes and of some of the major technological developments. Attention is now shifting to investigations into the dynamics of the evolving system. What ecological relations prevailed at various stages? What kinds of social-behavioral patterns were in operation? What were the interrelations of anatomical and physiological changes with cultural and behavioral developments? What has been the role of increasing use of tools and technology?

Table 16.1 The Elements of the Narrative

1. Between 8 million and 4 million years ago	Divergence from our closest living relatives, the African apes (estimate based on biochemistry)
2. By 4 million years ago	Adoption of bipedal gait Occupation of savannah habitats in Africa ( <i>Australopithecus</i> stage) <i>(order uncertain)</i>
3. Between 2½ million and 2 million years ago	First larger-than-ape-sized brains First stone tools First butchery marks More than one species in the same region (early <i>Homo</i> and robust australopithecine stage)
4. Between 1½ million and ½ million years ago	<i>Homo erectus</i> anatomical format More complex stone tools Spread into tropical and warm temperate Eurasia <i>Homo erectus</i> through early, archaic <i>Homo sapiens</i> stages
5. Between 50,000 and 30,000 years ago	Loss of skeletal robusticity Emergence of art, complex rituals, technical ingenuity Extensive stylistic differentiation and change Rise in population densities? Colonization of high Arctic, Australia, and later America
6. Between 10,000 and 5,000 years ago	Agricultural transition stage. These changes occurred independently and in different order in various parts of the world. Living in settled villages, towns, etc. Storage of food Cultivation of plants Herding of animals Great increases in population densities Accumulation of wealth, concentration of social and economic power ( <i>order variable</i> )

Transitions 1 and 2 involve "biological" mechanisms not too unlike those governing the evolutionary radiations of many species of primates and other mammals, while the last transition, the beginnings of farming, follows on the appearance of populations with all of the distinctive mental, social, and technological abilities of contemporary humans. This report concerns the quest for understanding the distinctive formative tran-



sitions that started with the appearance of the first larger-than-ape-size brains and the first stone tools (3. on Table 16.1) and ended with the loss of "brutish" muscularity (5.). This is what is styled origins of culture research.

### The Dynamics of Change

Scientific concern is currently shifting from concern with the narrative of change to an understanding of the dynamics of change; and a series of interconnected problems have come into focus. Along with this new perspective has come a whole battery of important new methods and approaches. The combination of focused questions and innovative methods will make the next ten years of research distinctive and exciting.

Here are some examples that illustrate the kinds of position and potential that now prevail. In each case a brief synopsis of the questions is followed by a prospectus of promising new approaches:

#### *How distinctive are humans?*

How different is our neurophysiology? Our cognitive and communication ability? Our digestive physiology? Our muscle physiology? And our socioreproductive system?

Strangely, although a great deal of research has been done on these topics for humans as an isolated category and on many different animal species, there has been very little careful scrutiny that compares humans with successively less close evolutionary relatives and which attempts to understand patterns of evolutionary differentiation. This is changing; scientists with anthropological interests are doing bridging research between biological and social science. Current studies include, for example, comparative brain function and development being done at Boston University, Cal Tech, Harvard, Rutgers, and UCLA. Comparisons of human and ape digestive physiology are being done, for example, at Michigan and at the University of California. Comparative reproductive physiology based both on humans with very different life styles and on animals is being done at various centers (e.g., Harvard, the University of Michigan, and the San Diego Zoo).

#### *Food*

What changes have occurred in diet and food acquisition? How have these influenced social behavior? How has diet changed in the recent past? How well are humans adapted to most or all of the major dietary patterns prevailing in the modern world?

During the past decade we have become aware that this topic is crucial both for understanding the past and for coping with the present. The questions are being attacked from many directions, and some major new methods and approaches hold great promise. These range from direct evidence about past diet to the quest for better understanding of ecology and food. In that order, the following examples can be listed:

1. Scanning Electron Microscope (SEM) studies of toothwear are beginning to provide information about feeding habits of fossil hominids (e.g., at Johns Hopkins and at SUNY Stony Brook).
2. It has been proven that the chemical composition of bone is modified in predictable ways by the foods consumed during the life of an organism. Strontium content can be indicative of the amount of meat in the diet, and the stable isotopes of carbon and nitrogen can be indicative of the relative importance of different kinds of plant foods and of the amounts of food from the sea.

These analytic methods are illuminating studies of the recent past, including the spread of maize agriculture in North America and the beginnings of farming in the Middle East. However, attempts are being made to develop ways of applying these methods to more ancient, highly mineralized fossil human bones. These methods are among the most portentous and exciting to come over the paleoanthropological horizon since the advent of potassium/argon dating in 1960. Successful application of these new methods will revolutionize understanding of the past. Various labs are at work on this; e.g., at Harvard, Los Alamos, Madison, Northwestern, Philadelphia, UCLA, and Wisconsin.

3. The discovery of stone-tool-inflicted cutmarks on fossil bones from the same two-million-year-old layers that yield the oldest hominid specimens with larger-than-ape-sized brains and the oldest stone tools. These new discoveries settle long debates over when human ancestors began to eat more meat than do their living relatives, but we still do not know how frequent meat-eating was, whether it was acquired by scavenging or hunting, and what social implications this class of food may or may not have had (see below). A foment of new enquiry involving systematic analysis of stone age faunal remains is getting underway (e.g., at Chicago, Johns Hopkins Medical School, the Smithsonian, and the University of Wisconsin).
4. Actualistic studies of kinds of food available in the various habitats occupied by early human ancestors with analysis of the implica-

tions for ranging patterns, social groupings, and social behavior. A new concept and methodology, termed Optimal Foraging Theory, is providing a useful frame of reference (see below under ecology). For instance, pioneer studies have begun to look at scavenged meat and at tubers as potentially influential and distinctive foods.

This is a field of enquiry which is in crucial need of development. A very large number of researchers need to tackle different classes of food and different ecological and geographic zones. Some initial centers for this kind of work exist at the University of California/Berkeley, University of Georgia, Harvard, and University of Utah.

5. Renewed efforts to recover and study archaeological traces of food refuse, including small fragments of plant remains that can be recovered by careful field methods. Laboratories concerned with ancient bone, shell, and plant refuse as indicators of diet are developing and intensifying their research across the United States. New studies are revealing that in North America, peoples generally rated as hunter-gatherers began to domesticate various plants much earlier than previously known. The same may prove true elsewhere when comparable studies are done. Centers for this research include the University of Arizona, University of North Carolina, Harvard, Kampsville, Illinois, Michigan, and the Smithsonian.
6. Comparative physiological and ecological studies on plant-eating animals have revealed strong correlations between body size and foods consumed, with larger animals able to thrive on coarser, lower-quality foods. Human ancestors have tended to increase in size over the last 4 million years of evolution, but yet seem to have specialized in high-quality foods. The physiological implications of this anomaly and its possible connections to intensified use of tools and equipment are currently under study.
7. Investigation of links between kinds of food, technology, population densities, and health have begun in recent years and were brought into focus by Esther Boserup's *The Conditions of Agricultural Growth* (1965) and by Mark Cohen's *The Food Crisis in Prehistory* (1977).

Studies of the range of foods taken before and after the beginning of farming and research on evidence for stress and pathology begin to inform our scientific sense of what are suboptimal human diets.

*Evolutionary Ecology*

Scientists concerned with understanding the origins and elaboration of human mental and cultural abilities explore the operation of broad ecological principles and processes as a first line of explanation. Modern ecology, in its turn, has been transformed and this is also reflected in its application to human origins research. It is now recognized that in order to understand the life of any organism, including human ancestors, one must work out a complex set of costs and benefits, with energy and reproductive success as the currencies. The ultimate evolutionary audit is concerned with the differential survival of offspring on down the line of generations. This last proviso gives advantages in some circumstances to individuals who become organized into cooperating societies. New theories are restlessly being explored concerning the costs and benefits of being social. Theories concerning the relationships between physiological processes, ontogeny, learned behaviors, and social patterns are under scrutiny.

Several important lines of research are in progress and others are getting underway.

1. Field studies of the feeding, ranging, and social interaction patterns of primate species under a broad range of conditions. At least a proportion of such studies need to be long-term in the sense of spanning substantial parts of the life cycles of individuals of known identity. This is particularly important for apes and other species where we have mounting evidence on long-term strategies and accumulated experience and tradition.

Some analysts are beginning to explore the influence of the characteristics and distribution of food on female social groupings, with male patterns argued to represent a further adaptation to the combined distribution of food and of females.

2. Field studies of the ecology of humans who acquire food by gathering and hunting need to be done in ways that are in part comparable to or link up with the counterpart primate studies. Until recently, few such studies existed and they were almost all purely qualitative. Now optimal foraging theory frameworks are being applied in gathering new data. Some examples of centers for this kind of work are the University of California, Berkeley; Cornell University; Harvard; University of Michigan; University of North Carolina, Chapel Hill; University of Utah; University of Washington, Seattle; and University of Wisconsin.
3. Studies of the properties of primate and nonagricultural human foods, notably their spatial and seasonal distribution in the wild,

associated acquisition and processing costs, energy and nutrient returns, and the problems caused by toxins and secondary compounds. A few exploratory field projects have begun, for instance, on meat and fat in scavenged carcasses and on wild tubers, and problem-oriented studies of particular issues, like coping with tannins and their effect on food choice.

This research cross-connects with that on paleodiet already discussed.

4. Patterns of food acquisition are critical for predicting the ecology and social configuration of any species, and aspects of the human case raise particular questions. At some stage in evolution human ancestors joined an unusual class of animals which carries food to what zoologists call central places (thereby creating some confusion vis-à-vis the same term used differently by economic geographers) where the food is imparted both to young (provisioning) and also to other adults (sharing). This fundamental component of human social behavior is commonly also a central symbol of sociality. It is without parallel among nonhuman primates and it is rare among mammals, with the partial exception of beavers and some social carnivores. It is common only among birds, for whom transport costs are low and for whom the young are particularly helpless. Social insects are the other major set of examples.

The question is, when did human ancestors begin to be involved in these kinds of behavior? What were the ecological and food choice contexts of the shifts?

Archaeologists have focused attention on the issue of the antiquity of home bases and food-sharing, and good progress can be expected over the next decade. There are clear cross-connections between this topic and the issue of the development of human mating systems and "family" units.

5. Knowledge of changes in the habitats and environments that supported successive stages of human ancestry. Enquiry into this topic is a longstanding pursuit, but as understanding of predictable responses to ecological circumstances grows, so will it be necessary for paleoenvironmental research to become more problem-oriented. This is already happening through quantification of mosaic patterns in faunal communities associated with hominoid fossils and by new enquiries into changes in stability and seasonality. Actualistic studies of modern environments comparable to those of the past are needed to calibrate the stratified record, and these are now beginning.

6. Understanding of how the fossil and paleoenvironmental record forms is now recognized as crucial if we are to be able to refine our knowledge of life in the past. This is called taphonomy and it is becoming an active and varied field of enquiry.

### *Technology*

Until recently archaeologists were content to compile narratives of tool forms and to note a tendency for complexity to rise through time. As a part of the analytical ecological thrust of contemporary research, there is a new emphasis on understanding function and on considering connections between technology and the emergence of the other distinctive human characteristics. New methods and approaches are being sought and have begun to emerge.

1. Use damage and use-wear develop on stone tools in ways that are distinct depending on the action and on the substance worked. Chippage patterns develop, and in particular, distinguishable, microscopically visible polishes develop near edges that have been used. The main analytic techniques were pioneered by L. Keeley, working first at Oxford, later at the University of Illinois in Chicago. Application of the method is burgeoning in many labs, but it will be some time before we have a full history of stone tool usage, but already the first applications to 1½ to 2 million-year-old tools imply that getting at a distinctive, high-quality food was an important function from the start—namely, extracting meat and marrow from large animal carcasses. However, some of the earliest tools were used for shaping wood, perhaps for making other non-stone tools, such as digging sticks or spears.

Steady patient application of the research methods should help elucidate connections between technology, food, and ecology.

2. At some stage in human ancestry control over fire became a crucial, novel technology. It had, and still has, importance in several ways, notably: (a) modifying vegetation patterns, generally through the enhancing of grasslands and clearings which then tend to favor increased biomass of large ungulates; (b) providing warmth and protection and a social focus; and (c) helping to detoxify foods rendering them more digestible, so that potential diet breadth is expanded.

Claims that control over fire may go back 1½ million years to the early days of *Homo erectus* have focused archaeological attention on the antiquity of fire control and its implications. Research, urgently needed, is just getting underway.

3. In addition to being agents of adaptation, artefacts can provide measures of mounting mental abilities and are repositories of learned traditions ("culture"). Problem-oriented experimental studies of skill and design are a new arrow in the archaeological quiver and are developing rapidly in scope and sophistication. Experimental work has recently helped demonstrate that the tendency to right-handedness goes back at least 1½ million years to early *Homo erectus*.
4. Archaeologists are developing ways of using ancient artefacts to assess aspects of the scale and character of information transmission systems and networks (see below).

Making tools, therefore, is not simply one curious aspect of human abilities. It may well be integral, in the sense that the very existence of human social systems may be predicated on the exploitation of widely dispersed, high-quality, portable foods (e.g., meat, marrow, large tubers, and much later, grain) and that these foods could not be exploited without tools.

#### *Residential Groups, Networks, and Reproduction*

Modern humans typically live in some kind of family household group, with individuals participating in many different kinds of wider networks of relationships, interaction, and interchange. This is not characteristic of any other primate or indeed of any other animal. How long has this kind of social matrix existed? By what steps and stages did this develop?

Archaeology has a hard time providing answers to these questions, but research attention is now focused on them and steady progress is being made. It can be said that from the time of the oldest archaeological sites 2 million years ago, down to the beginnings of farming 10,000 years ago, sites are of a similar size. This is consistent with day-to-day social groups in the range of ten to thirty individuals. Taken at face value, this may mean that for the vast majority of the human past, groupings were small. Population densities were also surely low.

The oldest definite traces of modules that we would take to be household or "family" units come after the post-Neanderthal loss-of-robusticity transition. Whether or not this is when human-ancestral societies began to be so segmented is the subject of current research.

Also appearing the first time after the loss-of-robusticity transition in the Upper Paleolithic are many signs of wider and more diversified networks, evidence of long-distance exchange of exotic items, evidence of aggregations at ritually important places, and the like. This is also the phase of prehistory where populations with distinct stylistic regional tra-

ditions first appear—some kinds of ethnic entities, one would judge. This phase also gives signs of involving huge increases in the volume of information being generated and transmitted within human systems.

All these emergent human characteristics are currently coming under active, problem-oriented research.

Patterns of social grouping and networks have cross-ties to patterns of mating, breeding, and demography. Human socioreproductive systems are distinctive in ways that involve both physiological and variable cultural components. In humans, unlike most primates and other mammals, ovulation is concealed and there is no coming into heat. Equally, human males expend much more effort than do other primate males in feeding and sustaining their mates, offspring, and kin. Careful comparative studies of the physiological and social workings of the human kind of system and of its ecological, evolutionary origins are needed and are getting underway. There are also some signs of relatively recent major shifts in reproductive physiology; for instance, the recent suggestion that Neanderthal females (and presumably all early proto-humans) may have carried their fetuses to 11- or 12-month terms.

Presumably this shift, if confirmed as real, relates to matrices of increased social nurture for mothers and their more helpless "premature" infants, and also perhaps to the need for a longer duration of growth and learning in a culturally complex milieu.

In recent years the proposal has been advanced that another major change in reproduction occurred when human groups began, perhaps 10,000–15,000 years ago, to be less mobile and nomadic. The argument is being investigated that this led to a reduction in birth spacing and hence a surge of population increase that in turn was part of the context of the beginnings of farming.

### *Aggression*

Opinion has varied greatly as to whether human ancestors were predominantly noble, gentle savages or were, as Robert Ardrey thought, weapon-wielding killer-apes. This important topic is coming under sober enquiry from a number of angles.

Again, comparative primate studies are important. Monkeys and apes, especially males, do fight; so do modern humans, so there is no need to suppose human ancestors never did, and the question becomes one of scale and intensity. How important were weapons? The Guggenheim Foundation has recently funded two anthropologists to do a careful study of fossil hominids seeking evidence of interpersonal wounds. Other studies with this kind of orientation will also be needed.

Fieldwork on chimpanzees has recently brought to light indications that chimps share with humans the dubious distinction of being animals



amongst whom organized coalitions of males engage in lethal intergroup aggression to achieve territorial gains. Confirmation of this is needed, as well as careful studies of its context and its evolutionary and sociological implications.

The oldest archaeological evidence of serious armed warfare, as opposed to skirmishes, is perhaps a 12,000-year-old cemetery in the Nile Valley. Otherwise, signs of warfare follow on in developmental stages where landscapes had become "full" of farmers with wealth in the form of investments in capital improvements to land.

### *Language*

This is the most important component of culture as a mode of adaptation and also the most difficult to study in an evolutionary framework, because there are no living proto-languages and the phenomena of speech do not fossilize directly.

Comparative enquiry into relations between language, symbols, and brain function are a major enterprise that needs to be intensified further. Already, studies show that although human linguistic brain function is firmly rooted in a basic neurophysiological system that we share with other primates, the "hardware" and the "software" of the brain and the way they develop in early childhood are probably very different and very distinct.

Recent advances in experimental neurobiology have opened new avenues of research into the question of human neurological uniqueness. These include investigation of behavioral physiology, neural connection patterns, neurotransmitter anatomy and physiology, hormone interactions, neural development, and gene expression in the brain. Also of current and future importance are ever more thorough and more careful quantitative analyses of brain structures in different primate species, including humans. The studies should continue to provide new insights, especially with respect to brain size, gestation, life-span, metabolism, diet, and other physiological-ecological variables.

Field studies of the communication and signaling systems of non-human primates are underway and begin to contribute insights. Likewise, ecological studies are needed of the circumstances under which evolutionary advantages would accrue to individuals with information exchange abilities beyond those of monkeys and apes.

The antiquity of language is difficult to determine, not only because language leaves no fossil artefacts, but also because the available indirect evidence is confusing. There is now widespread agreement that both the initial stages of brain expansion and stone tool manufacture began about 2 million years ago. But there is also some evidence of a relatively recent change in the structure of the vocal tract which coincides with the

loss in robusticity that distinguished anatomically modern humans from the Neanderthals and with modern-size brains which they had replaced by 30,000 years ago. Deciding which of these data are most relevant must await a more thorough investigation of both vocal tract anatomy and brain structure.

The socioecological pressures that gave rise to the evolution of language are equally uncertain. Competing hypotheses about what made language-like communication useful range from considerations of foraging strategy to problems of mating and infant care. Information sharing could have played a critical role in organizing central-place foraging, in strategies for extractive feeding, in cooperative hunting or intergroup conflict, and even in maintaining stable mating and provisioning relationships between pair-bonded mates as well as between different mating groups. These and other hypotheses can be tested only by pursuing both archaeological evidence for dwelling and foraging patterns (e.g., home-base or tool caches), and integrating these findings with more thorough studies of the dwelling and foraging strategies of modern foraging peoples and other primate species. In this way the study of language origins places the strongest demands on our capacity to integrate paleontological, neurological, ecological, and behavioral studies into a coherent picture.

### *Finding Fossils, Digging Sites*

In popular imagination, dramatic finds of fossilized ancestors are the centerpiece of enquiry into human origins. Given strong public interest, new discoveries are announced through newspaper headlines and arouse great excitement.

New discoveries are important, but the acquisition and study of fossils is now only a part of a much broader realm of enquiry. Often specimens are no longer important just as additions to the human family tree, but as sources of samples for chemical paleodiet analyses, tooth-wear studies, etc., and for the ecological evidence that their context provides.

Searches for new specimens need to continue. In particular, we need fossils from the 4–8-million-year time range that will inform us about the beginnings of bipedalizing and from the 2–3-million-year time range that will better document the first appearance of the larger-than-ape-sized brain. We also need to find a fossil record for our closest relatives, the African apes.

Archaeological enquiry into the role of technology in adaptation, the acquisition and processing of food, the antiquity of home bases, etc., depends on a steady continuation of programs of survey and excavation. There are, of necessity, undertakings that require care and patience.

Fortunately, they too arouse strong public interest that helps sustain investigation in spite of the inevitably slow accumulation of the requisite data.

### Conclusion

It might fairly be said that no society can consider itself civilized unless it has a sense of origins. A set of ideas and beliefs about origins are effectively a universal feature of ethnographic and historical cultures. Before the application of scientific enquiry to the subject, these commonly took the form of myths, legends, and sacred allegories. Now, the findings of biology and geology make belief in the literal truth of those allegories less possible, and people yearn for a replacement in the form of a satisfactory narrative of origins based on material evidence, which can be elaborated and improved upon by the application of the procedures of science.

The thirst for knowledge about human origins makes for avid public interest in this whole realm of enquiry, as evidenced by packed lecture halls and a spate of books and magazine articles. The sums of money spent on human origins research are modest, and this may well be the sphere of science where return of public interest and sense of reward are greatest per unit dollar.

The main role of this field of enquiry is surely education. Chiefly, it helps provide perspectives that make aspects of the human species, its accomplishments, and its predicament seem intelligible. However, it is also the case that as human origins researchers move away from simply being fossil-finders with interests largely in the narrative of bodily changes, so their enquiry into the dynamics of evolution takes them into realms where their research also has strong practical importance for the conduct of life today. This is evident in the range of topics now being researched and which are discussed above: ecology, brain functions, diet, population increase, etc. This shift brings human origins researchers increasingly into dialogue with medical researchers on neuroscience, orthopedics, and nutrition, and with social science scholars of demography, human ecology, and economics.

### Summary of Needs

Research on human origins and the rise of culture has grown greatly and broadened over the past one or two decades. Given continued growth, this endeavor is in a position to make expanding contributions to social

science and to provide links between social and natural science. Support of various kinds is needed:

Laboratory research on

- Comparative neurophysiology and anatomy
- Comparative digestive physiology
- Reproductive physiology
- Chemical composition of bone as an indicator of paleodiets
- Microscopic analysis of toothwear, cutmark, tool use damage

Problem-oriented ecological field studies

- Primate feeding and ranging patterns
- Primate communication systems
- Human acquisition of various nonagricultural foods
- The distribution and characteristics of feeding opportunities available to early proto-humans

Field studies of the stratified record of the past

- Especially searches for evidence from 4–8 and 2–3 million years ago
- Evidence of ancient ecological relations
- Evidence of changing diet
- The development of technology as a mode of adaptation
- Changes in social groupings, networks, and breeding patterns
- Use of satellite images for finding sites and interpreting ecology

## Appendix: Human – Plant Relations in Prehistory

A true revolution is in progress in paleoethnobotany in the United States. Within the past five years more new domesticated indigenous plants have been discovered from remains of prehistoric archaeological plants than at any time in the past and even more are anticipated with additional research. Furthermore, archaeobotanical scientists are working to explain how prehistoric Indians modified both the natural landscape and the genetic constitution of many of the plants prehistoric people used.

At one time it was felt that only a few domesticated plants were introduced from Mexico into the United States in prehistoric times and that these formed the basis of an agricultural economy. Then in the early 1930s two pioneering ethnobotanists, Melvin R. Gilmore and his student Volney H. Jones, noted that several plants native to the eastern United States were different from herbarium specimens and were possibly domesticated. Within the next twenty years two of them, sunflower (*Helianthus annuus*) and sumpweed (*Iva annua*) were confirmed as domesticated. The other plants awaited detailed research. The question of independent domestication or stimulation for domestication north of Mexico could not be resolved until the late 1970s and early 1980s. First, conventional radiocarbon dating of squash rind (*Cucurbita pepo*) from Phillips Spring, Missouri, confirmed that this Mexican domesticate preceded the genetic alteration of indigenous plants. However, the actual antiquity of introduced agricultural plants was not appreciated until the accelerator (TAM) method allowed small samples to be dated. Then it was discovered that squash was in the eastern United States for some 7,000 years but, surprisingly, that corn was less than 3,000 years in the Southwest. With previously held assumptions thoroughly challenged, new efforts were dedicated to the problems of domestication. The revolution was under way.

This Appendix was written by Richard I. Ford, Professor and Curator, University of Michigan, Museum of Anthropology.

The combination of a new dating method and the Scanning Electron Microscope (SEM) led to a refined reassessment of archaeological seeds. A number of scientists in several institutions have been engaged in this research.

Dr. Bruce Smith (Smithsonian Institution, Washington, D.C.) used the SEM to demonstrate that *Chenopodium berlandieri* was domesticated by Early Woodland time and in use in Georgia. Smith has discovered that the same plant was grown in Kentucky and Ohio.

Ms. Gayle Fritz and Dr. Richard Yarnell (University of North Carolina, Chapel Hill) have reexamined dried plant collections from the Ozarks which Gilmore originally suspected were domesticated. Fritz has identified both *Chenopodium berlandieri* ssp. *nuttalliae* and *Amaranthus hypochondriacus* as domesticated in this region. Ms. Nancy Asch and Mr. David Asch (Center for Archaeological Investigations, Kampsville, Ill.) have discovered still other plants that might be domesticates. These are the grass *Hordeum pusillum* and *Polygonum erectum*. Research on these species is continuing.

Dr. Vorsila Bohrer (Eastern New Mexico), while investigating prehistoric Indian use of cool season grasses, has found phenotypic changes in *Hordeum pusillum* also suggestive of domestication or changing selective pressure.

Dr. Gary Nabhan (Arid Lands, University of Arizona) has discovered that Sonoran panic grass (*Panicum sonorum*) may be a domesticate in the lower Colorado River area.

With the exception of the sunflower, all of these plants have been abandoned today. They are found only in archaeological contexts and remain silent evidence of the extensive manipulation of plants by prehistoric Indians. Two questions naturally occur under these circumstances: why were they domesticated in the first place and why did extinction follow so quickly?

I have investigated the relative subsistence importance of domesticated plants in the East and the Southwest. An ecological model considering human population density, territoriality, and dependability of natural biomass accounts for much of the difference. A low biomass in the Southwest made maize a welcome addition to the economy and, by transforming the land into cornfields, new habitats were created for pioneer annual plants. Thus, the overall biomass was increased unlike what was available in natural stands. I have also examined early maize production and the evolution of corn in the Southwest after its introduction from Mexico. Plant remains from Bat Cave and Jemez Cave, New Mexico, have provided the data bases for analysis.

The agriculture history north of Mexico is far from complete. In the East research is beginning with giant ragweed (*Ambrosia trifida*), little bluestem grass (*Andropogon gerardi*), and maygrass (*Phalaris caroliniana*). In the Southwest a close examination of ricegrass (*Oryzopsis hymenoides*), beeweed (*Cleome serrulata*), and others must still be undertaken. A study of these products of human-plant interactions may yield additional prehistoric domesticates, and when combined with those recognized in the last half decade, they may contribute new ideas for again altering their ancestral parents to feed a hungry world.

Interactions with plants do not end with single species' genetic transformations, however. Whole plant communities are altered or created by human agency. These changes must still be investigated more widely than they have been. Humans have used fire to encourage certain plants or they have limited succession in order to increase useful yields over time. The dynamics of such relationships require exploration before scientists can venture what the natural landscape of the United States actually was. Botanists often forget that people transplanted exotic plants, cultivated others, and made new plant communities for their use. In some cases the results of then prehistoric actions are still evident.

Finally, human-plant relations cannot be separated from cultural practices and the prehistoric social order. We have learned that most new varieties of domesticated plants *follow* cultural changes, that new plant communities result from modifications of subsistence practices, and that widespread clearance accompanied the shift from gardens to field agriculture. The explanation, then, of why plants were domesticated, why they were forgotten, and why whole segments of the landscape were modified depends upon an anthropological interpretation of the botanical data identified by paleoethnobotanists. A botanical description of a new domesticate, for instance, is interesting but uninformed without a scientific anthropological explanation. In conclusion, finding solutions to these problems forms an exciting agenda for future scientific research into anthropogenic plants and ecosystems.

## Summary of Consultations

### 1. With a diversity of colleagues at Harvard:

David Pilbeam, fossil record and evolutionary processes

Irvén DeVore, human ecology, primates, behavior

Terry Deacon, brain function comparative studies, and origins of language

Peter Ellison, comparative reproductive physiology

Margaret Schoeninger, paleodiet research

2. Richard Potts, Smithsonian, archaeology and paleoecology
3. Nicholas Toth, University of California, Berkeley, experimental studies of early technology and ecology  
Kathy Schick, University of California, Berkeley, studies of how early sites form
4. Richard Ford, University of Michigan, plant remains and diet
5. John Speth, University of Michigan, archaeology and paleodiet
6. Katherine Milton, University of California, Berkeley, ecology and paleodiet
7. Margaret Conkey, SUNY, Binghamton, on changes in social networks
8. Robert Blumenshine, Rutgers University, early hominid feeding and ecology
9. A group of archaeologists at the SAA meetings in Denver especially concerned with ecology and actualistic studies, including John Yellen, NSF; J. O'Connell, University of Utah; J. Sept, Harvard; K. Jones, University of Utah; and various younger researchers
10. Andrew Sillen, University of Pennsylvania, strontium and paleodiet studies
11. Sherwood Washburn, University of California, Berkeley, paleoanthropology



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## Emergence of Social, Political, and Economic Institutions

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The study of institutions has a long tradition in the social sciences. The present renewed interest stems from the cumulative findings of studies that have given us new insights into the way in which different institutional arrangements affect societal performance and from a recognition that models that do not include institutions are of limited relevance to societies. Institutions have been defined differently. Some scholars regard them as comprising the framework of rules and norms of behavior within which human beings interact; others have also included organizations in their definition. In either case, however, institutions constitute the structure that defines interpersonal relationships. To understand how institutions emerge, change, and decay is at the heart of all the social sciences.

Research on institutions can be expected to answer three major questions:

1. What are the relationships between institutions and performance and what are the consequences of different institutional arrangements upon political systems, economic performance, family structure, stability, and group activity?
2. How do institutions modify the models used by political scientists, economists, and economic historians to provide more complete answers to traditional questions?
3. How do institutions evolve through time, that is, what determines their emergence, stability, and decay?

Recent research can be classified (although with some substantial overlap) into four broad categories:

1. Micro-studies, which consist of detailed studies of specific institutions in particular empirical contexts.
2. Experimental studies, which attempt to create the incentive conditions hypothesized to underpin natural environments and to explore ways in which different institutional arrangements may produce different outcomes.

3. Cross-disciplinary and comparative studies, which attempt to examine through cross-sectional analysis and field study the interaction of institutions in different environments or through time.
4. Historical studies, which attempt to explore over very long periods of time the way by which institutions evolve, change, and decline.

We will attempt to outline a number of promising research frontiers (among many others) in which ongoing research has shown interesting and promising consequences for our understanding of how societies work.

### Micro-Studies of Specific Institutions

#### *The Family as an Institution*

The orthodox view among many twentieth-century social scientists on the institutional fate of the family has been that preceding the commercial and industrial revolutions, there was a kind of family that was extended, patriarchal, based on arranged marriages, and deeply rooted in the traditions of religion and community. Then, under the demands of geographical and social mobility imposed by commercialization and industrialization, this traditional kind of family gradually eroded, to be replaced by a system that was mainly nuclear and isolated from extended kin, based on companionate love, more secular, with greater equality between the sexes and between parents and children. In the eyes of some, this system is more fragile than the traditional form, perhaps auguring the disintegration of the family as an institution.

For the past two decades or so, scholars from the fields of sociology, history, economics, historical demography, and gender studies have complicated and enriched this view of the history of the family. They have uncovered evidence that extensive nucleation of the household preceded any serious industrial development in some places; that patterns of extended kin support persisted even when mobility was increased; that the phases of life are complex and variable and the "stages" such as adolescence, mid-life, and old age are not in any way natural but appear and are consolidated as a product of economic and educational forces; and that complex forms of both gender and generational inequality have persisted even when some economic trends (including increased participation of women in the labor force) and political forces

(measured by the vitality of the feminist movement) have appeared to point in the direction of increased equality.

At the same time, other studies (Fuchs, 1983, Becker, 1981) have elaborated on the disintegration of the traditional family structure as the participation of women in the labor force has grown and many of the traditional functions of the family (education, insurance, and production) have been taken over by government and the market. All of these recent studies of the family together provide us with new insights into the structure and tensions of the most basic of societal institutions.

### *Intentional Communities*

In numerous historical periods, groups of people have been forced to or have decided to leave the societies in which they were socialized to set up a new kind of life on their own. Zablocki (1980) has identified communal movements from the early Roman Empire, in medieval and then Renaissance Europe, and in American history, including the "Shaker Influx" from 1790 to 1805; a utopian socialist period from 1824 to 1848; a mixed but mainly radical political movement around the turn of the twentieth century (1890-1915); and, by far the most significant, the great countercultural burst of the late 1960s and the 1970s.

Communes have reflected a variety of ideological stresses, ranging from anarchism to authoritarianism, from religious to secular, from celibate to free love. They have pulled from cultural sources everywhere to give meaning to their collective efforts at establishing their way of life. Whatever their diversity, communal efforts throughout history have moved away from some type of preexisting society that they found oppressive and have renounced many of the rules and norms around which that society was organized. Among the most explicitly anarchistic and totalistic in their rejection of organized rules were some of the hippie communes of the 1960s.

The best research (Zablocki, 1980) on the communes since the 1960s has seemed to demonstrate that the aspiration not to have any rules has been virtually impossible to achieve. At the very least an organized group of human beings must evolve minimum rules regarding the division of labor, how to handle offensive or threatening behavior (even if by agreeing to "tolerate" it) and problems of intimacy. A special situation is created for those groups with an anarchistic bent; when the pressure to establish even minimal conventions or rules is applied, this peculiarity in itself may become a special difficulty for the group.

It may be that the movement back toward organized community in the communal experiments goes through a set of rough stages. Certainly

an important early stage has to do with the rise of charismatic leaders, whether as exemplars, forceful personalities, or gurus, who in effect impose a kind of order, even though this stage may be accomplished while maintaining the ideological commitments to freedom and without imposing formal rules. Subsequently, charisma seems to be transformed into a number of developing commitments, whereby individuals actually give up a certain amount of their autonomy. In her study of nineteenth-century communes, Kanter (1972) discovered a large array of mechanisms by which this commitment is built and sustained: sacrifice, renunciation, mortification, investment of time and energy, rituals of communion and confession, and "institutionalized awe" of the group, its leaders, and its traditions. Even those communes that remained essentially anarchistic over long periods of time, such as those studied by Berger (1981), still had to come to terms with and develop normative understandings about the mothering and fathering of very young children, as well as sexual access and jealousies.

### **Experimental Research— Methods and Hypotheses**

Experimental research focuses on devising frameworks within which the consequences of different sets of rules on outcomes may be explored. Much of this research is concerned specifically with different kinds of allocation and the consequences they have for performance.

We refer here to an "allocation institution" as an arrangement or set of rules, among or applying to participants, that specifies how to value items or services and how to assign costs or responsibilities among the participants. It is useful to distinguish two very broad types of allocation institutions: (1) market institutions (more-or-less organized or structured); and (2) bilateral or multilateral contracting institutions (these include relational elements).

#### ***Market Institutions***

Market institutions apply to recurrent exchanges among parties who could well be anonymous, that is, the governing rules apply to any transacting party, and the relationships of transacting parties end with the exchange (except, of course, for the occurrence of fraud—misrepresentation of either the commodity by the seller or the exchange medium by the buyer).

Theory hypothesizes that such an institution is characterized by: (1) a language of agent communication (messages) and/or of choice (agent de-

cision actions); (2) a set of process rules, where appropriate, that govern message interchange; and (3) a set of allocation rules that carry agent final outcome messages or actions into individual allocations and cost imputations. An important implication of this theory of institutions is that economic agents do not make or choose allocations, they choose messages or actions; it is the institution (via its rules) that determines allocations, given agent choices. One does not choose to buy something at auction but to bid \$50, which may or may not produce an award under the rules of the auction. A fundamental hypothesis of this theory is that institutions matter (in determining allocations), because institutional rules affect agent incentives in choosing messages, or actions. Two examples follow.

In *auctions* the language consists of bids. In the English auction, a bid (oral or written) is admissible only if it is higher than the standing bid. The allocation is to the last and highest bidder at a price equal to the amount bid. In the second-price sealed bid auction (written) the award is to the highest bidder at a price equal to the second highest bid.

In posted-offer retail markets the language consists of price offers and quantity acceptances. Sellers choose, and publicly post, prices. Buyers choose sellers and privately announce quantity bids. Sellers choose the quantities to be delivered, which may or may not correspond to a buyer's bid quantity, because of stock outs or rationing (e.g., one to a customer while they last).<sup>1</sup>

### *Contracting Institutions*

Contracts define not only the language, process, and allocation rules that apply to the participating parties but also the relationship of the parties over time, the elements that require further or continuing consideration for agreement, the contingencies that, if realized, permit reopening of the contractual agreement, and so on. In short, contracts create a continuing relationship in exchange in which all parties have an interest in maintaining the relationship. Contracting institutions apply where (classical) market institutions would fail or would not be feasible, for example, when the product is not well defined, the risk of precommitment for uncertain exchanges is too great, transactions are nonrecurring, inputs are highly specific, and/or there are common outcome (public good) elements that must be shared by long-term agreement. Since transaction costs are considerable, especially in multilateral contracting, the value of the contract must be substantial for all parties to justify the

<sup>1</sup>See the discussion and bibliographies in Plott (1982) and Smith (1982) for citations.

large set-up costs. In contrast, where recurrent transactions occur for a standardized product such as securities or groups of similar commodities such as art objects, some form of organized market or auction house can spread the set-up cost over a large number of exchanges. Where exchange requires investment in specialized (often site-specific) long-lived assets, efficient contracts require precommitments whose credibility is hypothesized to be assured by the creation of hostage assets. In theory such contracts may effectively eliminate or greatly reduce market risk due to opportunistic behavior by buyers or sellers and yield a de facto form of vertical integration.

Under power *pooling* agreements, as many as twenty-five or more firms enter by unanimous consent into a long-term agreement (unanimous consent) that defines rules, pricing and quota formulas, and compensation terms.

Under a long-term bilateral contract, Firm A, a distributor, enters into a contract with Firm B, a producer. Firm A agrees to take two-thirds of B's output under a "cost of service" pricing formula, yielding substantially lower unit costs to A than the cost of proximate sources. Firm B uses this contract to help obtain financing and proceeds to contract for the construction of its product. In this contractual arrangement Firm B pledges a productive asset entirely to Firm A, and Firm A pledges cash resources to B sufficient to cover all costs. The risks of unrecovered capital costs due to extended outage are shared between A and B.

Ceremonial exchanges among stateless peoples, such as the potlatch, kula, moka, and abutu, not only bought valuables with other valuables in ordinary internal or external market exchange, but also, with the exchange of nubile women and men, created a reciprocal hostage exchange in the short run and kinship ties in the long run. The ceremonial exchange therefore can be interpreted as buying political stability and a property right environment that made ordinary exchange and specialization possible.<sup>2</sup>

### Cross-Disciplinary and Comparative Studies

Studies that have examined the price of oil over the past twenty-five years, from the oil import quota system established by President Eisenhower in the 1950s through the recent rise and decline of OPEC, show

<sup>2</sup>Contributions and surveys in the research areas identified above are represented by such papers as Hurwicz (1973), Smith (1982), and Williamson (1981).

that the price and the quantity of oil have been fundamentally influenced by political-economic structures. Studies by Barzel and Hall (1977) on the oil import quota system and by Kalt (1981) on the political economy of oil prices in the postembargo system provide a new set of insights into the way in which oil prices have developed. These studies show that particular rules and the outcomes of those rules as well as the committee structure of Congress and the ideological conviction of congressmen and their views with respect to allowing markets to work fundamentally shaped the way in which oil prices changed throughout this period. Moreover, they show how these particular institutional arrangements, by influencing the price of oil, in turn altered the political-economic systems of both the oil exporting and the oil importing countries. These studies point the way toward the development of a political economy that integrates economic and political theory with a developing theory of institutional structure.

Contemporary attempts to implement market solutions to key policy problems provide additional real-world demonstrations of the interaction between political and economic institutions. One set of examples is generated in the "privatization" and deregulation movement in developed industrial countries. Another is offered by the process of structural adjustment and financial stabilization in the developing world. The study of these policy trends offers a means of founding the study of political economy on firm ground. In addition, both sets of policies represent the *choice* of market forms, and an in-depth study of the political process by which market mechanisms are chosen in preference to government regulation would give insight into the political dynamics that lead to the creation of markets, the most basic of economic institutions.

### Historical Studies

The perspective of archeology, unique within the social sciences, spans tens of thousands of years. Thus archeologists are in a position to examine the rise of the first sociopolitical institutions. They can consider such issues as which simpler institutions preceded the more complex institutions, what the conditions for and the long-term consequences of each institution were, and what the selection pressures for the emergence of a given institution might have been.

Some current research concerns the origins of agriculture and village life; social ranking and hierarchical society; and the state or stratified political societies. The institutions involved include corporate village organization; hereditary ranking or nobility; long-distance exchange; war-



fare and tribute collection; kinship; state religion; economic reciprocity and redistribution; marriage and military alliance; and the institutional role of descent and the ancestors.

A series of recent historical discussions on the transition from one broad type of societal organization to another—the decline of the ancient world, the rise of feudalism, the transition from feudalism to capitalism—give especially interesting insights into the rise, establishment, and fall of institutions. The advantage of historical work on problems such as these is that it allows us to focus on change over time—to carry through analyses of processes of evolution, not merely exercises in comparative states. Here, we shall concentrate on the rise, development, and fall of feudal institutions. This is, in part, because there is such a distinguished line of work in this field, going back to the classics by Marc Bloch (1961), Pollack and Maitland (1895), and others and continuing up to the present. But it is also, in part, because the substantive material provides a particularly helpful counterpoint to our analyses of modern institutions.

Recent work on the origins of feudal institutions has emphasized the lateness of their emergence and the difficulty of their establishment. This work has focused on two inextricably related processes: (1) the organization of institutions by which lordly groups constituted themselves as self-producing groups; (2) the creation of institutions that allowed the lords to impose regular levies or taxes (feudal rent) on the already well-established peasant communities. Thus, Duby (1968, 1974), building on the work of Bournazel (1980), Fossier (1982), Poly (1966), and others, has brought out the fact that the warrior bands that would ultimately constitute the basis of feudal society were, for centuries, unable to establish a regular relationship of rent appropriation from the peasantry. They were obliged to maintain themselves instead by more or less regularized raiding expeditions. The establishment of stable feudal groups seems to have depended on a long process of trial and error, which gave rise to a series of interrelated institutional and technological innovations. There was, first of all, the creation of the feudal bond—an institutionalized relationship of service in exchange for income and protection—which took various forms and which linked overlords to vassals. Second, there was the overlord's construction of castles, which provided the permanent base for the retinues of mounted vassals (knights) who gathered around them. The increased cohesiveness and military capacity of the lordly groups that resulted from these innovations allowed them to establish permanent positions vis-à-vis the peasants.

Research on the evolution of feudal institutions has shifted from an earlier emphasis on the rise of an embryonic urban commercial society

within the framework of rural agrarian feudalism to a focus on agrarian organization at the level of the peasant village and especially the development of the feudal warrior class. Thus, important studies by Postan (1972) and Le Roy Ladurie (1975) have emphasized the self-reproduction of peasant communities constructed upon peasants' strong control of subsistence plots and community regulation of the peasant economy, and, in turn, the peasants' lack of responsiveness to commercial stimuli. These authors hold that population growth, rooted in the peasants' desire for children to provide social insurance in old age, provides the main pressure for socioeconomic change instead of commercialization. Population growth leads, in Malthusian/Ricardian fashion, to subdivision of holdings, rising land prices, rising food prices, and falling wages. But since the producers do not respond productively to market signals with a reallocation of resources and with new specializations and innovations, population ultimately reaches a ceiling and, following Malthus, enters into decline, leading to the opposite economic trends—the buildup of holdings, the fall in land and food prices, and rise of wages. In sum, a cyclical economic pattern replaces the earlier evolutionary model.

At the same time, studies of the lordly class (Searle, 1980; Hilton, 1966) have moved away from a previous understanding of the lords as above all organizers of agricultural production. Exposing the lords' difficulties in carrying out agricultural improvements, they have stressed their preoccupation with military and political organization and with institutional and technological innovation in the field of military-political competition made necessary by the ever-present military-political competition. Recent historians have thus brought out the increasing sophistication of state institutions over the feudal epoch: for the earlier feudal period, they have concentrated on the development of the legal relationships by which the lords regulated their relationships to one another and to the peasants; for the later period, they have focused on the construction of the administrative apparatus of tax collection and on the buildup of standing armies.

Correlatively, recent work on the decline of feudal institutions has comprehended institutional change as stimulated by crises: the (temporary) inability of lords and peasants to maintain themselves in the accustomed manner, under pressure of the crisis of feudal production; the resultant intensification of conflicts between lord and lord and between lord and peasant to redistribute the product; and the ultimate search for a way out through the creation of new institutional arrangements (Bois, 1981). Specifically, the rise of absolute property, freed from the control of peasant possessors and peasant communities and immune from parasitic, politically based extraction, is seen to result from conflicts in which

neither lords nor peasants succeeded in achieving their ends—in which lords failed to impose either serfdom or an absolutist tax state on the peasants and in which the peasants failed to keep the land from falling into the lords' hands.

The study of the rise, evolution, and decline of feudal institutions—in relationship to received understandings of modern institutions—has given rise to certain broader generalizations and hypotheses. Once established, the various basic institutional arrangements that constitute different property systems will determine fundamentally different paths of socioeconomic evolution. Moreover, once established, these property systems tend to be, within limits, powerfully self-reproducing and resistant to self-transformation, so that the problem of explaining the transition from one system of social-property relations to another is quite distinct from the problem of explaining the evolution within a given property system.

1. Under different, established property systems, individual economic actors will find it in their rational self-interest to adopt different and quite distinct strategies as the best means to maintain or improve their condition. For example, peasants in full possession of their land and tools may have diversified rather than specialized to avoid the risk of market dependence (given the low level of productivity and the ineffectiveness of markets) and thereby may have avoided subjection to the whims of the market. They did not, that is, maximize exchange values, as economic actors can be expected to do under a capitalist system of property relations, where everyone is separated from their means of reproduction/subsistence (both organizers of production and direct producers).
2. Under certain established property systems even those economic actors who wish to increase or improve production may find powerful barriers in attempting to do so. For example, under feudal property relations, the fact that the peasants possessed the means of subsistence posed very great difficulties for lords who wished to organize them in production, but who could get them to work only through coercion or prohibitively high material incentives; lords therefore found it quite hard to organize production efficiently or to carry out improvements. Similarly, potential accumulators and improvers may have found restricted markets in land or labor power and/or markets, thus facing "overcompetition" from those who are producing not for profit but merely for maintenance/subsistence.

3. When it is difficult to improve production, individual economic actors will find that if they wish to improve their income they must appropriate it coercively. This places a premium, in relative terms, on developing coercive means to redistribute the product, rather than on developing the means to produce. Under feudalism, then, lords may have found, given their inability to get the peasants to develop productive forces, that developing their military capacity through alliances with other lords and the buildup and improvement of weaponry was the best way to maximize wealth.
4. Once established, different property systems give rise to different sorts of fields of competition, in which very different sorts of adaptive behavior are "selected." Under the feudal property system, lords and peasants were shielded from competitive production requirements by their possession of the means of reproduction. However, widespread dispersal of the means of force with the premium on redistributive methods of acquiring wealth tended to subject the lords to political-military competition. Thus, investment in arms and political organization superseded the investment in productive capacity that we find under capitalist property relations, where the economic actors have no choice but to produce for exchange and therefore must produce competitively.
5. Under most property systems, individually rational patterns of economic behavior can be expected to lead to aggregate increases in productive efficiency only to a quite limited extent, in part because most property systems appear to encourage diversification rather than full-fledged specialization, as a shield from productive competition. Under most landed property systems, then, the scope of the invisible hand will be quite limited, the impact of prisoners' dilemmas quite pervasive.
6. Individual economic actors will not, as a rule, find it in their rational self-interest to attempt, on an individual basis, to transform the overall property system in piecemeal fashion, even where they could feasibly do so. For example, feudal lords would not find it to their advantage to free and dispossess their peasants and, in that manner, constitute capitalist property relations within their units. This is because, having made such a change, they would be without laborers in a society still without a labor market.
7. Groups of economic actors will not, as a rule, find it rational to seek to transform the overarching property system for economic development. This is not only because of lack of information and

the difficulties of coordination but also because of the problem of envisioning an alternative structure more favorable to economic growth (unless of course such a structure has already emerged elsewhere).

The above summary suggests the valuable directions open for further research.

1. The study of the family and intentional communities provides us with a set of building blocks that through elaboration can provide us with in-depth micro-studies of particular aspects of societal relationships.

Evident changes in the family give the study of this central social institution great value for both scientific and policy purposes. The extraordinary increase in divorce rates, the great increase in household formation of types other than the "standard" family of parents and children, the great increase in proportion of mothers in full-time jobs in the labor force, and the increase in proportion of couples choosing to have no children are indicators of these changes.

It is clear that there also has been an implicit change in the constitution created at the time of marriage. Certain aspects of this change such as greater equality between the two partners are evident, but others are less clear. What change is there in the fraction of individual rights that are put under the jurisdiction of the couple? What change is there in agreements about the kinds of actions that are cause for dissolution? What changes are there in the relative rights and responsibilities of parents and of the state vis-à-vis children? Studies based on interviews and observation of the implicit institutions established by couples in more and less traditional social settings can aid both the understanding of constitution-formation in general and the understanding of the directions in which the family as an institution is moving.

A second institutional area related to the family is the local community. The local community can be regarded as an intangible and "emergent" institution, which despite its evanescence can exercise great force on the behavior of individuals within its scope. The principal means by which this occurs are informal norms and the social processes through which sanctions are imposed to enforce norms.

The changes that are occurring in this institution are largely due to two kinds of social changes, both technologically induced. One is the dispersal of interests and activities of members of a local community largely due to transportation changes which separate locus of occupation from that of residence. A second is invasion of community activities from the outside, largely through television and other communications gen-

erated outside the community. These two changes affecting the structure of local communities have their principal effect in reducing the strength of community-generated norms and in substituting norms generated from other sources.

Research on the processes of norm-formation in local communities can both illuminate the changes that are occurring and anticipate the further normative consequences of additional changes in the structure of local communities. This research would build methodologically on a tradition of community studies in sociology and political science, but with a much greater focus than most studies in that tradition and with a greater emphasis on comparative work. The interplay of theory concerning norm-formation and empirical research focusing on norm-formation and on sanctioning mechanisms should be especially productive.

2. If experimental methods are to contribute to our understanding of the emergence of institutions, two potential research areas not yet studied are in need of exploration. One area concerns experiments in which economic agents have the right to choose from a prescribed menu of institutions, which structure(s) they wish to act under. Since any institution will have its own subjective transaction cost characteristics to be weighed against its efficiency or other valuation criteria, this experimental environment will allow such trade-offs, or comparative institutional value, to be studied empirically. The second need is for experiments in which subjects are allowed to invent and try out their own rules or contractual arrangements, perhaps within a "constitution," in which only general rules of procedures and agreement specify the institutions under which they will operate.

3. Recurrent interaction of political and economic studies, such as those briefly outlined with respect to the price of oil, suggest the direction of a great deal of current research in political economy. This research needs to apply the tools and techniques of transaction costs analysis, game theory, and other new theoretical insights to the evolving interrelationship of political structures and property rights in our society, so as to give us in-depth studies that will allow us to make sense of the current policy prescriptions and performance characteristics of economies and political systems.

We are just beginning to understand that institutions play a fundamental role in our ability to create hospitable environments that will smooth the path and improve the performance of Third World economies and societies. Anthropologists once assumed that residence and kinship were the primary institutional bases from which the political order emerged and that political communities, even in industrial countries with well-developed class systems, were ordered on local constituencies.

In the twentieth century neither kinship nor locality has retained primacy. The speed with which people are freeing themselves from locality, as capital has freed itself, is intensifying. International migration, international markets, international capitalism, international terrorism, are reflections of this reordering of the institutional basis of social life, a revolution associated with the revolutionary communication systems that move people, goods, capital, and information (Robertson, 1984; Colson, 1982).

Comparative work needs to be done on the speed with which new institutions are arising in Africa and Asia. Populations have responded to freedom from colonialism and the realization that political power gives access to massive resources far beyond those at the local level by associating themselves in power blocks, increasingly based on appeals to ethnicity, which become more deadly as the stakes appear to increase (Gellner, 1983). In contrast, those who feel themselves vulnerable to the new order are also finding solace in appeals to the protection of cults, which draw people together on the basis of some common affliction or belief. Here they find the fellowship and support that the old generalized interests of kinship and local communities can no longer ensure, giving them extended horizons of their world. In North Africa and elsewhere in the Islamic world, there has been a resurgence and spread of Sufi orders, which have a comparable role. Various long-term anthropological studies have monitored such changing rules of association and have examined the interplay between political and economic forces in the insecurity of individuals. Studies now in place need to be continued and others now engaged in initiating field research should be encouraged to adopt a research design that will make further monitoring feasible and productive. Out of such long-run anthropological studies, combined with new insights into political economy, we can hope to provide a solid base for more imaginative, constructive, and worthwhile projects to be of assistance in the Third World.

4. Historical research must proceed in a rigorous manner from explicitly theoretical perspectives and test explicit hypotheses that follow from the theory. Required would be the cooperation of significant numbers of theoretically concerned historians and historically involved social theorists. Such cooperation would represent a giant step forward, for today social scientists and historians proceed largely in separation from one another. Historians ignore social scientists because the social scientists' hypotheses, all too often, seem so distant from the historians' materials; and social scientists ignore historians because the historians' research, all too often, proceeds without explicitly stated concepts and hypotheses, let alone concepts and hypotheses derived from social sci-

ence. Ideally, historians and social scientists would constitute teams aiming to investigate specific, though relatively broad problems, such as the origins, evolution, and decline of feudal society. All members of such teams would have to be individuals sensitive to both historical and theoretical dimensions of the problem. The collective would be constituted of specialists in regional/national histories and specialists in certain disciplines (for example, economic history, demography). The team would be required at the start to state its broad theoretical perspective and to specify the hypotheses, at varying levels of generality, it proposes to test through comparative study. For example, to further investigate feudal evolution, the team might examine the following questions: is there, as there appears to be, a specific demographic dynamic bound up with the characteristic property structure? Under what conditions do peasants respond to market opportunities by maintaining their diversification? By specializing? Is there, within feudal society, a general tendency to the development of larger and more complex states? What determines interregional variation within this broader evolution—the conditions under which states tend to be (or not to be) constructed and the conditions that make for success and failure?

For such work to be feasible, the members of the team would have to reside together for certain specified time periods to develop their ideas and, where they are complementary, to coordinate their research.

The above summaries of research make clear the kinds of resources that will be needed to carry on useful work on the evolution of institutions. Given the structure and rewards system of the social science disciplines, this research may not be forthcoming without specific financial support to overcome the following disciplinary limitations:

1. Much social science research on institutions tends to be parochial and narrow in focus. Such research fails to recognize the great diversity of institutions.
2. Other social sciences, notably in economics, political science, and economic history, operate with models that have no institutions and strongly resist "diluting" their disciplines by incorporating institutions into the analysis.
3. There is a lack of interchange between social scientists of different disciplines.
4. There is a lack of interchange between empiricists doing field work on institutional change and theorists developing models of institutions.



In order to overcome these parochial biases, we need financial support to create institutional frameworks that encourage the kind of research outlined above. Specifically, we recommend support for two kinds of research centers.

1. Cross-disciplinary research centers that bring together scholars from different disciplines. We think it is particularly important to provide support for scholars from different disciplines to learn the techniques, methods, and modes of modeling of other disciplines. Without this investment, fully integrated cross-disciplinary research is unlikely, for such cross-learning provides little immediate payoff in the normal publish-or-perish environment. Special inducements will be needed to encourage scholars to break out of the easier research patterns. The research centers should provide not only support for cross-disciplinary research for senior scholars but also three-year postdoctoral fellowships to give young scholars such opportunities and to encourage the development of scholars with these kinds of skills for the future.

2. Experimental laboratories, which are currently underfunded in the social sciences (though common in the physical sciences). The special advantage of experimental research is that one can control the environment by specifying the reward (value, induced demand) conditions of an experiment, and by assigning property rights to subjects that correspond to the specification of the institution. One then observes the behavior of individuals and of the market (prices, allocations) to determine the manner in which behavioral performance is affected by components of the institution or of the environment or by their interactive combination. An example of an economics laboratory developed for these kinds of studies is the computerized economic science laboratory at the University of Arizona. In operation for many years, the laboratory at Arizona has recently been funded on a continuing, separate, annual budget line by the state legislature. No continuing federal support for laboratories of this kind now exists.

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## Causes and Consequences of Demographic Change

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## Background

Demographic change includes changes in population size, growth rates, age/sex distribution, and geographic distribution. These variables in turn are a function of a population's past and present levels of fertility, mortality, and migration. The study of demographic change is distinguished from most social science research in several major respects:

1. The variables of central interest are outcomes of biological processes. The study of demographic change therefore involves important elements of epidemiology, reproductive biology, and medicine/public health. These disciplines are particularly important in understanding the intermediate variables affecting levels of fertility and mortality.

2. The variables of central interest are properties of a collectivity rather than of individuals. This orientation has led to more emphasis on macro-level analysis than is now common in the social sciences, although population analysts also rely heavily on information about households and individuals (micro-level data) in order to elucidate the factors associated with demographic change.

3. Population size, growth, and structure are related in ways that can be expressed in a series of mathematical identities. Demographers are continuing to develop and refine these identities (and approximations to them), which are useful in the measurement of demographic processes. This use of mathematics in population studies is quite distinct from that in most social sciences, where its function is to show the implications of certain behavioral assumptions.

4. Demographers are much closer to the data production process than are most other social scientists. Data produced by and for demographers are widely used throughout the social sciences. Perhaps as a result of their familiarity with data collection activities, demographers tend to be more skeptical about data quality and to spend a higher fraction of research effort in data evaluation.

5. Some of the most significant demographic changes today are occurring in developing countries. As a result, the leading population re-

search institutions are devoting a majority of research efforts toward developing-country studies, a higher concentration than in other social sciences except anthropology.

6. A large fraction of the research on causes and consequences of demographic change occurs in interdisciplinary population research centers within universities. About twenty such centers are currently in existence. Other major research centers are the Population Council, the Rand Corporation, and the Urban Institute. Compared to centers in other social science areas, population research centers tend to be well-funded. This situation reflects two essentially unrelated developments. First, the widespread perception of overrapid population growth in developing countries has directed substantial funds into the field from the Agency for International Development, the United Nations, the Hewlett Foundation, and other foundations and intergovernmental agencies. Second, domestic population research is supported by the National Institute of Child Health and Human Development, whose budget has been protected from many of the incursions in other social science areas.

## Emerging Research Areas

### *Changes in Family and Household Structure in the United States*

Demographers have documented major changes in family and household structure over the past three decades (e.g., Cherlin, 1981; Thornton and Freedman, 1983). Rates of nonmarital cohabitation, out-of-wedlock births, and divorce have risen sharply while rates of marriage and child-bearing within marriage have fallen. In one sense, this declining significance of family boundaries means that there is less reason to be concerned with family relations. But societies continue to rely on families to accomplish many social goals. In particular, family members are often expected to help in the financial support of members of the family who are not self-supporting. The flow of resources among members of the same family, whether or not they are living together, is poorly documented. So is the flow of other forms of material and emotional support among family members. Much of the information that does exist derives from the University of Michigan's Panel Study of Income Dynamics, a study that is constantly threatened with elimination.

For informed social policy decisions, it will be vital to have information on the degree to which existing relationships are satisfactory in sup-

porting groups of special concern. One can foresee in particular a major social policy issue over the next decade regarding the degree to which the financial costs of children should be borne by parents and the degree to which these costs should be covered by the state. This concern will be fed by fears of population decline, with below-replacement fertility levels having already persisted for over a decade; by the realization of the difficulties in an aging population of supporting social programs heavily directed toward the elderly; by increasing refusal of parents (especially fathers) to pay the full costs of childrearing; and by recognition that mothers deserve compensation for their efforts in performing a socially valued task.

A major question will be how social policy can intervene to help children realize their potential without weakening the remaining strong points of the family. It would be unfortunate if the social sciences were unable to offer sound studies as a guide through this policy thicket. Studies will be needed to document both the extent of kin networks and coresidential arrangements and their significance in terms of resources exchanged. These include the increasingly complex structures produced by family dissolution and recombination. Existing survey and census data-gathering activities, based on coresidential households as the ultimate sampling unit, are often ill-equipped to obtain information on the new forms of family relationships; new data-gathering strategies will be needed. The required data are also highly personal and sensitive, since they have a bearing on legal obligations (e.g., paternity, child support, or alimony) and sexual practice. The consequences of changes in family structure for child development will require either prolonged panels or extensive retrospective questionnaire designs. New data, new concepts, and new methods of analysis are needed to progress in this important task.

A related issue is why the family changes have occurred. Major explanations involve rising income levels, increased earnings opportunities for women, changes in social policy, and ideational/cultural change (Espenshade, 1985). While valuable insights have emerged from research on this subject, it has typically been based upon a single and relatively narrow disciplinary paradigm. The alternative explanations have not been evaluated vis-à-vis one another and have rarely been addressed in a systematic way individually. The role of ideational and cultural change is particularly poorly understood and the means for studying this role are poorly developed. Veroff et al. (1981) have clearly documented that the manner in which Americans think about family roles changed sharply between 1957 and 1976; but explanations of additudinal shifts



have not been persuasive. Obviously, progress in this direction is important for reliable prediction of the effect of family policy on family functioning.

### *The Volume of Immigration and the Acculturation of Immigrants*

Information on the volume of net migration to the United States is shockingly poor. This poor quality of data affects not only the measurement of this component of population change, which may account for more than half of recent U.S. population growth; because migration is a major component of population change, poor migration data also mean that identities describing population change are subject to major measurement error. The Census Bureau has used this argument, among others, to explain why it will not adjust areal census counts for enumeration error. Means do exist to improve knowledge of this element of population change, as described in a National Academy of Science publication (Levine et al., 1985).

Beyond the issue of the volume of migration are issues of social policy. If fertility remains as low as it has been for the past fifteen years, a major discussion is likely to ensue regarding immigration as a means of ensuring population stability or growth. Much of the potential movement to the United States originates in Latin America, the Caribbean, and Asia. It will be important to have examined carefully the experiences of recent immigrants from these areas in order to assess the advisability of increasing or reducing migration flows. The degree of assimilation of these new migrant groups with respect to schooling, language, labor force, and other dimensions will prove centrally important in this discussion. If the new streams of migrants are assimilating at rates similar to old streams, then the background for policy decisions is likely to be quite different than if they are not. Comparative work on policies and experiences in other developed countries, especially Sweden, France, Italy, and Germany, would also help inform U.S. policies to the extent that it analyzes the effect of immigration from poorer regions on wages, returns to capital, social welfare expenditure, and population growth.

Work on migrant assimilation and effects of immigration is not being done in any concentrated way and represents a high priority for the next decade. Some of the needed work can proceed along well-developed lines but other work will need to improve methods for reaching, gaining the confidence of, and interviewing illegal migrants. Massey's (1985) demo-ethnographic research demonstrates the feasibility of acquiring

detailed information about the motives and effects of illegal migration among the migrants themselves, both in sending and in receiving areas.

### *Social/Behavioral Aspects of Major Health Problems*

American mortality is strongly affected by factors that lie within the control of individuals and households. Patterns of prenatal medical attention, nutritional supplementation, age at maternity, alcohol consumption, and smoking are some of the major influences on infant mortality, many of them working through low birth weight. Suicide, homicide, and motor vehicle accidents are the major causes of the high death rates at ages 15–24. Cigarette smoking is responsible for around half of the enormously large sex differences in mortality at the middle and older ages (Waldron, 1985), which in turn has produced disturbingly unbalanced sex ratios at older ages.

It could not be more obvious that the social and behavioral sciences have a major role to play in studying ways to improve health. Demography in particular has considerable integrative potential because of its attention to population-based changes in the distribution of risk factors and their implications for various diseases and causes of death, while taking account of competing risks and the underlying population structure, which itself is changing in response to mortality change. The well-developed and carefully elaborated mathematics of population processes are well-suited to framing research questions and demonstrating the full implications of changes in risk factors for various important social variables such as age structure and the prevalence of morbidity (e.g., Manton and Stallard, 1982). Demography has clearly come to play this kind of central role in health and mortality studies in developing countries. But the U.S. health research establishment has been slow to recognize this potential of the social sciences, relying principally on the “medical model” of disease. This model dominates the National Institutes of Health, which has done little to encourage systematic application of the social sciences to improving American health.

### *Factors Influencing Fertility in Developing Countries*

In developing countries, much of the research attention over the next decade will continue to be focused on an issue with which it has been preoccupied for the past decade: determinants of fertility levels. This research focus follows directly from policy concern with rapid population

growth. The past decade has seen great progress in sorting out the biometric features of the fertility process. For example, the sharply increasing returns to improved levels of contraceptive effectiveness as an influence on birth rates has been clearly demonstrated through birth interval models, as has the key role of breast-feeding as a fertility-suppressant (Bongaarts and Menken, 1983). Many useful behavioral studies of fertility have been conducted from virtually every disciplinary angle. Much of this work has been catalogued in a volume commissioned by the National Academy of Sciences, *Determinants of Fertility in Developing Countries* (Bulatao and Lee, 1983).

We believe that a key to research advance over the next decade is careful analysis of specific cases of fertility decline in their full social context. That context embraces family economics, local and national administrative systems, cultural change, and, not least, deliberate governmental measures aimed at reducing fertility. Explanations of fertility change are still quite unsettled. Some explanations emphasize the elimination of "unwanted" childbearing as a central factor accounting for fertility decline; others, the importance of structural factors that influence the perceived costs and benefits of children; and still others, the diffusion of fertility control behavior as an innovative/imitative activity, often closely connected with the spread of Western ideas through the developing world. The importance of this latter process was recently underscored in a review of the fifty-odd countries that were subjects of the massive World Fertility Survey program (Cleland, 1985). Its role was also supported by the results of the massive European Fertility Project, one of the largest comparative historical social science projects ever conducted (Coale and Watkins, 1985). This study established the inadequacy of social-structural explanations of fertility decline in Europe and the importance of linguistic boundaries as "firebreaks" in the spread of fertility control.

Particularly promising are more careful analyses of existing government programs to reduce fertility. These programs are an "exogenous" variable to households and hence help to identify statistical relations that are otherwise obscured by interdependent household decisions. Many nations are conducting experimental interventions whose careful evaluation would shed a good deal of light on determinants of fertility. Unfortunately, the standards of evaluation are quite poor. Often, goals of a project are stated in terms of service delivery rather than demographic impact, so that substitutions of contraception supplied by an official program for preexisting forms is given equal weight with new contraception. Usually, no control group is identified and no imagination used to find a surrogate. If evaluation is done at all, it is usually conducted by

the same agency that manages the program, introducing many possible biases. We believe that scholars must be more active in involving themselves, sometimes uninvited, in analysis of program effectiveness.

Additional insights can be gained through ethnographic methods, which are only beginning to be used in fertility research. The quantitative dimensions of the fertility process in developing countries are known fairly well in the fifty or so countries that conducted a survey under the World Fertility Survey program and from a second round of this activity. But the list of potential explanatory variables available in these surveys is very short. And little effort has been made to elicit from respondents their own views of the childbearing process, what role it plays in their lives and in that of their families. Such insights are necessary in order to elucidate the manner in which macro-level institutional and cultural factors impinge on individual decisions. Intensive interviewing with open-ended questions over extended periods of time, preferably in an internationally coordinated comparative program, could correct some of these deficiencies. At the same time, we should stress that ethnographic research has much to learn from demography. It is critical that the research be conducted in ways that permit generalization to some social aggregate whose size and extent are known. Furthermore, such research is of greatest value when it is connected to known demographic characteristics of individuals and groups (e.g., fertility levels).

### *Monitoring and Predicting Demographic Change in Developing Countries*

In developed countries, vital registration systems provide accurate and current data on the magnitude and sources of population change. These systems do not function adequately in most developing countries. In their place, demographers have devised indirect means of estimating levels and trends in fertility and mortality. These are based on two strategies. One is to find ways of asking questions on censuses or surveys that secure reliable information on the volume of vital events. The work of Brass (e.g., 1975), now widely exploited in the developing world, has shown that better information can be obtained on the volume of vital events in a lifetime (e.g., "how many children have died among those you ever bore"?) than on events in a specific time period. Others have shown these lifetime events can be reliably "dated," so that repeated surveys can supply useful information on trends in a relatively well-defined period.

The second strategy is to make use of population identities to infer from observed population features (especially the age distribution and growth rate) the value of unobserved ones. For many years, the identities invoked were those of a "stable" population, one with vital rates fixed over several generations. Recently, it has been shown that the stable equations are a special case of a more general set of equations pertaining to any populations (Preston and Coale, 1982). This more general set replaces the total population growth rate with a set of age-specific growth rates, observable from two censuses. These new equations are being exploited in demographic estimation for developing countries and are also being used to estimate hard-to-observe parameters in the United States (e.g., the expected duration of a marriage).

The two strategies are not independent but are best applied in tandem. The field of demographic estimation for developing countries is continuing to develop rapidly, as witnessed by the fact that a recent important compilation of methods by the United Nations (1983) is already in significant respects obsolete.

There is an eager audience for the methods among scholars and governments in developing countries. The methods are widely applied. Nevertheless, there is considerable difficulty in finding resources to support the development and testing of these procedures because the research is programmatic in nature. No one can predict what methods will be developed a year hence, and funding agencies are reluctant to commit resources to activities that cannot promise results. This problem is shared by the discipline of statistics, but there seems to be a greater acceptance in that field (presumably by necessity) of programmatic activities whose expected quality is most reliably judged on the basis of a researcher's track record. Social scientists steeped in hypothesis-testing modes of inquiry are much more uncomfortable with this approach, and consequently the field of indirect estimation has no grant-based support.

A related area, one that is not adequately addressed by present indirect estimation procedures, is urban growth in developing countries. A United Nations survey of governments, undertaken in 1980, lists population distribution and migrations as their first concern in the field of population, even before the question of population growth. Spatial distribution policies are almost always synonymous with measures to control the growth of the large cities. At the same time, the data on city size are obsolete by an average of six years or so, since it typically derives from decennial censuses with a one-year publication lag. Data on urban rates of growth lag by an average of eleven years, since nearly all

are derived from comparisons of the two most recent censuses and pertain to a date centered approximately halfway between them. The costs of making national and urban policy based on such perpetually obsolete data are almost certainly high.

Most countries in the developing world do not have sufficient resources either to take more frequent censuses of their populations or to institute population registration systems, like those of Sweden, the Netherlands, Japan, Taiwan, and South Korea. Nor do they have the resources to develop conventional regional population estimation capabilities, which have been notably successful in the United States and the United Kingdom in detecting shifts in population redistribution trends. Some large, important countries in Africa, such as Nigeria, Zaire, and Ethiopia, are even finding censuses difficult to take, especially at ten-year intervals. Remote sensing of urban areas is one possible alternative method of monitoring the growth and size of human settlements over time. Put simply, samples of population density are taken on the ground ( $P$ ), e.g., from censuses, and from the air ( $A$ ). A function is then developed to predict  $P$  from  $A$ .  $A$  is then measured subsequently from the air and  $P/A$  multiplied by  $A$  to obtain  $P$ . The conversion equation can be recalibrated intercensally by areal sampling. The evidence is promising that this method can yield basic head counts of a precision similar to that of the traditional census (Kraus et al., 1974; Lindgren, 1971; Adeniyi, 1983). This is not to say that remote sensing has not been criticized as a method of estimating populations (see Morrow-Jones and Watkins, 1984, and reply by Paul, 1984). A full-scale test of the method has never been done, despite the potentially quite high returns if the method is found to be reliable, and despite a fairly well worked out methodology for using remotely sensed data in population estimation. At the moment, the research applying remotely sensed data to the problems of the social sciences has been uncoordinated, slow, and underfunded.

A second area of inquiry for which the application of remotely sensed data might have very large returns is that of resource analysis. Remote-sensing technology is now available to produce timely and precise information on deforestation, agricultural land, crop yields, et cetera (e.g., Tucker et al., 1985). Such information would, among other things, create much tighter constraints on the terms of the debate over resources and population that has sputtered on and off in the demographic literature without resolution. For example, there is such a wide range of deforestation estimates that practically all points of view on the subject can be backed up by statistics. This disparity exists even though there are enough Landsat images to answer precisely what the rates of deforestation are in the world. Better satellite imagery will soon be available from the French

satellite SPOT. Military and intelligence satellites, data from which are presently unavailable to social scientists, have even higher resolutions.

### *Predicting the Consequences of Medical Change in Developing Countries*

A final promising research area in developing-country demography relates to predicting the demographic, social, and economic effects of impending mortality changes, especially in sub-Saharan Africa. Vaccines against malaria and rotovirus (the cause of a substantial fraction of diarrheal deaths) will likely be deployed in the near future. Such advances are likely to be far more beneficial for health and mortality in Africa than all food relief efforts even if these were sustained indefinitely. Malaria is probably still the major cause of death in sub-Saharan Africa, although the absence of reliable data on causes of death makes any such judgment speculative. An effective vaccine against malaria, widely deployed, is likely to accelerate rates of population growth sharply in many areas. Africa already has the highest population growth rates in the world, despite a very weak institutional and human capital base. With a life expectancy at birth around fifty years, the potential for accelerating these growth rates is considerable.

How such changes will be distributed temporally and spatially, and what effect they are likely to have on local ecosystems, labor force size, patterns of land use, rates of urbanization, labor productivity, and capital formation are questions with important connotations for human welfare. National and international efforts in such matters as land reform and family planning programs require reasonable projections of the impact of these public health programs so that policies can be anticipatory rather than merely reactive. The impending mortality declines also provide an opportunity to test a variety of economic-demographic hypotheses and refine models of economic-demographic processes.

### **Desirable Organizational Changes**

We suggest two major institutional changes that we believe would improve the capacity to perform research on these and other important issues. One requires a reorganization of existing resources; the other, an increment in resources.

The first is an integration of existing demographic data bases into a central facility. Ideally, this data library would permit on-line access by cooperating American institutions, but the feasibility of this option

needs to be explored. Even without such access, the case for centralization is compelling. Many data sets are being separately maintained in a dozen or more American institutions for years and even decades on end, in anticipation of some future use, however remote this possibility may be. Some centers have thousands of such files (including several permutations of the same file). As in other libraries, items are added to tape libraries but rarely subtracted. At the same time, many valuable data files are available at only one installation and are unknown to those elsewhere who could profit from them. Some centralization of function is clearly desirable. It would facilitate comparative research and permit scholars with a new approach to find the data needed to explore its validity. There is, of course, the danger that such a facility would encourage mindless number-crunching by analysts who are unfamiliar with the country and data set being exploited. But we do not want to discourage the development of tools because they may be misused.

A possible mechanism for such a reorganization is the center grant program of the Population Research Center, National Institute of Child Health and Human Development. This organization funds about nine centers, almost all of whom receive substantial funding for computer personnel and for data management. If centers would agree to releasing some fraction of their computer funds back to NICHD for centralization of data bases, it is possible that no additional funds would be needed. At the same time we believe it inequitable and inefficient to limit access to such a centralized facility to the nine presently funded centers. Some means should be sought for incorporating other research institutes into this program. Clearly, such a center could also develop into a disseminator of software and technical expertise on a wide variety of computer-related subjects in population studies.

We are aware that the Interuniversity Consortium for Political and Social Research at the University of Michigan serves a similar function for the general social science community. But we believe that the needs and interests of the population research community are sufficiently distinct that a separate facility is justifiable.

Our second suggestion relates to developing-country demography. A very high fraction of what we presently know about biosocial aspects of demography in developing countries derives from one source: the Cholera Research Lab (now the International Centre for Diarrheal Disease Research, Bangladesh (ICDDR, B). This facility maintains an up-to-date register of approximately 200,000 individuals, whose demographic characteristics and vital events are carefully monitored and recorded. This population register serves as a base for an enormous variety of detailed investigations. For example, ICDDR, B has been the principal contrib-



utor to knowledge of the importance of childhood diarrhea to growth retardation and excess mortality and of the importance of lactational amenorrhea as a fertility inhibitor. It has also been the site of the most carefully evaluated set of family planning program interventions, as well as of many other intervention programs. The Centre has produced the most accurate life tables for South Asia, hence can be used as a model for similar populations. From its inception, ICDDR, B has maintained a strong demographic and social science core, including demographers, anthropologists, and sociologists. It houses many visiting scholars annually, most of whom are supported by their own funds.

A second institute, the Institute for Nutrition in Central America and Panama (INCAP), was highly successful in these same areas, although its demographic staff was weaker. Recent political events in Guatemala have impaired INCAP's functioning and illustrate how fragile such institutions can be in developing countries.

In view of the great success of these institutions, now about two decades old, we are surprised and concerned that their example has not been followed. There is nothing remotely similar in size or scope on the continent of Africa or Western Asia or South America. The shortage of field research sites in developing countries is partially responsible for the fact that U.S. researchers on developing-country demography usually have little, if any, developing-country experience. This trait biases research toward secondary statistical analysis of large data files and away from primary sources and cultural analysis. We believe that this bias is badly in need of correction.

Even one additional site of this type would be expensive and would require substantial funding. We believe that such an activity would be cost-effective. In relation to our list of important research areas, for example, the site could serve as a base for the ethnographic surveys that we believe to be badly needed; as a locale for carefully evaluated fertility intervention programs; and, if in Africa, as a place for predicting and investigating the demographic, social, and economic impacts of the new vaccines. It would also bring together demographers and epidemiologists.

A third form of organizational change we expect to happen more or less spontaneously, although it could be fostered or inhibited by funding authorities and research institutions. The greatly increased capabilities of microcomputers have changed the relative costs of performing various aspects of the research process. In particular, they have reduced the costs of and increased the scope for data-gathering activities. Direct key-to-file data entry promises more accurate and timely data generation, and it expands the range of information that can be gathered. It allows more complex questionnaires, more open-ended questions, and more

subtlety in interviewer–respondent relations. Large-sample questionnaires can be reduced dramatically in size by eliminating the prespecified categories of variables. Much of the tediousness of self-enumeration can be avoided, and more questions can be asked in the same questionnaire space. (For nationally representative surveys, of course, large-scale surveying organizations will still be necessary.)

We expect that these capabilities will lead to more intense researcher-initiated data generation activities, and correspondingly to less reliance on secondary analysis of preexisting large data files. Just as the development of macrocomputers gave a comparative advantage to secondary statistical analyses of large data files, and hence to the large institutions that were most efficient in managing them, so too has the development of microcomputers given significant capability back to the individual researcher for designing data files most appropriate to particular research issues. Since population researchers have been more closely involved in data generation than have other social scientists, we can expect to observe some of the first effects in this area.

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# 19

## Family and Domestic Relations

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The family is the only social institution that has excited increased public interest as it supposedly diminishes in importance. This is because the family—whatever it actually is—embodies deep, possibly universal human values, so that all modern societies consider it vital to their well-being that the family be understood, nurtured, and strengthened. Fortunately, the family has emerged in the past decade as an area of marked interdisciplinary progress. Once interpreted as a formal institution, the family more recently has been treated in an increasingly rich and varied fashion, aided by technical advances in demography, survey research, and statistical modeling. A common interest—inquiry across time, particularly longitudinal inquiry—seems increasingly to unite the several disciplinary approaches to the family and close relations, and to provide an ideal point of entry for multidisciplinary study. The family is a human arrangement highly engaged in transmitting codes across time: culture, property, genetic materials. From the individual perspective, this means that the family is ineluctably involved in human development.

There is now rough interdisciplinary agreement in conceptualizing the family as a somehow-structured set of processes among a shifting set of individual actors, whose choices are constrained both by institutional, cultural, and material structures and by the patterns of interaction and shared meaning evolving within family units. What the family is depends on how it is bounded, and how it is bounded depends on how it operates in contact with other institutions. Families occur in time, and require longitudinal study. It is not just that the operation of families at particular moments in time may be unrepresentative of what they do at other moments: rather, it is that much of what families do that is interesting takes place over time.

Why should the family, one of the most traditional subjects in a number of disciplines, have become a focal point for new scholarship? The answer has two distinct components. The first deals with the internal development of the field within its traditional disciplinary boundaries—sociology, psychology, anthropology, and, to a lesser extent, demography. In these realms, an initial (often functionalist) focus upon the family as an institution whose properties were treated as substantially uniform within a given society was followed by a second, more behaviorist phase.

At this point, study was guided by a microscopic concern with intrafamilial processes, and an enlarged concern with the determinants of differentials in these behaviors. We now stand at the beginning of a third phase: the micro-level insights of the second phase demand integration with the more macroscopic concerns initially directing study.

The second reason for a new family scholarship is the intense interest taken in the subject by disciplines that until recently had ignored it; economics and history are most prominent among these. Economics has proposed explicit micro-level models of family behavior drawn from theories of the firm and of human capital. Such inquiry has treated family members as producing both marketable and nonmarketable goods and services within the family, and demanding from the family both material and nonmaterial satisfactions for which near-substitutes may be found elsewhere. The contribution of the discipline of history, by contrast, has been on the macro-level, indicating that the family has had a more complex, less unidirectional and determinate relationship to other elements of the social structure than assumed by functionalist accounts.

## The Emotional Content of the Family

### *Recent Scholarly Concerns*

Nothing has dominated family study more than the assumption that over time the family has lost various of its functions: productive (most prominently), protective, even educative. The family is said to have become primarily a tight, nuclear unit that has persisted primarily because it gives its members emotional satisfaction, and because the powerful feeling tone in the family provides the setting in which children can be socialized to the stern demands of a competitive world.

In the 1970s and early 1980s skeptical work accumulated around this proposition. Historical study has placed the emergence of an emotionally tinged childrearing at an earlier date than the conventional interpretation would allow. Sociological study has pointed to a possible loosening of the connection of sexuality and the energies that bind today's family. Close examination of patterns of intergenerational transmission of values in contemporary Western families has challenged our notions of how and why families teach their children, and the specific role of intrafamilial emotion to that end has come under closer scrutiny. Cross-cultural examinations of socialization practices have made us aware of the practical interpenetration of a variety of societal institutions in the intimate operation of the family.

Emotions are elicited and expressed primarily within the context of social relationships, particularly within the context of close relationships such as those between romantic partners and among family members. Emotions have powerful influences on interpersonal perceptions and behavior. A number of disciplines have expanded their examination of emotions in recent years. Recent work in psychology indicates that emotion may be particularly likely to arise in relationships that are especially interdependent but that such relationships do not necessarily evoke high degrees of expression of emotion. The sociology of the emotions, thus far mainly treating the theoretical level, has opened a debate over how emotions are socially constructed. Within demography a series of empirical studies have demonstrated how specific ideological systems imply particular patterns of emotional satisfaction in childrearing. Some of these satisfactions may—as modernization theories usually hold—have acceptable or preferable substitutes in goods or services available through purchase, but others may not. For example, when the latter is the case, fertility will not drop off sharply in response to reduced mortality and improved old-age economic security.

Until recently, psychologists examining emotions have focused mainly on intrapersonal effects of emotions, but a recent shift to a more social focus has had important implications for our understanding of families. After years of studies of individuals in isolation or of studying relationships of two people who had never seen one another before and probably would never see one another again, psychological research on relationships has begun to change. Empirical work has established that our own emotional state influences both how we perceive others and how we behave toward them. People in positive moods report feeling sociable and receptive to others; feelings of happiness are associated with positive social relationships and a willingness to help others. Others' displays of emotion correspondingly influence our behavior toward them. People who express sadness are likely to be seen as helpless and dependent, and are more likely to be helped, but only in the context of a close relationship. Research now underway is focusing upon the circumstances under which people strategically present emotions to others.

### *Likely Trends in Scholarship*

Research on how the emotions are underpinned ideologically and culturally is barely under way. A part of the inquiry we propose will satisfy a particularly pressing current concern in the United States: the question of how—apart from legal or other formal considerations—a family differs from other enduring affective arrangements among persons.



Questions of public policy no less than social theory lead us to ask whether there is something special about contemporary marriage that distinguishes it from long-term cohabitation, and, if so, why. Does a biological mother feel differently about her infant than an adoptive mother does, and why? Do loose and shifting confederations using fictive kinship terms share material and nonmaterial goods as effectively as kinship groups constrained by more conventional rules, and why? Policy questions are relevant, for a good deal of policy looks to "the family" as a target, while others assume its existence as a point of distribution or leverage.

We need to know with what feelings families make fertility decisions, decide whether the wife should enter the labor force, celebrate an anniversary, buy a house, elect to eat at a restaurant rather than prepare dinner at home, decide not to divorce but to try to weather the current marital storm. We need to know what emotional ties come to bind friend to friend, lover to lover, husband to wife, parent to child, kin to kin. We need to know whether rules govern how one is supposed to feel about a mate, a child, an uncle, or whether rules govern how persons in these relationships are *supposed* to evolve their own rules governing such feelings. Social, personality, and physiological psychologists have recently made important advances in identifying individual differences in reading one's own and others' emotional states, in attending to one's own and others' emotional states, and in emotional reactivity. We now need to know how such individual differences interact with the cultural rules governing expression and perhaps production of moods and emotions in relationships.

We know just enough about the feelings that operate in middle-class families, to recognize how rich will be the reward for asking questions about the emotional lives of American families that are outside the "mainstream": about how and why subcultural and variant ideological notions are transmitted from generation to generation, about how children's feelings may be structured by the shallow, sometimes debasing work of their garbage-man father.

## Needs

What is called for is a multipronged, multidisciplinary inquiry, which we wish to encourage here. Most particularly, we propose microstudy of contemporary families, with a combination of methods incorporating observational and other nonobtrusive techniques, but guided by a concern for possible change over time in what families do, and for the pow-

erful fact of great variety in what families do. We do not believe that survey research ought have a methodological monopoly in the study of family and close relationships. The experimental method most commonly employed in the study of emotions often seems to have ripped the feelings in question from the context in which they operate, and from the time frame in which they develop, but this need not remain so. Obviously, naturalistic techniques are sometimes too intrusive, but depth interviewing, oral history, and other methods deserve application. Psychologists have become much better at nonobtrusive systematic studies. The older procedures of interviewing have been joined by constrained observation (for example, when parents and children are given specific tasks to solve) and by quasiexperimental procedures.

Do the institutional mechanisms required to carry out such study currently exist? We are not sure. Family study centers and centers for the study of child development might carry on the enterprise, but both of these tend to be disciplinarily focused. We also are not persuaded that the content of the research agenda is yet clearly enough specified. For the moment, we would encourage an agenda-setting initiative, taking advantage of a relatively open mechanism such as the Social Science Research Council. An obvious first step is a workshop engaging a group of scholars with a primary interest in the theory and methodology of the study of emotions with a number of counterparts whose expertise is in the behavior, development, and social construction of families.

## **Dynamics of Family Formation, Adaptation, and Dissolution**

### ***Recent Scholarly Concerns***

Marked progress toward understanding the family from new perspectives has been achieved by focusing on the dynamics of family formation, adaptation, and dissolution, as seen from a life-course perspective. This approach encourages a linking of micro-level processes within the family with macro-level processes beyond it. We have begun to learn about the ways in which families carry their pasts around in their internal processes. The relevant past includes individual histories of the marital partners, their shared premarital experiences, as well as the larger societal past of which the circumstances surrounding their marriage are a small part. Thus American marriages initiated by young partners have been subject to more subsequent breakup, other background factors

being equal. And, everything else being equal, marriages formed in periods of obvious external stress, such as wars and depressions, are likewise more subject to eventual breakup.

Like divorce, wives' growing commitment to the labor force represents a considerable enlargement of a pattern that began emerging several decades ago. We have begun to discover that adequate explanation for the commitment of married women and mothers to gainful employment requires exploration of the way in which personal and societal history intersect in conditioning subsequent family behavior. The enlargement of unmarried women's work opportunities early in the twentieth century carried no revolutionary implications at the time, and the temporary expansion of work opportunities for women during World War II was overtly premised upon the extraordinary circumstances that occasioned it. Nevertheless, the exposure of young women to gainful employment often led to subsequent commitment to the labor force. Over time, assumptions about women's spousal and maternal roles were modified, family by family. Finer-grained research has indicated that depending upon the internal role structure of the family in which wives worked, such episodes led either to a catastrophically disordered set of familial roles, to a triumphantly expanded set of individual and familial capacities, or to very little long-term change within families.

### *Likely Trends in Scholarship*

The great pluralism of family processes in our society is the most evident and yet the most unexamined fact within our purview. Investigation has begun to move from the level of discerning and linking central tendencies to exploring variation, asking about the determinants of differential "susceptibility" to trends, and about the intrafamilial consequences of particular choices. As in the study of recent trends in divorce and wives' work, this approach asks how a range of adaptations at any given moment affect the way in which the family institution fits into the larger society.

To a considerable extent, this question is one about culture, about shared, normatively evaluated meanings. Investigation may start with a behavioral or functional question, but ultimately we will move from the question of who makes what transitions, when, under what circumstances, to what understandings particular transitions entail within given families, and, over time, within a culture. Whether work—whether welfare receipt—supports or challenges the family depends upon what proper family behavior is understood to be, whether the action in question is seen as sustaining the family or violating its boundaries.

To understand the way in which families define and redefine themselves, we will need to look at the evolution of relationships (a rapidly growing concern of social psychology), the timing of marriage (a recent focus of life-course study in sociology) and its constitution through symbol and social support (anthropological concerns), and the way in which the newly married couple evolves joint values and procedures. All this will allow us to follow the process by which the couple adds children to the family, develops economic strategies to fit its circumstances, remains intact, or breaks up. Obviously, a complicated longitudinal data set will be required.

### *Needs*

A fair start has been made in these kinds of investigations, enough to predict likely progress in the next decade. Yet such progress requires the commitment of larger and somewhat centralized resources to the effort because it is dependent upon longitudinal data sets. We urge both the maintenance of existing longitudinal data sets and argue for their supplementation.

The pioneering work has depended to a great degree upon fortuitously gathered and maintained documentation. Somewhat perilously, too, a fair amount has depended upon inferences from cohort data rather than data upon individuals, upon retrospective rather than longitudinally derived data, and upon reports on family configurations and processes by a single family member. If the full value and power of the perspective is to be realized, if the kind of interdisciplinary linking of micro-processes and macro-processes is to be furthered, more appropriate data will have to be created and made available. Cross-sectional analyses of age-specific transition probabilities only remotely capture the essence of a process that engages the emotional life of the individuals involved and that changes the boundaries of the family unit.

The major longitudinal data sets now in existence, except perhaps the long-term studies of human development initiated under a child-development aegis, either assume the individual to be the unit of analysis or treat the family as an operating unit of unified purpose and unproblematic internal mechanisms. These data sets also assume that a single informant is capable of reporting adequately upon whatever system properties of the family are of interest. For some purposes, these limitations are not hobbling, but for others they shortly will be. We propose an initiative, with the substantive focus indicated above, that will seek to devise methodologically appropriate structures that may be added to ongoing longitudinal data sets.

We argue for a strategy that supplements the more conventional longitudinal surveys at strategic points with instruments and procedures designed to examine those properties of the family as a system that help explain variation in family formation, adaptation, and dissolution. This will involve a number of distinct subsamples.

In one, several family members, including children, will be respondents. Multiple respondents should be asked separately to report on family transactions of material, labor, and emotional resources. They likewise should be asked separately to report on one another's understanding and performance of family roles. The permeability of family boundaries and the interpenetration of family roles with other roles can be understood only when we know the extent to which family members understand these boundaries and roles in common, and over time.

The basic longitudinal data sets should also be supplemented by small subsamples examined by observational methods—for only direct assessments by trained observers can provide accounts of the operations of the family as a system, at given times. Subsequent requests for self-generated family documentation—written and material as well as retrospectively interpretive—should supplement this effort.

A third supplementary subsample would involve field study and even less naturalistic methods (including constrained observation and perhaps laboratory methods) to allow researchers to bring to bear highly relevant, formalized concepts drawn from social psychology. The overall research design is intricate, but to be adequate to the examination of plural family solutions to varied situations it obviously must be complex.

## Historical Inquiry in the Several Disciplines

### *Recent Scholarly Concerns*

Under the initial inspiration of highly precise "family reconstitution" techniques devised for the study of precontraceptive fertility and of a highly evocative and broadly inferential school of French economic history, American historians took up the study of the family almost two decades ago. For historians, the crucial initial discovery was that the histories of nonelite families—not just the social institution of the family—could be studied empirically. Early work involved describing aspects of the demography and composition of the families of various subgroups at various points in the past, patched together by reference to standard sociological concerns with the impact of industrialization and

urbanization on the family. Typically, more continuity of family forms was found than theory had led us to anticipate. The nuclear family was not the product of modernity. Indeed, the opposite may have been the case: the nuclear tendency of Western European families as they emerged from the medieval period provided the locus in which children were raised in such a way as to be mobile, risk-taking, "untraditional." The argument, while depending on initially uncertain inferences from detailed microscopic study, is challengingly macroscopic.

The historians' findings moved sociologists and other social scientists who were consumers of sociological theory on the family to question whether in fact they understood the mechanisms linking family behavior to the social environment. The implications for the study of contemporary Third World family and demographic change were especially intriguing.

The sparseness of accounts focusing on behavioral aspects of families shortly came to bother historians, as did the relatively narrow time-bounds within which most work in family history had been carried out. Collaboration and cross-fertilization outside the discipline, as well as the discovery of new sources that might be turned to the study of the family, encouraged scholars to begin to assay interpretive accounts treating to some extent the internal dynamics of families, and the cultural ideologies that in part governed these dynamics. A key goal was to discern and interpret substantial family change in a time frame that did not a priori correspond to the modernization paradigm.

A particularly satisfying set of studies already accomplished deals with the relationship of family processes and family ideology to agricultural practices—both peasant and nonpeasant—and the implications for family of the initial impact of market-oriented capitalism. The new focus shifted attention away from a simple typology of family structure, demanded a continuous treatment of change over time, and suggested a view of the family that focused on the dynamics of family formation and adaptation. Family systems, we have learned, are cushioned from change in the surrounding systems by the variety of ways (within many societies) in which they are coupled to the economy, by their variety of composition, and by the practical difficulties in most societies of sanctioning closely and effectively the kinds of affect these systems ordinarily contain. Thus local variation in inheritance customs in Western Europe survived the Napoleonic reforms and, indeed, much of nineteenth-century statemaking to affect the timing of marriage and childbearing, the adoption of contraception, and the organization of the family economy down to the present century. And yet, the historical record also contains accounts of rapid change, with new stable configurations establishing themselves relatively suddenly. The migration of Europeans to

British North America was one such instance. We now ask whether change since World War II constitutes another.

### *Likely Trends in Scholarship*

Historical study (across disciplines) needs concerted work in defining agenda, in creating synthesis, and in formulating shared scientific standards for evidentiary and inferential adequacy. Historians—increasingly well-versed in quantitative methods—need to move toward studies that cover relatively long time periods and that make explicit connection with some kinds of more or less standard sociological propositions. They also need to become sensitive to cross-cultural methods and to the findings of observational and experimental methodologies from which they can draw insights. Students of family across disciplines need training in archival methods and more concrete exposure to the ongoing effort to capture the historical sweep of family change over the last several centuries. Field workers need acquaintance with the methods and findings of historical demography and with related findings in contemporary social demography. The several disciplinary approaches to treating time and change need close comparison and mutual critique. Computer modeling of long-term family and demographic change has already begun, and is strongly to be encouraged.

As scholars seek new explanations of apparent rapid family change not narrowly attributable to economic and demographic causes, though clearly linked to them in some way, they look to a complex set of issues indexed by such terms as "culture" or "ideology." Changes in sex roles, and particularly the roles of women in both occupational and domestic domains, changes in the formation and constitution of extended families created by divorce and remarriage, and changes in concepts of appropriate sexual expression all reflect changes in the rate of occurrence of various forms of behavior. But whether that constitutes structural change depends on whether there has been change in the conceptual—cultural or ideological—structures that are operative in each particular case being studied. Practitioners in a number of disciplines have come to recognize that a long view is necessary if we are to devise an adequate understanding of how, in general, family and social change are related and how, in particular, we are to gauge the pace and nature of recent change. Inevitably, we muddy the issue when we contrast modern trends to "the traditional family." Two things are now needed: the construction of a more adequate baseline and a careful, probing analysis of contemporary assumptions that guide behavior carried out in or for the family.

### *Needs*

We propose here not joint research but joint efforts at synthesis around the question of long-term change in the ideologies that define family and proper family behavior. Sensitization in training will help here. But the specific initiative we propose is to assemble an earnest and select group of practitioners for extended work on the problem of synthesis. Questions of method and inference obviously will have to be addressed, as well as questions about linkage mechanisms between institutional change and changes in the society and culture. We propose calling together, under the aegis of the Center for Advanced Study in the Behavioral Sciences, or some like setting, a dozen or so practitioners from the various fields to spend an academic year on the subject. We would not presume the exact product of this year, but we would encourage that both actual syntheses and methodological critique would emerge in published form.

### **Storage and Dissemination of Varied Materials**

Closely related to the question of subtle documentation that underlies all our suggestions is the matter of storage and cataloging of data. A wide variety of routinely and spontaneously generated documentation bearing upon the subtle aspects of family and intimate relations is now available but is nowhere collected or indexed. One thinks of case records, the field notes of social investigators, personal documents such as diaries and letters, account books, and family memorabilia such as surround family reunions. Questions of representativeness naturally arise, but to a degree such questions can be confronted only in the course of the proper preservation, documentation, and indexing of archival materials.

The pervasiveness of the family and close relationships no less than the elusiveness of the phenomenon makes a concerted effort at documentation and dissemination imperative. The initiatives we have proposed are all multidisciplinary. Ready availability of appropriate data of the several sorts we have discussed, indexed together, will press toward this end. Archiving, thus conceived, is clearly a preserving action, and is more cost-effective than collecting fresh data again and again, but we emphasize that it is more as well. It is a creative intellectual activity, calling for a broad conception of families and adequate intellectual and economic resources to bring off a rough synthesis. This step will both draw upon and further the other theoretically bold steps outlined above.



Technological advances make it unnecessary for archiving to occur in a single place. As we envision it, a single center, perhaps as part of a center for family studies, might serve as the residual archival location for those materials. But the main role of this central location would be to conceptualize the appropriate purview of such a multi-location, multi-medium archive—extending from the critically important supplemented longitudinal machine-readable data sets through anthropological field notes and psychological and clinical observational accounts, to spontaneously created correspondence and diaries.

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## Urban Transformation and Migration

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The study of urban areas and of the forces that affect their growth and decline has become more technically focused and theoretically advanced since 1960. At the same time, the practical importance and visibility of issues raised in the study of metropolitan regions has been attenuated. On the one hand, basic inquiry has provided real insights into such complex phenomena as household and firm location, mobility, neighborhood change, and interregional migration. On the other hand, the recognition of interconnections by policymakers has led to a stronger role in urban and regional development by higher levels of government and to the expectation that the impact of alternative policy proposals can be evaluated credibly. Arguments for revenue sharing, community development grants, and special purpose grants developed in the 1960s and 1970s, as well as the "New Federalism" initiatives of the 1980s, may be closely tied to social science models of individual and local government behavior and to models of the political economy of urban areas.

In this report we review several important strands of behavioral research in the urban microcosm; the economic, demographic, and social transformations of American cities; and the migratory flows of households in response to differential opportunities in U.S. metropolitan areas. This review is itself highly selective and is confined to only a few illustrations of the outpouring of intellectually exciting research in the urban transformation. The few topics we discuss below were chosen because they represent areas in which considerable real progress in understanding has been achieved in the recent past and because they hold unusual promise for improved understanding during the next decade. They represent the class of intellectual challenges for social science research that combines theoretical analysis and empirically based quantitative approaches. They also illustrate the close relationship between high-quality abstract analysis and the more concrete concerns of policymakers and planners. This linkage increasingly characterizes social science research in the urban transformation and in population flows.

## Research Review and Agenda

### *The Long-Distance Mobility of Households and Firms*

The analysis of migration, in particular, has witnessed an outpouring of studies, mostly at the macro level, investigating the determinants of population flows between origin and destination regions (Greenwood, 1981; 1985). Since the late 1960s the focus of research has moved away from attempts to compare the relative importance of conditions at origin and destination regions in motivating the decision to move. More comprehensive theories of migratory behavior, based broadly on the notion of migration as a form of human capital investment, have shifted the research paradigm to more specific investigations of the differences in economic conditions at origin and destination pairs and their effects upon the decision to migrate (Isserman et al., 1986). At the same time, more general theories of the spatial distribution of labor have been advanced (Carlino and Mills, 1987; Massey, 1984). Together, these more comprehensive behavioral theories have led to hypotheses about the education and skill levels of those more likely to respond to differing economic opportunities and to theories linking migration to the life cycle of the individual, the household, and the behavior of the firms.

These more comprehensive representations of behavior have led, quite naturally, to inquiries regarding the nature of the equilibration of spatially distinct labor and housing markets (Robeck, 1982). For example, theoretical and empirical analyses have been framed comparing the gross average income gains to employed workers from migration with the net income gains expected when the probability of unemployment is properly accounted for (Todaro, 1969). Analyses investigating differences in well-being across origin and destination pairs have provided insight into the role of variations in the cost of living and urban amenities in affecting population flows (Graves and Linneman, 1979).

The theoretical and empirical analyses of migration undertaken since the mid-1960s have, in part, reconciled a variety of heretofore anomalous facts: cities and regions with large immigration flows are also those experiencing high levels of outmigration; and variations in real income and unemployment rates across regions are likely to decline only slowly, despite high levels of place-to-place migration.

Although much progress has been made in modeling and in predicting the flows of place-to-place migration, more research is necessary to understand the mutual impacts of these flows on the operation of local labor and housing markets and of these markets on the migration flows.

Attributes of local labor markets are increasingly seen as important to the location decisions of firms. As capital has become more mobile and as scale economies in production have come to be achieved at lower levels of output, interactions between population and firm mobility have become more important. The role of differing kinds of employment change (e.g., production versus service employment) in promoting broad-based regional growth has received much recent attention, especially among geographers and planners, and a number of case studies are suggestive of the leading role of certain service sectors (Markusen et al., 1986; Massey and Meegan, 1982; Noyelle, 1985).

It should be noted that most of the actual empirical analyses of the place-to-place migration of households have been macro in character, that is, they have been based upon the analysis of aggregate data on the location of groups of individuals at two points in time. Relatively less empirical research has used individuals or households as units of observation. This reduced emphasis on micro-level migration research has been characteristic despite recent advances in statistical methodology and despite reductions in the cost of computation. As a result, we still know relatively little about the relationship of directly stated preferences and intentions to subsequent migratory decisions. Similarly, we know little empirically about the role of information and of specific networks of communication in affecting the long-distance mobility decisions of individuals and households. Because empirical analysis relies heavily upon aggregate cross-sectional data, evidence on staged migration and on return migration is still quite fragmentary.

Many of these issues have been addressed theoretically by social scientists, and a variety of interview and anthropological evidence has been accumulated (Cowles, 1972). Nevertheless, well-executed statistical analyses incorporating these insights have been quite rare.

In large part, these deficiencies can be traced to the information available for analysis. In any period of analysis, migration is an unlikely event, and migration between many specific pairs of origins and destinations may be quite rare. Thus, even rather large special-purpose samples of households are often inadequate for estimating sophisticated models of long-distance migration. The relative rarity of specific individual moves also means that release of raw micro data by government agencies could indeed jeopardize the confidentiality of individual respondents. It is quite clear, however, that tabulations of such data (e.g., moment matrices formed from observations on individuals) could be made available cheaply in such a way as to observe scrupulously the principles of individual confidentiality but also to facilitate micro-level research. The availability of adequate data for migration research is but



one conspicuous example of the most important and specific research need in the fields of urban and regional analysis.

### *The Determinants of Urban Spatial Structure*

Since 1960 an increasingly general set of theories has been articulated to explain the patterns of location and land use within metropolitan areas and to provide economic and geographic forecasts of urban change (Muth, 1985). These theories of location and urban form have, moreover, been subjected to a variety of empirical tests by economists, geographers, planners, and sociologists using spatially disaggregated data from a wide variety of city types.

Although the theoretical development can be traced ultimately to the German economic geographers of the early nineteenth century, the heart of the modern theory is only about twenty years old (von Thünen, 1826; Muth, 1969). The key theoretical insights are deceptively simple, namely, that, in choosing a place to live, households make a trade-off between the time and money costs of commuting to work and their desires for more spacious living accommodations (Alonso, 1964; Kain, 1962). House builders are also presumed to substitute more capital for land in the production of housing as the price of land increases. A rather comprehensive set of propositions about the spatial structure of cities can be deduced from these postulates. For example, since the desire for housing increases more than proportionately with income and the cost of commuting increases less than proportionately with income, it follows that higher-income households choose to locate further away from downtown job concentrations than lower-income households. Locations further from the central city are less valuable, since they save less in commuting costs. Hence, it follows that land prices decline with distance from downtown. As land gets cheaper, it is used more intensively relative to capital in the production of housing. Thus residential densities decline with distance and average lot size increases. As land prices decline, so then must the unit price of housing (that is, the price per unit of service). Since capital can be substituted for land, however, the relative decline of land prices exceeds the relative decline of housing prices. Finally, the model suggests that the poor, who consume small quantities of housing service, will live in locations where land and housing prices are highest. In contrast, the well-to-do, who consume large quantities of housing, will locate further away from downtown where housing is cheaper.

As noted, this simple but powerful model has been subjected to extensive empirical testing (Kain and Quigley, 1970; 1975). Its predictions

conform, on average at least, to the European and North American experience. The model emphasizes rising incomes and reduced transport costs as driving forces in the observed decentralization and suburbanization of population, in contrast to a variety of popular villains alleged to cause "urban sprawl." The model thus implies that suburbanization is not at all a recent urban phenomenon. Subsequent empirical analysis, using a variety of data sources, time periods, and cities, has confirmed that the decentralization of population has been proceeding for as far back in time as we have data (Mills, 1972).

This model of the economic geography of metropolitan areas is highly stylized, to be sure. Nevertheless, insights drawn from the model have proved useful in informing and analyzing policy choices and in making predictions. For example, the model correctly predicts the great expense of renewal programs intending to bring higher-income households back to the city by subsidizing their housing; the model suggests that the emergence of two-worker households will have centripetal impact on urban form. Less intuitively, when motor fuel prices rose substantially in the 1970s, many observers argued that these higher transport costs would have an important centralizing effect on urban densities and metropolitan form. The straightforward application of the social science model described here suggested, in contrast, that the effect of a doubling of gasoline prices would be offset by the growth in real incomes in a couple of years (Quigley, 1979). This simple model has yielded a variety of rather important insights.

This analysis is not without serious limitations. There appear to be two major shortcomings of existing neoclassical models, shortcomings we may expect to overcome in the next decade. First, the model is essentially static in its approach; many of the most important intellectual and social problems of urban transformation are manifestly dynamic in character. The forces giving rise to neighborhood change, to industrial relocation, and to local residential mobility are quite difficult to treat explicitly by the model. Second, the model emphasizes the long-run equilibrium spatial character of the urban area—long-run in the sense that the physical capital stock has fully adjusted to its equilibrium position.

Extending the model of urban spatial structure to accommodate these realities is a major challenge, but there are clear signs that much progress can be made in the next decade. First, there already exists a variety of prototypical models of urban development and land use that are explicitly dynamic or evolutionary in character (Kain and Apgar, 1985). These developing models are often oriented toward simulation, or else they explicitly incorporate a dynamic or control theoretic framework.

Second, with declines in the real costs of computation and with advances in minicomputers, there will be more experimentation with these approaches, and results will become more widespread.

A related development has been the diffusion of so-called computable general equilibrium (CGE) techniques (Scarf, 1973). In many ways, these CGE methods are ideally suited to urban spatial analysis and permit the development of models with far greater realism. In particular, they facilitate analysis of the effects of long-lived capital stock and infrastructure on current choices and opportunities. These developments may also facilitate the integration of micro and macro aspects of urban location and spatial structure. Both of these lines of development promise to make urban models more realistic, more catholic, less neoclassical, and more broadly social scientific (Ko and Hewings, 1986). These approaches do, however, increase the complexity of modeling efforts and may make them harder to evaluate. The challenge is to generalize existing models and yet to make them transparent.

This challenge is especially important in urban research. In contrast to many other areas of social science inquiry, urban research has been characterized by the rapid diffusion of theoretical results into the more practical realms of planning and evaluation. This has been quite evident in the models of urban form and location developed since the 1960s. Insights derived from these approaches have been used by government and planning agencies to analyze policy alternatives, for example, housing subsidy programs at the national level and highway investment programs at the local level. The promise of improved planning and evaluation techniques is one motivation for increased research efforts.

### *Residential Mobility and Neighborhood Dynamics*

Research on local residential mobility and its effects upon the changing character of urban neighborhoods provides some contrast to that undertaken to understand long-distance migration and urban spatial structure. Research on local mobility and neighborhood change has been more inductive and more interdisciplinary. It has also reflected the split between micro and macro levels of analysis and has not yet fully integrated the two. At the micro level, urban sociologists and others have developed and tested a variety of models of the individual household's decision to move within an urban area (Rossi, 1980; Hanushek and Quigley, 1978). Research has also attempted to analyze the outcomes of this process. Specifically, theories have been put forward relating local residential mobility to identifiable aspects of the household life cycle and to changes in households' demands for housing, space, and transport

(Brown and Moore, 1970; Quigley and Weinberg, 1977). Many of these models have been estimated and evaluated by using place-to-place flows and measures of the propensity to move by population aggregates, as well as by exploiting individual data on local mobility and on intentions to relocate within metropolitan areas.

Research has documented the importance of housing conditions, public service attributes, and demographic factors in conditioning local residential mobility. Research also indicates the importance of perceptions of external change in the environment in the mobility calculus of urban dwellers. So far, social science research has been less successful in analyzing the macro implications of these micro studies, that is, in understanding the cumulative effect of local mobility upon spatially defined neighborhoods within urban areas. One of the major problems in analyzing neighborhood change, of course, is that the subject matter—the neighborhood—is a social and a spatial unit lacking formal institutional or bureaucratic concreteness (Palm, 1981; Keller, 1968). Despite the researcher's problem in defining the neighborhood, its reality and significance for the well-being of individuals and of the city as a whole are undeniable. Households live in neighborhoods, and their setting in particular neighborhoods promotes or restricts access to urban amenities and to the services provided by public agencies and private firms. Location in a given neighborhood also affects the life chances of an individual.

Neighborhood change is occasioned by variations in the housing and physical capital stock (due simply to its aging or to private and public decisions about new investment) as well as by shifts in population composition. The latter are affected by regional in- and outmigration, but the largest component of such shifts is the geographical mobility of local residents (Adams and Gilder, 1976). Neighborhoods are also interconnected in a complex and incompletely understood process. Large-scale economic investment decisions, whether private or public sector decisions, clearly affect the neighborhoods in which they are located. But they may also have profound impacts upon other neighborhoods, hastening the rise or decline of neighborhoods far removed from the site of the investment or disinvestment decisions themselves (Bradford and Rubinitz, 1975).

There have also been substantial and important analyses of neighborhood composition. For example, sociologists have undertaken a variety of descriptive and normative studies defining and comparing population composition and documenting levels of residential segregation by race and ethnic group—within neighborhoods in urban areas, among U.S. metropolitan areas, and across historical periods. A variety of studies has indicated the relationship between racial or ethnic prejudice, its eco-

conomic consequences for housing prices and commuting patterns, and the well-being of minority households in America (Kain and Quigley, 1972; 1975; Yinger, 1979). The research provides important evidence about the magnitude of the social costs associated with patterns of segregated living conditions, costs that are distressingly large even when narrowly defined.

Studies of the role of market intermediaries (e.g., financial institutions, real estate agents, developers, and appraisers) have provided evidence on the operation of institutional constraints (Palm, 1976; Yinger, 1986). Such constraints are not easily captured in models relating the economic and other resources of the household to the housing opportunities in the metropolitan environment (Palm, 1979).

Theoretical research has provided insights that help to understand the process by which neighborhoods decline or improve and the process by which individual decisions to move or to invest in a neighborhood affect the decisions of subsequent actors as in a form of contagion. These models indicate the extent to which drastic neighborhood change may be the unintended result of quite benign micro decisions made by individuals or public agencies (Schelling, 1978).

Social science research has been less successful in applying these insights empirically to develop predictive or descriptive empirical models of neighborhood change itself. In large part, of course, this is because individual decisions are strongly conditioned by expectations about the future and by perceptions of the availability of other opportunities for residential living or for profitable investment.

Current work on households' perceptions of alternatives and of households' search processes promises to yield important insights on these points. Much current work on local migration seeks to integrate two different theoretical perspectives: "structuralism" and "behaviorism" (Palm, 1986). In the former approach, emphasis is placed on the ways in which the political-economic structure constrains the decisions of the individual or household—the effects of the organization of the economy, the provision of housing within the economy, political decisions concerning the allocation of housing, and the development of neighborhoods on the structure within which individuals or households make decisions (Johnston, 1984). In the latter approach, emphasis is placed on studying the ways in which households' perceptions of alternatives as well as their search processes affect location decisions and the evolution of neighborhoods. For example, researchers are exploring the role of "anchor points," such as the locus of current residence, in conditioning perceptions about alternatives and in affecting the search process (Brown and Holmes, 1971; Onaka and Clark, 1983). These analyses of perceptions and the process of searching are even more important in

light of evidence suggesting that the number of residential searches actually undertaken by moving households within metropolitan areas is quite small (Smith et al., 1979; Huff, 1982).

The integration of micro and macro forces is perhaps nowhere more necessary than in the analysis of local mobility and residential change (Clark, 1987). Only by a better understanding of the forces that make for "gentrification" (rapid neighborhood improvement) or for cumulative decay can we hope to evaluate policies intended to stabilize neighborhoods by public investment in infrastructure or by improved public services.

### **Research During the Coming Decade: Priorities and Payoffs**

There are clearly a variety of intellectually exciting, theoretically challenging, and empirically demanding research tasks that offer great promise for the next decade. For most of these projects, we expect that the principal advances will be obtained by traditional means, that is, by individual investigators or small groups of researchers working with individual grants. There are at least two areas, however, in which support from a larger-scale effort may be appropriate.

#### ***The Availability of Data***

First, there is the drastic need for a more rational and systematic approach to data availability in support of urban research (Isserman et al., 1982). For the public sector, a wide variety of data is currently collected for other purposes and stored at substantial expense. Many of these data sets—observations on individuals, households, and firms—contain geographically identifying information that would make them of great value to urban research. For a variety of reasons they are not currently made available. As the costs of computation and data processing have declined substantially and as advances have been made in statistical methodology and in econometric and multivariate statistical techniques, deficiencies in data availability have become more apparent. This is especially important in the analysis of urban and regional phenomena. As noted above, many of the most promising research directions involve analyzing individual data from small geographic areas such as neighborhoods, census tracts, civil divisions, cities, and metropolitan areas. Currently, data are stored in ways that make them less accessible to researchers; often they are inaccessible for micro analysis.

In large part, the current situation results from inadequate resources and insufficient attention to data requirements and to the opportunities for urban research using existing information. In part, however, the situation results from the very real concerns of government and private agencies to protect the confidentiality of data suppliers. On the one hand, urban research could benefit immeasurably from the availability of geographically disaggregated micro data on individuals, firms, and households. On the other hand, the routine availability of such data would seriously compromise the anonymity of respondents, at least in certain cases. It is also clear, however, that a variety of existing techniques could be used to make micro data, even from small areas, available to the research community. These same techniques could also guarantee the confidentiality of individuals.

Consider, for example, the data collected and disseminated through the U.S. census. For 1980, data are available at the individual level describing households, their demographic characteristics, their economic resources, current housing and commuting characteristics, and their residential location five years ago. The raw data also indicate the current location of each household down to the city block of residence. As such, their potential utility for the study of regional migration, residential location and commuting decisions, and local mobility, to mention only a few, is enormous. The practical utility of such data in making planning and investment decisions is also undeniable. When these data are released, however, they appear in aggregate form. For any metropolitan area, they are published only in highly summary fashion by census tract, and only selected tabulations are available. Before release, the so-called public use sample of individual household responses is also stripped of meaningful spatial information. Similar procedures are followed for important and valuable data collected by geographical area by the Departments of Commerce and Treasury and a variety of other government agencies.

Many of these data could be made far more useful for urban researchers with absolutely no sacrifice in confidentiality. At least two approaches are possible. First, modern statistical techniques could be utilized to permit multivariate analysis of spatially disaggregated data at the individual level without release of individual data. Second, the availability of remote-access terminals at low cost could be exploited to allow researchers access to individual data for statistical and other purposes not inconsistent with the utter confidentiality of respondents.

The design, organization, management, and execution of such procedures and their dissemination throughout the research community are not trivial undertakings; they would require a substantial effort. To be

successful, moreover, such an undertaking would require strong leadership from within the government statistical community as well as the social science community. We propose a substantial commitment to support efforts to make existing micro data available in a geographically disaggregated form. To be successful, such an effort must insure that coordinated data collection efforts at the federal level will make location-specific information more accessible while scrupulously observing the confidentiality of respondents.

Such an effort is best organized by the research community, probably under the auspices of the National Research Council (NRC), and should be closely coordinated across disciplinary lines. The success of this effort depends crucially upon the involvement and leadership of the government statistical community. At least initially this project will require the full-time efforts of highly competent government officials and a methodologically trained support staff, including the talents of information specialists, computer scientists, and statisticians as well as social scientists.

Day-to-day management of such an undertaking is probably best undertaken by the government statistical community acting in close liaison with the NRC. In the long run, the continuing commitment to the objectives of this project can perhaps be insured by ample fellowship support and a small permanent staff in Washington. The level of initial commitment and the breadth of technical talent to insure meaningful change in the availability of data are, however, quite substantial.

It should be stressed that the output of this investment in research infrastructure is itself a public good available to all at little or no cost. As such it is quite unlikely that the efforts of isolated researchers will be directed toward these ends in the absence of a visible and well-financed central program. This is thus the classic case for a collective effort. The single most valuable contribution to urban research would be a serious effort to foster the availability of spatially disaggregated micro data in an ongoing fashion.

### *The Support of Focused and Integrated Analysis*

As noted throughout this discussion, a distinguishing methodological characteristic of urban research is the key role played by the spatial dimension of analysis. To a great extent, moreover, the field is characterized by an interdisciplinary focus, or at least a multidisciplinary tolerance. Both of these observations suggest that great intellectual progress can be made in the context of a specific geographical region and at a scale large enough to demand collaboration and integration. The his-



tory of research progress in the field bears witness to this. It is quite astonishing, for example, to recall the methodological and intellectual contributions made by the last large-scale integrated urban project, the so-called New York Metropolitan Region Study, completed in 1959. That study, a three-year effort funded by the Ford Foundation, culminated in some nine volumes of analysis, chiefly political, economic, demographic, and geographic, as well as an elaborate set of economic and demographic forecasts to 1985 (See Hoover and Vernon, 1959). The forecasts and the forecasting techniques were themselves influential in charting regional decisions about public investment; the silver anniversary of the effort was commemorated at the 1984 annual meetings of the American Economic Association (Netzer, 1985).

The methodological contributions of the New York study were considerable. It pioneered in the regional application of input-output analysis, in demographic projection techniques, and in the application of regression techniques to the analysis of government expenditures. The New York study extended the theory of shift-share analysis and provided its first empirical application. In short, the study was of fundamental importance to the technical development of regional science (Berman et al., 1960).

In retrospect, moreover, many of the ideas that emerged during the 1960s and 1970s and that shaped current urban research are direct descendants of seminal insights first put forward in the context of the so-called New York study. For example, the modern analysis of housing and residential location and the factors affecting the location of industry were clearly foreshadowed by the less technical but highly ambitious contributions of the New York metropolitan study. In some research areas, current work has been defined along a trajectory begun with the analysis of relationships in New York in the 1960s.

This is the right time to undertake several substantial, focused, and collaborative efforts in the spirit of the earlier integrated analysis. First, urban areas have changed a great deal in the past twenty-five years and the metropolitan areas chosen today for analysis are likely to offer quite a contrast in form and orientation. They would be less centralized, of lower density, and more homogeneous internally. One or more of the cities chosen would be young, located in the sunbelt, with rapid income growth, and so forth. Second, more precise and comprehensive theory would guide a large-scale integrated effort. Third, advances in computation, data processing, and statistical technique would permit hypotheses to be framed with more precision and with greater powers of discrimination. Fourth, the existence of these integrated efforts would provide a powerful and continuous stimulus for the improvements in

data availability noted earlier. Fifth, since the days of the New York study, there have been major advances in the theory and methodology of program evaluation and their applications in the urban and spatial context. These developments, arising principally from economics and regional science, have provided direct support for decision making for public expenditure programs, environmental policies, engineering and transport projects, and regional planning. Integrated and geographically focused research projects devoted to improving basic understanding of urban processes would also provide the opportunity to diffuse techniques and technology for improved decision making to governmental agencies and planning authorities.

We propose that several such integrated urban research projects be undertaken. Each of these projects should be focused on a particular metropolitan area and should be undertaken by a university-based research group. Projects should be of certain duration, say, three years, and research teams should consider their cities to be urban "laboratories"; they should be required to produce and disseminate analytically descriptive information early in the project's life. Preference in funding should be given to teams that promise an integrated disciplinary approach and a balance of theoretical and empirical analysis. Funding should be at a level sufficient to insure the full-time attention of the principal investigator and a senior social scientist with a proven track record, and should include funds for advanced graduate students and/or postdoctoral training.

The exact number of focused and integrated projects to be undertaken remains problematic. There are clearly more than a handful of university-based research groups that could credibly mount such a project. At minimum we recommend that three or four such "laboratories" in different geographical areas be established and that they be well funded for a fixed period. We also recommend that the program be evaluated and perhaps enlarged after the first set of integrated research projects is completed.

As an integral part of this strategy, we propose that funding agencies encourage the collaboration of social scientists working toward an integration of theory and empirical work across two or more of these urban research projects. This should be organized and funded apart from the integrated city-specific projects, and need not require the extensive financial commitments envisaged for the intraurban research activities. These collaborative efforts should use the interurban comparisons to facilitate inquiry by integrating structural and behavioral (or macro- and micro-level) factors. These efforts could involve a series of commissioned

working parties or a series of conferences linking smaller working groups at selected sites. The program should be evaluated after the first set of integrated research projects is completed.

Although the success of such an initiative in dramatically increasing our understanding of urban processes and outcomes is not certain, the probability of success is quite high. Moreover, given the rapid rate of diffusion of social science findings in this area, it is likely that data assembly and research insights about urban processes could be used by local authorities in the regions studied to improve local planning and decision making.

This initiative has a high potential payoff in improving basic understanding of urban phenomena and in integrating the analysis of urban economy and its social structure. Such a program may also be of real practical importance to planning, evaluation, and decision making. The initiative warrants support from those agencies and organizations concerned with basic research in the social sciences.

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## Legal Processes

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Observers of contemporary society frequently are struck, and sometimes dismayed, by the apparent increase in recourse to law and formal legal processes. In recent decades, and particularly in the United States, statute books and the Code of Federal Regulations have swollen at a startling pace. Parents bring lawsuits against school systems, rejected mistresses sue their former paramours, patients file malpractice claims against their doctors, and business corporations hire teams of specialists to deal with a growing web of regulatory obligations and liability rules (Friedman, 1985). Government bodies are drawn into litigation by Indian tribes, civil rights organizations, prisoners, and public interest law firms (Cooper, 1988; Schuck, 1983). The law, traditionally regarded as a source of order and stability, now seems more often to propel institutional change, reorder values and priorities, and disrupt established patterns of activity.

But what is the precise extent of these apparent changes in law and legal processes? What are their causes, and what do these developments portend for the fabric of social and political life? These questions have intrigued scholars interested in legal behavior and in the relationship between law and society, stimulating the promising lines of research that are the subject of this report.

### **The Social Scientific Study of Law**

The systematic study of law in society reaches far beyond recent interest in a "law explosion." In the 1930s, American law professors known as "legal realists" attacked the conventional understanding of judicial behavior. "Legal rules don't decide cases," they contended, "men do." But the legal realists had difficulty producing convincing answers to the questions, "How do men decide? And what difference does it make?"

In the ensuing decades, the quest for reliable knowledge about the way in which legal institutions actually operate produced a major expansion in social scientific research. Today, numerous scholars on law school faculties and in university departments of anthropology, economics, history, political science, social psychology, and sociology study legal systems empirically. The behavior of legal institutions is viewed in terms



of a reciprocal interaction between the law and political, economic, organizational, and cultural forces.

Two theoretical problems have dominated this research. The first concerns the social functions of law. When, many scholars have asked, do societies, or individuals within them, turn to distinctively legal mechanisms—as compared to other modes of social control, political action, and dispute resolution—to solve social problems or vindicate their interests? How do legal processes differ from other methods of social regulation and dispute settlement? What are the social consequences of increases (or decreases) in recourse to legal mechanisms?

The second theoretical focus has been nourished by empirical studies of variations *within* legal institutions. A single legal tradition, court system, or law enforcement agency, it has repeatedly been found, varies procedurally (from the formal to the informal) and substantively (in responsiveness to different political, social, and economic interests). Moreover, it is likely to vary along both dimensions over time. Accounting for such differences and assessing their social consequences has been a pervasive concern of social scientific legal research.

A brief discussion of three prominent research topics will illustrate the kinds of strides that have been made in recent years.

### *Legal Decision Making*

Reflecting the challenge raised by the legal realists, much progress has been made in the behavioral study of legal decision making. The scope of research has been extended beyond high court judges to include lower criminal courts (Eisenstein, Flemming, and Nardulli, 1988; Wheeler, Weisburd, and Bode, 1982; Feeley, 1979); juries (Kalven and Zeisel, 1966; Shanley and Peterson, 1983; Hastie, Penrod, and Pennington, 1983); welfare administrators (Mashaw, 1971); regulatory inspectors (Hawkins, 1984); police officers (Skolnick, 1966; Muir, 1977); attorneys (Rosenthal, 1974; Katz, 1982; Sarat and Felstiner, 1986); insurance claims adjusters (Ross, 1970); neighborhood mediation boards (Felstiner and Williams, 1978); and the disputants themselves (Merry and Silbey, 1984). Studies have analyzed whether decisions of particular courts and administrative agencies emphasize strict adherence to legal rules rather than discretionary or negotiated decisions (Levin, 1972; Wilson, 1968; Shover et al., 1984), or favor "haves" over "have nots" (Galanter, 1974; Wheeler et al., 1987), or advance liberal as opposed to conservative values (Gottschall, 1986; Wilson, 1982), or discriminate against members of minority groups (Spohn, Gruhl, and Welch, 1982; Baldus et al., 1983; Thernstrom, 1987). Second, in attempting to explain such variations, the

search for a single outcome-determining variable—such as “legal culture,” or the decision maker’s social background, or “capture” by particular economic interests—has been superseded by multivariate analyses of numerous factors that affect legal decisions; these include organizational pressures on officials to manage caseloads, the economic consequences of decisions, the differential capacities of litigants, the political environment of the decision-making organization, competing role conceptions and types of training, and the specificity of the legal rules themselves (in addition to studies cited above, see Gibson, 1983, 1980; Grabosky and Braithwaite, 1986; Kagan, 1978).

### *Legal Mobilization*

Before legal decision making can occur, the political system must have brought the social interaction in question into the ambit of legal control, and particular actors must have sought a legal resolution of the matter. Thus a second important research focus, as noted above, has concerned the factors that induce societies, or individuals within them, to mobilize (i.e., turn to) legal forms and institutions—as contrasted with other modes of social control, political action, and dispute resolution (Black, 1976). Some researchers have begun to develop measures of litigation rates over time and across regions and nations, along with theories explaining observed variations (Galanter, 1983; Friedman, 1983). Others have undertaken in-depth studies of particular communities, using survey and ethnographic methods, to determine the incidence of legally cognizable grievances, how they vary across social classes and arenas of social interaction, and when legal processes are mobilized to help resolve disputes (Mayhew and Reiss, 1969; Engel, 1983; Merry, 1979; Harris et al., 1984). A good deal of progress has been made in illustrating how economic considerations, legal complexity, subcultural norms, the availability of legal counsel, and other factors affect patterns of legal mobilization.

### *The Consequences of Legalization*

As noted at the outset, in recent decades industrialized democracies, and the United States in particular, have granted citizens a broader array of legal rights and expanded opportunities to invoke them. The consequence, in popular terms, has been the “legalization of society,” “the due process revolution,” or more pejoratively, “overregulation.” Not surprisingly, a third social scientific focus has been the impact of legal change on social, political, and economic life. A skeptical starting point was provided by William Graham Sumner’s nineteenth-century dictum

that law ways cannot change folk ways. But that extreme formulation has been transmuted into a more sophisticated series of questions. Researchers have shown how the capacity of legal mandates to "penetrate" or change social practices varies with social organization and attitudes, sanctions, modes of enforcement, and compliance costs (Muir, 1967; Wirt, 1970; Snortum, 1984; Kelling et al., 1974; Whittaker et al., 1985). Economic analysts have made major strides in estimating the impact of laws and regulations on economic efficiency (Ackerman, 1975; Posner, 1977; Crandall and Lave, 1981; Portney, 1982). Other researchers have shown how variation in methods of enforcement affects the disposition of citizens and business organizations to comply with law.

### **The Institutional Structure of Research on Legal Processes**

During the rapid growth of the 1960s and 1970s, the social scientific study of law has been served reasonably well by a highly decentralized institutional framework consisting of widely scattered university-based scholars, operating for the most part without the benefit of ongoing research institutions or long-term, large-scale studies. Again speaking generally, the primary research agenda has been the accumulation of individual case studies, often largely descriptive, focusing on particular courts, agencies, communities, or samples of litigants.

For case studies of discrete legal institutions, small-scale, loosely coordinated research has been quite appropriate and should continue. Most legal decisions are carefully recorded, facilitating archival research by individual scholars or small interdisciplinary teams. Similarly, observational and field interview techniques, which have been extremely valuable in the study of decision making by police, regulatory officials, and some kinds of attorneys, are well suited to research projects conducted by individual scholars. The same is true of the increasingly realistic jury simulation studies conducted by social psychologists.

Some coordination of conceptual frameworks, research questions, and coding instruments has been accomplished via communications links and peer review processes associated with academic meetings (primarily the Law and Society Association), quarterly periodicals (e.g., *Law & Society Review*, *Journal of Legal Studies*, *Law and Policy*, *American Bar Foundation Research Journal*), and the National Science Foundation's invaluable Law and Social Science Program. Nevertheless, this decentralized structure needs to be supplemented insofar as the field is now striving toward (1) the integration and coordination of case studies; (2) systematic development and testing of theory using substantial data bases; and (3)

the extension of research to topics that require more extensive survey research, for example, the social patterns that affect the mobilization of legal action, and the manifold social consequences of legalization.

The need for more systematic knowledge of the sources and consequences of legalization is not adequately met by the professional legal community. In contrast to the field of medicine and public health, the relevant clinicians and professional instructors—practicing lawyers, judges, government enforcement officials, law school teachers—are not closely integrated with a large well-funded body of “epidemiologists” and researchers who study the empirical causes and results of practitioners’ interventions. Law schools do not perform the role of centralizing, funding, and disseminating scientific research that is performed by leading schools of medicine and public health.

### **Research Needs**

The principal requisites for accelerated progress are: (1) institutional mechanisms for coordination and replication of research; and (2) systematic creation of data bases and facilitation of their use. Ideally, the coordination requirement would be met through long-term funding of three regional, university-affiliated research institutes for the study of (1) litigation and mobilization of formal legal processes; (2) legal decision making; and (3) the consequences of legal change. Coordination would be enhanced by support for communications networks linking scholars working on similar topics. A more fundamental form of cohesion would be promoted by annual informal conferences of senior scholars and graduate students, analagous to the Gordon Conferences, which have stimulated development of the natural sciences.

In terms of data base creation, the regional research institutes could serve as centers for the longitudinal study of legal processes, attitudes, and impacts in designated “index communities” and as sites for interaction between researchers and representatives of courts and other legal agencies.

### **Illustrating Research Needs: Three Exemplary Lines of Research**

The case for establishing ongoing research institutes, networks, conferences, and index communities can be supported by reference to three exemplary topics: longitudinal studies of litigation; the “rights explosion” and its consequences; and informal versus formal dispute resolution.

In each of these areas, substantial progress has been made recently, laying a foundation for exciting further advances. But in each area, basic theoretical or empirical problems exist due to weaknesses in available data bases and in institutional arrangements to support the necessary research.

### *Longitudinal Studies of Litigation*

Theoretical development in the study of legal processes has often been impeded by the difficulty of systematic data acquisition. Consider, for example, the relationship between economic change and legal processes. Researchers have been interested in the hypothesis that litigation increases as a function of economic growth and have sought to specify that function more precisely (Friedman, 1983). The underlying assumption is that economic expansion creates social mobility and differentiation, new exchange relationships that are not clearly governed by traditional norms, and breaches of settled expectations; hence economic growth generates more disputes that resist informal resolution. A competing theory would stress intervening political factors: litigation rates reflect conscious political struggles between those who wish to preserve and those who wish to readjust existing power relationships through manipulation of litigation costs and liability rules. How might the relationship between economic growth and litigation rates be tested?

Some measures of economic growth are readily available, not only at the national level but also in disaggregated regional units. Economic historians have constructed economic indicators reaching back into the nineteenth century. Moreover, some measures are available for more specific relevant trends, such as business failure rates, debt delinquencies, and various kinds of accidents. In contrast, obtaining comparable data on litigation rates is fiendishly difficult, especially in the United States. Until recently, few state governments kept systematic tabulations of lawsuits filed in lower courts. The records that do exist often cannot support longitudinal or cross-state comparisons because of changing jurisdictional rules (Daniels, 1984). Moreover, official records, designed for court caseload management rather than social research purposes, rarely break the data down into type-of-case categories that are meaningful for social scientific analysis. To chart trial court litigation, therefore, researchers must uncover case files and docket books stored in county courthouse basements and conduct labor-intensive counting, sampling, and coding operations.

In the 1970s, several researchers, acting independently, took on such tasks, concentrating on one or two counties in different states (Friedman and Percival, 1976; McIntosh, 1980–81, 1983; Daniels, 1982; Selvin

and Ebener, 1984). (More recently, one researcher conducted an intensive three-county study in West Virginia [Munger, 1986] and another scholar, John Stookey, is examining four counties in Arizona.) These studies have provided eye-opening, if selective, mappings of the course of civil litigation in the past century, indicating that lawsuits per capita have indeed increased for personal injury cases, but not for commercial, property, and debt cases. Along with recent research on litigation rates in other countries (Haley, 1982; Wollschlager, 1985), these studies suggest that the interplay between economic growth and trial court litigation is variable and complex. Yet controversy exists concerning the exact relationships (Krislov, 1983; Marvell, 1985), largely because of huge gaps in the relevant data.

First, data gaps stem from divergences in research techniques, concepts, and measures employed by different American, European, and Japanese researchers. Partly because of a lack of coordination among researchers, studies have employed different sampling intervals, type-of-case categories, ways of coding outcomes, and different baselines for litigation rates (e.g., lawsuits per 1,000 persons, lawsuits per 1,000 adult citizens).

A second data weakness stems from the limited range and density of existing research. The studies have indicated that aggregate litigation rates confound contrasting trends for different kinds of cases and conceal short-term shifts caused by sudden economic, political, and legal changes. To adequately chart the incidence of legal conflict and its correlates would require studies employing finer breakdowns by type of case and litigant; denser sampling intervals; more locales, reflecting regional variations in law and economic trends; and comparisons between the United States and other industrialized nations for closely defined categories of cases.

Third, the groundbreaking studies mentioned above focus exclusively on entities called "courts," whose jurisdictions and functions change over time. But contemporary societies regularly create less expensive functional alternatives to courts, including police departments, regulatory agencies, administrative tribunals, conciliation offices, and privately operated grievance-resolution and arbitration systems. Recourse to "courts," it has been shown, is affected by changes in litigation costs and counsel fee arrangements, and the accessibility of alternative forums for the processing of legal claims (Verwoerd and Blankenburg, 1985; Rosch, 1987; Blankenburg and Rogowski, 1986). Understanding the relationship between economic change and "litigation," therefore, must require data on these additional factors and forums, impelling researchers to range far beyond the archives of single courts.

The present institutional structure for social scientific studies of litigation seems ill-suited to the task of filling the data gaps just itemized. Longitudinal studies of legal contestation are labor-intensive, particularly if they seek to include the finer substantive breakdowns, data on costs, and alternative dispute resolution institutions mentioned above. But thus far, research has been funded on a small scale, ad hoc basis. Lack of resources and coordinating mechanisms has inhibited the instigation of multidistrict or cross-national longitudinal studies at the trial court or administrative agency level.

First steps toward coordination are emerging. The recently established Comparative Legal Indicators Project, coordinated by Professor Erhard Blankenburg at the Vrije Universiteit of Amsterdam, has established a network of primarily European scholars interested in comparative studies of litigation. Professor Frank Munger at the State University of New York at Buffalo has organized a network of scholars working on historical studies of trial courts. Both, however, lack funding, adequate mechanisms for meetings and communications, and the needed visibility. Systematic means of sustaining and publicizing such special-topic networks, accordingly, would be extremely valuable.

More fundamentally, significant opportunities for progress would stem from the creation of regional research centers that would be guaranteed a ten- to fifteen-year existence. By serving as repositories of research expertise and accumulated data, as clearing houses for information on planned and in-progress research, and as sites for regular research conferences, the cumulation of experience and comparative focus needed for theoretical development would be greatly enhanced. In addition, "flagship" centers for research on litigation, conceived broadly, could speak with authority for the research community in urging court administrators and administrative agencies to keep case records in formats useful for long-term and theoretically relevant research purposes.

### *The Rights Explosion and Its Consequences*

In many areas of law, individuals have won stronger legal protections in their relationships with the corporate and governmental bureaucracies that bulk so large in modern society. New rights have been granted to employees with respect to occupational safety and health, questions of job security, and protection from discrimination; to consumers vis-à-vis merchants, banks, and finance companies; and to injured persons as against liability insurance companies. School districts are legally obligated to provide notices, hearings, and appeals to parents of learning-disabled children; social welfare agencies must accord similar procedural

rights to their clients, as must public works and facility-siting authorities for neighborhood and environmental groups.

In theoretical terms, these new rights challenge long-standing assumptions concerning the function of law in preserving existing hierarchies of wealth and power. An important focus of research, accordingly, has been the social consequences of attempts to affect, by law, existing economic and power relationships, and the processes by which new legal rights are invoked or mobilized.

One persistent finding is that the translation of the law on the books into social behavior cannot be taken for granted. The weakest link in that chain, in most cases, is the process by which intended beneficiaries learn of their rights and mobilize legal institutions to enforce them. Welfare recipients, poorly educated or vulnerable in their dependency, often fail to invoke rights to hearings (Handler, 1979; Mashaw, 1971). Poor consumers remain ignorant of legal rights established by debtor protection statutes (Caplovitz, 1967; Whitford, 1979). School administrators learn to satisfy the forms of due process without surrendering real influence to most affected parents (Weatherly and Lipsky, 1977).

Repeatedly, however, such effects have been shown by other researchers to be variable; some categories of citizens invoke their rights effectively and with some frequency (Neal and Kirp, 1986; Miller and Sarat, 1980–81). Moreover, even “incomplete” mobilization, if it results in a certain level of sanctions, can have powerful deterrent effects, inducing affected organizations to provide the intended protections as a matter of routine (Bardach and Kagan, 1982). However, the identification of such tipping points, through the study of deterrence, detection, and sanctioning processes, is still in its infancy.

Another line of research has highlighted the unintended effects of legal change. Expansion of debtors' rights, some studies indicate, has resulted in higher litigation costs for creditors, increased acquiescence in delinquencies, and loss-shifting to debtors who pay on time (Kagan, 1984). Due process protection for clients in some social benefit programs has led to tougher, more mechanical eligibility standards and longer delays for new applicants (Simon, 1983). Legal changes facilitating medical malpractice claims have resulted in more “defensive medicine,” less trust between doctor and patients, and higher medical costs (Greenwald and Mueller, 1978; Danzon, 1985). While some class actions brought by public interest groups have had little more than symbolic effects (Handler, 1978), others have achieved significant reforms (Mnookin et al., 1985), and still others have forced government agencies into wasteful reallocations of scarce resources (Melnick, 1983) or into “reforms” that reduced service or protection for some classes of beneficiaries (DiIulio, 1987; Rothman and Rothman, 1984).



These divergent findings and emphases reflect an emerging consensus: under some circumstances, new rights can effectively change hierarchical relationships, but the effects of legal change are variable and contingent. The next stage, many scholars would argue, will be to specify the conditions under which different categories of individuals come to regard themselves as rights-bearing citizens in their relationships with organizations. What factors—including the modes of communication of legal rights, methods of enforcement, the social ties between parties to potential disputes—affect the frequency with which legal rights are invoked? Finally, how do these and other factors—including the availability of insurance and methods of spreading compliance costs—affect organizations' propensity to institutionalize compliance with new legal obligations?

The questions just listed will be difficult to answer without appropriate data bases. These must be *comparative*, encompassing responses to different laws in different types of communities. They must also be deeply *contextual*: law is only one factor affecting behavior, existing research has reemphasized. Hence isolating the effect of *law* requires data not only on how claimants and regulated entities respond to legal stimuli but also on their responses to a broad array of economic, cultural, and organizational factors. Third, because the effects of law as well as other factors often change over time, *longitudinal* data are necessary.

The current decentralized research structure does not adequately facilitate such comparative, contextual, and longitudinal research. As noted earlier, it tends to yield case studies using different methods, conceptual models, variables, and measures. It fails to provide sufficient support for replication of case studies in different socioeconomic settings, much less for larger-scale comparative or longitudinal studies.

One strategy for facilitating contextual, longitudinal studies is to focus research on designated "index communities" (National Academy of Sciences, 1976). In this concept, a group of communities (including sub-communities in larger cities), selected to vary on theoretically significant dimensions, are "studied both historically and anthropologically over an extended period of time." This continued or reiterated in-depth study is calculated to reveal how "a variety of social and cultural trends are articulated in the lives of community members" (p. 50). Index communities would, of course, be useful for many strands of sociological, political, and economic research. But they would simultaneously help develop understanding of changes in rates and modes of mobilization of law and formal legal processes, and of the cumulative social consequences of specific legal change efforts.

Logistically, established programs for recurrent studies of designated index communities would promote the cumulation that fails to emerge

when individual researchers choose different communities for study. Under uncoordinated research structures, some students of the consequences of legal change sample individuals, some sample affected organizations, and some study legal institutions responsible for enforcement. The index community concept would encourage joint research at all three levels.

Case studies of specific laws cannot easily determine whether observed social responses are stimulated primarily by attitudes toward the particular law or by underlying attitudes toward the legal system as a whole—what might be called the legal culture. By providing the promise of recurrent research efforts, the index community concept not only would draw together diverse researchers but also would motivate them to view their own research in close relation to that of others, thereby encouraging attention to the underlying legal culture of the community, along with broader political, economic, and cultural changes.

### *Formal Versus Informal Legal Dispute Resolution*

Research on a broad range of substantive and institutional arenas has persistently revealed that only a small proportion of initial legal claims or violations result in formal prosecution, adjudication, or rule application. A variety of ethnographic, archival, and survey research techniques have been employed to describe the systems of mediation, negotiation, and discretionary judgment that operate in the shadow of formal adjudicative and bureaucratic systems. This research has shifted the primary focus of efforts to understand legal systems from the peak of the legal iceberg—the relatively few dramatic high court cases—to the huge, below-the-surface sector of uncontested, informally resolved, or negotiated cases. Nevertheless, controversy still exists concerning the precise causes and consequences of legal discretion and informalism.

Some research indicates that discretionary decision making and informal dispute resolution, while reflecting standards derived from formal law, provide higher levels of litigant satisfaction along with greater accessibility, economy, celerity, and flexibility (Danzig, 1973; Kelman, 1981; Mnookin and Kornhauser, 1979; Sarat, 1976). Other scholars, in contrast, conclude that informal and discretionary decision making tend to induce claimants to accept incomplete vindication of their legal rights, eroding the law's deterrent effect and its potential as a vehicle for political change (Abel, 1982; Fiss, 1984; Silbey, 1984).

Once again, these contrasting interpretations reflect, in large part, certain weaknesses in the data. Researchers conducting geographically separate case studies often have used somewhat different measures of

outcomes, participant motivations, decision-making method, participant satisfaction, and consequences. At this stage in the field's development, major gains would stem from closely coordinated comparative studies that contrast informal and formal dispute resolution and law enforcement, dealing systematically with issues of accessibility, participant motivation and satisfaction, compliance, and regulatory effects. Institutional mechanisms for coordinating and funding such efforts, however, have been limited.

One model was provided by the Civil Litigation Research Project (CLRP), funded by the Department of Justice and housed in the University of Wisconsin, Madison. CLRP coordinated the efforts of a group of leading law and social science scholars from different universities. Through systematic surveys, it showed how often citizens in a sample of communities experienced different kinds of legally cognizable grievances, the frequency with which those grievances were pursued through informal and formal dispute processing channels, what degree of success these efforts had (as self-reported), and what litigation cost (Trubek et al., 1983). Unfortunately, no permanent mechanisms for the continuation and cumulation of the CLRP effort were established, and the ensuing dispersion of researchers, skills, and techniques makes the systematic extension of the project difficult. Continued funding for research networks and centers for the study of legal processes would provide a strong foundation for perpetuating the momentum and skills developed in such collaborative projects.

Another model for studying dispute resolution has been indicated by a series of detailed ethnographic studies of citizens' attitudes toward and use of both informal and formal processes in selected neighborhoods, towns, or subcommunities (Baumgartner, 1985; Ellickson, 1986; Engel, 1984; Greenhouse, 1982; Merry and Silbey, 1984). Full development of this model, however, would require funding and coordination mechanisms for systematic replication. Funding research networks of scholars working in this vein, along with reiterated projects on a series of index communities, would be a powerful mechanism for institutionalizing the useful neighborhood study method and for expanding its focus to assess the social control effects of informal versus formal decision methods.

The degree to which informal dispositions deviate from the standards of positive law and the capacity of informal dispositions to evoke compliance present difficult data-gathering and analysis problems. A few researchers have used statistical techniques to identify cases with similar issues and party characteristics and then to compare dispositions of mediated cases with dispositions of formally adjudicated cases. This approach is feasible only when researchers have been granted access to

and have the resources to code case files and, in some instances, to track down and interview parties after case dispositions. Again, ongoing regional centers for the study of legal processes that encourage cooperative relationships with court administrators, administrative agency officials, and mediation services would greatly facilitate access to and accumulation of the necessary data.

Finally, a major gap in the data on formal and informal decision processes stems from the paucity of in-depth studies that draw on the experience of key legal decision makers, including lower court judges, arbitrators, regulatory officials, and administrative hearing officers. As legal and regulatory processes have come to operate in a more politicized environment, many officials have become more cautious in dealing with social science researchers. Ongoing centers for the study of legal processes could help legitimize such research, partly by encouraging more visible, high-quality research and partly by serving as centers for conferences and other projects that encourage direct participation by legal decision makers. A practical side benefit would be the creation of fora in which legal decision makers could openly discuss the use of discretion and develop theoretically informed principles of professional conduct with respect to discretionary and informal legal action.

### **A Final Word About Research Centers, Networks, and Conferences for the Study of Legal Processes**

Because legal processes change over time, sometimes quite rapidly, and because they often vary considerably across legal jurisdictions, communities, and clienteles, the study of legal processes necessarily must remain sensitive to local contexts. A high degree of centralization of research clearly would be counterproductive. The centers for the study of legal processes that we recommend would exert their influence not through monopolization or control but by providing examples of excellence, opportunities for coordination, and locations for accumulation of data and research experience.

It is precisely because of the variability of legal phenomena and the importance of decentralization that three geographically separated centers, free to set their own agendas, free to compete and specialize as well as to cooperate, would be preferable to a single large and powerful center. On the other hand, multiplying the centers beyond this number would exacerbate the structural problems referred to earlier: few law schools currently are committed to social scientific studies, and relevant research is scattered among various disciplines. A handful of "flagship"

centers would achieve high visibility in the field, thereby encouraging coordination, excellence, and wide dissemination of findings.

The centers need not be elaborate and expensive institutions. There would be no need for expensive equipment. They would probably use the mainframe computers at affiliated universities. Space needs would be small: the primary requisites would be office space for researchers, a conference room, and a library that could also serve as a data bank and repository of coding and survey instruments. Some of these elements may be available in existing university buildings.

The principal governmental commitment needed would be for continuity of funding—ideally for twenty years—for a small staff, for periodic released time for senior scholars, and for certain basic, ongoing research operations, such as recurrent index community surveys, yearly conferences for scholars and for legal officials, and regular gathering of court data. More elaborate projects housed in the centers would be funded through applications by individuals or groups of scholars to traditional funding sources.

Can existing research institutions fill this need? We think not. Organizations such as the Federal Judicial Center and the Center for State Courts do valuable work, but they are attuned overwhelmingly to practical court management concerns rather than to theoretically important issues. The same is true, in a somewhat different sense, of the National Institute for Dispute Resolution. The RAND Institute for Civil Justice and the American Bar Foundation, while engaged in excellent social scientific work, may be too closely linked to particular constituencies, sources of support, and practical concerns. The most appropriate settings, therefore, would be leading universities (perhaps one on each coast and one in the Midwest) in which a critical mass of visible law and social science scholars with close links to foreign scholars now work.

As important as regional research centers would be, the basic social organization of law and social science research must remain decentral-ized. Most researchers will spend most of their time in their own universities, taking advantage of research opportunities in local legal institutions, often working in isolation. Research centers, therefore, cannot substitute for the other coordination mechanisms we recommend: organized communication networks and conferences.

Networks of collaborative scholars might be organized in terms of research topic, as in the case of the European comparative legal indicators network mentioned earlier, or in terms of region. One example, rewarding to participants and fruitful in eliciting research efforts, is a workshop on legal process and legal ideology that has drawn together scholars from Amherst, Wellesley, the University of Massachusetts, and

other colleges in New England. It does not take a great deal of money, but does take some, to fund the administrative aspects of such networks and to make the sessions visible to interested scholars. But on the basis of past experience, the payoff in terms of the collaborative definition of questions, methodological gains, and mobilization of energy seems to be large.

The annual meetings of the Law and Society Association, initiated in 1974, have been enormously important in creating a sense among participants of a scholarly community dedicated to the empirical and theoretical study of legal systems. These meetings now are busy and exciting four-day affairs, attended by over 350 scholars, featuring scores of panels, at which formal papers are delivered and discussed. Success breeds its own limits, however; there seems to be hardly enough time left for the kind of sustained discussion that would yield consensual priority-setting and coordination of research efforts. The success of the Gordon Conferences in defining research frontiers in the natural sciences provides an intriguing model for extending and deepening the role of the Law and Society Association annual meetings. That model entails extended annual summer sessions, attended by approximately 100 scholars and graduate students, in which informal presentations predominate. By creating the time in busy schedules for sustained discussion and debate, and by simultaneously creating close ties between senior and junior scholars from around the country, such conferences, for a modest investment, could pay enormous dividends in focusing and stimulating research on the role of law in modern society.

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PART

**V**

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**SOCIETIES  
AND INTERNATIONAL  
ORDERS**



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# 22

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## Culture and Ideology

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Our work group had a particularly challenging task not only because its two terms of reference are among the most contested in the literature—the term culture, for example, has been subject to voluminous and recurrent definition in anthropology alone—but also because the subject matter is not self-contained. It implicates itself as it reflects upon the researcher. Thus the reflexivity, the intense self-examination, that has been going on in the behavioral sciences over the last decade becomes inevitably a part of this topic. The familiar parodic declension suggests the problem: “I have considered views of social reality, you have political opinions, he has an ideology.” The Archimedean point from which one examines this topic or any social science question is thus brought into question, and the struggle for objectivity as regards it is the greater. Nevertheless, the topic helps us to focus on core problems for the social sciences as it focuses upon core problems in the conduct and method of social science. Recent work, as we will show, offers us increased understanding of those cultural influences that have so often been implicit both in the behavior we seek to understand and in the balanced professional practice we seek to achieve.

But much more than that is the contribution that those culture and ideology approaches offer to our understanding of the dynamics of national and international life. As we look around, we see how misleading any literal reading of the oft-discussed prediction of an “end to ideology” is. Everywhere we look, and particularly if we include implicit—culturally embedded—as well as explicit—culturally confrontational—ideologies, we see ideologies at tension and in conflict. The pervasiveness of this “vital yet enigmatic” phenomenon in the modern world, at home and abroad, continues to argue for the concentrated attention of behavioral science.

### **The Cultural Approach in the Social Sciences**

Indeed, in the last fifteen years there has been a surge of cultural studies in the social sciences transforming our thinking about these sciences



and their subject matter. This development builds on older emphases on the sociology of knowledge in German sociology (Mannheim, 1946), on the elementary cultural forms in French and British social anthropology (Mauss, 1954), and on cultural configurations (Benedict, 1961) and on worldview (Redfield, 1953) in American cultural anthropology. Work by Berger and Luckman (1966), by Douglas (1978, 1982), and by Geertz (1973) have been particularly influential in reorienting work in the social and behavioral sciences towards this understanding of the "cultural construction" of the social realities we believe in and enact. Something we take to be as "real" as illness and disease can be shown to be culturally constructed (Ohnuki-Tierney, 1984). This approach we also ought to say is most revelatory when it is intensely ethnographic and based on long-term participant observation. This is true not only for questions of illness and disease but for something as tenuous as the study of elites (Marcus, 1983). We will treat the most recent developments in the cultural approach in four topic areas of study:

1. Culturally embedded ideology in political and policy studies.
2. Dynamics of ideology in complex civilizations of the Third World: the cases of South Asia and Latin America.
3. Cultural studies of historical transformations.
4. The cultural study of modern American society.

### *Culturally Embedded Ideology*

The most pervasive dissemination and reinforcement of ideology in the contemporary world may stem from ideas implicit in the everyday cultural practices that are rarely recognized as ideological at all, especially language, science, art, and architecture. Research of the last several decades on the processes by which common activities convey embedded ideologies has made it clear that this area is a fruitful one. Such masked ideology is likely to be more potent than explicit ideological argument because it rarely evokes skepticism and rebuttal and because it more fundamentally shapes people's conceptions of the social world and of themselves.

Language is probably the most potent source of embedded ideology. Michel Foucault (1972, 1977, 1978) has shown through historical investigations that both everyday talk and the scientific language of esteemed professions and disciplines (psychiatry, penology, medicine) constitute ideologies that justify control and domination all the more effectively because they are accepted as science. Following Foucault's lead, Mi-

chael Shapiro (1981) has analyzed the ways in which styles of writing and speaking incorporate and disseminate ideologies through the categories they employ and avoid and through the subtle resonances of terms. Joseph Gusfield (1981) has demonstrated that our everyday talk about drinking/driving disseminates beliefs about the character of people involved in accidents and about the reasons for automobile accidents that are not supported by the facts; they overstate the role of irresponsible people and understate the role of unsafe cars and road conditions. Gusfield also analyzed the ideological character of the "scientific" testimony and research about drinking/driving, showing how science is, in an important sense, a literary form. Murray Edelman (1977) has explored the reinforcement of conventional beliefs through banality in political language and the ideologies implicit in the language of the helping professions and in bureaucratic language.

We have a theoretical basis and some appropriate methods for pursuing more rigorous, extensive, and systematic research on political language and ideology; such work would help to illuminate aspects of the social world that are difficult or impossible to explore in any other way. Work by anthropologists in the 1970s on the rhetoric of social life (Sapir and Crocker, 1977) explores this same problem in non-Western societies. It is *language* about politics that people experience rather than political events themselves; even political happenings that are close by take their meanings from the language used to depict them. The language constructs "spectacles" featuring leaders, crises, persistent problems, and enemies, all of them with meanings that vary with people's material situations and that both reflect and reinforce ideologies. The perceived threats or enemies, for example (nations, races, ethnic groups, social classes, genders), vary with the time, the place, and the social and economic conditions of observers; many groups attacked as enemies (Jews in Nazi Germany, women persecuted as witches in seventeenth-century Massachusetts, Nisei in the United States in World War II) do no harm. We have hardly begun to understand the full range of social consequences that flow from the choice of language forms, but recent research has made clear that the topic is fundamental.

Other cultural practices involve embedded ideologies as well, and our understanding of them may be more embryonic than in the case of language. Several studies have analyzed buildings and architectural spaces as social and political symbols, showing that they help evoke beliefs about the authority, competence, power, and dignity of people and institutions (Edelman, 1978; Lasswell, 1979). The monumentality of some public buildings and corporate office buildings, differences in the lavishness and comforts of spaces according to the kind of client they accom-

modate, and devices that inhibit or encourage communication or define people as under surveillance all illustrate the reinforcing link between architecture and ideology.

Science is still another aspect of the culture in which ideology is potent because it is accepted as nonideological. For quite a long time there has been lively debate over whether the assumptions and methods of positivist science incorporate and reinforce the dominant political ideology of observers by understating the degree to which observations reflect the presuppositions of observers and the conventional social assumptions (Bernstein, 1978). There is reason to believe that the conventional views of science perpetuate inequalities in gender roles (Vickers, 1982) as well as other forms of inequalities. As already noted, a number of the studies of political language find that science is a source of embedded ideology. Two books analyze the sense in which this is true of economics, probably the social science with the most impressive reputation for sophistication and objectivity (Gudeman, 1986; McClosky, n.d.).

Art as a cultural form is more complex from this point of view. Works of art are often explicitly ideological, and many that are not are sources of embedded ideology in the sense already discussed. At the same time, there is a persuasive argument that art can complement research in helping us to understand ideology and free us from implicit ideology because the work of art is itself a powerful demonstration that realities are constructed and because great art liberates the mind from conventional presuppositions (Marcuse, 1978).

Fetishism is a theme common to all these probings of embedded ideologies: the recognition that people project into their environments problematic beliefs, which then seem to have an independent existence and come to dominate the thought of their creators. This recognition has wide-ranging ramifications for other research projects as well because research itself is one of the cultural institutions it can illuminate. It is clear that interdisciplinary work and a resort to diverse methodologies are appropriate and necessary.

***Dynamics of Ideology in Complex Civilizations of the Third World:  
The South Asia and the Latin American Cases***

The large-scale literate civilizations of the non-Western world, such as those of China, India, and Latin America, pose the question of how ideas, values, and belief systems are created, transmitted, and shared among vast human populations that have traditionally lacked the social mobility and communications technologies of the West. Here, the ideo-

logically unifying aspects of ancient civilizations seem to coexist with a great diversity of regional, religious, and linguistic subgroups whose contribution to the overall structure of society is both complex and unclear. The study of these complex civilizations reflects this paradox of unity and diversity. Some scholars emphasize the overarching unity and others insist upon the diversity.

European sociology in the late nineteenth and early twentieth centuries sought to define the timeless "essence" of Chinese, Islamic, and Hindu/Buddhist civilizations through macrosociological or ideal-type analysis (Weber, 1958). American scholarship, when it focused on these major peasant cultures, tended to examine empirical processes of social and symbolic interaction "on the ground." In this regard, the work of Redfield (1941, 1956) on the levels of sociocultural complexity in the Yucatan peninsula has exerted an enduring influence.

Redfield's insight was to recognize that the premodern peasant civilizations of the Old and the New World must be viewed as systems of cultural communication. Somehow, aesthetic standards, irrigation technologies, medical systems, legal codes, poetic genres, and mytho-religious traditions—to name but a few examples—are created, codified, and communicated across hierarchically separated levels of society and over vast geographic distances. To what extent do elites dictate ideological doctrines and cultural values "from the top down" in such traditional civilizations? Do some ideas from the more isolated, less literate, politically weaker and socially stigmatized groups in society—the Little Traditions, in Redfield's terms—eventually "trickle up" to the intelligentsia in the cosmopolitan urban centers which Redfield identified as the locus of the Great Tradition? These questions about cultural consensus and dissensus, ideological integration and conflict, remain central issues today.

To illustrate, let us first consider research on complex civilization in South Asia. After World War II intensive holistic fieldwork conducted in the Indian subcontinent produced a rich picture of rural society based upon caste hierarchy and agrarian patron-client relations. What was still lacking, however, was a dynamic model of the ways in which rural peasants participated in, and were influenced by, the larger civilization. Influenced by Redfield, Marriott (1955), and Singer (1972), recent researchers have made major advances on this problem by documenting the movement of cultural ideas and beliefs both up from the villages and down from the cosmopolitan centers. Despite its seeming rigidity and traditionalism, South Asian society was seen to be riven by the tensions of intercaste rivalry and status competition, a tendency manifested in many regions by lower castes imitating the lifestyles and religious taboos

of the Brahman priestly caste (Srinivas, 1952). This suggestion that Brahman priestly ideals were the most widely respected values of Hindu society foreshadowed recent theories of pan-Indic ideological consensus based upon the Hindu religious and metaphysical tradition.

In the mid-1960s the Great Tradition-Little Tradition work encountered a major challenge from the French structuralist school in the work of Louis Dumont (1970). Dumont insisted upon a set of logical principles governing the ideas and institutions of Hindu society. He criticized the prevailing Anglo-American tendency to dwell upon the singularity of discrete, empirically observable constituent units and postulated a single dominant Hindu cultural belief system ("ideology" in his terms). This ideology explains the caste ranking and the diverse religious behavioral patterns of nearly 600 million Indian peasants. This controlling, fundamentally religious ideology is set radically apart from the West by the rules of purity and impurity, by the supremacy of ascribed religious status over political or economic power, and by the Hindu penchant for social inequality which these cultural axioms generate.

In response to Dumont's Gallic theory of South Asian civilization, a rival school emerged in the 1970s under the banner of "Hindu ethnology," led by several anthropologists and historians at the University of Chicago (Marriott, 1976). This approach emphasized a distinctive transactional logic in South Asian behavior. Just as the ancient Hindu concepts of Ayurvedic medicine stress the constant need to manipulate diet, work, worship, and sociability in order to achieve a healthful equilibrium between the fluctuating humors of the body, so the ethnosociological theory contends that all Hindus (and many non-Hindus) follow implicit social interaction strategies that result in the mixing, sharing, or conserving of their distinctive individual and group qualities, which they believe to reside in their blood and bodily tissues ("bio-moral substance").

These global theories of South Asian ideology, of metaphysical unity and cultural consensus (Fernandez, 1965), have brought a resurgence of interest in a processual approach to the problems of unity and diversity within a complex civilizational tradition. Scholars are now singling out specific occupational castes for intensive study. The study of untouchables is particularly important, for they represent the ultimate test of the power of ideology and cultural belief to unify hierarchical social systems. Several writers (Berreman, 1971; Mencher, 1974) have maintained that cultural consensus is weak or nonexistent at the bottom of the caste hierarchy and that untouchables are either ignorant of, or hostile to, the Hindu religious doctrines that legitimize their low status in the eyes of the higher castes. Moffatt (1979), however, found a surpris-

ing degree of "replication" of high caste ideas among an untouchable caste in southern India. He suggests that Dumont may be correct about the pervasiveness of purity/impurity ideology, even among those who are its most stigmatized victims. Research elsewhere has shown that local-level political and economic factors sometimes significantly limit the ability of untouchables to perform the rituals and maintain the religious institutions that would enable them to participate in the dominant high caste ideology (McGilvray, 1982, 1983).

What these contesting studies make evident is that both large-scale cultural consensus and fragmented patterns of regional, sectarian, and ethnic dissensus must not merely be noted and mapped, but *explained*. The basic questions raised by Max Weber and Redfield do not so much concern fixed ideological beliefs as ideological *processes*, grounded in historical and behavioral realities, which must be understood in terms of a sociology of knowledge.

That this work on ideology and consensus is of the greatest importance is seen in South Asian communal violence. Hostile linguistic, religious, and ethnic communities—Sikhs versus the Hindus in the Punjab, Assamese versus Bangladesh immigrants in Northeast India, high castes versus low castes in Gujarat—are currently engaged in bloody conflict, despite the fact that South Asia has been a cradle of some of the world's most celebrated nonviolent religious and political movements. We need to understand how explicit, politically focused ideologies of hatred are mobilized and manipulated in the heat of ethnic and sectarian conflict. These ideological problems pose a major threat to schemes for social change and economic development, as shown in the case of Sri Lanka, where fighting between the Sinhalese and the Tamils is now undermining major domestic and international programs in irrigation, export industry, and tourism.

This dynamic of ideology and the (often violent) struggle for consensus in South Asia is repeated throughout the Third World and is everywhere a challenge to our understanding. Peasant studies have become a particularly vigorous field again as, under the influence of dependency theory, neo-Marxism, and symbolic anthropology, the ideological struggle on the international stage as reflected on the local level has been made a part of research. Recent research in Latin America has asked how peasants actively construct their social life and their identities in reaction to the various ideologies, explicit or embedded, that claim their allegiance, and how they make these constructions within the material constraints that provide the context for their everyday activities.

Taussig (1980), for example, has examined how local peasant beliefs in the power of the devil are closely linked to the rise of commodity

production and the expansion of international capitalism and its mercantile ideology in the rural area of Colombia. His work draws on Nash (1979), who, adopting a more materialist perspective, shows how a Bolivian people respond to the brutalizing conditions of wage labor by drawing on and transforming their traditional culture.

To focus on a single country, studies from Guatemala have examined the ways in which Catholicism was initially used ideologically to subordinate native peoples. Later it was transformed by these same peoples to fit their local situations. Now, by a curious turn, it has become the idiom by which many are voicing their critique of the existing system and their hopes for increased control and liberation within society (Britnall, 1979; Hawkins, 1984; Warren, 1978). One of the theoretical issues addressed by all this work concerns the dialectical relations between peasant conceptions and the hegemonic forms of thought imposed by a dominating society that is itself integrally linked to one or another international ideology.

### *The Historical Transformation of Culture*

Anthropology and history have been in fruitful interaction for more than two decades now. Anthropologists demonstrated the power of the culture concept by showing in detailed ethnographies the power of indigenous belief to construct social practice. Historians, for their part, challenged the synchronic approach of anthropology by posing the problem of societal transformation. Recently, young historians influenced by anthropological methods and modes of explanation have produced a dynamic social history especially attuned to matters of ideology and culture. Anthropologists, in their stead, have recently been writing a much more historical ethnography. The result has been a reexamination of the traditional view of the coherence of culture, and of the necessary consensus that enters into social processes. Very close inquiry is being conducted into how culture constructs practice and practice transforms culture. We are obtaining new understanding of consensual processes.

Especially in highly complex societies with stratified social and political systems, cultures are rarely fully coherent or truly consensual. The different groups that compose such societies tend to have distinct subcultures, or at least to understand the dominant culture in quite different ways. Struggles between these various groups will therefore be in part ideological struggles over the symbols and meanings of the culture, and hence are likely to transform overall cultural patterns.

The notion that cultural understandings are negotiated and transformed in the course of social struggles is particularly prominent in the

work of anthropologically minded European historians. A classic example is Carlo Ginsburg's *Night Battles* (1983). Ginsburg shows the consequences for peasant practitioners of an obscure but ancient fertility cult in remote Friuli of being investigated by the Inquisition. Over the decades of these investigations (from the 1570s to the 1640s), subtle changes developed in the cult. By the mid-seventeenth century their beliefs and rituals had assimilated many elements of the witchcraft they had learned from their persecutors. Here is a clear case of the cultural patterns of the marginal and poor being transformed by contact with the dominant culture into a distorted version of the dominant culture's ideological categories.

But dominated groups or classes are not necessarily passive in cultural struggles. William Sewell, in *Work and Revolution in France* (1980), shows how subordinate classes could appropriate notions from the dominant class and transform them into weapons to be used in struggles against that very class. Confronted by the individualism of the victorious liberal bourgeoisie after the Revolution of 1830, French workers responded by adopting the language of liberalism—above all the idea of freedom of association—transforming it into a justification for corporate trade regulation, class solidarity, and eventually socialist control over the means of production. In this case, the language of the liberal and revolutionary tradition was transformed, with momentous consequences for French and European society, by the innovative cultural practice of the working class.

Another example is William Reddy's *The Making of Market Culture* (1984). This book shows how the regime of labor relations in French textile factories was produced by a complex interaction between the divergent expectations of workers and factory owners, in the context of international market forces and a circumambient ideology of laissez-faire individualism. Reddy's study demonstrates that even the factory, the presumed epitome of economic rationality, was in fact a culturally constructed institution, a culturally constructed mode of production. Its inner workings owed as much to the shared beliefs and standards of textile worker communities as to the logic of capital or the will of the entrepreneur.

Anthropologists for their part, and under the stimulus of historians, have confronted, first, the problem of consensus in society, which is to say the variations in ideology and, second, the transformative implications of such variation. We saw this problematic already in Moffatt's study (1979). But Rosaldo's (1980) work in an isolated headhunting society shows variation and transformation from generation to generation in the meaning and specifics of this practice. Headhunting is culturally



constructed, but these constructions are transformed over time. Bourdieu (1977) and Sahlins (1985) are two anthropologists who have focused on this dynamic process whereby cultural structures constrain human action and therefore shape human history while themselves being subject to change whenever brought into action. This developing, interactive notion of praxis (Ortner, 1984) represents a major shift both in social theory and in the practice of social science. It is seen also in new theoretical work on worldview (Kearney, 1984), a mainly anthropological concept closely related to ideology.

The convergence of such empirical studies as those of Ginsburg, Sewell, Reddy, and Rosaldo with the theoretical reconceptualizations of Moffatt, Bourdieu, Sahlins, Ortner, and Kearney make the problem of cultural transformation one of the most exciting areas in contemporary social science. The already brisk pace of innovation in this area could be hastened above all by greater cross-disciplinary contact. Scholars in different academic specialties are not sufficiently aware of work in neighboring disciplines. For example, historians working on problems of cultural transformation still tend to look to earlier synchronic conceptualizations rather than to more recent dynamic models. The constitution of interdisciplinary working groups drawn from anthropology, history, sociology, and political science could significantly increase the communication between convergent developments in these different fields.

### *The Cultural Study of Modern American Society*

The advances we have so far laid out in the process of this report—our growing ability to analyze the cultural biases in politics and policymaking, in Third World unrest, in historical transformations—are in interaction with our growing ability to study culture and ideology in modern society, particularly our own.

For one thing, we have begun to think about how a common set of cultural resources can be appropriated and altered when different groups make use of them. In *Getting Saved from the Sixties*, for example, Tipton (1982) has analyzed how the youthful religious cults of the 1970s drew on central themes in American culture—utilitarian individualism, confidence in a voluntary social contract, and hope for a spontaneous cooperation among like-minded others—and reworked them to fit the distinctive social dilemmas of those in differing social locations. Thus Werner Erhard's EST blended utilitarian self-interest with the expressive hopes left from the counterculture, adapting them to a world of young single adults, trying to feel "ok" about themselves and learning to "keep agreements" both personal and professional.

Persuasively combining the study of social practices with a sophisticated structuralist approach to culture, Hervé Varenne (1977) has examined American culture not by looking at attitudes and beliefs but by looking at the ways in which Americans understand the creation and destruction of social bonds. Beginning with an analysis of the break-up of a local chapter of the Farm Bureau Federation in one small town, and moving to an analysis of how children leave home, how young people's friendship circles form, and how politics and church are arranged, Varenne shows that the Americans he studied act as if it were only through the voluntary cooperation of free, like-minded individuals that social bonds could be formed.

A related work, the much-discussed *Habits of the Heart* (Bellah et al., 1985), also deals with American culture, but it expands the theoretical reach of cultural studies in significant ways. First, the authors do not follow the traditional ethnographic approach by studying a single, self-contained community. But they also do not move to the standard sociological solution of random sampling, losing a sense of the particular contexts in which culture is shaped. Rather, they see themselves as taking cultural traditions rather than individuals as their units of analysis. Then they strategically select communities and individuals in whom those cultural traditions might be most fully explored. Thus they study local community volunteers in a New England town and a San Diego suburb to explore the varied meanings of the American tradition of civic voluntarism. They study middle class suburban couples to understand ways of thinking about love and personal relationships (see also Swidler, 1980). Furthermore, they are able to show the dynamic ways in which established cultural meanings are appropriated by different groups and put to different uses in different circumstances. Thus the therapeutic ideology of self-actualization can be used by married couples to justify communication and working things out together. For others, it justifies a sense that one can be responsible only for oneself. Finally, *Habits of the Heart* examines the ways in which, at times, a culture fails those who try to use it. Frequently, individuals attempt to express commitments that the ordinary language of their culture is inadequate to describe. Thus moments of cultural confusion, when people back themselves into corners they cannot get out of in trying to think about why they live as they do, become fruitful topics for social-scientific investigation.

Such insights, connecting particular symbolic or ideological elements to specific, differentiated parts of the social world, have produced a flowering in the study of science (Crane, 1972) and art, as well as religion. Under the rubric of the "production of culture" (Peterson, 1976), such analysts have produced a wealth of work on how particular social con-

texts actually change the possibilities and the meanings of specific items of culture. Recent work ranges from Wendy Griswold's (1981) study of the influence of American copyright law on the distinctiveness of American novels, to an important study of how the industrial concentration ratio in the popular music industry directly affected innovation and diversity in American popular music (Peterson and Berger, 1974). In a series of important papers, Paul DiMaggio (1982a, 1982b) has explored new relationships between class, status competition, and culture. He has examined how the nineteenth-century American upper class, worried about finding new ways to mark the boundaries of its class membership, constructed what we now take for granted as the difference between "high culture" and "popular culture."

An area of cultural studies that has shown dramatic progress recently has been the study of gender as a cultural and/or ideological system. Ortner and Whitehead (1981) have suggested that social scientists studying both "sex roles" and power/prestige differentials between the sexes tend to assume that gender differences are largely grounded in natural, biological differences. They argue that notions about gender differences, in our own culture as well as cross-culturally, must be treated as embedded in larger cultural patterns. Thus Kristin Luker (1984) has shown how pro- and anti-abortion views among American women are embedded in different complexes of cultural assumptions about "reality," and that these complexes in turn are grounded in different socioeconomic configurations for the two groups. By taking a cultural approach, Luker illuminates a debate that has been plagued by both emotional heat and conceptual opacity.

Added to the revival of work on particular American communities (Kornblum, 1974; Rubin, 1971; Rieder, 1985; Hannerz, 1969; Horowitz, 1983), the work on popular culture and on gender cited above signals a new era in cultural studies, marked by theoretical approaches powerful enough to deal with the complex interactions of culture and social structure in differentiated, modern societies.

### Recommendations

Ideology is a concept of enormous importance in human affairs, for ideologies are capable of both great good and enormous harm. The fact that ideologies are often implicit, on the one hand, or subject to unquestioned allegiance, on the other, makes this "vital but enigmatic" subject difficult to study. But that in no way limits the importance of this study. It makes it all the more important if our object is "human betterment" (Boulding, 1985) and a more harmonious world order.

Indeed we have shown in four areas how a cultural approach has made significant advances in our understanding of the dynamics of ideology: in politics and public policy; in the Third World; in history's understanding of ideological transformations; and in everyday life in American society. In view of the understanding offered by this recent work we believe it is time to institutionalize these efforts in two related ways. We also propose funding of one specific resource for the study of ideology and culture.

First, we recommend the establishment of *de Tocqueville Centers* in one or several American universities. Since ideological and other cultural biases and commitments are so often hard to understand by those who hold them, scholars from other cultures are often the best sources of insight into our own cultural images and assumptions. Foreign observers of American life from de Tocqueville himself to Harold Laski have given proof of that. But these have always been exceptional and individual efforts and, in recent decades, few and far between. What we propose are centers that will sponsor foreign social scientists interested in doing studies of American life and comparative research on matters of culture and ideology in consultation with American scholars. We already have a few examples of these studies. We have mentioned Varenne (1977), but we could all profit in a number of ways from a more constant input. We could gain increased understanding of the cultural assumptions in our own behavior. We would gain a better understanding of foreign perspectives upon the world and upon ourselves (Basso, 1979) and the cultural and ideological sources of these. Embedded ideologies would become more explicit and better negotiable.

Whereas there has been a unilateral tendency for studies to proceed from Euro-American centers to other parts of the world, *de Tocqueville Centers* would act to make social science inquiry more multilateral and more justly distributed. Thus we recommend that Third World scholars whose "modes of thought" significantly differ from those of the West (Karp and Bird, 1980) be especially encouraged to participate in this program. We cannot forget that a major problem in international social science in recent years has been the problem of access (Prewitt, 1984). Encouraging the access of foreign scholars to our own society—not simply as students *learning* social science but as fully enfranchised investigators *doing* social science among us—can only help, through the persuasion of reciprocity, to ease that access problem. We should also mention that this idea of a *de Tocqueville center* or centers, though enthusiastically supported by our work group, is not new to it. The Social Science Research Council has for a number of years been interested in just such a program.

Second, we recommend the provision of research centers for the ethnography of American culture, in which interdisciplinary work groups deriving from across the behavioral sciences and from the relevant humanities would be able to cooperate on common ethnographic projects. While the traditional ethnographic approach has tended to be the exclusive work of individual scholars occupied with single, and usually simple, societies, complex societies require the collaboration of a community of scholars. We need, therefore, cross-disciplinary centers where scholars from different fields can systematically explore and develop research approaches to American society. We recognize that the two centers we propose might very well function under one rubric.

Third, we wish to recommend support for more systematic tools for cultural research. One impressive example is *ARTFL* (American Research on the Treasury of the French Language), a University of Chicago based linguistic data base containing hundreds of French texts from the seventeenth through the twentieth centuries. This data base, comprised mainly of literary works but also including works of philosophy and social and political theory, makes possible the systematic study of changes in the meanings of the words and verbal expressions constituting French culture. Specifically for our area of interest we would recommend support for the creation of a compendium of key terms (concepts) relevant to ideological discourse and to other forms of articulated worldview. A literary version has already been undertaken by Williams (1976), and a more systematic effort has been made for the German language by Bruner et al. (1979). Both these efforts enable the researcher to focus quickly upon the lexical core of ideological belief and argument. This resource would, in addition, be valuable because these terms are contextualized. This resource would also be computerized. We would recommend support for such a compendium in the major European languages.

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## Religion and Political Change

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There is now a great deal of empirically grounded, theoretically significant research on religion and politics that has been developed in different disciplines. Hence, this is an appropriate time to encourage and stimulate communication and collaborative work.

Across the board in the social sciences, scholars working on widely varying cultural and religious traditions and regions display remarkable convergence in theoretical concerns. Researchers are showing a general interest in reassessing and going beyond the concepts and assumptions inherited from the classic works of Weber, Durkheim, and de Tocqueville, and much of the more recent, influential literature of the modernization school, which has built on a version of these foundations (e.g., Smith, 1982, and Vallier, 1970; for earlier reconsiderations, see Glock and Hammond, 1973).

Progress has been especially notable in the challenge to the conceptual foundations of analysis. Anthropologists, historians, political scientists, and sociologists have worked to build new theoretical insights, to identify new methods and sources of data, and to develop new tools for reassessing the historical record. A particular concern for understanding the significance of popular religion runs through much recent work, along with an interest in assessing the dynamics of its long-term links to institutions and social movements.

In theoretical terms, the equilibrium model that dominated discourse for so long has been superseded. At the same time, conventional assumptions about the relation between process and structure, the expectation of secularization as normal and inevitable, and the relative status of religion as epiphenomenal or as a primary source of action and change are being revised. These developments have also led to a new analysis of the link between levels of change, for example, between institutional transformation and patterns of everyday life.

This progress in theoretical understanding has two major sources. First, there has been a reaction to the long-held assumptions, rooted in classical liberal and Marxist thought, that make religion epiphenomenal, stressing instead more immediate and supposedly real or rational social, economic, or political forces. The theories the old assumptions inspired lacked consistency and explanatory power and faced more and more un-

resolvable anomalies. The normal progress of scientific work in these areas has spurred doubts about this body of theory and dissatisfaction with its empirical assumptions, most notably those that make religion epiphenomenal and assume an inevitable decline in its salience in public and private lives, and the related assumption that modernization is associated with an inexorable trend in the public separation of religion from politics. A second, equally important source of these shifts is the press of critical events. The vitality and clear salience of religion in Latin America, Africa, Asia, and increasingly in the United States, have underscored the inadequacy of received theory. Both sources are important: recent research is not simply a reaction to current events. Indeed, much of the work we reviewed was in progress long before the rush of public concern. The study of religion and politics thus offers a good example of conceptual reassessment and empirical change going hand in hand.

Continuing research across the social science disciplines shows that religion is in a constant, dynamic process of change. A dialectic of adaptation and resistance between religion and politics appears in all areas and traditions. Hence the equilibrium model stressing the reproduction of homeostatic balances is superseded.

Abandoning the assumption that religion is epiphenomenal also means shedding the belief that religious movements are necessarily transitional, prepolitical way stations. Much early work in the modernization school saw religious movements in this way—as irrational alternatives to politics (Bastide, 1978). This perspective distorts the understanding of such movements and hinders analysis of the sources and patterns of change. Recent research stresses instead the long-term character of religious movements and their continuing creative links to politics at all levels (Fields, 1985; Mainwaring, 1985; Comaroff, 1985; Ranger, n.d.; Scott, 1977; Bruneau, 1982; Levine, 1981, 1986).

The neo-evolutionary hypothesis linking modernization and development with increasing secularization has been definitively exploded. Instead, the relation of religion and politics is seen as a process that assumes different forms over time. This point of view makes for stronger analysis of phenomena such as fundamentalism, which can now be seen not only as reaction (which tends to explain it away) but also as a creative, dynamic movement of its own.

Recent research also advances understanding of how religion links institutions and everyday life. Religion provides material and symbolic linkages, and is itself often an object of manipulation by political institutions and the state. Much attention has been given to the capacity of religious movements to operate “above” and “below” manifest social in-

stitutions and pathways—its scope for grassroots mobilization, international migration, the formation of diasporas, etcetera (Comaroff, 1985; Gellner, 1983; Fields, 1985; Ileo, 1979; Tai, 1983; Mainwaring, 1985; Smith, 1982).

There is renewed concern with analysis of cultural questions, but without the static assumptions and qualities that marked much earlier research. Recent work stresses transformations, and consistently deemphasizes the extravagant and archaic, which for so long dominated studies in the field. There is less work on millenarian movements and "irrational" sects, and greater attention to the ways in which religion organizes everyday life and provides systematic links to larger structures of power (Bruneau, 1982; Ileo, 1979; Kselman, 1983; Davis, 1974; Scott, 1977; and especially Fields, 1985; Tai, 1983, is somewhat of an exception).

Methodological progress in the study of religion and politics has been founded above all on reassessment of historical and ethnographic methods and data. Institutions have also been subjected to more dynamic and behavioral analysis. Totally new methods and intensive development of quantitative data analysis have yielded little product.

Most theoretical development in this field has been empirically grounded. The search for *general* theories arouses little interest; where it has been undertaken, the results are meager (Martin, 1978).

### Case Studies

Bruneau (1982) has undertaken a path-breaking, multileveled study of religion and social and political change in Brazil. He has used organizational contexts that are meaningful to actors (parishes, dioceses) at many levels to trace the sources and processes of change in religion. Religious ideologies are located in specific social and institutional contexts. This study presents a rare combination of national survey research with institutional and ideological analysis.

Mainwaring (1985) also presents a multileveled study of religion, churches, and popular movements in Brazil. This work is not so innovative methodologically but is important for the reworking of ethnographic methods and specific linking of religious change to hypotheses on social movements. It shows the unique capacities of religion to legitimate and empower social movements, and demonstrates sources of its important role in resistance to authoritarianism and in the promotion of democratization in religion *and* political life.

Levine (1981) conducted a comparative study of linked transformations in religion and politics, and of their implications for future change in highly contrasting national cases. He emphasizes the need for research to assume a phenomenological perspective. This means working within the categories religion provides, not subsuming these to an externally imposed logic. The relation between ideological and institutional change is stressed, with particular emphasis on uncovering sources of change in religion and tracing how these lead to different ways of seeing, organizing, and acting in the world. Data are based primarily on systematic elite interviews and institutional analysis. The study is multileveled, with stress on interconnections.

Levine (1986) is a collection of studies focused on conflicts within both religion and politics to control and orient popular movements. These studies reconsider traditional theories and assumptions about popular religion, and explore the links of such traditions and organizations to class, and to economic and political transformations in a series of cases.

Smith (1982) presents a multileveled study of Catholicism and political change in one key case. The study is set in the theoretical context provided by the analysis of churches as complex organizations whose structural traditions shape how they interact with politics. The potential and limitations of religious involvement in politics is thus well illuminated. The potential for conflict within institutions is shown by analysis of elite data at different levels.

The study by Berryman (1984) is not scientific in form and shows little interest in social science theory. It is, rather, an instance of *engagé* scholarship. Nonetheless, it is valuable for demonstrating linkages between religion and politics in everyday life at the grassroots and institutional levels. Berryman shows how new religious ideas (such as liberation theology) were worked out in organization and action, and how the resulting organizations took on critical roles in the Central American political struggles.

Fields (1985) provides a significant reanalysis of the origins and political meaning of religious movements in colonial Central Africa, through a case study of the Watchtower movement. Fields stresses the need to go beyond the assumption that religious movements are epiphenomenal or prepolitical. Like many recent scholars, she takes a phenomenological perspective, looking at religious movements from the "inside out," searching for why converts did what they did, not simply why they did not do some things of interest to researchers. Fields reanalyzed the historical record, with particular stress on sorting out the position of converts

and new religious leaders from that of missionaries and colonial authorities—those who wrote the “official” record. Fields offers an important critique of generations of anthropological research, showing how religious organization and early anthropological discourse were bound up in the fabric of colonial domination, and hence were inevitably political.

Comaroff (1985) is a study of cultural and political transformation in Southern Africa through analysis of the Church of Zion among the Tshidi. Comaroff places the analysis of missionary religion in the context of colonial domination of economic, political, and cultural life.

Comaroff and Fields both stress not seeing religious movements as archaic, irrational, or prepolitical. They take them instead as vital movements of transformation with a logical organization, continuing links to other social spheres, and the ongoing creation of political methods and purposes. If such movements are hard to fit within conventionally political ideologies such as nationalism, the authors see this as a useful index of the validity or non-validity of nationalist perceptions. Both stress giving clear, independent, analytical weight to the links of religious movements with the politics and economics of colonial domination. This is a vital point for correction of the historical record, and furthers understanding of current and likely future changes. (Similar points on Islam are made by Gilsenan, Gellner, Fischer, and Arjomand.)

Ileto (1979) conducted a study of popular movements in the Philippines. Ileto examines their links to colonial authorities (Spanish and American) and to the local nationalist movement. He reanalyzes the historical record to show how popular movements drew on the pervasive tradition of passion plays for models of leadership and action. He demonstrates the continuing mobilizing power of such models and groups. The great value of this work is its methodological reorientation of the study of popular movements, another instance of seeing them “from the inside” and of going beyond the official written record to understand history “from the bottom up.” The point is made in more general and comparative terms by Scott (1985, 1977), who argues that religious movements commonly provide a source and basis for protest against structures of power and domination.

A valuable analysis of the origins and ramifications of millenarian movements was carried out by Tai (1983). Tai shows how such movements arose as dynamic expressions of local needs, and in the process created vigorous, effective organizational and symbolic responses, which took on key roles in the struggle for power. Tai provides a clear analysis linking ideology and organization in the creation of a religion, and establishing religion’s centrality as an organizing principle in an area of intense conflict and change. The study advances understanding beyond

the traditional assumption that millenarian movements strive simply to recreate a past golden age, but remains somewhat wedded to the view that they are a "lesser," prepolitical stage of organization and action.

A considerable body of historical scholarship has developed in recent years, reassessing the links of religion to social and political transformation in early modern Europe. This work creates new data by exploring the sources for understanding movements by nonliterate groups in the past (Davis, 1974; Ginzburg, 1980, 1985), and throws new light on the dynamics of political movements stemming from religion, above all in work on the English Puritans (Hill, 1982; Zaret, 1985; Hempton, 1985).

There is a large body of recent work on Islam, which needs to be sorted out from the poor instant scholarship that has recently flooded the bookshelves. Scholars like Gilson, Gellner, Voll, Arjomand, and Fischer provide theoretical and methodological advances. They are especially sensitive to linkages between the levels at which religion and politics interact and affect each other. Most of this work sheds assumptions of secularization, and helps to understand phenomena such as "fundamentalism" not as simple reaction but also as creative adaptation with a specific and understandable appeal to different social classes and groups.

Tipton (1982) gives us a study of moral codes of three new religious movements in the United States—Zen, Jesus people, and EST. Tipton's work reflects a renewed interest in sociological analysis of culture in the United States, and especially in exploring creative aspects of religion as a central cultural expression.

A study of the accommodation between religion and modernization is presented by Hunter (1983), with particular concern for changes in fundamentalism and evangelical groups. Hunter challenges the secularization model as classically formulated, and stresses instead a process of adaptation that changes religion as religion itself changes the culture. A similar concern for the creative process of change in religion and for its key role in linking public and private life is found in Bellah et al. (1985).

## Recommendations

1. The surge of recent innovative work on religion and politics has created a critical mass of research and researchers across the board in the social sciences. But thus far, most scholars have remained within the boundaries created by disciplines, geographical specializations, and cultural/religious traditions. For this reason, we feel that now is a particularly appropriate moment for efforts to maximize communication among

researchers while creating and extending resources for further interdisciplinary work. Such investment in new resources would have immediate and extensive payoff in the quality of future work.

2. We therefore propose a short-term five-year program that would sponsor collaborative, interdisciplinary research through a series of workshops among scholars active in the field, with workshops specifically tied to publication. We see this as a low-cost program, whose funds would promote conferences and also organize publications. The possibility of a publication series on religion and politics has been discussed informally with several presses.

3. The program could be housed in an interdisciplinary, interregional committee, located, for example, in the National Endowment for the Humanities or the Social Science Research Council. The latter has considered the creation of a committee on Islamic studies. This is too narrow—our proposal is specifically cross-cultural, interregional, and interdisciplinary.

4. Given the mass of work now available, a short-term program seems appropriate. The problem is not to create new work but to spur conceptual integration. After five years, the program could be reassessed, perhaps through reassembly of the working group, or some similar collection of scholars.

5. In addition to promoting conferences and publications, the committee we propose would also sponsor a short-term program aimed at improvement of data in three areas: (a) a survey of relevant library collections, now isolated and scattered; (b) promotion of exchange of sources and bibliographies; and (c) development of an aggregated data base on religious or religiocultural indicators. This should also include references to existing data sets and archives.

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## Social Studies of Modern Science and Technology

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For a quarter-century after World War II, American science and technology led the world, but since the early 1970s that hegemony has been increasingly challenged. Economic and scientific competition from Japan and Western Europe grow stronger every year, while the Soviet Union continues to raise technical problems for American national security. Both economic and military circumstances have thus revived the post-Sputnik concern over the strength of American science and technology, particularly with regard to the recruitment of talented people, the vitality of the research enterprise, and the nation's technological resourcefulness in both the military and the economic arenas.

At the same time, among the general public, science and technology are no longer automatically equated with progress. Instead, a cloud of distrust has settled over both. Science may once have been considered a form of objective knowledge and, as such, a reliable arbiter of disputes, but in recent decades it has increasingly been perceived as a partisan weapon, deployed primarily in the interests of those who wield it to defend deleterious effects on the environment, sanction acceleration of the arms race, or challenge the merits of democratic decision making. Such perceptions have inevitably given rise to political and legal disputes, and these disputes in turn have had an impact on public policy for research and development and therefore on the scientific and technological enterprise itself.

The design of sound policy for both the promotion and the control of science and technology is thus one of the most pressing and difficult problems confronting the United States government today. There is no overarching theory applicable to the problem, but that fact does not render us helpless. Successful policy may be formed from good judgment anchored in knowledge of how science and technology work in relation to each other and to the larger society. Such knowledge may be gained from well-considered personal experience, but even more so from historical and sociological studies, which can identify effective connections among events and forces of which the historical or contemporary actors were not aware. The ability to find these connections derives not from any special gift of intelligence but from a commitment to examine the complete record so far as it is accessible—to search diligently

among government documents, scientific publications, patent records, correspondence, biography, instrumentation, computer files, movies, newspapers, magazines, and television-news tapes.

Such studies not only reveal the sources and consequences of policy decisions in the past, they can also advantageously clarify policy alternatives and costs in the present. (The current debate about the proposed superconducting supercollider, for example, would profit greatly from a consideration by historians of the political economy of particle accelerators since the 1950s.) More generally, they go behind corporate and cultural myths to illuminate how science and technology work, and to expose the relations among scientific advance, industrial development, and the socioeconomic impact of technology. Former Secretary of Education William Bennett has declared—rightly so—that knowledge of the history of science and technology should be an essential component of education in the modern world. This knowledge is indispensable to an understanding of how the modern world came to be, the nature of the scientific and technological enterprise, and the policy decisions that it entails.

As historical and sociological studies have compellingly demonstrated, policy options arise from the fact that neither science nor technology is autonomous; both are in practice conditioned by, and subject to, the dominant forces in the wider society. Modern science and technology originated in a complicated play of forces, some of them intellectual, others social, political, and economic. While science has provoked changes in human self-perception and technological capacity, the requirements and organizations of industry, government, and philanthropy also have shaped the scientific and technological enterprise. Private and public activity has provided funds for research and training, instrumentation and information, programmatic goals and employment. Technological development itself has usually been encouraged with consideration for public purpose or private assessments of market opportunity. In the great laboratories—industrial, governmental, and academic—science and engineering have increasingly been conducted in groups organized around indispensable technologies (see, for example, Heilbron and Seidel, 1989; Koppes, 1982; Hewlett and Duncan, 1969).

The scholarship that has displayed these general patterns constitutes a solid base for deeper study and analysis of major topics in the history and sociology of modern science and technology, all of them with major implications for contemporary policy choices. These topics fall into four categories for investigation: (1) growth and vitality in science; (2) the development and role of the agricultural and biomedical sciences; (3) the

sources of technological innovation; and (4) the impact and control of science and technology.

### Growth and Vitality in Science

The most promising problem for the policymaker is how best to encourage the various fields of science to ensure their growth and vitality in ways that advance long-term national interests. To prepare informed answers, it is imperative to foster historical and sociological studies of scientific institutions, laboratories, research groups, and scientific disciplines; recruitment, training, rewards, stratification, and stultification; patronage, politics, and public policy. These studies should be pursued wherever possible and appropriate through cross-national comparisons rooted in quantitative data. Spokesmen have long argued that vitality in any branch of science depends upon the manpower and economic resources invested. However, such claims do not hold up well under historical scrutiny of a quantitative, cross-national type.

In 1975 a cross-national study (Forman, Heilbron, and Weart, 1975) of the personnel, funding, institutes, and productivity of physics in universities and other higher schools of Europe and the United States around 1900 discovered some facts which no one would have guessed—that the financial investment in academic physics, reckoned as a fraction of the national income, and that the manpower investment, reckoned as a fraction of the national population, were the same to within a few percentage points in France, Germany, Great Britain, and the United States. Productivity, however, as measured by number of papers published in the leading journals per dollar or per person, differed considerably from one country to another, from a high for Germany to a low for the United States. At the turn of the century, American physicists were claiming that poor funding accounted for their relatively low international standing. Clearly this was not the case.

These results prompted some scholars to explore the sources of scientific quality by using quantitative and cross-national methods. Contemporary analyses by the sociologist Robert Merton and his students (Cole and Cole, 1973; Storer, 1973) had already pointed to the importance of hierarchy in the scientific enterprise. Historical studies of the American physics, chemistry, and mathematics communities and of the British and American genetics communities (Kevles, 1978, 1979, 1980, 1985) indicate that quality hinges heavily on the arrangements and control of institutions, particularly societies, journals, and elite centers of research, as well as upon cultural context and scientific tradition. The

desirability of cross-national comparisons in historical analysis is of course no discovery. What this recent work demonstrates is that the force of such comparisons is considerably increased when they are anchored in quantitative data.

Quantitative studies have proved to be valuable even when applied to scientific communities in a single country. Noteworthy among recent works of this type is the quantitative data set developed by Arnold Thackray and his students for the history of chemistry in the United States since the late nineteenth century (Thackray et al., 1985) and Spencer Weart's exploration of American physics in the 1930s (1979). Quantitative data have also been deployed with useful results in studies of the social origins of American scientists (e.g., Bruce, 1972; Knapp and Goodrich, 1952) and in a major work examining the role of women in the American scientific community (Rossiter, 1982).

These recent studies have provided data concerning growth rates in their respective disciplines, rates of investment in research and training, the distribution of productivity over practitioners and institutions, and patterns of employment in academia and industry. They suggest that certain minority groups in the United States—Jewish Americans historically and Asian Americans more recently—participate in the technical enterprise to an extent greater than their weight in the population, while the converse has been the case for other minority groups and women. These studies have also confirmed the fundamental point advanced twenty-six years ago by the late Derek J. de Solla Price (Price, 1963) that a disproportionately high volume of significant science is produced by a relatively small fraction of the practitioners in a given area of work. Perhaps national strength in science is not achieved by producing an ever greater number of Ph.D.s; the marginal contribution of the  $n$ th Ph.D. in a given field diminishes rapidly as  $n$  increases, and the marginal utility of additional investment and growth in science falls off sharply beyond a certain limit. The policy implications of this fact would seem to be considerable.

Quantitative data do not exhaust the material from the past necessary for prudent planning for the future. They must be accompanied by the information drawn from the traditional documentary materials of the historian and the sociologist. Explanation of the uneven participation of women and different minority groups in American science, for example, requires attention to the cultures of the groups themselves, to their histories and opportunities, and to the policies and practices of educational and scientific institutions. Similarly, patterns of growth in the scientific disciplines vary from one to the other. While chemistry, for example, was strongly connected with industry from the late nineteenth century,

atomic physics was largely an academic endeavor before World War II, and so was molecular biology until the 1970s. To understand the patterns, each discipline must be explored in its own socioeconomic setting. Furthermore, recent sociological investigations (e.g., Latour and Woolgar, 1980; Knorr et al., 1981) have revealed that analysis of how scientific facts and ideas are brought to light and developed can profit from close scrutiny of life in the laboratory itself.

### **The Agricultural and Biomedical Sciences**

Both agriculture and medicine have an immediate relevance to social need and suggest the complexity of relationships that surround the application of natural knowledge—either in the clinic or in the farmer's fields. Innovation and adaptation in these areas are induced and articulated through a maze of research, educational, and developmental institutions. Charles Rosenberg's studies of genetics and agriculture (1976, 1977) suggest, for example, that certain institutions—in this case, America's agricultural experiment stations—mediated in a sometimes faithful and sometimes compromising way between the utilitarian demands and expectations of society and the practice of the basic biological sciences.

Like agriculture, medicine and the biomedical sciences have their mediating institutions (research institutions, medical schools, hospitals, clinics, departments of health, pharmaceutical firms). The relationship among basic science, clinical investigation, technology, and medical care is fundamental to the development and understanding of modern medicine. Before the turn of this century, increases in life expectancy were largely the indirect results of improvement in housing, diet, and other factors not specific to any particular disease. But beginning at the end of the nineteenth century, the introduction and diffusion of medical technologies, simple in retrospect but impressive and powerful at the time, began to alter patterns of morbidity and mortality (Buck and Rosenkrantz, *in progress*). Techniques associated with differential diagnosis and prevention of disease, X rays, bacteriology, and immunology were particularly important. Since the 1930s, prospects for further improvement in medical practice have been closely bound up with innovation in technology and the related basic sciences—and consequent applications to patient care and management. Scholars are ready to look further into this important collection of relationships.

Among the subjects now demanding attention in the history of the agricultural and biomedical sciences during the past century are the ways in which biochemical and biophysical knowledge and its instru-



mentation made their way into concrete clinical settings. We have in mind even such discrete and time-hallowed innovations as the X ray, antiseptic surgery, and clinical pathology. And we must know more about the historical impact of modern medical science. Contemporary criticism from every part of the political spectrum has made us increasingly aware of the costly and ambiguous relationship among technological growth, medical care, and users as well as supporters of the medical system. Historians and sociologists disillusioned by the moral and economic costs of modern medicine have tended to treat it as fundamentally a marketplace phenomenon. Yet this too is a product of the peculiar historical evolution of medicine, its intellectual and technological armamentarium, and the unmet expectations created by its record and its claims.

Contemporary concern with agricultural policy has caused a growing interest in the social context of agricultural science and its social impact. (The literature in this area has been usefully summarized by Busch and Lacy, 1983.) A long-standing scholarly absorption in plant breeding, especially hybrid corn, provides an appropriate background to contemporary debates over "biotechnology." The interactions between pure and applied research, between the public and private sectors, between laboratory findings and their potential social and economic impact, which seem so pressing today, have had a long, revealing, and often contentious history. A recent outpouring of criticism in this area not only constitutes a significant phenomenon in itself but also emphasizes the historical roots of the difficulties that lie behind it. (See, e.g., Hightower, 1973; Rossiter, 1975, 1979.) It is certain, however, that the history of agriculture in the past century is incomprehensible without an understanding of the ways in which science and technology reshaped practice. Lampard's study of the Wisconsin dairy industry provides a useful example (1963). Without an understanding of the role of chemistry and bacteriology, of the role played by the University of Wisconsin, the State Experiment Station, and extension services, no scholar could begin to explain the evolution and economic history of this important agricultural industry. Economic and agricultural history provide a natural source of data and trained scholars for future research in science and science policy.

In the biomedical sciences, a few key studies underline the potential for future research. For example, Gerald Geison's attempt (1978) to deal with the multiple factors—social, institutional, and intellectual—that account for the origins of the distinguished Cambridge University school of physiology in the late nineteenth and early twentieth centuries suggests the necessary breadth of approach demanded by a contextual ap-

proach. Recent studies of the origins of molecular genetics (Olby, 1974; Judson, 1979) indicate the need for cross-national comparison as well as the intensity of interest surrounding this area of research and development. Robert Kohler's study (1982) of medical biochemistry and its sources of support analyzes retrospectively the kind of interactions that should be considered prospectively in the next decades. And even more attention has to be paid to the ways in which technological advances are applied in clinical situations; social, institutional, and economic variables all help shape such applications. Recent surveys by Stanley J. Reiser (1978) and Audrey Davis (1981) provide useful first steps in the area of biotechnology as related to clinical medicine. Louise B. Russell's studies (1979) of the diffusion of technical innovation in the hospital illustrate a policy-oriented economist's approach to this central problem.

### Sources of Technological Innovation

No sound public policy for technology is possible without knowing to what extent technological innovation has been called forth by forces operating in the larger society. The problem may be analyzed in terms of broad categories and institutions of obvious importance—notably overall governmental policy, national security and the military, the economy and industry, social values, and the work force—and in terms of case studies of particular technological areas that cut across these categories.

Since the federal government has played so major a role in fostering innovation, a major point of inquiry must be the shaping of federal technology policy itself since World War II, including how professional associations and business groups have combined with government agencies to frame it. The military has been responsible for stimulating a great deal of research and development in the United States, especially since World War II. Thus it is essential to explore the broad range of activities through which the armed forces have promoted, coordinated, and directed technological change and have thereby (sometimes directly and sometimes indirectly) affected the course of modern industry. Particularly important here is the vexing question of spinoffs from military to industrial applications and how they affect economic growth, labor-management relations, and the structure of modern industry.

A large fraction of research and development (R&D) has been produced by a relatively small number of corporations pursuing nonmilitary markets. By a complex process involving numerous stages from basic research to applied research, pilot plants, and intensive coordination with corporate marketing departments, new materials, devices, and pro-

cesses come to find a place within the market economy. This process is among the most important but least understood items on the agenda of the history of science and technology, business history, and economic history. The modern pattern of R&D dates from the late nineteenth century and expresses a long-standing interconnection between the communities of pure and applied science (Galambos, 1979; Reingold, 1972). It also lies at the heart of our ability as a nation to compete successfully in world markets. Important entry for its analysis is to be gained from scholarly investigation of the processes by which new products come to market; industrial involvement in R&D and industrial connections in the R&D area to different federal agencies; industry's impact on federal R&D policy; the relationship of labor to the environment in which new technologies are introduced in the workplace. Equally important would be studies of the evolution of technological regions, for example, Route 128 around Boston. Here one needs to examine, among other things, the shaping of industrial/academic relationships; recruitment of technically trained manpower; and the flow of knowledge between profit-making and non-profit-making institutions.

An exemplar of what needs to be done is a history of the exploration, exploitation, and regulation of the radiofrequency spectrum. In the technological history of the twentieth century, the exploitation of the radio spectrum should occupy the same central position as the frontier movement did in the historiography of the nineteenth century. The spectrum is an indispensable resource, and one that cannot easily be used up. It would be advantageous to know how the spectrum has been allocated among civilian and military users and among different commercial users, and the relationship between the regulatory environment and technological development.

The history of technology, long absorbed with the nuts-and-bolts evolution of particular technological forms, has more and more been turning to the sorts of issues outlined above. An important school, which deals with technology as an expanding form of knowledge, is distinguished by its emphasis on the technical, managerial, and epistemological aspects of technology. Works include treatments of innovation as well as the emergence of industrial systems. Particularly encouraging in some of the newer work is its attentiveness to technological development in an international and cultural context. Notable are David J. Jeremy's study of the transfer of technology (1981); David P. Billington's investigations of art and engineering (1983); Edward Constant's approach to technology as knowledge (1980), and Edwin T. Layton's research on the historical relations of science and technology (1971a, 1971b). Thomas P. Hughes's remarkable study of the development of electric light and power in the

United States, Britain, and Germany (1983) is the first book to deal with the subject across national lines and to treat the invention and evolution of integrated electrical power systems in their interactive technological, financial, organizational, market, and distributional facets.

A different, and especially stimulating, school has approached the history of technology as a social product largely shaped by the cultural setting in which it originates and accordingly reflective of the values and ideologies of its creators. Examples of this approach are the writings of David Noble (1984), Anthony Wallace (1978, 1980), Edwin Layton (1971a, 1971b, 1979), and Merritt Roe Smith (1977). Studies of this genre treat technological change in the workplace as well as its impact on industrial communities, geographic regions, and national cultures. They also confront the relationships of technology to class interest, economic power, and social change.

A start has been made recently in the study of the military's historic role in R&D and technological innovation (Smith, 1985). In recent years, scholars have begun to explore the role of industrial firms in the same area. Many are influenced by the seminal work of business historian Alfred D. Chandler, Jr. (1962, 1977), of economist Nathan Rosenberg (1972, 1982), and of several historians of technology such as Hughes. In addition, some companies that pioneered in R&D, notably American Telephone and Telegraph, General Electric, and Du Pont, have created archives and hired company historians or provided funds for academic research projects to deal with the evolution of their efforts in R&D.

### **The Impact and Control of Science and Technology**

Many disputes have arisen in recent years over research priorities, toxic waste, teaching of evolution versus creationism, and regulation of new technologies (pesticides, food additives, chemicals in the workplace). The disputes have arisen largely from discontent with the impact of technology on the quality of life in such areas as nuclear power, toxic waste disposal, air and water pollution, and, of course, the arms race. However, analysis suggests that these disputes reflect a larger range of concerns, including the implications of science for religious values, the distribution of costs and benefits, and, above all, the implications for social and political choice (Nelkin, 1984a, 1984b).

An important and fruitful area is the history of the impact of technology upon the environment. Model studies of this type have been done

in the area of water resources (Baumann, 1969; Blake, 1956; Tarr et al., 1984). They demonstrate that large quantities of unused data are available for environmental studies and that these data can be exploited for "retrospective technology assessment," that is, the examination of the social and political consequences, anticipated and unanticipated, of past technological developments. Similar studies would be in order for nuclear power, toxic waste disposal, and air and water pollution.

The dissatisfaction with scientific/technological imperatives suggests that much needs to be learned about how research priorities are established; the participants in the decisions; the criteria used to justify decisions; the alternative interests or priorities neglected as priorities are established. On the side of the public, a variety of questions needs exploration: What does the public know and understand about science? How do people deal with complexity and uncertainty? How do people find technical information and interpret it? What shapes the social perceptions that ultimately bear on social policy? What problems of access to technical information and expertise are faced by workers and others concerned about health?

It would aid debate over technical issues if public disputes concerning them were monitored with the aim of understanding the nature of public concerns (the values at stake, the issues of equity, the moral and religious concerns); the development and political expression of social movements organized around opposition to science and technology; the role of scientific experts in controversies and the complex social construction of evidence in disputes; and the nature of dispute resolution by the courts, legislatures, and regulatory agencies in different political cultures.

Risk assessment has burgeoned as a research area, but mostly in a technocratic form. A great deal more work should be done on the distribution of risks; the way in which cultural, ideological, economic, and political predilections shape perceptions of and responses to risk; and the social construction of facts and theories that preclude the resolution of risk disputes.

Questions of the social responsibility and public accountability of scientists persist. Recently we have seen an increased tendency toward external regulation of the scientific community through review boards, budgetary control, and legal and legislative measures. How are these implemented, and what is their effect? What are the implications for the traditional autonomy of science and for scientific research? What are the consequences for the recruitment and careers of scientists? How are scientists and engineers to be made sensitive to questions of social responsibility and accountability?

Profound institutional changes seem to be occurring in the relationship between academic and industrial science as venture-capital firms as well as established corporations seek more efficient and competitive technological development, drawing upon highly advanced work in such areas of science as molecular biology, biochemistry, and computer-related fields. The changes are being complicated by the major reentry of the American military into the support of some of the same fields in science and engineering. Sociologists have begun to examine these changes (Nelkin, 1984a, 1984b; Kenney, 1986), and it has been argued (e.g., David Dickson, 1984) that these trends may make it more difficult for members of the lay public to influence the formation of research policies. Many observers have also noted that these trends may also threaten the vitality of academic science, not least because of the restrictions of secrecy that may be imposed on research projects for reasons either of proprietary interest or of national security. These contemporary trends deserve ongoing scholarly attention, and so do their historical backgrounds.

### Needs for Investigation

An essential requirement of the study of science and technology here emphasized is the accumulation of quantitative historical data bases for important branches of the scientific and technical communities. Such data bases should include time series concerning manpower, funding, training, employment, and publications. Unfortunately, the data bases exist only in fragmentary form or not at all for periods before 1945. Efforts are under way at a few centers—Berkeley for physics, the University of Pennsylvania for chemistry—to develop appropriate quantitative data sets. These efforts need to be assisted, and a national program ought to be mounted to ensure that all important areas of science are covered.

Equally deserving of national attention is the formation of a biographical data bank in the area of technological innovation, which would collect biographical data on twentieth-century engineers and innovators. Also important would be the acquisition and collection of patent statistics. In the area of the social impact of science and technology, it would be useful to have a centralized repository of information on legal cases and public disputes involving science and technology, on mechanisms for dispute resolution, on new institutional arrangements in academia, and on measures affecting secrecy and the public's right to know.

We heartily applaud and urge the opening of corporate archives concerning R&D as an area essential to the study of modern science and

technology, historical or otherwise. At the same time, we stress the importance of maintaining academic freedom in the rules governing the use of these archives.

The volume of records generated by the activities of twentieth-century science and technology is enormous. We urge continuation and expansion of efforts—for example, at the American Institute of Physics, M.I.T., the Hagley Museum and Library, the Center for the History of Chemistry, and the Bancroft Library—aimed at framing guidelines for the retention of these records. It is necessary to sensitize professional organizations as well as corporations to the importance of establishing sound archival policies. It is also essential that federal agencies be guided to develop better record retention procedures than those they now follow in technical subjects. A subject of new importance for all agencies is the formulation of policies for the retention of records of a machine-readable nature, which do not necessarily become records in the ordinary sense because they are stored in a central memory, read on a screen, and then, very likely, erased.

One of the most important types of "record" for studies of the development of science and technology is the artifact, whether experimental or usable in nature. Some of these artifacts are now housed in museums and private collections throughout the United States. The Smithsonian Institution is currently developing a computerized catalogue system for artifacts. With appropriate planning and coordination, such a system could be the foundation for a national catalogue of artifacts. Because of the centrality of artifacts to the understanding of design and production processes and because it is very difficult to publish or replicate artifacts, historians need a catalogue of items readily available for historical study. Such a catalogue would also help scientists and historians to monitor the condition and preservation needs of such materials.

Historians and sociologists of modern science and technology need to approach their subject in a manner different from what is now customary. The basic reason is that the object of research—the recent past of science and technology—has been created by teams. Galileo and his historians can face one another one-on-one; the lone historian of the Brookhaven National Laboratory or of genetic engineering faces a hopeless task. What scientists have created in teams historians and sociologists must analyze in teams. An individual has neither the expertise to control material generated by interdisciplinary groups of scientists, engineers, technicians, and managers nor the time to survey the mass of extant relevant information. (It is estimated, for example, that the available documentation for a study of the Lawrence Laboratory now under way at Berkeley probably exceeds the total of all extant archival material relating to the scientific revolution of the seventeenth century.) In short,

the study of modern science and technology requires long-term collaborative effort.

Historians and sociologists of recent science and technology could well organize in task forces or teams just as scientists do. The teams should be centered on strong existing academic, archival, or museum programs in the relevant disciplines. Graduate students might participate in research projects at a level appropriate to their training, and to gain experience in interdisciplinary team research. The centers should have rich library resources and good computing facilities.

Again following scientists' practice, projects might engage postdoctoral fellows, who are now much underutilized in our profession. Although it might be desirable to break a large project into a few integrated smaller ones, we insist that to succeed the parent project must be integrated; mere juxtaposition of separate undertakings, as in work published together in conference proceedings, would not achieve the ends we have in mind.

Provision should be made for distributing project results where people other than fellow experts are likely to see them: in journals of public affairs, programs for public television, material for secondary schools, as well as in scholarly books and articles. Research teams should include a person able to help in outreach programs. The overall magnitude of the cooperative historical and sociological research into the affairs of modern science and technology would be small compared with that of the technical enterprise itself, but the potential gain in understanding would be considerable.

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## Social Knowledge- Producing Institutions

### Working Group

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While other working groups considered research by social scientists, ours considered research *about* social scientists as well as by them. We inquired into the present state and future prospects of research about the institutions through which social knowledge is produced and disseminated—institutions in which all of us are participant observers as well as scholarly practitioners. Our deliberations also encompassed research on the relations between the social sciences and other parts of society, especially government.

Our working group brought together scholars with contrasting outlooks on the nature and significance of the social sciences and benefited from their varied substantive interests and methodological skills. Martin Bulmer is an applied sociologist who has done work on the history of the social sciences in America in the 1920s, the utilization of social science in public policymaking, and the methodology of social policy research. Thomas Juster is an economist who heads an empirical research institute at the University of Michigan; he periodically reflects on the place of social research in society. Donald McCloskey is an economist and economic historian, a specialist on British agricultural and industrial history; he has also written on economic theory and, most recently, on literary approaches to economic science. Dorothy Ross is an intellectual historian who writes on the history of the behavioral and social sciences in America. Theda Skocpol is a political sociologist who is doing comparative and historical work on the development of social knowledge in relation to the interventions of welfare states. Arnold Thackray was originally trained as a physical scientist and has published a variety of studies in the history and sociology of scientific knowledge, most recently a study of chemistry in America from 1876 to 1976. Carol Hirschon Weiss is a sociologist at the Harvard Graduate School of Education; she has done a series of studies on the influence of social science on executive and legislative policymaking at federal, state, and local levels, and recently completed a study on the reporting of social science in the media.

The working group took full advantage of these diverse experiences and orientations, while aiming at a coherent final report. Members produced individual memoranda prior to a single all-day meeting for collective discussion. During and after that meeting, members contributed to

drafting the report, which was then pulled together by the chair and circulated for revisions before the final version was prepared.

In this report, we assess current research and recommend resources that could further the most promising lines of existing research and stimulate desirable new endeavors. Overall, we believe that research priorities fall into two major substantive areas: studies of the relations between social knowledge and public policymaking; and studies of the cultural orientations and social structures of the social sciences, including attention to the intended and unintended effects of funding patterns. The following two sections present developments in these areas and outline opportunities for future progress. The final section justifies our recommendations for the future.

### **Studies of Social Knowledge and Public Policymaking**

In the eyes of practitioners and the educated public, the social sciences have always been partially justified by their expected contributions to social progress. Many of their contributions, moreover, are supposed to come through uses of social knowledge by policymakers, those who direct the resources of large-scale, authoritative organizations, especially subnational and national governments. A considerable body of research has accumulated exploring the ways in which social knowledge actually has figured in public policymaking (see the helpful bibliography in Weiss, 1979a). Recent developments in the literature point especially to the diffuse, indirect, and subtle effects of social knowledge on policy formation. These developments highlight the need for further investigations of, on the one hand, broad cultural and institutional influences and, on the other, the diverse paths by which social science actually enters public discourse and policy debates. A rich conceptual basis has already been achieved to inform such research, yet important challenges remain to be faced in the organization and execution of appropriate studies.

#### ***Moving Beyond the Instrumental-Decisional Model***

Along with the efflorescence of "evaluation research" that followed in the wake of the Great Society programs of the 1960s, there emerged a model of how political decision makers and applied social researchers might work together for societal betterment. Martin Rein (1983) has described this "instrumental-decisional" model, and one can see its as-

sumptions and its basic methodological implications for applied social scientists ably spelled out in, for example, Rossi and Berk's "The Scope of Evaluation Activities in the United States" (1983).

The instrumental-decisional model has not proved adequate as a mapping of reality, yet it has inspired telling empirical studies leading to important conceptual reorientations. There have been some systematic surveys of the attitudes of policymakers toward social scientific research and of their actual experiences in using it. These include *The Use of Social Science Knowledge in Policy Decisions at the National Level* (1975) by Nathan Caplan, A. Morrison, and R. J. Stambaugh; *Social Science Research and Decision-Making* (1980) by Carol Weiss and Michael Bucuvalas; and "Policymakers' Use of Social Science Knowledge: Symbolic or Instrumental?" (1977) by Karen Knorr. There have also been studies of the contributions of social research and researchers to the making of particular policies (e.g., Aaron, 1978; Hayes, 1982; Rein and Hecl, 1980; OECD, 1980; and Rich, 1981b). Although many interesting points about the policy applications of the social sciences emerge, these studies, taken as a whole, have *not* found that empirical research findings are routinely adapted to the straightforward solution of well-defined policy problems, as the instrumental-decisional model implies they should be.

On the contrary, these investigations have led toward the formulation of a new frame of reference to guide research on more complex and indirect effects of social knowledge on public policymaking, a frame of reference to which many scholars have contributed (see especially Bulmer, 1982; Holzner, Knorr, and Strasser, 1983; Husen and Kogan, 1984; Kallen et al., 1982; Rein, 1983; Rich, 1981a; Tarschys, 1983; Weiss, 1979b, 1979c, 1980, 1982; and Weiss and Bucuvalas, 1980). As Weiss (1982, pp. 620–21) has argued,

Rarely does research supply an "answer" that policy actors employ to solve a policy problem. Rather, research provides a background of data, empirical generalizations, and ideas that affect the way that policy makers think about problems. It influences their conceptualization of the issues with which they deal; affects those facets of the issue they consider inevitable and unchangeable and those they consider amenable to policy action; widens the range of options they consider; and challenges some taken-for-granted assumptions about appropriate goals and appropriate activities.

The paths through which social knowledge can influence the decisions of policymakers range, for example, from the generalized absorption of ideas and assumptions for public discourse, through the intercession of staff members trained to draw upon social research, to the direct com-



missioning of studies to evaluate policy options or the effects of an established policy. The paths operative in any given setting may depend as much on the workings of governmental organizations and the political process as on the quality or availability of logically appropriate social knowledge.

Recently, some political scientists have begun to reconceptualize the determinants of policy outcomes in ways that open the door to more systematic analysis of the various effects of social knowledge. For a long time, interest groups or parties or agencies were thought to determine policy outcomes simply in terms of their conflicting goals and the resources each could bring to bear in competition or coalition with others. Lately, however, more cognitive and non-zero-sum processes have been recognized as crucial to the political process. Some political scientists now systematically investigate how "political agendas" are set (e.g., Banting, 1979; Kingdon, 1984; and Walker, 1981). Others probe how "political learning" over time encourages administrators and others to rework existing governmental measures, correcting for the widely perceived shortcomings or mistakes of previous policies (Heclo, 1974). While such studies have not investigated the input of social science as such in any depth, their emphases on "agenda setting" and, as Hugh Heclo nicely puts it, on the role of "puzzling" as well as "powering" in politics naturally direct attention to informational and conceptual inputs to politics, including those that may come directly or indirectly from the social sciences.

Of course, not only policymakers themselves are affected by social research; so are politically relevant publics. This happens when concepts or findings from the social sciences are conveyed—accurately or with distortions—to broad audiences. For example, both policymakers and their political supporters and opponents may be more affected by social knowledge as filtered through the media than by data or conclusions directly from research itself. Moreover, the teaching of the social sciences to successively larger cohorts of middle-class Americans may have profoundly affected public policymaking throughout the twentieth century by shaping educated people's sense of what social problems could be addressed by governmental action, and how.

Basic cultural assumptions, as well as currently taken-for-granted beliefs that were once explicit social choices, pervade social research and public policy debates alike. Some fascinating studies have explored such matters. Judith Innes De Neufville's *Social Indicators and Public Policy* (1975) reveals how seemingly objective and neutral statistics embody policy-relevant social assumptions. Constance Perin's *Everything in Its Place: Social Order and Land Use in America* (1977) examines the

worldviews that underlie politics and planning for land use. And Joseph Gusfield's *The Culture of Public Problems: Drinking-Driving and the Symbolic Order* (1981) reveals the cultural and ideological beliefs that imbue all aspects of social research, legislation, and criminal proceedings about drunken driving in the United States, with some telling contrasts to other nations.

For the future, we badly need investigations that trace the full array of interconnections between social knowledge and policymaking over long periods of time. Such investigations should attend carefully to the concrete processes by which social knowledge enters into the framing of issues and the making of particular policies; moreover, policies should be traced through their actual implementation and through the subsequent assessments of policymakers and political publics about the contributions supposedly made by social knowledge. Along with studies on many of the topics mentioned above, the following are examples of studies that could be pursued.

1. Long-term investigations of the contributions of social knowledge to the ways in which particular state governments or the U.S. federal government have handled—or mishandled—selected policy areas, such as education, taxation, the environment, or income supports for the poor, sick, unemployed, and elderly. The idea would be to discover and weigh the direct and indirect contributions of social science knowledge in relation to other determinants of policy debates and outcomes. In the process, attention should be paid to concepts and findings that were potentially available but did not actually influence policymaking.

2. Long-term investigations of influences from social science, with a focus not on particular policies but on major societal phenomena, such as the rise of selection by merit as a principle of organizational design and social mobility, or shifting patterns of race relations in the United States. These major societal phenomena have, of course, been objects of government policymaking, but their roots and significance go beyond that. In what ways, exactly, have the theories and research of social scientists contributed to cultural, social-structural, and policy changes in these areas? Prior scholarship has certainly addressed these topics, but we lack full and analytically precise overviews of the roles of the social sciences as such. And such overviews, if they could be achieved, would have seminal influence for the sociology and politics of social knowledge.

3. A comprehensive overview of the actual and perceived contributions of the full range of the social sciences during the national emergency of World War II. What did social scientists and their theories and data accomplish? How clearly were their successes and failures appreciated during the war, as well as afterward, at a watershed point for the institutionalization of a federal commitment to the funding and applica-

tion of scientific research (see Miller, 1982; and Klausner and Lidz, 1986)? In particular, it would be useful to compare the experience and fate of the social and natural sciences in the United States; and useful cross-national comparisons to Britain and elsewhere could also be made.

### *Toward Comparisons Across Nations*

There is a pressing need for more cross-national and comparative-historical research into social knowledge and public policymaking. Comparative studies of the impact of social knowledge on policy are essential if we are to understand what are relatively general conditions of knowledge use in government—holding good, perhaps, across all modern Western democracies—and what are the conditions specific to a particular nation and period, such as the United States since World War II. On a nation-by-nation basis, patterns of mobilizing and applying social knowledge have been described and analyzed in a preliminary way, yet systematic cross-national comparisons are still few and far between. Nevertheless, sufficient beginnings have been made to suggest the likely fruitfulness of further work. Martin Bulmer (1983b) and Hugh Heelo (1974) have found that governmental commissions differ systematically in their structures and functions and thus mobilize social research to different effects in Britain, Sweden, and the United States. Margaret Weir and Theda Skocpol (1985) have analyzed economists' relations to the national state in the same three nations in order to help explain the origins and contents of "Keynesian" policies of macroeconomic management. Bulmer (1983a) and Walter Korpi (1980) have offered hypotheses about interrelations between governmental approaches to poverty and the conceptualization of social research on poverty issues in the United States as contrasted to Western European nations.

Despite these beginnings, most research remains focused on single nations with occasional gestures toward describing patterns from other nations. Much more comparative work needs to be done on a full range of research topics from the most macro-historical to the micro-decisional. We need to examine how national states have influenced the emergence and organization of the research-oriented social sciences (an issue sketched in Skocpol, 1987), and how the policy choices of governments have affected the research agendas of the social sciences. In turn, we need to compare the ways in which the conceptual and factual dimensions of social knowledge have influenced public policymaking in various nations. It may turn out that policymakers in some nations readily appropriate the data and specialized findings of certain areas of the social sciences, while policymakers in other nations are influenced only indirectly, say, by findings highlighted in the public media. Compari-

sons could help us to pinpoint such variations and to formulate hypotheses to explain them.

What is more, research on various nations can go beyond juxtapositions of nations as separate systems and take account of the transnational linkages through which ideas have diffused or been deliberately transplanted from one nation to others, especially for use by governments in their pursuit of internationally significant goals. Processes of imitation or reactive adaptation to internationally prestigious ideas—for example, economic theories ranging from mercantilism, to “free trade,” to Keynesianism, to various developmentalist models—have not only tied Western intellectuals and policymakers together. They have also linked Western policy intellectuals to research facilities and governmental planning agencies in the nations of the Third World, helping to shape many aspects of the strategies for development followed by these countries.

Further rationales and agendas for comparative research could be indicated, but the basic point is straightforward: Few of the most important questions about the interrelations of social knowledge and public policymaking can even be properly framed—let alone adequately investigated in a systematic way—without carefully designed cross-national studies carried through by analytically oriented comparative scholars or by multinational teams. Such studies promise to enrich our understandings of national singularities, of cross-national regularities, and of the processes that link bearers of social knowledge and policymakers across national boundaries as well as within them.

### *Research on Intermediary Institutions*

In the United States especially, such nongovernmental organizations as think tanks and policy research institutes, along with some university-based research centers, self-consciously position themselves between basic research and government (see Critchlow, unpublished). For example, the Brookings Institution plays an important part in the development of policy. Members of Brookings stay in close touch with congressional staff, executive-branch staff, White House staff, interest groups, and other scholars; thus the Brookings people sustain policy-relevant discourse that draws self-consciously on social science research. The American Enterprise Institute and the Heritage Foundation pursue similar activities from different perspectives. To some degree, American grant-making foundations also function as intermediary institutions, especially when they shape program priorities to their sense of the society's needs for improved public policies. In addition, some foundations (like the Guttmacher Institute) make special efforts to disseminate research results in usable forms to officials and politicians.

Another important set of intermediary bodies is the offices of research and analysis within the federal agencies that do their own research and draw on the work of others in proposing and modifying policies. On a more temporary basis, governments may set up commissions to marshal research in relation to particular problems on the public agenda. And they may contract with enterprises specializing in "evaluation research" to obtain studies of public programs and policy options.

Students of the social sciences and public policymaking have done some careful work on all of the aforementioned types of intermediary institutions (see Aaron, 1978; Biderman and Sharp, 1972; Cannell and Kahn, 1984; Critchlow, 1985; Karl, 1969; Karl and Katz, 1981; Komarovsky, 1975; Meltsner, 1976; and Orlans, 1973). Especially for those who build on these studies in the future, perhaps the key challenge is to develop better ways of assessing the actual contributions of intermediary institutions to the development and application of social knowledge. As long as research remains devoted to case studies of single organizations, there is an all-too-natural tendency to accept each foundation's or agency's subjective reading of its own role and impact. While such evidence should not be ignored, entire arrays of often competing organizations need to be examined, and ways must be found to assess their collective and individual impact on well-defined policy areas.

Finally, aside from intermediary organizations, it may be useful to direct more research attention to an emerging "intermediary profession"—that of public policy analyst—and to the techniques—such as cost-benefit analysis—that are routinely taught in public policy programs as ideally neutral, scientific, and apolitical methods for solving problems. Why have public policy programs flourished so suddenly, and especially in the United States? What impact do these programs and their techniques have on the research agendas of the academic social sciences, and on the workings of the political process? Among others, Nathaniel Leff of Columbia University is doing work on these issues, concentrating on the impact of cost-benefit accounting techniques in the World Bank and other policymaking bodies.

### **Studies of the Cultural Orientations and Social Structures of the Modern Social Sciences**

Important as it may be to study the applications (and nonapplications) of social knowledge to public policymaking, even more fundamental questions need repeatedly to be posed and answered about what we social scientists are up to in our own right. One may wonder how well we can

be trusted to do this job without special pleading or override idealizing our own enterprises. Yet the social sciences have social structures and cultures just like the rest of society; thus we fall in our own intellectual jurisdiction, so to speak. As brief glimpses at relevant literature are about to show, social scientists are fortunate to have small but growing numbers of historians of science, intellectual historians, and other humanists also interested in studying our past and present folkways, perhaps with more distance from the normative content of those folkways than we ourselves can—or should—muster.

### *The Emergence of Social Science Professions*

In recent years, scholars in the humanities and the social sciences have produced excellent studies of the emergence and institutionalization of modern social science disciplines. Many of the best studies have been sociological in the generic sense of paying attention to all relevant social structures and encompassing connections among institutions and categories of people that transcend intellectual biographies of great persons or accounts of single universities or professional associations. And all of them have been historical in the sense of tracing processes of change over time and examining alternative possibilities, including those eventually bypassed and forgotten.

The structure of national university systems, and the changing organization of industrial economies and of governments at levels from the national to the local—these features of societal context figure in the best available recent studies of the emergence and development of the modern social sciences in the United States, Britain, France, and beyond. Conceptually, reinterpretations of “professionalism” are at the center of many of these studies. They reveal the processes that forged new career patterns and new definitions of properly “objective” conduct for producers and disseminators of modern social knowledge, and they show that the processes and patterns of professionalization varied considerably across the major Western nations.

Excellent examples include Thomas Haskell's *The Emergence of Professional Social Science: The American Social Science Association and the Nineteenth-Century Crisis of Authority* (1977), Mary Furner's *Advocacy and Objectivity: A Crisis in the Professionalization of American Social Science, 1865–1905* (1975), and Robert L. Church, “Economists as Experts: The Rise of an Academic Profession in the United States, 1870–1920” (1974). From such studies (see also Ross, 1979) one can see why the modern United States has become the preeminent home of specialized, university-based academic disciplines, whose lead-

ing practitioners understand themselves to be doing basic research that is at once "nonpolitical" and in "the public interest."

In contrast, somewhat different organizational configurations and substantive orientations have characterized groups of social knowledge-producers in other nations, as exemplified by Clark's (1973) discussion of the close involvement of French social science professions with the state, and by Abrams's (1968) and Bulmer's (1978, 1985) dissections of the complex interplay of pragmatically oriented empirical research with the reformism and piecemeal social intervention characteristic of the British state in the nineteenth and twentieth centuries. Indeed, more systematic *comparative* studies, going beyond histories of developments in single nations, would surely lead to important new insights about the ways in which the various structures and activities of national states have historically helped to shape the disciplinary organization and the substantive research agendas of the social sciences.

### *Cultural Authority and Professional Discourse*

Precisely because the preeminent authority of science is now so taken for granted, it is important to understand its historical roots and cultural mainsprings (see Rosenberg, 1976, and Haskell, 1984, for works that pose fundamental issues). Currently, some scholars are probing the meanings and authority of "science" specifically in the realm of social inquiry. David Hollinger (1984) is exploring how John Stuart Mill and his Victorian colleagues developed a view of science as a moral discipline that could take the place of religion in the modern world; this view was then propagated and accepted in America, including in the social sciences. Dorothy Ross (1984) is also working on a book that will illuminate the cultural underpinnings of American social science. She is probing the reasons why, during the early twentieth century, the most influential American social scientists rejected the full implications of historicism and instead modeled their disciplines on a positivist understanding of natural science. Ross argues that the roots of this choice lay in the profoundly ahistorical conception of American exceptionalism that pervaded American culture, and with it, American social science.

Recently, historical interpretations such as these have been supplemented by the use of insights adapted from cultural anthropology and literary criticism to study the meaning systems invoked by contemporary social scientists in their routine "discourse," that is, in their writings and their discussions with one another. Thus, Donald McCloskey (1985) has dissected *The Rhetoric of Economics* to reveal the disjunctions between official and actual discourse and to plead for an acceptance

of the inherently "metaphorical" nature of much economic thought. Renato Rosaldo (forthcoming) and others (e.g., Clifford, 1983; Marcus and Cushman, 1982) have examined the discourse of anthropologists to see what it can tell them about the nature of anthropological knowledge and the ways in which anthropologists convince one another and broader audiences that they have arrived at truthful insights about other peoples. This newer research on the discourse of anthropologists joins and complements an older tradition of historical reflection on the ideas and practices of anthropologists, a tradition especially embodied in the work of George Stocking (1968, 1983) and others at the University of Chicago.

Historical and cultural studies of the social sciences along the lines just surveyed offer critical perspectives on the fundamental premises and institutions. Such studies can demonstrate the existence of structural biases that are invisible to social scientists themselves. They can help to restore taken-for-granted truisms to the uncertainty and complexity of the social choices that they historically were, and thus suggest the existence of alternative possible meanings and institutional arrangements for the social sciences in the present and the future. Such studies reopen thinking not only about the strategies social scientists have adopted to achieve useful knowledge but also about the criteria they and their societal audiences have chosen to judge knowledge useful.

In sum, historical and cultural studies of the social sciences serve a critical self-reflexive function for those of us who work as producers of social knowledge, and we believe that the training of social scientists ought routinely to familiarize them with the latest debates in these areas. Nurturing more of this kind of scholarship, even though much of it will inevitably be done by historians or by nonmainstream social scientists, ought to be an accepted priority of the social sciences, for the sake of their own self-understanding. Beyond this, ways need to be found to link the insights offered by such "interpretive" scholarship with the results of more orthodox social-scientific analyses of the contemporary social sciences and of the relations between social knowledge and public policymaking.

Finally, new historical and cultural studies of the social sciences will open the door to more sophisticated future comparisons of the natural sciences and the social sciences. Perhaps the most widely influential study in the contemporary history of science has been Thomas Kuhn's *The Structure of Scientific Revolutions* (1970), which invoked a comparison of natural and social sciences. We urgently need more of such comparative work to improve our understanding not only of the social sciences but also of the natural sciences as disciplinary cultures and social enterprises.



### *The Sociology and Politics of the Research System*

Our own discussions, along with some of the responses to the Committee on Basic Research's original request for suggestions of important research topics, point to the need for further investigations of the sociology and politics of the research system. Funding patterns and incentive structures are areas that need attention, with an eye to possibilities for reorganizations in the future if ways can be discovered to improve the inner workings of the social sciences.

Research attention needs to be paid to the consequences of funding policies for the development of the social sciences—both their substantive orientations and their methodological styles. This has both a historical and a contemporary aspect. Historically, research has already demonstrated the formative role in several U.S. disciplines played by private foundations, which were the principal funders of research in the 1920s and 1930s (see Bulmer, 1984; Bulmer and Bulmer, 1981; Church, 1974; Grossman, 1982; Karl and Katz, 1981; and Robinson, 1983). Nevertheless, to get at the actual overall impact of the activities of the foundations, further investigations are called for, some macroscopic and others focused on particular research areas. What expectations did the foundations have about the practical utility of research areas and methods they favored, and what were the results in specific areas such as early studies of voting and civic participation; studies of the social determinants of crime and delinquency; the rise of macroeconomics and the development of adequate information bases for public policymaking; and the "social indicators" movement? Moreover, to what extent were the developing social sciences as a whole really shaped by foundation funding? In what ways were the foundations in the lead, and in what ways were they responding to trends already well under way in the universities or elsewhere? Were there important theoretical or methodological developments that proceeded quite independently of input from the foundations?

For the period since World War II, all of these questions can be asked again, but the scope of investigation must be broadened to include the predominance within the overall funding system of the federal government. The joint impact of the National Science Foundation (see Riecken, 1983), the National Institutes of Health, and the various mission agencies that fund research is vital to understand. Again, both the overall array of research funding and topically defined areas (such as research on the workings of labor markets or research relevant to mental health) should be examined, so that we can move toward a better understanding of the impact of federal funding on the substance and meth-

odological styles of research. Which areas have been favored at different times, why, and to what effect? The organization of research also affects the kinds of knowledge produced (as Juster, 1970, suggests). By deliberate policy or in effect, what kinds of projects have been favored in what sorts of settings (for example, individual research efforts in university departments as opposed to collective projects in research institute settings)?

The most basic queries about the effects of funding patterns probably would have to be approached through cross-national comparisons and comparisons across major time periods. What determines the willingness of funders to give to the social sciences, and how are the agendas of private and public funders set, either pluralistically or according to uniform fashions? Is it healthy (in specifiable ways) for the social sciences to have truly pluralistic sources of funding by fully competing private and public agencies? What difference does it make for the intellectual and methodological progress of social science to have much of the available research funding at a given time tied to overt political priorities (as some nations other than the United States do)?

Finally, we would like to see investigations of incentive structures and career patterns in the social sciences, coupled with assessments of their effects not simply for individuals in the aggregate but for the development of the social sciences as a whole. Incentive structures include, most basically, patterns of social recruitment into the various disciplines. We need to know how these have changed over time, and what difference it might make if, for example, the ethnic and class mix of sociologists has shifted, or if historians are nowadays more likely to be the children of academics.

Incentive structures also include patterns of funding as seen from the perspectives of individual applicants, along with opportunities for publication and the normative standards that are applied in judgments about promotions and other allocations of scarce career rewards. Do present incentive structures—especially those that channel grants for research—promote excessively scattered and short-term efforts? Do these efforts come at the expense of longer-gestating projects aimed at reconceptualization and synthesis of available knowledge, or aimed at improved measurement, data collection, and validation of knowledge on well-defined issues?

Perhaps there should be new research and critical reflection on these issues in the near future, with an eye to convening a national, interdisciplinary commission that could recommend policy and organizational changes to public and private funding agencies and to universities. In addition to the funding agencies and the universities, the major inter-

disciplinary centers at which many social scientists spend fellowship years—especially the Center for Advanced Study in the Behavioral Sciences and the School of Social Science at the Institute for Advanced Study—should also figure in the deliberations and recommendations of such a commission, for they have provided important individual incentives and helped to shape the substantive agendas of the postwar American social sciences (see Thackray, 1984).

### Undertakings for the Future

In the previous sections we have discussed current intellectual frontiers and new questions that ought to be addressed. In this final section we offer three recommendations that, in our opinion, would further the most promising of existing trends and stimulate needed new research.

1. We recommend sustained studies of the social sciences and their interactions with public policymaking. In addition to worthy individual projects, such programs should include the activities of cross-national and interdisciplinary working groups of scholars.

There is currently no governmental agency or private foundation that places a programmatic priority on research about the development of the social sciences or their interactions with government, and we feel that this situation should be changed to further the promising substantive agendas we have surveyed.

The National Science Foundation might broaden the mandate of the existing Program on the History and Philosophy of Science; at present this program primarily promotes research on the natural sciences. Alternatively or additionally, the N.S.F.'s basic sociology program could place more emphasis on studies about the social structures and cultures of the social sciences, perhaps in explicit cooperation with the National Endowment for the Humanities. And the N.S.F.'s basic political science program could place greater emphasis on studies about the contributions of the social sciences to public policymaking and, conversely, on the impact of state structures and governmental actions on the social sciences. The National Science Foundation could make research about the social sciences in a societal context a higher priority and clarify for potentially interested scholars where, in its programmatic structure, to direct various sorts of proposals.

Private foundations and the major interdisciplinary centers can probably do even more to further the research frontiers discussed in this report. Some foundations, such as the Russell Sage Foundation, might find it a meaningful expression of their own historical identities to insti-

tutionalize programs designed to encourage a steady flow of new research about social knowledge and public policymaking. The Woodrow Wilson Center in Washington, D.C., is ideally situated to facilitate cross-national comparisons of the role of the social sciences in public policymaking. And the National Humanities Center and the Institute for Advanced Study are logical locales for interchanges between humanists and social scientists interested in studying the historical development and contemporary characteristics of the social sciences. Finally, the Social Science Research Council has in the past and will continue in the future to facilitate interdisciplinary investigations on the social sciences in societal context. In particular, the recently created S.S.R.C. Research Planning Committee on States and Social Structures has established working groups for the comparative-historical study of "States, Social Knowledge, and Social Policies."

We do not mean only to encourage more individual research in the areas we have identified as substantively promising. In addition, there should be more genuinely collaborative research. Repeatedly in the above discussion, we have pointed to needs and possibilities for projects that compare national or other "cases" or that would require the achievement of challenging syntheses of the skills and substantive knowledge of various specialists. Unfortunately, present funding structures favor either individual, relatively short-term research projects or else one-shot conferences at which individuals present pieces of their preexisting work and, at best, end up contributing to an edited volume of loosely coordinated articles. We feel strongly that much of the further progress possible in the areas we have surveyed depends upon breaking out of this dialectic of individually conceptualized research and one-time collective gatherings. Especially for comparative research on social knowledge and public policymaking or on the professional structures and cultures of the social sciences, we must move toward the establishment of small to medium-sized "working groups" that could meet repeatedly, from the early conceptualization of research through its execution. Such working groups might sometimes design and carry through one big research project. More often, they might closely coordinate projects actually carried out by individuals or subgroups of scholars. The important point is that working groups should meet repeatedly over several years, moving from the early stages of research conceptualization to the actual execution of a well-worked-out plan.

A slightly different approach to forming working groups for sustained collaboration would entail use of facilities at the Center for Advanced Study in the Behavioral Sciences, the Institute for Advanced Study, the Woodrow Wilson Center, or the National Humanities Center. At times,

perhaps, sets of scholars interested in sustained research collaboration could be jointly scheduled for a year's residence at these centers. Yet we also urge the possibility of three-month stays for working groups, especially at the point where joint conceptualization of issues and hypotheses needs to happen. Such limited joint stays could be ideal for the initial formation of cross-national or interdisciplinary teams, because shared time is often needed simply to establish effective communication.

2. We recommend the establishment of a "Historical Social Science Board" to promote training for social scientists in the conceptual possibilities and methodological requisites of historical and comparative-historical research.

At present, many current research frontiers in the area of our charge lie at the conceptual and methodological intersections of the social sciences and history. We have suggested the promise of investigations of medium- to long-term processes to reveal the indirect and contextual effects of social knowledge on public policymaking, and we have highlighted the promise of studies of the changing social structures and cultural orientations of social science professions in various nations. Excellent research in these areas requires that social scientists, while not abandoning their own theoretical agendas or methodological skills, also learn the analytic possibilities and methods of archival research, of oral history, and of chronological reconstructions of complex sequences of social and political change.

Such excellent research in the areas of interest to us also requires that specialist historians and social scientists alike learn the uses of genuinely *comparative* history to define researchable problems, to sharpen contrasts, and to examine hypotheses and sort out causal configurations when only a few "cases" are available for analysis. Genuinely comparative history should not be confused with the culturally particularist history often done by "area specialists" who confine their attention strictly to one country or region of the world. Valuable as this kind of scholarship is, it has been disproportionately encouraged at the expense of explicitly comparative scholarship that builds findings out of systematic cross-national and cross-cultural comparisons.

The model we have in mind is the Mathematical Social Science Board (MSSB) that was established in 1964 with its headquarters at the Center for Advanced Study in the Behavioral Sciences. This active and influential enterprise aimed to infuse more mathematical expertise into the social sciences, and to that end it planned and arranged for the funding of various training institutes and substantive seminars. Like the MSSB, a new historical social science board, headquartered in an appropriate transdisciplinary body, could inspire and help arrange funding for var-

ious training enterprises. Possibilities include: (1) university-based curricular projects designed to improve historical methods training for graduate students in sociology and political science; (2) special training courses (such as "summer institutes") for established scholars wishing to learn and experiment with particular methods, such as oral history or comparative history; and (3) topical seminars for scholars interested in reconceptualizing particular research problems (such as studies of decision making in a public policy area) in more historical terms. Particular projects such as these might often be suitable for funding by the National Science Foundation, and we feel that it would be salutary for the NSF to take an active interest in carefully planned efforts to make historical methods widely and reliably available in the social sciences.

Obviously this recommendation for the creation of a historical social science board addresses methodological needs and research possibilities not only in our group's substantive jurisdiction but also in jurisdictions of other groups. We acknowledge this, and urge the Committee on Basic Research to consider whether the time is not ripe for this kind of effort to enrich the possibilities for historical and comparative-historical research on many substantive topics of priority interest to social scientists.

3. We recommend the establishment of university-based programs or centers devoted to research in and teaching of the history and sociology of the social sciences.

The capacity of the social sciences to reflect upon their own development and linkages to government and the broader society could be greatly enhanced through the development of interdisciplinary research programs or centers at major universities, especially once the kinds of measures embodied in our first two recommendations begin to bear fruit. At universities with many scholars interested in the study of the social sciences, an interdisciplinary center or program could gather archival sources for the history of the social sciences, engage in appropriate training of graduate students from both history and the social sciences, undertake oral histories, gather and refine the historical statistics on the social science community, encourage comparative and integrative studies of the social science disciplines and of the philosophy of the social sciences, and host collaborative research projects and conferences. National centers of this kind are already in place to study the history of various scientific and engineering fields, including physics, chemistry, microbiology, computing, and electrical and chemical engineering. In some ways these centers are models for what we have in mind, yet we feel strongly that the social sciences should be studied together, not discipline by discipline, because historically formed and cross-nationally varying disciplinary configurations should themselves be an object of analysis, not a taken-for-granted framework.

The aim should be to create or encourage appropriate efforts in several universities with faculty who are already interested in the social sciences and their societal roles; ideally this should encompass faculty from departments of history, history of science, sociology, political science, and economics. If public policy people could also be involved, so much the better. The great advantage of interdisciplinary, university-based centers and programs lies not only in their function of graduate training but also in the promise they offer for continually inspiring new, broadly conceived and transdisciplinary research of the sort we have repeatedly called for throughout this report.

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## The Outlook for Comparative International Social Science Research

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Recent developments in both comparative and international research have important implications for social science research in general. A range of research previously conceived as "domestic," or as concerned with analytical propositions assumed invariant across national boundaries, clearly needs to be reconceptualized in the light of recent comparative/international findings. In this report we have not tried to survey the field. Instead, we have illustrated the importance of comparative/international research by choosing a small number of areas in which significant work has been done over the past ten years and that show substantial promise for future contribution.<sup>1</sup>

We have looked first at research on the evolution of the international financial system. A discussion of the internationalization of production follows. Here we have focused, first, on the organization and flow of capital and technology and, second, on the internationalization of labor and its consequences for the organization of the labor process. A central theme in all three of these sections is the interaction between state policies, both foreign and domestic, and the new internationalized systems of finance and production. Our final focus is on the impact of global cultural models on domestic institutions and policies. In each of these areas we have noted lines of inquiry that need to be pursued if the heuristic promise offered by the progress achieved to date is to be fulfilled. Even more important, in our view, is an understanding of the support strategies necessary to promote further scientific progress in comparative international research more generally. We have focused on these in the final pages of the report.

All of the specific areas of research that we will discuss have contributed to a general movement toward new ways of conceptualizing the relation between structures and systems that operate at the global or international level and those that operate at the domestic or national level. It has become increasingly clear that comparative and interna-

<sup>1</sup>Obviously our selective strategy has had some costs. We have not been able to deal with a number of interesting and relevant developments (e.g. the work on "international regimes" being done by Keohane (1981), Krasner (1982), and others), but preserving the possibility of going into a few areas in slightly more depth seemed worth the sacrifice of greater breadth.

tional research needs to be integrated. This reconceptualization forms the fundamental theoretical context in which specific findings must be understood and is therefore worth reviewing quickly.

Traditionally, comparative research has focused on the ways in which analytically defined relations among social science variables are modified by different national settings. International research, on the other hand, has focused either on events or structures that are transnational, occurring across or outside societal boundaries, or on relations among nation-states. To a large degree, these two sorts of investigation represented different research traditions, conducted by different communities of scholars.

In the last fifteen years, there has been a growing realization that this separation was doing violence to the reality of both comparative and international phenomena and producing distorted results. Scholars began to look at the ways in which differences between societies were produced by the ways in which transnational systems impinged on domestic structures. At the same time, those concerned with analyzing global systems began to take more seriously the importance of comparative evidence collected at the national level to understanding the construction and operation of international systems.<sup>2</sup>

Increased integration of comparative and international research has not, of course, occurred simply as a result of new intellectual insights. It has been prompted as much by real changes in relations between individual societies and the international system as by the evolution of our intellectual perspectives on these relations. Dramatic increases in the relative economic importance of international trade, burgeoning levels of international indebtedness, the increasing rapidity with which policies initiated in one state are borrowed by others, and the pervasive penetration of even the most isolated societies by global patterns of popular culture all help draw our attention to the fact that comparative work must also be international.

Despite its importance, taking an integrated approach to comparative/international research is by no means easy. Comparative work has

<sup>2</sup>Two key contributions to the integration of comparative and international work are Katzenstein's (1977) edited volume which looked comparatively at domestic political structures as part of the explanation for international economic policies and Gourevitch (1978) which proposed Waltz's "second reversing the image" and looking at the international economy as a means of explaining domestic structures. Also very important was the work of Wallerstein (1974) and those who followed his lead. While this work placed a heavy theoretical emphasis on the extent to which the evolution of particular societies should be seen as derivative of the evolution of the international system, it utilized a detailed historical method of exposition and evidence that stimulated comparative analysis at the same time.

of necessity been constructed on the foundation of "area studies," which tend to emphasize an "idiographic" approach stressing historical, cultural, and structural uniqueness. Trying to introduce more rigorous analytical models into this kind of analysis is, to say the least, a challenge. On the other hand, work on international systems has tended toward the construction of analytical models. Expanding these models to incorporate the "thick descriptions" that form a basic part of the evidence in much comparative research is equally challenging.

Overlaying the problems caused by differences in approach between the two research traditions are additional challenges caused by the inherently multidisciplinary character of work that integrates comparative and international perspectives. The comparative tradition has always emphasized the interaction of economic factors with social, political, and cultural ones. Since a substantial subset of work on international systems is dominated by economic models that see social and political factors as largely "exogenous," bringing together comparative and international work implies a qualitative increase in the need for economist/noneconomist dialogue.<sup>3</sup> These problems must be confronted if the promising beginnings that have been made in comparative/international research are to be exploited, but before suggesting how they might be more effectively surmounted it is worthwhile illustrating the substantial progress that has been made despite them.

### **International Finance and Domestic Policy**

Finance is perhaps the most thoroughly internationalized arena studied by social scientists. While international financial flows have always been a central feature of the international system,<sup>4</sup> the degree to which financial institutions have become transnational is unprecedented. The Eu-

<sup>3</sup>A very analogous and extremely important problem that our working group has not dealt with involves the relation between security issues and domestic politics. Currently, a deep intellectual gap separates the experts on defense, armaments, and strategic doctrine who know a great deal about the technology of security, and have constructed a variety of complex cognitive models of security-related interactions among nation-states, from experts on the domestic politics of individual countries who know a great deal about society, culture, economic structures, policy formation, and interest groups. Security issues involve complex linkages between these two areas of research. Russett (1985) provides some suggestions for linking security.

<sup>4</sup>See, for example, Tilly (1975) on the role of international finance in the formation of European nation-states.



romarket and various "offshore" banking institutions have given transnational banks the capacity to operate outside the jurisdiction of individual nation-states. Advances in telecommunications and information processing, the increasing proportion of global production that is traded across national boundaries, and the internationalization of production all contribute to the power and importance of these transnational financial institutions (see Bryant, 1980, 1984; Pecchiolo, 1983).

This increased transnationalization of finance has had contradictory results. On the one hand, it has clearly limited the options of states. Their perceived inability to regulate the Eurocurrency market has meant a partial loss of control over monetary policy, as has the international debt crisis resulting from Euroloans to developing countries during the 1970s (Wallich et al., 1983; McKinnon, 1984). Foreign policy has also been affected (Cohen, 1984). The breakdown of the Bretton Woods system and the introduction of floating exchange rates have introduced greater volatility and have increased the impact of international investors on domestic economies. Examples include capital flight in France, undermining the Mitterand expansion attempt, and the high value of the dollar playing havoc with U.S. trade policy (Richardson, 1983; Maris, 1985). In the view of at least some economists (e.g., Dornbusch, McKinnon), monetary policy can no longer be pursued nationally but must explicitly take into account policies of all industrialized countries.

This is not to say that state apparatuses have ceased to be key actors in international finance. Transnational banks continue to depend on core nation-states to maintain the currencies in which they do business. In addition, greater mobility of financial capital can provide states with new degrees of freedom. It is now easier to attract capital for investment purposes or to finance budget deficits, and floating exchange rates do provide somewhat more leeway for differing macroeconomic policies than did the fixed exchange rate system of the past.

Even in the Third World, where the power of the international financial community has grown substantially over the past decade, the effects on state apparatuses have been paradoxical. Here, much more than at the core, state policies have become constrained by the exigencies of international finance. Yet, at the same time, increased availability of international finance also helped place state apparatuses in a central position vis-à-vis the organization of their domestic economies (cf. Frieden, 1981; Evans, 1985) and in certain respects has increased the state's capacity for autonomous action (cf. Stallings, 1982).

These findings, paradoxical in terms of traditional zero-sum models of the power of nation-states and transnational actors, underscore the need

for new ways of conceptualizing the interaction of public and private institutions in an internationalized economy. They also serve to underscore the need for interdisciplinary dialogue. Prevailing models of the international financial system tend to be derived from general economic theorizing, while comparative analyses of the institutional consequences of changes in international financial parameters tend to be constructed by political scientists and other noneconomists.

We have a number of sophisticated deductive models of how changes in international financial parameters should affect individual countries (e.g., McKinnon, 1984; Dornbusch, 1980; and others on "open economy macroeconomics"). We also have a number of good analyses of the complexities of the institutional structures that are in fact in place (e.g., Aronson, 1977; Hawley, 1983). What is needed to push forward existing work on internationalization in the financial area is more dialogue and collaboration between economists and noneconomists in order to produce analyses that are firmly grounded in economic theory while at the same time sophisticated in their institutional assumptions and sensitive to the consequences of diverse institutional arrangements.<sup>5</sup>

### **The Internationalization of Production and the Role of the State**

An impressive body of research that has emerged over the past fifteen years documents the extent to which production is organized along global lines and the consequences of this form of organization for the political economies of individual countries. Much of this has focused on the institution of the transnational corporation (TNC) and its effects on Third World countries. This literature has succeeded in demonstrating certain negative consequences of TNC penetration,<sup>6</sup> but it also has demonstrated substantial intersections between the interests of states, concerned with maximizing a broad range of goals at the local level, and those of TNCs concerned with maximizing a much narrower range of objectives globally.

Work on extractive industries will serve as an example. We know from Moran (1974), Becker (1983), and a host of others that transnational

<sup>5</sup>For a suggestive exploration by an economist of the value of introducing more institutional analysis into discussions of international finance, see McCulloch (1983).

<sup>6</sup>See Evans (1981) for a review of some of these findings. See also Bornschieer and Chase-Dunn (1985) for a more recent assessment of the evidence.

mining companies are likely to frustrate national development plans by their reluctance to invest their returns in the expansion of local operations, especially where forward integration (and by implication an upward shift in the country's position in the international division of labor) is concerned. Yet these same studies also show that states may diminish their chances of realizing other objectives (e.g., access to stable markets) by eliminating TNC participation (cf. Shafer, 1982). Likewise, numerous studies have shown that it is possible for states with political will and technocratic competence to induce TNCs to conform more closely to certain local goals (see especially Becker, 1983). The literature on internationalization in extractive industries points, in short, in the same dialectical direction as the literature on the internationalization of finance.

There is also, of course, a substantial literature on states and TNCs in the manufacturing sectors of Third World countries. This literature reinforces the conclusion that states not only remain important actors in the new internationalized organization of production but also, precisely because of the necessity of bargaining with multinationals, have become more rather than less important.<sup>7</sup> It also contains some very suggestive findings with regard to the way in which the interaction of Third World states and TNCs in the manufacturing sector has influenced the international economic system as a whole.

Certain Third World countries have succeeded in dramatically shifting their position in the international division of labor. The movement of Newly Industrialized Countries "NICs," such as Korea and Taiwan, from the export of traditional labor-intensive light manufactured goods to the export of sophisticated high-technology products or, like Brazil, from coffee to steel, autos, and airplanes, exemplifies the process. Its consequences for trade patterns have been documented by economists (see, for example, Fishlow, 1981; Helleiner, 1981; Cline, 1984), but the institutional dynamics of the process itself are not well analyzed.

Convincing explanations of the institutional processes that enable countries to change position in the international division of labor are central to any theory of the international economic system as well as to

<sup>7</sup>Again there are conflicts between the consequences of multinational penetration and developmental goals. See, for example, Newfarmer and Mueller (1975) and Newfarmer (1980) on the anticompetitive consequences of TNC dominance in local manufacturing. See also Evans (1979) and Gereffi (1983) on differences between TNCs and local interests over the location and direction of technologically innovative activities. And see Newfarmer (1985), especially the contribution by Shepard, on consequences of TNC penetration for demand creation. But there are even more clearly positive sum possibilities for state-TNC bargaining, as, for example, in the petrochemical industry (Evans, 1979, 1981, 1982).

models of national development. In order to arrive at such explanations, the experience of core as well as peripheral countries must be examined.

Conventional wisdom (and a good deal of social science theorizing) fifteen years ago assumed that core countries that were "home" to TNCs (e.g., the United States) would be the beneficiaries of TNC expansion. Subsequent research by economists and political scientists raised serious questions with regard to that assessment (e.g., Gilpin, 1975; Musgrave, 1975; Bergsten, Horst & Moran, 1978), anticipating the anxious policy reassessments that dominated consideration of the new international economy in the United States in the 1980s (e.g., U.S. Labor Department, 1980, on U.S. competitiveness). The question of whether the position of the United States in the international division of labor may somehow be "slipping" is, of course, a matter of intense policy concern, but it must also be seen as part of a broader theoretical issue.

Analyses of apparent U.S. inability to construct an effective institutional response to changes in the international division of labor stand in contrast to the picture that emerges from research done on smaller European countries (see Katzenstein, 1985). Here we have a picture of countries that are acutely dependent on the international economy but seem not only to have developed institutional mechanisms for adapting to the shifts wrought by new flows of technology and capital but also to have engaged in conscious attempts to exploit these flows.

Japan is, of course, an even more oft-cited exemplar of institutional arrangements that have enabled dramatic shifts in international economic position (cf. Johnson, 1982; Borrus, 1983; etc.). Curiously, however, despite interest in utilizing the Japanese "model," Japan's success is much more likely to be attributed to unique national characteristics than to be incorporated into an analytical schema that would allow greater comparative and theoretical leverage.

Overall, there is an impressive body of work on the responses of a range of countries to the emerging "new international division of labor." The question is how to build on this work in order to move toward more general theoretical understandings. At least two strategic guidelines seem obvious:

1. *Breaking down the barriers that separate research on the First and Third Worlds is a logical first step.* If we are really going to understand the institutional mechanisms that allow one country to improve its position in the international division of labor while its neighbors slip, we need an approach that examines both First and Third World countries through the same analytical lens, recognizing that changes in both sets of countries are part of a single process while at the same time doing

justice to the fundamental institutional differences that point the way in which the process evolves in a given societal context.

The problems with bringing together work on the First and Third Worlds are, of course, daunting. Since this work has been conducted by surprisingly distinct sets of scholars, distinct conceptual frames have evolved for handling "development" as opposed to the political economy of advanced industrial societies. In addition, the sharp differences in the quality of data available on Third World countries as opposed to that available on advanced industrial societies is a barrier to comparability. Still, these problems are surmountable. Indeed, some interesting new work is being done along these lines. Cumings (1984), for example, sketches an integrated analysis of the development of the Northeast Asian regional economy that considers both developed countries actors (Japan and the United States) and less developed ones (Taiwan and South Korea). Similar analyses that include the United States both as an actor and as a comparative case are badly needed. Especially germane would be work on the relation between the rise of the NICs and the changing position of the United States in the international division of labor.

2. *Better models of how state apparatuses function as institutional actors are imperative.* Although TNCs are new social institutions, we have much better models for analyzing their behavior and decision-making processes than we do for state apparatuses. We know very little about why some states are able to construct effective institutions for dealing with the new international economic environment while others are not. Since the success or failure of state apparatuses to construct such institutions shapes the structure of the international economic system itself, better models of how states function as actors and institutions are central to further theoretical progress.

### Labor and the Labor Process

Flows of labor are no less important to current processes of internationalization than flows of capital and technology. They raise a number of the same conceptual and methodological issues that we have already discussed. For example, since labor migration simultaneously affects both sending and receiving societies, the necessity of an approach that integrates concern with First and Third World development is obvious. Looking at the internationalization of labor also raises a host of new issues. We will not try to cover the full range of issues here. The conse-

quences of the "brain drain" of Third World professionals to the United States, the developmental consequences of emigration for sending communities and a variety of other aspects of labor flows will be left aside. We will illustrate the issues involved by focusing on some questions raised by the influx of low-wage labor into the U.S. labor market.<sup>8</sup>

Recent research shows that international migration does more than insert a new set of workers into the existing production organization. It changes the way in which the labor process itself is organized. Immigrants, especially illegal ones, are much less likely than domestic workers to be employed in large factories. Instead, they work in small highly mobile shops, at home under a piece-rate system, as sharecroppers in agriculture, or as itinerant wage laborers under the gang system (see, for example, Sassen-Koob, 1980, 1984; Waldinger, 1984; Wells, 1984). Old industries like garment and footwear production, and new ones like electronics, have become increasingly "informalized" through their reliance on immigrant labor.<sup>9</sup>

The changes observed in the organization of the labor process as a result of internationalization are of fundamental theoretical significance to a whole range of social science thinking, from economics to sociology, that has assumed a secular trend toward increasing homogeneity of labor organization and a movement in the direction of large-scale bureaucratically organized production. Since the changes observed do not move along the lines of a "modernization" of productive organization, as that has traditionally been defined, but in the reverse direction, they suggest rethinking our basic models of social change.

Despite the theoretical importance of research on the internationalization of labor, analyses of the labor force, labor markets, and stratification in the United States have often proceeded without taking into account what may be the most important change of the last two decades. Illegal immigrants working in the informal sector are not, almost by definition, included in official statistics. Gathering data on these groups is difficult and costly. Since substantial amounts of reliable, accurate data are a prerequisite for the construction and testing of more sophisticated analytical models, there is an understandable tendency to focus atten-

<sup>8</sup>For a discussion of different types of immigration see Piore (1979), Portes and Walton (1981) and Portes and Manning (1984). For a discussion of developmental consequences for sending communities see Portes (1982).

<sup>9</sup>While this research is primarily based on U.S. data, there are intriguing parallels between the U.S. results and observations in at least certain areas of Europe, most notably the Emilia-Romagna area of Italy as reported by the joint U.S./Italian research project undertaken by MIT-Harvard and the Universities of Modena and Parma (see, for example, Sabel, 1982, and Brusco, 1982).

tion on that part of the work force that is most easily captured in existing quantitative data. With a few notable exceptions, systematic empirical studies of the internationalization of labor and its consequences for the evolution of the organization of the labor process in the United States are lacking.<sup>10</sup>

Support for research aimed at surmounting the difficulties of data gathering on the internationalization of labor, especially with respect to illegal workers and the informal sector, must be a priority if we are to avoid developing analytical models whose elegance is achieved at the expense of ignoring a central aspect of the evolution of the role of labor in the new internationalized system of production.

Equally important in the analysis of the consequences of internationalization for labor is renewed attention to the noneconomic aspects of the organization of labor markets and the labor process. One of the strengths of immigrant groups is their ability to make use of noneconomic ties, such as ethnic and kin networks, as instrumentalities in the labor market. At the same time, traditional organizational structures not only have proved ineffective at mitigating the impact of internationalization on labor but also have themselves been undermined. There has been some very interesting recent work taking a more "cultural" perspective on current changes in the organization of the labor process (see, for example, Sabel, 1981), but, as internationalization continues, political and ideological responses to the dislocations it causes will without question demand additional study.

### **Constitutive Consequences of Global Cultural Processes**

Internationalization, especially in its economic aspects, tends to be conceptualized in terms of increased flows from one nation to another. The cultural aspects of internationalization can, of course, be conceived in

<sup>10</sup>Two collaborative efforts at systematic comparative data collection are worth noting here. First, there is the project organized by a sociologist (Sassen-Koob) and three anthropologists (Fernandez-Kelly, Sala, and Pessar) investigating the position of female immigrant workers in the apparel and electronics industries in New York, California, and Florida. Second, there is the international collaboration organized by Castells, Cornelius, and Portes along with other economists, sociologists, and anthropologists from the United States and three foreign countries examining the role of the informal sector in the Iberian countries and the role of Latin American immigrants in the emergence of similar labor arrangements in the United States. This project includes comparative city studies in Madrid, Montevideo, Bogota, Miami, New York, and Los Angeles.

the same way: as ideas, information, technology, or consumption patterns flowing across national boundaries. Recent research on the isomorphism of cultural patterns across countries suggests, however, that these conceptualizations understate the extent to which societal institutions are derivative of global cultural models rather than simply affected by them. It is not simply a matter of institutional borrowing but a much more active process through which societies reorganize around world models as they enter the international system, (Meyer, 1980).

This perspective challenges theoretical models prevalent in both comparative and international research traditions. Ideas of national uniqueness and tight intrasocietal coupling of institutions need to be rethought. So do models that view the evolution of societal institutions as produced primarily by adaptive responses to the political and economic environment and therefore consider it predictable on the basis of deductive theorizing. Some specific examples will clarify the nature of the challenge.

Cross-national data on educational systems ( see, for example, Meyer et al., 1977; Ramirez and Boli, 1982; Inkeles and Sirowy, 1983) have shown that the expansion, structure, and content of educational systems reflect world models evolving over time as much as the economic or political situation of the individual country in which they are located. Many other aspects of state structure and policy have this same property. For example, the content of national constitutions has been shown to be remarkably similar in prescribing the rights and duties of citizens, with similarities increasing sharply in the mid-twentieth century (Boli-Bennett, 1979; Boli-Bennett and Meyer, 1978). Likewise, the standard institutions of the welfare state (at least as legal principles) spread rather quickly throughout the entire nation-state system and continue to evolve in roughly common directions in the most disparate countries (Collier and Messick, 1975).

The same is true in the economic arena. Occupational changes in the contemporary world system show surprisingly isomorphic changes. The service sector and its professional component expand in parallel fashion in countries at all levels of development (see, for example, Fiala, 1984). Even the response of state apparatuses to TNCs is derived in large measure from a global culture of bargaining norms. The bargaining position of Papua New Guinea vis-à-vis international copper companies reflects a global culture created by the accumulated experience of other Third World countries as much as by its own political history or economic circumstances (see Evans, 1985).

Finally, of course, there are the less systematically studied but clearly powerful effects of global cultural models on patterns of popular culture



throughout both developed and less developed countries. The increasing pervasiveness of common patterns of popular culture is impressionistically obvious, but rigorous comparative analysis of its consequences for the evolution of local social and cultural institutions is still lacking.

The case should not, of course, be overstated. Local institutions and histories shape the constitutive consequences of global cultural patterns. As in the analysis of internationalization of production, the aim must be to elucidate the interaction between local structures and processes, on the one hand, and global systems on the other. The recent work of Orloff and Skocpol (1984) and Weir and Skocpol (1985) on national variation in the emergence of the welfare state among advanced capitalist countries provides a nice example. The complex interaction of "orthodox" and "nationalist" economic models in the policymaking process of Japan, Korea, and Taiwan, going back at least to the 1920s and extending to the present, offers equally interesting case material.

Likewise it is important to remember that global cultural models may be generated in particular societies in response to particular local challenges and that this may occur at the periphery as well as in the center. Indeed, one of the most interesting examples of peripheral problems generating global cultural models may be found in the field of comparative international research itself. The "dependency approach," essentially the product of Third World scholars trying to understand the problems of their own societies, has emerged as one of the most prominent global models for understanding the process of internationalization (Cardoso, 1977; Palma, 1978).

In short, the point is not that some kind of Hegelian cultural logic determines the course of institutional evolution in individual countries. It is rather that researchers trying to explain the behavior of apparently "domestic" organizations and institutions are likely to produce badly misleading models and theories unless they take into account the extent to which key properties of their objects of study are constituted through global cultural processes rather than at the level of the individual society.

Building this insight into future research means focusing more attention on the processes by which cultural models emerge, evolve globally, and interact with national institutions and societies. How do certain policy choices made in core countries become worldwide models while others remain unimitated? What role do international institutions (e.g., the World Bank, the United Nations) play in such processes? Future research should also be much more sensitive, in analyses of national policy processes, to the role of external cultural pressures that may support a policy choice, provide information and analyses to make this choice ap-

pear more reasonable, and support the status of professionals who act as local spokespersons for the established lines of action.

### Recommendations

New resources are not the principal requisite for taking advantage of the leads offered by recent comparative/international research. But, if future theorizing, model construction, and data collection in the social sciences are to take into account the undeniable fact that political, social, and economic processes are international in character, substantial restructuring of the way in which resources for social science research are allocated will be required.

Current funding structures pose a weighty institutional barrier to anyone attempting to undertake the integration of comparative and international research. Within most social science disciplines (with the exception, perhaps, of anthropology) the quest for universalistic theories has carried in its wake a surprising degree of unacknowledged parochialism. Deductive models are implicitly assumed to apply everywhere. The sanctions against departures from disciplinary norms of methodological elegance lead researchers to test their theories only in those sites where the quality of the data is commensurate with the rigor of the models. This militates against including the range of cases necessary to determine whether theories and models are in fact applicable beyond the confines of the contemporary United States.

The tendency for comparative analysis to cross disciplinary lines constitutes an additional disadvantage for comparativists. Work that draws on more than one discipline is likely to be viewed from within each discipline as not taking full account of the theoretical and methodological resources offered by that discipline. Past attempts to circumvent these obstacles to comparative research have focused on the support of "area studies." Unfortunately, this avenue poses equally grave problems, foreshadowed in our initial discussion of the differences between comparative and international research. The institutional character of area studies programs and centers tends to reward detailed command of specific local knowledge. More broadly theoretical approaches, such as those required to integrate international and comparative perspectives, are often seen as taking inadequate account of the uniqueness of the local area.

Progress in comparative/international research requires allocation of resources through organizational structures explicitly designed to promote work that moves across disciplinary and area boundaries. The necessity of providing such additional resources for comparative

international research has been recognized in the past, in the Ford Foundation's efforts of the 1950s and 1960s or in the International Education Act, but the vision of these earlier efforts was never followed through to its fruition. Without advocating any grandiose new programs it is still possible to generate the sustainable long-term consequences that these earlier efforts aimed at. Reallocation of even a small proportion of the funding currently defined in terms of disciplinary or geographic categories would have a major stimulating effect on the efficacy of social science responses to the internationalization of social, political, and economic life.

The stimulation of additional research must be complemented by programs designed to foster creative reanalysis and reinterpretation of existing evidence and existing theoretical models. Testing existing theories and models against new evidence is obviously useful, but the heart of the process of constructing more adequate comparative/international understanding lies largely in reconceptualizing existing evidence and reworking existing theoretical models.

It would be unrealistic to expect individual scholars working on the basis of their own disciplinary and area expertise effectively to tackle such a task. Progress requires bringing together groups of scholars who can attack comparative/international issues simultaneously from several different disciplinary perspectives and draw on a variety of area expertise. Such groups rarely exist at one university. Two methods could be used to bring them together:

1. *Additional support for existing centers of international studies could be used to bring together small groups of scholars who share an interest in a particular theoretically promising problem to work together for an extended period of time.* This method would strengthen existing centers and have a high likelihood of producing collaborative research and writing, but its success is contingent on the simultaneous geographic mobility of a number of relevant scholars. In order to make it possible to attack problems where such simultaneous mobility is not possible for the relevant scholars, this approach should be complemented by:

2. *Support that would allow small groups of scholars with different disciplinary and area backgrounds to engage in work on a particularly promising problem and meet periodically at different locations over a two- to three-year period and share the results of their ongoing work.* Such support requires a small amount of resources relative to individual investigator grants and can have a strong synergistic effect on the contributions of several individual researchers in addition to stimulating collaborative research and writing.

These proposals are modest in relation to the scope of the research they are designed to foster, but their impact would be much greater than the resources they involve. Failure to institute some structural changes of the sort proposed here would, however, have serious negative consequences for social science research generally. If the internationalization of social, political, and economic life is to be reflected in general social science models and theories, funding structures must reflect the importance of this process. Otherwise, models and theories will continue to be "domestic" while the phenomena being explained are clearly not.

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## International Security and Crisis Management

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## Introduction

Most of us, scholars and citizens alike, are deeply concerned with current national security problems, primarily centering on Soviet-American relations. But the subject is not limited to a particular time period or pair of countries. Scholars may be as interested in understanding the security problems and policies of Athens and Sparta (see, for example, Kagan, 1969, 1974, 1981) or the origins of World War I (*International Security*, Summer 1984) as they are in problems of nuclear weapons. This would be true even if earlier eras did not shed light on current predicaments. In fact, they almost certainly do, at minimum in helping us see what is new about the nuclear era. Our concern with security also is not limited geographically—Third World security is as intellectually important as that of the superpowers. Furthermore, the two topics are clearly interrelated.

All fields of study have their own idiosyncratic difficulties. Three should be noted here. First, the world has not seen a nuclear exchange and has seen only a handful of major confrontations between the superpowers. Thus there is no direct evidence for such crucial questions as whether nuclear war can be kept limited, how decision makers would behave on the brink of war, and how the strategic nuclear balance influences the outcome of confrontations. A second difficulty is that national security studies always run the risk of being highly politicized. It is unrealistic to expect this situation to change, but it is important for us to manage it in a way that is least disruptive for scholarship. A third problem is that the study of security is—or should be—interdisciplinary. Although political science and international politics are at the core, disciplines such as history, economics, psychology, anthropology, law, and the physical sciences are important. Indeed, many of the best contributions have come from people outside political science, especially in the first generation of studies of nuclear strategy. These diverse perspectives make the field intellectually exciting, but in an era of increasing specialization it is both important and difficult to maintain the interdisciplinary nature of the enterprise.

A report like this cannot be comprehensive, and we realize that a different committee might have advocated other priorities. Indeed, the

field of security studies is characterized by a high degree of diversity. This is healthy both because the community has a range of interests, perspectives, and values and because no one can be certain which lines of research will prove to be most productive.

## Data Needs

### *Cold War History*

Although there have been no nuclear wars, there have been forty years of Soviet-American interaction as superpowers. Our knowledge about the history of this period is still sharply limited. We need to gather and make available as much information as possible about the evolution of American strategic doctrine since 1945, the ways in which American statesmen have thought about the use of force in general and nuclear weapons in particular, the American views of other governments, and the ways in which American military organizations have functioned. To the extent possible, we need similar data on other countries, especially the Soviet Union. At this point, we do not have a good understanding of how U.S. policy and Soviet-American interaction have evolved and of turning points in the Cold War—periods of time at which general trends were established, missed opportunities, or, alternatively, points at which we avoided disaster.

Works like George and Smoke (1974), Rosenberg (1979, 1983), F. Kaplan (1983), Herken (1985), and Blechman and Kaplan (1978) have proved very valuable. We have learned that there were great differences between American declaratory policy and actual war planning. While the former often stressed the concept of assured destruction of cities, the latter always stressed the need to hit a wide variety of military targets. Furthermore, there has been more consistency over time in war planning than in declaratory policy. Political decision makers have also kept closer control over the latter than over the former. Operational requirements and difficulties play a larger role in war planning than scholars had realized; what strategists desired and what war planners felt was possible were often quite different. We have also learned that presidents are torn between regarding nuclear weapons as extraordinary and divorced from international politics and seeing them as merely particularly powerful bombs. Indeed, President Eisenhower seems to have begun his presidency with the latter perspective and shifted to the former by the time he left office.

Much of the necessary information has been produced or gathered by the U.S. government. The problem is in getting it declassified and making it accessible to scholars. For already declassified information we need a system of coordination and sharing of information. But a great deal of the record remains classified and, while some documents are so sensitive that they should remain barred to the public, many are not. It is thus important for the government to take the initiative and release more information. A joint committee of government officials and scholars might be formed to provide for the speedier declassification of documents and the wider diffusion of information about what documents are available. This is in the interest of the government as well as scholars. Only rarely can civil servants take the time to analyze past documents; the government as well as scholars reap the benefits from a greater understanding of current dilemmas, which results from careful analysis of earlier cases.

The documents themselves rarely tell the whole story and should be supplemented by oral histories. Many exist but more are needed, especially of the lower-level officials who played crucial roles in such areas as American war planning and the analysis of Soviet military posture.

### *Quantitative Data Bases*

The four decades since the conclusion of World War II have witnessed an explosion in the data-based study of international politics and security. Questions concerning the relationship between national attributes and the domestic and foreign policy behavior of nations, the evolving structure of the international system, causes and consequences of international crises and wars, and the dynamics of international interaction have come under systematic scrutiny. The ability to ask and answer these questions, however, has been dependent upon the development of large data banks that document over time key attributes of and principal interactions between nations.

Thus far broad data collection efforts have focused on the quantitative and qualitative characteristics of (1) nations; (2) major conflicts and wars; and (3) interactive events between nations. The characteristics of nations were collected and recorded for a wide variety of years and nations by different researchers (see Russett et al., 1964; Taylor and Hudson, 1972; Taylor and Jodice, 1983; and Banks et al., 1971). In addition, a number of projects have produced data archives on various internal attributes of nations and the characteristics of interstate conflict (the Dimensionality of Nations project under Rudolph Rummel, Political Stability under Ivo and Rosalind Fierabend, the Comparative Study of Civil Strife under

Ted Gurr, and the Correlates of War under J. David Singer). The daily interactions between nations have been documented for the post-World War II period by at least three different projects, each concerned with slightly different forms of interaction. These include the World Event Interaction Survey begun by Charles McClelland, the Conflict and Peace Data Bank developed by Edward Azar and the Comparative Research on the Events of Nations directed by Charles Hermann.

The data movement of the past several decades has enhanced the methodological expertise for (1) the extraction of data from public sources; (2) development of indicators for basic concepts; and (3) quality control through reliability checks. This, together with the extensive technological advances of recent years in computer technology, makes feasible the future development of considerably more valid and reliable data sets.

Funding for data development, however, has been at best sporadic. The data sets referred to above are largely the work of individual researchers who are dependent on the vicissitudes of changing national funding strategies. There is no guarantee that they will be continued and certainly no clear opportunity for extending and developing the data compilations in response to the evolving needs of the research community. Furthermore, even though data collectors are generally aware of one another, there is no mechanism to integrate and compare their results. This has led to unfortunate duplications of effort and puzzling findings that may be due to differences in data sets rather than phenomena in the real world.

These facts—the research record, the currently ad hoc nature of data collection activities, and the available methodological/technological expertise—point to the desirability of a large-scale, integrated effort that would continue and further develop the data resources available to the research community of security scholars.

## Research Directions

### *Cooperation*

International politics consists of combinations of conflict and cooperation and our analysis must similarly combine the two. Thus a great deal of work in the field is built around the model of the Prisoner's Dilemma, in which each actor's first choice is to exploit the other. If this is not possible, each prefers mutual cooperation to unregulated competition. In this situation, however, the structure of the situation, the actors'

strategies, and misunderstandings may all combine to produce a third choice. A familiar problem in international politics is the security dilemma (Butterfield, 1951; Herz, 1950) in which the efforts by one country to increase its security have the effect—often unintended and undesired—of decreasing the security of other states. Since others are likely to react to a decrease in their security by increasing their arms, mutually unwanted arms races are possible.

Thus it is fruitful to examine the conditions under which this third choice occurs and the conditions under which adversaries are able to cooperate with each other (see, for example, Axelrod, 1984; Keohane, 1984; Jervis, 1978; *World Politics*, October 1985). Important here are the actors' time perspective and the value of the payoffs. Cooperation is most likely when the actors expect to have a long series of interactions, no one of which will be decisive; when the gains for exploiting the other and the losses for being exploited are relatively small; and when mutual competition is much worse for both sides than mutual cooperation. Cooperation is facilitated by contingent strategies such as reciprocity, which is based on the principle of cooperating with the other when and only when the other cooperates. Striving for too much of a unilateral advantage may be self-defeating.

Cooperation is also more likely when the actors can determine with some certainty whether the other side is cooperating or not, when they are willing and able to reply in kind to the other's behavior, and when the other side realizes this. Under these circumstances, the attempt to gain unilateral advantage will be less tempting because it will be seen as likely to provoke an undesired response. Similarly, if these conditions are met, states will be less likely to defect out of the incorrect belief that the other has done so previously or out of fear that the other will do so in the immediate future.

These arguments do not imply that cooperation is always possible or always good. Sometimes the security dilemma operates so strongly that there is nothing a state can do to increase its own security that does not simultaneously deeply menace the other side. In other cases, states primarily seek expansion and so mutual security is ruled out. In still other cases, two countries may cooperate at the expense of a third. Nevertheless, this research program can illuminate a number of security situations. Furthermore, it applies to other areas of international politics—for example, relations among international borrowers and lenders—and to other areas of social science as well, since it is related to problems of collective goods. Thus this approach provides a bridge between security studies and other fields of social science, something we feel is particularly valuable because the study of national security policy is too often intellectually isolated.

### *Conflict*

The field has accumulated a number of pieces of evidence concerning conflict and war that indicate that many maxims of security studies do not seem to be valid. Attributes such as a nation's power or its governmental structure cannot be shown to be directly related to its involvement in war. Being rich or poor, big or small, does not seem to make a country more or less war-prone. A country's internal difficulties also do not make it more or less likely to engage in conflict. Nor does population density push leaders into armed conflict to secure territory. Arms expenditures, on the other hand, do tend to be positively related to the amount of war engaged in; a strong military defense does not guarantee to keep a nation out of war. But arms races do not invariably produce wars. In fact, the concept of an "arms race" is undergoing reinterpretation. Arms races have typically been defined as accelerating military expenditures in the face of a potential enemy who is doing likewise. But it can be shown that the domestic setting can account for military expenditures far more clearly than the international context.

Relative power, particularly between bordering nations, influences the probability for war, but contrary to some of the arguments that have been advanced under the balance of power theory, wars are most likely between equal rather than unequal nations. One's intuitive feeling that wars are typically the consequence of accumulative, escalatory hostile interactions cannot be documented through numerous post-World War II crises and wars. The relationship between various attributes of the international system—its alliance configuration, polarization, power distribution, and status structure—and the level of conflict between the nations in the system have been studied extensively. While there is now no question that these structural attributes affect international conflict, the patterns that are emerging are complex and thus not easily summarized.

These represent some of the more significant results to have emerged, but they should be considered as illustrative rather than exhaustive. Although there is a long way to go, the ground has been prepared and we are now in a position to move ahead in a dramatic way.

### *Systems Perspectives on National Security Studies*

What is usually called the field of international politics is more often approached through the study of particular states' foreign policies. But only by looking at the interaction among the states can we realize that there is often an enormous disjunction between the goals that states pursue and their outcomes. The roles that are played in the interna-

tional system, the norms under which they operate, and the cycles of positive and negative feedback often are crucial. Some of the best known works in international politics have developed systems theories (e.g., Hoffman, 1965; Rosecrance, 1963; M. Kaplan, 1957; Waltz, 1959, 1979). We think these focuses will continue to be productive. A systems perspective leads to propositions like the following: the distribution of power among the states in the international system is a more important determinant of their behavior than are their domestic political and economic systems; the stability or instability of the international system cannot be deduced from the goals and intentions of the individual states; if each state tries to maximize its own security without taking due account of others' probable reactions, the result may be to make the state less, rather than more, secure.

Another systems argument is that the bargaining power of states is crucially affected by whether the system is bipolar or multipolar. The ability of small allies to force their larger partners to follow their lead by threatening to withdraw from the alliance is more potent in the latter world (Waltz, 1979; Snyder and Diesing, 1977). In a bipolar system, the superpowers have more freedom of action.

### *Domestic Factors*

While the international environment is an important constraint on the states' behavior, many of the specific impetuses for security policies come from within the state. One large and important argument—which the field still has not resolved—is the extent to which and the circumstances under which the policies that a state follows are systematically and strongly related to its domestic attributes (e.g., whether it is democratic or authoritarian, the nature of its economic system, whether the state is strong or weak vis-à-vis its domestic society). These questions seem especially important in four areas. First, the choice of security strategies that a state makes may be strongly influenced by its society and internal politics. Analyses of British and French security policies in the years between the two world wars have been fruitful (one of the first, and still best, is Wolfers, 1940; one of the most recent is Murray, 1984). Thus Wolfers shows that Britain and France disagreed on the crucial question of how strong Germany could be permitted to become before it would constitute a menace in part because of the differing constellations of party politics in the two countries. Murray stresses that neither fearful British public opinion (combined with British economic weakness) nor German strength prohibited a more active policy by Britain and France in 1938 and 1939. Weaknesses at the top, in both designing and executing policy, were crucial.



Second, states' domestic politics affect each other more and more not only indirectly through the security policies but also directly through the mechanisms of public opinion and transnational alliances. Third, economic issues are increasingly politicized, both within and between countries. Some cases directly involve security issues, such as controversies over East-West trade in advanced technologies. In other cases security questions are brought up indirectly. Thus the economic policies followed by each of the NATO allies can create or ameliorate frictions that affect security policies. Fourth, a great many international conflicts result at least in part from the uneven economic development within and among states.

In stressing the extent to which economic and domestic sources influence foreign policy, it is easy to overlook the fact that causal relationships also run the other way (Gourevitch, 1978). Thus a state's economic well-being is greatly affected by the success or failure of its security policies. More generally, the nature of a country's society and government can be strongly influenced by the security setting in which it finds itself. For example, historically strong and democratic state institutions are most likely to develop when the environment is threatening enough to require major efforts of self-protection but not so overwhelming as to prevent pluralism and decentralization. The question of possible conflicts between democracy and national security needs is a pressing issue for both academic research and public policy.

### *Decision Making, Beliefs, and Cognitions*

Much progress has been made in recent years in understanding the decision-making process and the ways in which statesmen think about national security issues. In this area, of course, there is special need for interdisciplinary research since the theories and findings from psychology are particularly informative. Both individual differences in perceptions and common aspects of the ways in which people think must be grasped if we are to understand how statesmen behave. For example, work in cognitive psychology, which stresses the importance of packets of beliefs (called scripts or schemata), helps explain both specific intelligence failures (e.g., Pearl Harbor, Iran) and the general tendency for statesmen to be very slow to change their images of other countries. More generally, decision makers, like people in their everyday lives, use shortcuts to rationality that conserve their cognitive resources by oversimplifying the world. But this mode of information processing also leads to random errors and systematic biases as statesmen employ information that is readily available and relatively easy to grasp even if it is not the most relevant for the task at hand. Affect plays a role, and "de-

fensive avoidance" (Janis and Mann, 1977) can also operate. Statesmen who are under great pressure to undertake a course for action—for example, to challenge or try to block another state—are likely to develop an unwarranted belief that this course of action will succeed. The result may be conflict or war (Lebow, 1981).

Two particular problems are ripe for further research. First is the question of cross-national and cross-cultural barriers to accurate perception. It is frequently argued that many statesmen, especially American, engage in "mirror imaging" and assume that others see the world more or less as Americans do. We need to determine whether this proposition is correct, whether it holds for other countries in other periods of time, and under what conditions it operates most strongly.

Second, we need to develop a fuller understanding of how statesmen set policies in the security area—for example, how and why they perceive others as threats, how images of other states are established and altered, the ways in which conflicts among important values are treated, how statesmen decide that certain threats are so implausible that they can safely be dismissed, and the biases and methods of simplification that characterize various stages of information processing. At some point we may be able to develop good computer simulations of decisions and artificial intelligence replications of the processes involved.

### *Study of War Itself and Military Institutions*

Although military organizations have been the subject of many studies, we think that two aspects need more attention. First is the possibility that war could arise out of the unintended interaction of peacetime military operations, especially during a crisis. (For a study of nuclear alerts, see Sagan, 1985.) In incorporating modern technology into their military establishments, the United States, the Soviet Union, and their respective allies have transformed their military organizations, instituting a large number of standard operations that were neither necessary nor feasible in earlier times. Many of these activities are conducted daily on a global scale, and as far as operational procedure is concerned they serve to keep the world continuously and quite literally on the brink of war.

Soviet-American tension has been little studied in terms of the interactions between the complex organizations that the military establishments have become. Military operations cannot be entirely reduced to strategic logic. They must be examined in their own right. Theories of organizational behavior generate a number of empirical questions about the internal operations of current military establishments and the inter-

actions that arise between them: the type and frequency of operational interactions between forces; the responsiveness at different levels of organizational hierarchy; the flexibility for changing operations; the conditions that would trigger changes; and the differences between normal peacetime and crisis operations.

A second subject needing more attention is the way in which military organizations function during wartime: What makes them effective when they are called upon to fight to secure their nation's political objectives? The question why individual enlisted men fire their weapons or otherwise take risks in combat has received considerable attention from sociologists. How can this work be extended to help us understand the individual performance, for example, of fighter pilots whose proficiency varies widely from country to country? What have been the effective ways of selecting and training officers? Both the German and Israeli armies have produced unusually capable officers, though they have done so in very different ways. War planning, before and after the outbreak of conflict, plays a key role in effectively utilizing the resources available for war. Why have some countries done well in planning for war in peacetime or been successful in adjusting their plans to the unpredictable events that inevitably occur in war?

What makes war a unique form of collective action clearly is the fact or possibility of combat. Decisions and plans must be formulated and implemented that will lead to killing on a large scale, often while that killing is taking place. How civilians formulate policy under these circumstances, sustain courses of action or abandon them, how military organizations respond to orders: these are all questions very different from the kinds of questions posed, for example, by the students of strategic communication or bargaining. Similarly, psychological analyses of decision making in small groups need to take these special factors into account.

### *Integrating Separate Levels and Approaches*

Insufficient work has been done with respect to the need and possibilities for integrating individual, national, and systemic levels of organization and to the study of linkages between them. As developed by Schelling (1960, 1966), Riker (1962), Snyder and Diesing (1977), George and Smoke (1974), and others, the concepts and processes of bargaining, leverage (side payments, inducement, means of persuasion or influence) as applied to competition, conflict, coalition formation, adversarial identification, and group decision making (Cyert and March, 1963) seem to provide useful starting points. The examination of such processes within

and between states offers a way of clarifying the extent to which international conflict and war are explained by systemic factors (including balance of power and related phenomena) and how much they are determined by domestic factors such as specific national capabilities, needs, and demands, by factors of decision making such as beliefs (including ideologies) and changes in leadership, and by interactions among the three.

### *Institutional Needs*

Because national security policy is so important for public policy it now receives major funding from private foundations. This will be of great assistance to the field, but three potential problems should be noted. First, there is a danger that the new funding will increase the politicization of the field, with liberal foundations supporting liberals and conservative foundations funding conservatives. Furthermore, if scholars believe—correctly or not—that certain foundations have strong political preferences, they may be tempted to present proposals of greater political appeal than scholarly value. A second potential danger is that the desire for policy relevance will lead to the production of analyses of pressing problems at the expense of basic research and the development of intellectual capital of the field.

A third problem is that while the level of short-run support is very high, the long-term prospects are uncertain. “Feast or famine” is as bad for intellectual enterprises as it is for public policy. Increased funding and increased activities are drawing many new people into the field. This welcome development will increase the size and the diversity of the community. But at this point it is not clear whether there will be sufficient jobs to permit these people to continue working in the security area. The academic market, although expanding a bit, is sharply limited, and it is hard to know the extent of the nonacademic market. Thus it is important that we learn what the job prospects are and that funding be related to the overall employment situation. Greater progress can be made if there is an increased number of researchers in the field, but this requires long-term if not permanent positions.

The increased knowledge, interest, and number of people in the field mean that it is especially important to develop efficient means of communication and to explore new ways of bringing people and ideas together. Preprint series, electronic mail, and electronic bulletin boards are fairly inexpensive possibilities. Teleconference systems, which permit interactions among people working on the same subject, need to be encouraged. But for many purposes, face-to-face meetings are neces-

sary. Repeated discussions among people who are working on related problems are often the most effective way to make intellectual progress, and we need to be able to facilitate such meetings.

Many researchers are finding inadequate the traditional medium of large periodic conferences where formal papers are presented and short responses are given. The need to produce formal papers means that the research usually is reported long after it is completed and the conference setting does not allow for the type of sustained discussion necessary to provide researchers with an adequate understanding of each other's work. We need to explore alternative formats that could solve these problems, which of course are not unique to the security field.

It may be fruitful to bring together scholars with overlapping interests for several meetings a year to allow for discussion that is sustained enough to provide real benefit. Between meetings extensive exchange of views and papers could also be encouraged. On other occasions and subjects, smaller and less structured meetings might provide the intensive exchanges that can deepen the understanding of all participants.

More than is true for many other areas of social science, maximum progress in studying national security requires the involvement not only of university scholars but also of people in think tanks and the government. Many people who are not involved full-time in security studies have special skills, knowledge, and interests that enable them to learn from and contribute to continuing research in the field. Developing institutions and means of communication for tapping such a diverse group is not easy and calls for greater attention than it has received thus far.

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PART

**VI**

**Data and Analysis**



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## Large-Scale Data Needs

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One of the central trends in the social sciences since the 1960s has been a spectacular increase in the size of the data bases employed in research. Rather than a passing developmental phase, that trend can better be seen as a reflection of progress toward improved understanding of human behavior and social processes and as a requirement for continued progress. The need for very large data bases has come as a direct product of growth of knowledge. The foreseeable future is unlikely to bring diminution in the magnitude and complexity of data needs in the social sciences; the greater likelihood is the need for more and still larger and more complex data collections.

The effort to meet large-scale data needs reflects still other indicators of growth and maturation in the social sciences. These include increased agreement among social scientists in identifying areas of primary research importance and the growth of a measure of consensus where methodological approaches are concerned. The creation of new, large, and complex data bases has also involved the development and maintenance of technical and administrative support facilities needed for the full utilization of the data.

The reasons underlying the development of very large data bases can be broadly summarized in terms of four sets of considerations. *One* is interest in monitoring, explaining, or predicting change. This interest requires multiple measurements involving the same or comparable research subjects—individuals, families, organizations, and so on—over extended periods of time. Comparability of subjects is dictated, among other reasons, by the need to differentiate patterns of change that are intrinsic to the research subjects, whether they derive from the impact of environmental factors or from the consequences of cumulative experience and life history. Whether pursued through panel or successive cross-section designs, the study of change leads to larger and larger data bases.<sup>1</sup>

<sup>1</sup>A few examples serve to illustrate this and the following points. The American National Election Study conducted in 1952 comprised over 235 variables and involved interviews with approximately 1,900 individuals. In its entirety, the 1952 study amounts to approximately 1.36 million characters of data. The 1984 study involved over 1,140 variables, interviews with more than 2,250 individuals, and comprises some 5.23 million characters.

A *second* set of considerations lies in the complexity of human behavior. The phenomena of concern to social scientists—human attitudes and behavior, social and political processes, or the functioning of organizations—are explicable only in terms of multiple factors. In many cases, moreover, measurement of each such explanatory factor requires multiple indicators. The consequence is the requirement that data bases include large numbers of variables.

The *third* set of issues derives in part from the second. Since the phenomena of concern to social scientists involve multiple factors, research data bases must include large numbers of cases to support generalization or to sustain adequate analytic controls. Large numbers of cases are also required by frequent concern for small “special” populations—particular ethnic groups or employment categories—and with “rare” events—particular types of crime or particular forms of political participation.

Although of a different order, a process of augmentation creates a *fourth* set of considerations underlying growth in the size of data bases required for research in many areas of the social sciences. Given the highly inductive nature of social science in its present stage of development, research rarely leads to the identification of measures that are no longer needed. Instead, the more important research findings often lead to the identification of additional factors that must be taken into account in explaining social processes and phenomena. This dictates the addition of new measures to data collection efforts while the demands of continuity and comparability dictate the maintenance of existing measures as the new measures are being added.

Individually and in combination, these factors impose a demand for large-scale data bases. It is worth noting that the same factors are appar-

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The series of election studies for the entire period 1952 through 1984 includes almost 10,000 variables, reflects interviews with approximately 28,000 individuals, and amounts to approximately 48 million characters of data. As a second example, the first wave of the Panel Study of Income Dynamics, conducted in 1968, included slightly more than 450 variables and interviewed a sample of approximately 5,000 families composed of about 18,000 individuals. In total the data amounted to over 14 million characters. With the completion of the seventeenth wave of the study in 1984, the data collection includes over 10,000 variables, reflects interviews with more than 7,000 families composed of more than 20,000 individuals, and amounts to over 440 million characters of data. The third example, The General Social Survey, included in the first wave (1972) approximately 120 variables and 1,600 interviews, and amounted to slightly fewer than 400,000 characters of data. Fourteen waves have now been conducted, with the most recent in 1987. The cumulative file includes 1,650 variables, reflects over 21,000 individual interviews, and amounts to 36.7 million characters of data.

ently at work where data to meet the monitoring, bookkeeping, and allocative and reallocative needs of governments are concerned. Here too, the need for comparable data across extended time periods, growth and awareness of complexity, concern for special populations and rare phenomena, and the combined demands of measurement continuity and innovation have led to ever larger data bases. And without elaborating the point, similar processes are clearly at work in large areas of the private sector.

The large-scale data needs of the social sciences bring with them a number of more or less obvious problems. The most obvious problem centers on matters of cost. Massive and complex data bases of the sort increasingly required in the social sciences are costly to assemble, process, and maintain.<sup>2</sup> One consequence is that the large majority of social scientists cannot expect to meet their research needs by collecting data *de nouveau*; such an expectation would be analogous to expecting each physicist to maintain his or her own particle accelerator. Rather, data must be developed and employed cooperatively, and maximal use of data resources will involve the subsequent work of scores of researchers. We will return to this point shortly because it is central to any policy supporting the development and maintenance of large-scale data bases.

### Accomplishments

Important accomplishments have been made in addressing these problems and needs. They include the extension and improvement of governmental data collection efforts and provision of more effective access to the fruits of those efforts. A second consequence has involved development and maintenance of facilities to conserve and provide effective access to data collections. A third and, to our mind, particularly promising development has involved initiation of collective approaches to data collection. Each of these developments is discussed below with particular emphasis upon the last.

#### *Governmental Data*

Use of large-scale data bases is, of course, by no means a completely new development in the social sciences. Various domains within the social sciences have long relied upon the massive data produced by gov-

<sup>2</sup>The costs of the American National Election Studies and the Panel Study of Income Dynamics are both now well in excess of \$1 million per year.

ernments—whether through censuses and other enumerations or as by-products of ongoing bookkeeping and accounting activities—as primary research materials. Here, the importance to the social sciences of data collected through the decennial censuses of the United States or the Current Population Surveys well illustrate the point.

In recent years these data sources have become progressively more useful for research in the social sciences. The data produced by established governmental data collection mechanisms have become steadily more varied and extensive and thus increasingly relevant to growing numbers of researchers. New data collection efforts in new areas have also broadened the relevance of governmental data collection for social research. The National Crime Surveys (NCS), the various health and epidemiological surveys conducted by agencies of the Department of Health and Human Services, and the more recently initiated Survey of Income and Program Participation (SIPP) all illustrate the point.<sup>3</sup>

At the same time, new effort has been devoted to the technical characteristics of data and approaches to data collection. It is our perception that federal data collection agencies now more frequently consult with experts in the academic and private sectors in the design and conduct as well as in the specification of content of data collection efforts. Here, the redesign of NCS and the design of SIPP are again cases in point. While the results of such consultation remain less than fully satisfying, they do look in the direction of improved data and increased utility of data for the purposes of social science research. Their continuation and extension should surely be encouraged.

<sup>3</sup>For descriptions of these data collections see, as examples, U.S. Department of Justice, Bureau of Justice Statistics, *Criminal Victimization in the United States, 1985, A National Crime Survey Report, NCJ-104273* (Washington, D.C.: Department of Justice, Bureau of Justice Statistics, 1987); National Center for Health Statistics, "The National Health Interview Survey Design, 1973-1984, and Procedures, 1975-1983," *Vital and Health Statistics*, series 1, no. 18 (Washington, D.C.: GPO stock number 017-022-00879-3); Joseph E. Fetti and Mary Grace Kovar, "A Multi-mode Longitudinal Study: The Longitudinal Study of Aging," *Proceedings of the Survey Research Section, American Statistical Association* (Washington, D.C.: American Statistical Association, 1987); National Center for Health Statistics, "Plan and Operation of the Health and Nutrition Examination Survey, United States, 1971-1973, in H. W. Miller, *Vital and Health Statistics*, Department of Health Education and Welfare Pub. No. (PHS), 79-130, series 1, nos. 10A and 10B; J. Coroni-Huntley, et al., "National Health and Nutrition Examination Survey—Epidemiologic Followup Survey," *Public Health Reports* 98 (Washington, D.C.: National Center For Health Statistics, 1983), pp. 245-51; and U.S. Department of Commerce, Bureau of the Census, *Survey of Income and Program Participation, 1986: Selected Papers Given at the Meeting to the American Statistical Association in Chicago, Illinois, August 18-21, 1986* (Washington, D.C.: U.S. Department of Commerce, Bureau of the Census, 1987).

Similar effort has been directed to improving access to federal data. Many governmental agencies now provide data in forms that are better suited to the requirements of university-based researchers. Heavier reliance has also been placed upon academic agencies to provide access to data and to facilitate their use. An early example is provided by the labor market experience data generated by the Bureau of Labor Statistics. These data became identified as the "Parnes" data because of the role of Professor Herbert S. Parnes of Ohio State University in reformatting, reorganizing, and documenting the data for dissemination to the research community.<sup>4</sup> The use of the facilities of the Inter-University Consortium for Political and Social Research (ICPSR) at the University of Michigan to process and disseminate NCS and other data, as well as steps toward creation of a data center at the University of Wisconsin to facilitate use of the SIPP data, are further cases in point.

Efforts of this sort can only be applauded. They look toward the kind of cooperation that is needed to meet large-scale data needs in the social sciences and should be encouraged and extended. On the other hand, we can only look with dismay upon the often ill-considered reductions in support for federal data collocation activities that have been proposed and in some cases implemented. These can result only in reduced access to federally collected data and will aggravate the large-scale data needs of the social sciences.

### *Data Archives and Repositories*

It is difficult to establish the first efforts within the academic community to develop nongovernmental data for basic research. It is certain that one of the early efforts was launched by Elmo Roper when he and his colleagues established the Roper Public Opinion Center at Williams College to archive data produced by the various public opinion polls. In the early 1960s the ICPSR was formed to extend and expand the "data archive" concept embodied in the Roper Center. A central activity of the Consortium was the creation of data resources for use in basic social science research.<sup>5</sup>

The resources in the first instance were provided by the growing series of national election studies conducted by the Survey Research Cen-

<sup>4</sup>For a description of this collection, see Herbert S. Parnes, *National Longitudinal Surveys of Labor Market Experiences: Handbook* (Ann Arbor, Mich.: ICPSR, 1984).

<sup>5</sup>For a discussion of the role and origins of social science data archives and a selective list of such facilities in the United States and other countries, see Jerome M. Clubb; Erik W. Austin; Carolyn L. Geda; and Michael W. Traugott, "Sharing Research Data in the Social Sciences," in Stephen F. Fienberg; Margaret E. Martin; and Miron L. Straf, eds., *Sharing Research Data*, (Washington, D.C.: National Academy Press, 1985).



ter (and subsequently the Center for Political Studies) at the Institute for Social Research at the University of Michigan. Given the growing capacity of the computer to manage and process large quantities of data, the Consortium subsequently was able to provide the necessary technical and administrative support to complete a monumental collection of country-level election returns, census data, and congressional roll call voting records extending to the earliest days of the nation's history.

The Roper Center, the ICPSR, and other national and local data archives grew rapidly in the 1960s and thereafter. Their data holdings have become increasingly diverse and now include not only data collected by academic researchers but also extensive arrays of governmentally produced data. In some cases significant efforts are now underway to provide improved access to these holdings. These efforts look toward providing effective remote access as well as index, catalogue, and search capabilities to aid researchers in identifying and locating data suited to specific research interests.

It is important to stress the functions of data archives and other similar repositories in relation to the large-scale data needs of the social sciences. Their function is not limited to conserving and providing access to individual data collections, although these are important functions. These facilities also allow social scientists to combine data collections and to conduct temporal and geographical comparisons in pursuit of research goals that the individual data collections were not originally designed to meet. In this sense, these archival facilities contribute directly to meeting the large-scale data needs outlined at the beginning of this report.<sup>6</sup>

### **"National Resource" Data Collections**

The importance of governmental data for social science research and the role of data archives and dissemination services in meeting large-scale data needs are relatively well known. A third development, the creation of national data resources tailored to the basic research needs of social scientists, is more recent and requires more extended comment. These

<sup>6</sup>The diversity of the data now available to social scientists and the combinatorial possibilities which they present are suggested by the holding of the ICPSR. See ICPSR, *Guide to Resources and Services, 1987-1988* (Ann Arbor, Mich.: ICPSR, 1987). The ICPSR experience provides a further indication of increase in the scale of data collections employed by social scientists. In the course of the 1977-1978 fiscal year the ICPSR supplied slightly over 6,500 data sets which averaged approximately two million characters per data set. In 1986-1987 a little over 19,500 sets were supplied which averaged over 15 million characters per data set.

data resources are multipurpose and continuous in nature and are designed explicitly to support the research needs of scholars who are not directly involved in the data collection effort itself.

Three such data collection efforts have been mounted to date: the National Election Studies (NES), the General Social Survey (GSS), and the Panel Study of Income Dynamics (PSID). The rationale for each is to serve the research needs of one or another subset of the social science research community. These large-scale data bases are created for multiple usage in individual research projects that share, or have in common, a need for particular data. The specific projects are normally differentiated from each other by their need for additional variables beyond those of common interest, but such other variables are incremental or marginal additions to the data that satisfy shared needs. Given the multipurpose characteristic of the data collections, they tend to be multidisciplinary in orientation and content. Each is defined by a substantive problem area, not by a discipline, although the problem areas are themselves each somewhat more central to one social science discipline than to others. The problem-centered definition of the research enterprise encourages the creation of data normally identified with all of the disciplines that are relevant to the problem.

In addition to being multipurpose and more or less multidisciplinary, each of the newly constructed large-scale data bases is also defined to represent the parameters of large populations. The data collections are national in scope and embody relatively complete coverage of all elements of the populations that are relevant either for the testing of theory or for the application of theory to an entire national population.

The national resource data bases created thus far are also longitudinal in design. The data and the data collection activities extend over considerable temporal spans with continuous or serial (repeated) data collections. They are intended to embrace phenomena that occur only over a long time span (reflecting maturation or socialization processes) or to study some aspect of change that occurs only through time (such as changes in attitudes or values), or to study the consequences of changes in environment or context (such as occurs in the study of electoral behavior with changes in national conditions of prosperity and peace, or changes in candidates).

Given the multipurpose, large-scale, longitudinal nature of these data collections, they can be properly described as supporting programmatic research activity. They sustain research that must necessarily evolve as knowledge accumulates, measurement deficiencies are noted and overcome, and theory is elaborated. The unique feature of these data bases is that their content and design are specified by a collectivity of re-

searchers. Those responsible for overseeing the development of the data resources represent constituents who are the members of the otherwise "invisible colleges" of scholars whose individual research interests constitute portions of larger informal programs of research focused on problems of continuing, mutually agreed upon, importance.

These data bases reflect, then, a measure of consensus within each of several research communities as to research problems that are considered of sufficient importance to merit an investment of public funds. They reflect, as well, agreement on the data collection and methodological approaches that are appropriate to those problems. And they also reflect willingness to cooperate in the design and specification of content of basic resources to serve mutual research interests.

### *Problems of Establishing Policy to Govern the Development of Large-Scale Data Bases*

The work group benefited greatly from the transcriptions of discussions of the parent committee and from other communications we received from committee personnel, including a most helpful letter from Dean Gerstein. In attempting to respond to the many suggestions and observations that have been made, we have been led to conclude that there are some categories of very important questions that we do not feel can be usefully addressed by our work group, either because they are not intrinsically or uniquely related to the development of large-scale data bases or because the questions are posed at a level of detail appropriate only for substantively informed research scholars.

Any data collection that is not planned or justified as a multipurpose, multiuser national data resource should be evaluated in accord with standard criteria of the relevance, need, cost, or feasibility of data collection for the conduct of a specific research project. Only the proposal writer, or relevant research peers in the field, can speak to such substantive issues as the merits of using or adapting an existing data base rather than mounting a new data collection; using successive cross-sections or a panel design for longitudinal analysis; determining how long a panel or time series should be maintained; assessing the feasibility of matching and merging existing files; or determining whether farmers or farms constitute the desired unit of analysis. Clearly many such questions can be answered with the assistance of substantive, methodological, or technical expertise. The experience of professional information specialists, computer scientists, data archivists, and social science methodologists can be enlisted when the detailed specifications of the research proposal are known. Such questions are not unique to large-scale

data bases. Nor are they questions that can be answered in the abstract. The answers should be provided by the proposers of the research project and would properly be assessed in the normal process of peer review.

It also seems to us virtually impossible, and probably inappropriate, to specify the relative or comparative scientific value of the various multipurpose large-scale data bases. Instead, we believe that each must be judged for its contribution to the field or problem area it is intended to support. A positive judgment will certainly play a part in specifying the resources needed to support research in the problem area at issue. But the larger question of the relative scientific value of the research in a given domain can scarcely be judged by the data base used to support that work, except insofar as the ultimate research project is deficient because of deficiencies in the data base. If a given problem area merits high priority, for whatever reason, and if significant progress depends on creating a new data base—large-scale or otherwise—then it would seem to follow that the data base thereby acquires a high priority in the allocation of resources. At the same time, of course, the costs of creating such a data base, or anticipated substantive or technical problems that would limit its usefulness, may well modify the priority and resource allocation otherwise associated with the problem area. However, it is difficult to imagine a resource allocation strategy that would rank or otherwise compare data bases removed from separate and independent assessments of the research they support.

Nevertheless, experience with these evolving enterprises does suggest a number of criteria by which they, or others proposed to the committee, may each be judged without attempting to compare their relative scientific utilities. Unhappily, however, to our knowledge no systematic inquiry has focused on any of the large-scale data bases now in existence. Consequently, the best that can be done at this stage is to enumerate a host of considerations that experience suggests should be articulated in any assessment of a large-scale multipurpose generation of resources, either in existence or proposed.

### *Suggested Criteria for Continuation and Establishment of National Resource Data Collections*

Considerable discussion has been directed to questions of how decisions are to be made concerning initiation and continuation of national resource data collections. We agree that these discussions raise serious and highly troublesome issues. The size of the investments and the continuing obligations involved certainly exacerbate these difficulties. At

the same time, it seems to us relatively easy to specify criteria that would almost inevitably enter into such decisions. Difficulties will obviously arise in applying these criteria to specific cases calling for concrete decisions, but the difficulties themselves are surely not intrinsically different from those always confronted in allocating scarce research resources among too many meritorious research projects.

### *Research Value*

Surely the first and primary set of criteria to be used in making such decisions concern the importance of the research problems that the proposed data collection will address and the likelihood of significant research results. Discussions in this area often seem to presume that a magic touchstone, if found, would infallibly discriminate important from unimportant research problems and provide certain protection against all but major research payoff. We know of no such touchstone. All that we can suggest is that the explanation of and justification for the proposed data collection be evaluated and judged by social scientists who are informed and active in the field. We recognize that the peer review system is far from perfect. We recognize as well that scientists who are active in a particular field of specialization are also likely to consider worthy of support those activities that further research in that field. At the same time, social scientists do strive for objectivity, and in the case of competitive claims on scarce resources, third parties have at least an opportunity to exercise a measure of neutrality in judgment.

### *Cost-Benefit Considerations*

Although potential quality of work and status of the likely users are necessary first considerations, a large investment in a new data base may also be tested in terms of the numbers of research scholars who will use the data. Numbers are important because the costs of large-scale data collections must be judged, at least in part, in terms of the cost per analytic project using the data. As a matter of rational perspective it seems inappropriate simply to set the cost of a large-scale data base against the typical cost of an individual project; the large-scale collections should always be designed for use in a host of individual projects carried out by scores of individual investigators. The cost-benefit ratio must be determined in part by the numbers of those who benefit. The experience of the three ongoing projects (GSS, NES, PSID) is clearly that data collection costs for the research *output* from the projects (papers, articles, and books) would have been many times the actual cost

of the existing enterprises if the data resources had been funded on a project-by-project basis. Although it is true that there are very real economies of scale in data collection, the truly significant economies of scale related to the costs of large-scale data collections have to do with the scale of use.

Consequently, it is our view that large-scale data bases are involved in a "big science versus little science" contest only in terms of the intellectual scope of the research. The human scale of the analytic projects using the large-scale data base is not different from any other—it is always individuals or partners or small groups of close collaborators. The question is how many such analytic units are all supported by the single large data collection? The competition for resources is really between the many individuals who can share a data base and the many individuals whose research cannot benefit from such sharing.

There are also real economies in the scale of collection. It should be recognized that the unique needs of each analytic project that is dependent on a large-scale, multipurpose data base simply add an incremental or marginal cost to the price tag for collecting the large portion of data of common interest. In the case of the American National Election Studies, the line is sharply drawn between the core data, which constitute the continuing time-series element intensively reviewed every two years by the board of overseers, and the new data. The latter are introduced to upgrade quality or extend the content by responding to evolving research needs that are a part of the research programs supported by the ongoing data collection. Given the multipurpose status of virtually every datum collected, it is very difficult to press the cost-benefit argument to allocate the comparative costs for the larger body of core data and the smaller body of new additions. The important point is that prorating all costs across all analytic projects clearly results in a relatively modest cost for data collection per unit of completed research.

There is a third way in which the continuing large-scale data projects provide real economies where the unit of cost is the individual analytic project. The continuing nature of collections with multiple objectives lies in the standardization that naturally results for a whole host of solutions to problems of method, concept, and measurement. As individual research projects are more often supported by centralized data collection activities, the reduction in sheer number of data collection efforts enhances the likelihood of greater coordination and standardization across different research domains. In a similar vein, as data generation is closely reviewed because of its relevance for large numbers of data analysts, high standards are established for the quality of data, the uniformity of measurement, and efficiency in the methods of data collec-

tion. All of the current major data collection efforts are setting new standards for data quality in their respective fields.

### *Potential Problems*

Except in the unlikely event that a large-scale data base can be established with a fixed set of specifications that never change, proposals to develop new data collections should anticipate a series of interrelated problems of research statesmanship and administration. For example, one pressing preoccupation of the Board of Overseers for the National Election Studies (NES) has been defined by the alternatives of providing innovative leadership or predictable service to its research constituency. The NES, like the GSS, was established to ensure constituencies of continuity in their developing time series. However, since the time-series data are used for research in problem areas that are very alive intellectually, the demand for innovation and supplementation in study content and improvement in study design also calls for a response if longitudinal collections, each collection should develop a professional and highly experienced staff of administrators and technicians. This not only reduces the start-up costs that would be associated with many separate data collections but it also permits the efficiencies that result as experience is gained.

### *Substance and Method*

Insofar as the problem area and the relevant research permit, the creation of a large-scale data collection as the empirical foundation for programmatic development also provides an opportunity for experimentation with research design, as well as continued upgrading of the quality of data. Improving collection methods and measurement techniques may and should become an integral part of the research enterprise. Long-term funding and longitudinal base development make it feasible for scholars to devote continuing attention to the interrelated problems of method and substance. Social science is long on lip service to the notion of the interdependence of method and substance, but traditionally it has suffered from the very limited capacity of scholars to work both sides of the street with the data they collect. If data collecting methods and measurement techniques are at all open to improvement or modification, plans for attention to them should be a part of any overall plan to develop new data resources. Large-scale data collection projects, in part because of their scope but in part because of their multipurpose character, are uniquely valuable as vehicles for testing and improving research methodology.

A final benefit to be derived from mounting large-scale data the quality of the research is to be sustained. NES, in particular, has been characterized by a great deal of experimentation in measurement and by the addition of new content. Consequently, the Board of Overseers and the Principal Investigator are constantly engaged in balancing the need for continuity with the need for change. This, of course, demands close rapport between the Board of Overseers and other leaders in the research community to ensure the greatest possible responsiveness to the researchers' needs. At the same time, there is the real possibility that innovations responding to "state of the art" or "cutting edge" challenges may outstrip the capacity of many of the research community, and the overzealous response to demands for change may be at the expense of research that depends on stability and predictability concerning the data base.

Where the development of time-series measurements is a prime consideration, the governance of ongoing data collections will have to cope with the problem of maintaining the integrity and relevance of data, balancing, for example, calls for improvement in measurement against arguments for continuing comparability of measurement. However, given the speed with which social science is advancing, plans for long-term data collection efforts should, in most domains, explicitly include opportunities for improving the quality of measurement. The NES employs the device of a biennial off-year pilot study as a means for testing recommendations for new or changed measures. These carefully designed studies produce data of a quality adequate to sustain scientific journal publication, but their purpose is to provide a systematic basis for Board judgment on both the statistical and the substantive properties of measures that scholars propose to add to the data collection effort.

The cost of improvement extends to the problem of introducing new measures to replace old measures in any given time series. In principle, one might expect a period of overlap in which the differences between old and new can be calibrated through simultaneous data collection. However, this process may well be subject to substantial problems in ensuring comparability of measurement conditions. In any event, achieving comparability inevitably means some redundancy for measurement for the specific concepts in question, a redundancy that will always occur at the cost of forgoing collection of alternative data that could be included in the data collection instrument.

Where significant changes in data collection are a possibility, the relevant decision makers associated with a large-scale data collection must balance conceptual desirability, technological feasibility, cost, and likely level of use or impact on the research community. There is no magic



formula by which advice may be offered on such problems, but those proposing new data collection efforts should demonstrate their awareness that the world they intend to serve may change with disconcerting speed, and, through their control of resources, they must inevitably play a major role in determining the rate as well as the direction of change.

A final substantive topic of somewhat different order should also be noted. Although the family of large-scale data collections would seem to offer many opportunities for cooperation and collaboration across collections, the problem of limiting the autonomy of those making decisions on behalf of any given set of researchers in favor of some more general goal that might be served through "coordination" is not easily solved. The experience of NES and GSS has been that apparently minor differences characterizing some of their specific measurement operations appear to be major differences in the eyes of those using and designing the studies, and there has not been a high degree of success in coordinating measurement efforts. As with the problem of simultaneous use of old and new measurements, the response to suggestions for closer cooperation in specifying study content almost always calls for sacrificing other content in order to provide space in the data collection instrument for the cooperative venture. Although commitment to being multidisciplinary and favoring interproject cooperation sounds highly desirable, the very particular demands of research specialists usually ensure that the more general purposes that might be served get short shrift.

### *Governance*

Although many of the opportunities and problems related to the development of large-scale data bases can and must be discussed in the abstract terminology of research design methodology and theory development, the actual resolution of problems, or the fruitful execution of data collection, depends on a number of identifiable organizational arrangements. For example, the proposal to serve a community of scholars with a new data collection should contain explicit provisions for the representation of the potential users' needs. For the three major data operations now underway, this has taken the form of national "boards of overseers" who are trustees to their communities of users. They are charged with representing their colleagues' interests. In the case of the NES, its Board of Overseers has actively sought the participation of large numbers of the user community in the design of studies. Scores of scholars have been active participants in the conferences, in the conduct of pilot studies, and in the subsequent design of major data collections. Such participation of the user community may not be essential, but it does

provide one means of ensuring meaningful contact between the community and the data collection effort by which it is served.

Whatever form of organizational superstructure is designed to make authoritative decisions on the design and content of a multipurpose data collection, the roles assumed by the principal investigator(s) and the boards of trustees pose somewhat unusual problems for the individuals involved. Members of the governing boards may well have experience in exercising their individual professional judgments on the (proposed) work of others as editorial referees or members of review panels evaluating proposals. The role as overseer or trustee for a continuing data collection, participating in collective decision making, may well add dimensions of complexity, particularly if some part of the data collection is germane to the board members' own research. A somewhat different problem may emerge from the tendency of board members to become special-interest pleaders on behalf of a particular segment of the user community. Selection of board members, therefore, is not only conditioned by the need to reflect the full intellectual diversity of the users; selection must also be made in mind of the necessity of avoiding self-service or special pleading that could transform judgments on behalf of the external community into personalized and politicized argumentation. It is unlikely that such narrow perspectives could long endure without jeopardizing the overall venture, and the task of achieving agreement, if not consensus, on project decisions certainly becomes more painful in the presence of such nonprofessional if thoroughly human behavior.

The challenge to the principal investigator is more unique. This is particularly true if the precedent established by the three ongoing data collections is followed. Each principal investigator is an established research scholar. Each has been accustomed to preparing research proposals that reflect their own intellectual interests and their own professional judgments. In their new roles they are not simply constrained but need to participate as, at most, first among equals in collective decision making. Their ultimate goal must be that of serving a broad community of research interests rather than their own. The demands of the role may be heavy, in which case opportunity costs are an immediate consequence. It is, of course, possible to imagine chief responsibility residing in a professional research administrator who has no personal investment in the content of the research he or she is facilitating. However, given the dynamic nature of the research enterprise, it has seemed appropriate to the National Science Foundation to ensure that its national resource development efforts are headed by established scholars who are chosen because they have played major roles in the research programs that have now evolved into national resource efforts.

A further twist on the problems posed by these new roles in social science direction and administration is provided by the shared concern that participation in the design and conduct of the data collections not give undue personal advantage to those most directly involved. As a matter of policy the NES does not fund the research activities of the Principal Investigator, members of the Board, or members of the staff. Moreover, established policy dictates that none of these individuals has access to the data, beyond that required for efficient administration of an ongoing collection, until the data are in the process of being distributed to the entire universe of users. Inevitably, those involved in data collections relevant to their own interests have something of a head start in exploiting the data through the very process of shaping the data collection. Nevertheless, given the above constraints and the policy of distributing the data-collecting instrument as soon as it has been finalized and before data collection has been completed, the principle of equity among all those needing the data for their research seems reasonably well protected. The other side of the coin, however, and the one emphasized here for its implications for choosing principal investigators and boards of trustees, is that an unusual series of constraints are placed on the individuals responsible for the development of the new data resources.

A final thought in this general domain relates to the problems posed for the host institution. On the one hand, it is clear that the National Opinion Research Center and Michigan's Survey Research Center and Center for Political Studies all benefit from their identification with the major data collections they are carrying out. At the same time, they are organizationally responsible for providing the intellectual leadership and the administrative and technical personnel. Once the novelty of a new enterprise wears off, the opportunity costs of maintaining that enterprise become visible. As principal investigators change careers, retire, or otherwise reduce their commitment, the task of finding a replacement falls on the host institution. There are, in short, a variety of major consequences for the host institutions as well as for the individuals engaged in the enterprise. As with many other problems discussed in this report, it does not seem possible to recommend particular policies that will solve these problems, but certainly the inauguration of any new large-scale, expensive, longitudinal data collection should not be undertaken without some clear awareness of such ramifications.

Finally, we should note quite different sets of problems that arise from the actual conduct of data collections. There is a clear need for an organization of administrative and technical talent to carry out the many tasks associated with designing and executing a data collection. The centralization of data collection not only makes possible but makes manda-

tory the presence of methodological and technical experts on the staff. This becomes particularly important if the data collection anticipates significant changes in content, measurement technique, research design, or data collection methods. There need not be a large superstructure charged with the details of actually mounting a data collection, but there must be a staff composed of highly professional administrators and technical experts. It should be noted in this connection that it is quite possible to separate the functions of content and design specification from the actual task of data collection. Although all three of the ongoing enterprises have their own in-house data collection capabilities, it is quite possible, particularly with the emerging technologies involved in computer-assisted telephone interviewing, to contemplate decentralized data collection that nevertheless performs in accord with strict controls imposed by the central staff.

Beyond establishing the organizational links between the data collection enterprise and the user community, and in addition to the development of the administrative and technical staff responsible for the data collection, every viable large-scale data base must have an efficient and effective means of providing access to the data by the user community. Although the development of computer networks and the capacity for remote access is very real, it seems likely that the data archives must continue to be enlisted for the widespread dissemination of data. It is possible, of course, that special-purpose capacities can be developed, as with the Wisconsin Center, to enhance access to collections such as the SIPP data. The other option is to turn to existing archives that are proficient in providing the documentation, formatting, organization of data files, and data management essential to the widespread distribution of data to researchers located in very different institutions with very different data processing and computational facilities. The GSS data are distributed through the established archives of the Roper Center and the ICPSR, which maintain contact with the hundreds of institutions where the researchers using the data do their work. The ability to respond to the technical specifications of each user's installation is important for ensuring widespread access to the data. In the case of NES, the ICPSR archive not only disseminates the data but indeed plays a major role in providing the documentation and establishing data format and file organization conventions that maximize researchers' access to the often complex collections of data. Although the present cost of data dissemination is rather cheerfully borne by the archives that service the ongoing major data collections, it is possible to envision a data base with a more limited clientele and a product so expensive to handle that the cost of dissemination becomes yet another real factor in planning.

### Further Needs

By far the largest portion of this report has been directed to the initiation and development of national resource data collections and to the opportunities and problems presented by this relatively new approach to meeting the large-scale data needs of research in the social sciences. This emphasis has seemed justifiable and, indeed, necessary because this approach is relatively new; as a consequence it is, we believe, both unfamiliar and a matter of some controversy.

We do not wish to suggest that national resource data collection efforts now constitute, or are likely to constitute, the sole means by which social scientists meet large-scale data needs. The massive data resources collected by governments will remain of primary importance to social science research. Social scientists will continue to employ in the future, as they have in the past, extensive collections of voting records, budgetary data, and other "process produced" materials. By the same token, they will continue to employ and combine specific data collections held by data archives to carry out comparative research and address research goals that the data were not originally designed to serve.

But whatever the source of data—national resource, governmental, process produced, or the combinatorial resources of data archives—the direction of development in large areas of social science research is toward reliance upon larger and more appropriate data collections. This trend brings with it further needs, three of which we note in conclusion here.

It is sometimes assumed that once data are collected and are accessible from data archives, a national resource collection, or governmental sources, the costs of research have been met and research can begin without further expense. In fact, research use of these large-scale collections inevitably involves further costs—salaries, computer use, and the like—that are beyond the resources of individual researchers. It cannot be assumed, therefore, that the availability of relevant data automatically assures that important research problems can be pursued. The availability of basic data resources does yield important cost benefits, but the additional costs in time and money of meaningful analysis of such resources is often high. Thus further individual support, albeit of lesser magnitude, will sometimes be required to allow full realization of the research opportunities which these resources afford.

A second and sometimes forgotten need has to do with management and maintenance of data resources. Massive data resources usually require an investment in processing, organization, and documenting if they are to be disseminated for effective use. Maintenance of data col-

lections for continued use also involves a cost, particularly, as in the case of national resource data collections, as they accumulate with time. For example, as data collections grow in size, new means must be provided to allow researchers to search and retrieve specific data without being required to search manually through thousands of pages of printed documentation.

A further need lies in the area of computational resources. Available computer hardware and software are already inadequate to meet the analytic, management, and retrieval needs of some of the larger data collections that are now available. As the data collections employed by social scientists continue to grow in size, the adequacy of available computer facilities will be still further strained. In this general area the development of supercomputers is sometimes treated as a virtually certain panacea for requirements of users of very large data bases. At this writing, however, it is by no means certain that the supercomputer will save us. In light of the current direction of development, supercomputers will have capacity to carry out very large numbers of calculations at very high speeds, a capacity that will hold great advantages in meeting the mathematical needs of the physical sciences. But whether this course of development will provide the capacity to manipulate large quantities of data (the input-output capacity) to meet the requirements of the large-scale data bases of the social sciences is by no means certain.

## Appendix

### *Current National Resource Data Bases*

As of 1987, three national resources data bases were being developed with the support of the National Science Foundation. They bear description to illustrate various features that distinguish this class of data resources from other large-scale data collections that may also serve basic research needs. All three are nongovernmental in origin. All have emerged from large, continuing projects that had a substantial history prior to their transition to the status of national, multipurpose resources. All have been expanded or elaborated to serve research goals other than those that defined the original projects. They are, thus, public resources that are being developed with a more or less substantial transformation from their content as private research projects.

### *General Social Survey*

The General Social Survey originated in the 1970s out of the National Opinion Research Center's studies of the quality of life. These studies,

originally designed and executed by research scholars at the University of Chicago, have now been continued for a crucial monitoring of social change in the United States. The studies constitute a time series, with annual collections beginning in 1972 and, with the exception of 1979 and 1981, continuing to date. By mid-1985 the cumulative data file contained some 350 variables for 18,000 respondents collected in 12 national surveys. Although the data are of primary interest to sociologists, they cover a broad range of topics including social stratification, the family, religion, ethnicity, race relations, freedom of speech, national morale, and crime. The data are all in the public domain and are disseminated by the Roper Center, ICPSR, and other archives.

This program of research on national social change serves as a prototype for a multinational program of coordinated activities in England, Germany, and Australia as well as the United States. International cooperation has increased in recent years and will provide the basis for rigorously comparative international studies, in addition to the nationally oriented work that has gone on in the past. In general, the result of this program is to enhance understanding of social change, and rates of change, both nationally and differentially by social groups.

The General Social Survey staff has identified more than 1,000 books, articles, and papers—individual research enterprises—using GSS data. GSS is under the direction of two co-principal investigators and a fifteen-member board of overseers drawn from the national community of sociologists. It has a permanent institutional base at the National Opinion Research Center at the University of Chicago.

#### *National Election Studies*

The series of American National Election Studies is conducted through the University of Michigan Center for Political Studies. It grew out of the programmatic research on national electoral behavior that had been sustained by an unbroken series of biennial national election studies beginning in 1948. Following the 1976 study, the organizational format was transformed along with a commitment to long-term support authorized by the National Science Foundation Board.

Although the NES collection is of primary interest to political scientists, the range of topics included in the very large data collections associated with each national election provides source material for historians, social psychologists, and sociologists as well. The topics include studies of stability and change in group identifications; the formulation of social, economic, and political attitudes and perceptions; citizen participation in both electoral and other political activities; the role of values and events in altering policy preferences, and impact of the mass media on popular perceptions of political leaders. Although the initial

purpose of the data collection was to "explain the vote," the continuing elaboration of the nature of the antecedents to the vote decision has led to expanding theoretical developments concerning the interrelationships among the many categories and classes of explanatory variables.

With the beginning of NSF support the National Election Studies also embarked on a program of experimentation with methods and design. This program has included a major methods comparison in the use of the telephone as opposed to face-to-face interviewing; a design to test the comparative advantage of the use of panels or successive cross-sections for longitudinal analysis; major experiments with the continuous monitoring of change in political opinions and beliefs; and the use of supplementary contextual data for the elaboration of interpretations based on survey interviews.

The data are immediately placed in the public domain. The commitment to serving external constituencies of research scholars is highlighted by a long-standing policy that prohibits anyone involved in the data collection from having access to the data until they are being distributed to the entire national roster of scholars for whom the resources were developed. Either in its original guise as a Michigan project or in its new form as a national resource enterprise, the election studies have served as a model for research programs in Australia, Britain, Canada, Denmark, Germany, India, Japan, the Netherlands, Norway, Spain, and Sweden. These studies have had a major impact on the systematic study of public opinion and voting behavior, both in the United States and abroad, but they continue as a unique source of data for the elaboration of social science theory that is only incidentally focused on the voting decision as the continuing principal dependent variable.

As with the General Social Survey, the American National Election Studies are directed by a principal investigator supported by a permanent professional and technical staff under the executive direction of a ten-member Board of Overseers. Board members are active participants in every phase of decision making on study design and content. Their work is supplemented by large numbers of outside researchers in the design and content specifications for each new individual data collection. The American National Election Studies have in fact become a model for the cooperative design of research that now serves both the national and the international communities of research scholars.

#### *Panel Study of Income Dynamics*

As with the other examples, the Panel Study of Income Dynamics (PSID) originated as a research program, in this instance a program of the Economic Behavior Program of the University of Michigan's Survey Research Center. The design of PSID is extremely complex, involving a



continuously expanding panel of family units that, now in its fourteenth year, poses formidable problems as well as unprecedented opportunities for the study of many kinds of microeconomic behavior. Given the nature and the design of the data collection, the range of topics with multidisciplinary interest includes the national labor supply, occupational and residential mobility, family composition and change, fertility, child care, and retirement. Although it has a fixed institutional location at the University of Michigan, with a permanent staff, it is governed by an external board, which regularly reviews staff decisions and accomplishments.

All three of these resource development efforts are still in flux, but all three have demonstrated the feasibility of developing data resources to serve extended arrays of individual research interests through large-scale data collections that are centrally planned and administered.

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### Selected Research Topics

Selected research topics calling for new data archives or data banks, as recommended to the Committee on Basic Research in the Behavioral and Social Sciences, included the following (among more than 40 relevant suggestions):

Panel Data for Longitudinal Surveys: Ivor Berg, University of Pennsylvania

Socio-Economic Modeling and Forecasting: Kenneth Land, University of Texas

Applied General Equilibrium Analysis: Herbert Scarf, Yale

Life-span Human Development: M. Brewster Smith, University of California, Santa Cruz

A Center for Longitudinal Behavioral Study: Robert B. Cairns, University of North Carolina at Chapel Hill

Longitudinal Studies of Social Development: Robert B. Cairns, University of North Carolina at Chapel Hill

Life Course and Aging: Glen H. Elder, Jr., University of North Carolina at Chapel Hill

Research on Saving Behavior: F. Thomas Juster, University of Michigan

The Aging of the Baby-Boom Cohorts: Denis F. Johnston

Household Panel Data for Low Probability of Events: Eugene Smolensky, University of Wisconsin-Madison

Basic Behavioral Data in Many Cultures: H. Russell Bernard, University of Florida

Time-Use in Non-Market Settings: Denis F. Johnston

Comparability of Social Statistics Over Time: Mary G. Powers, Fordham University

Utilization of Social Science Archives: Lise Menn, Boston University

Improving the International Comparability of Selected Social Statistics: Denis F. Johnston

Data Development for International Research: Dina Zinnes, University of Illinois

Subnational Politics in Industrial Societies: Samuel C. Patterson, University of Iowa

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## Measurement and Scaling

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The social and behavioral sciences, like all sciences, depend upon experimental and observational discoveries as the "raw material" for increased understanding. The gathering of data is, however, only the first step in the quest for understanding. The data must then be analyzed and interpreted with the aid of theory. The task of theory is to develop models—often mathematical or biological, and in more recent years, computer models—for the mechanisms or processes that underlie the phenomena, to deduce, or in some cases to calculate, the properties of the models, and to test these properties through comparisons with the observations or experimental results. When such comparisons allow the acceptance or rejection of the model, our understanding has advanced.

The construction of theories is typically based on the results from mathematics and incorporates assumptions about the measurement and representation of the phenomena under consideration. Thus, measurement theory necessarily plays a central role in all of science.

Measurement theory has been an active and productive field in the last two decades. Mathematically minded social scientists, philosophers of science, and mathematicians have made many new and important contributions to the scientific literature.

There are three fundamental concepts in the modern theory of measurement. The first is that of *representation* of an empirical structure by a numerical structure: The empirical entities are represented by numbers or vectors of numbers, and the observed empirical relations are represented by relations among the numerical objects. The second is that of *uniqueness* of a representation. When several equally good representations exist for an empirical structure, the researcher is interested in describing how these are interrelated, and the full specification of such interrelationship describes the "scale type" of the measurement process. The third concept is that of the scientific *meaningfulness* of propositions stated in numerical form. Put another way, it is focused on the problem of describing precisely which mathematical statements reflect intrinsic characteristics of the empirical structure. All three areas are currently in a very productive stage.

As early as 1963, Suppes and Zinnes provided a readable account of measurement theory based on what was then known. Many important

discoveries, reformulations, and generalizations have recently fleshed out and increased our understanding. It seems likely that this momentum will produce further breakthroughs.

Scaling might be thought of as the somewhat more applied complement to measurement theory. It has provided a wide variety of models and procedures for the representation and analysis of data, with impressive, cumulative developments during the last generation. The rate of progress has recently accelerated so that many of the most important contributions have come in the last few years. As in measurement theory, the most important discoveries have been the result of a more precise mathematical understanding of the underlying structure of the models, which has allowed what were formerly thought of as rather disparate models to be condensed into a very small number of comprehensive, elegant mathematical statements.

Progress in measurement and scaling has been paralleled by an increase in understanding in fields and applications that are closely allied with or are particularly heavy users of measurement and scaling. This is especially true in disciplines that collect information from respondents in natural settings. The entire field of survey research is one such example. Another includes advancements in anthropological and sociological field studies based upon structured interviewing. The field of mental assessment, which has a long and venerable history in social science, has both contributed to and benefited from advances in the field. We will conclude the substantive part of this report with an applied example of an educational assessment program at the national level.

In this report these developments are summarized under the following headings: (1) measurement, including representation, uniqueness, and meaningfulness; (2) multidimensional scaling with an example; (3) survey research and structured interviewing; (4) National Assessment of Education Program as an applied example; and (5) recommendations.

## Measurement

### *Representation*

The theories of most basic sciences are stated in terms of quantitative models. One goal of the theory of measurement is to make explicit the qualitative assumptions that give rise to quantitative models. Though this sounds like a very simple task, many of the best minds in mathe-

matics and other sciences have grappled with its numerous unexpected complexities and subtleties for hundreds of years. It turns out, unanticipated by some, that the solutions adequate for the physical sciences may not always—or even usually—suffice for the unique needs of the social and behavioral sciences. As a result of these special needs, mathematicians and mathematically oriented social scientists have devoted a considerable effort to the problem of representation and uniqueness in measurement theory. We are in an era of rapid progress in this highly technical and complex field. We shall attempt a very brief description and summary of the ideas involved.

The first step is to state the qualitative assumptions in a precise way. Often this takes an axiomatic form. One might think of the qualitative assumptions to be the empirical laws that govern the empirical situation. There may be many ways of formulating such laws, so it is often difficult to make an optimal choice of empirical axioms. Once a set of axioms has been decided upon, the mathematical task is to relate these empirical assumptions to an appropriate quantitative structure. Mathematicians refer to such a relation between qualitative and numerical structure as a structure preserving mapping—an isomorphism when there is a perfect preservation of structure and a homomorphism when there is some loss of information. The set of such mappings forms what is called a scale of measurement.

### *Uniqueness*

How mappings within a scale are related to one another is called “the uniqueness problem,” and it is central throughout measurement and science because it profoundly affects the ways in which we can summarize scientific information. In particular, it is closely related to the structure of units of measurement found in physics. Two major findings illustrate recent research results in the area. First, the possible types of one-dimensional or scalar measures that are useful in science are necessarily very limited. Current indications are that on logical grounds the kinds of possible scale types for highly symmetrical qualitative systems are necessarily limited to ordinal, interval, ratio, and a few others that probably have little, if any, applicability in science. It is not yet clear what can arise and what will be useful for structures that lack such symmetry—an example of which is any probability structure. Second, once a type of scale is selected (or assumed) for a qualitative structure, then a great deal is known about that structure and its quantitative models.

### *Meaningfulness*

The concept of meaningfulness is potentially among the most useful in the modern theory of measurement. Results about meaningfulness have immediate application to assessing average performance, importance ratings, indices of consumer confidence, psychophysical scaling, statistical tests, modeling in decision making and social structure, and many other problems. The most remarkable development probably concerns the concept of meaningfulness of quantitative scientific statements. Not all mathematically valid expressions about a quantitative model are deemed acceptable by the scientific community as even potentially reflecting lawful relationships among empirical variables. Science has generally used as "laws" only those expressions that do not depend on what type of mapping within a scale is used to measure the empirical phenomena. In physics textbooks, for instance, the fundamental laws are typically quoted without any specification of the units (e.g., "the ratio of force to mass times acceleration is a constant"). In physics, such laws are spoken of as "dimensionally invariant." In measurement theory, such expressions are said to be *meaningful*, with dimensional invariance an important special case.

It is commonly recognized that it is not meaningful to say (1) "John weighs 10 more than Bill." This is because the truth of the statement depends on what scale that weight is measured in, that is, the statement may be true if weight is measured in pounds, but false if measured in kilograms. However, it is meaningful to say (2) "the ratio of John's weight to Bill's weight is 1.10," since the truth value of that statement does not depend on the particular ratio scale used to measure weight. However, in ordinary measurement of temperature (Fahrenheit, Centigrade), it is not meaningful to say (3) "the ratio of today's temperature to yesterday's is 1.1," but it is meaningful to say (4) "the ratio of difference between today's temperature and the freezing point of water to the difference between yesterday's temperature and the freezing point of water is 1.10." The reason why (2) is acceptable and (3) is not is that the measurements of weight and temperature have different abstract mathematical properties (in particular, weight is a ratio scale and temperature is an interval scale), and meaningfulness must necessarily reflect mathematical properties of the measurement. Such physical examples are well known and transparent. However, in the social sciences, the problem is usually much less transparent.

Consider the common problem of aggregating rater scores. At a sporting event, several judges assign numbers to contestants, and a rule for aggregating these numbers determines which contestant has per-

formed best. For simplicity, assume that each judge has a ratio scale for measuring performance. What is a reasonable rule,  $R$ , to assign an overall score to contestants so that those with higher scores are considered to have performed better than those with lower? There is much research in social science to show that there are many theoretical and practical problems in trying to get the judges to coordinate "their scales" (the problem of interpersonal comparison of utilities), so let us restrict our attention to rules,  $R$ , that do not assume that effective coordination of judges' scales has taken place.

In analogy to the previous examples, let us say that a rule is meaningful if and only if it produces the same ordering of contestants no matter what particular ratio scale each judge uses in outputting his or her score for contestants. (It is, of course, assumed that each individual judge uses the same scale for all contestants.) Under these assumptions, it can be shown that one typical rule employed is not meaningful, namely, giving as an overall score the arithmetical mean of the judges' scores. Interestingly enough, there is a rule that is almost never employed despite the fact that it is meaningful and takes full advantage of the assumption that the judges have separate ratio scales, namely, the geometric mean of the judges' scores. This simplified example shows how the counterparts of certain basic and transparent ideas of physical measurement still remain fundamental but become less transparent and in general require far more subtle analysis in social science research.

Once recent review of the field of meaningfulness summed up with the following words:

We have seen that the theory of meaningfulness has a wide variety of applications. These applications can affect the way we analyze data (for instance in using geometric means rather than arithmetic means). They can help us to understand what claims we can make on the basis of empirical evidence (as with performance measures). They can help us avoid pitfalls (such as using improper index numbers or correlations). They can help us to build models describing the outcomes of experiments (as in the psychophysical situation). They can help us to analyze models (as in pulse processes). Finally, they can help us understand the nature of scientific laws (as in dimensional invariance). The potential applications of this simple idea of meaningfulness are only beginning to be made. [Roberts, 1985]

### **Multidimensional Scaling with Example**

As in measurement, research in methods of scaling, particularly multidimensional scaling, has been very productive recently. The most active



area has been concerned with the development and fitting of alternative geometric and other related formal representations to reflect the interrelationships among a set of objects. These include spatial distance models and other spatial models, as well as various algebraic or geometric representations such as hierarchical tree structures. Typically, an ideal geometric and/or algebraic structure is assumed to underlie a given data set. Data on the interrelationships among a set of objects are then fit to the assumed underlying model. The extent to which the data corresponds to the model indicates the appropriateness of model.

Multidimensional scaling can handle a variety of types of data. These include the following nonexclusive types: *proximity* measures of similarity, relatedness, or other types of "closeness" of pairs of objects; *dominance* or preference data where judgments are made by individuals as to which one of a pair of objects is preferred (so-called paired comparisons judgments); *multivariate*, based on entities measured on each of a number of different variables.

Although there are many possible variations as to what formal representations could be developed and how they might be fitted, we point out four that have attracted substantial effort over the last twenty years:

1. A typical version of multidimensional scaling begins with a symmetric matrix of numerical proximity values reflecting similarity among objects of interest. The representation is a spatial distance model involving placement of the objects in a spatial picture, where the interpoint distances in the space are as consistent as possible with the original proximity values. The example on adolescent discipline presented below is of this type.

2. Individual scaling is concerned with multiple matrices (attributable, say, to different human subjects or experimental conditions), each defined on a common set of objects. Two interrelated spatial representations are constructed: the first provides a representation for the common objects, and the second specifies how each of the individual proximity matrices might differentially reflect the aggregate relationships present in the first.

3. The third type of representation, often used for either dominance or general multivariate data, is what is sometimes called a "joint space" representation, in which two (or sometimes more than two) classes of entities are represented in a single spatial model.

4. Network, and more specifically, tree-structure models are nonspatial. Here, the attempt is to represent proximity data through various distance functions defined on constrained graphical structures that are characterized by weighted links or connections among the original objects.

The whole field of scaling is at a critical juncture in terms of unifying and synthesizing what appear to be disparate contributions. Enormous progress has been made in generalizing the methods to multiway data as well as in providing various kinds of "hybrid" models that combine spatial with tree-structure models. Within the last year or two it has become apparent that several major analysis methods can be unified under the rubric of a single generalized mathematical structure. For example, it has recently been demonstrated that nonmetric multidimensional scaling, principal components analysis, correspondence analysis, and log linear analysis have much more in common in terms of deep underlying mathematical structure than was thought even a few years ago.

In an attempt to provide some intuitive feel for how multidimensional scaling aids in visualizing data we present an example.

#### *Adolescent Physical Punishment example*

The following scaling, drawn from a larger study, is relevant to a reinterpretation of theories about deviance and physical punishment among adolescents (Weller et al, 1987). The example addresses the question of whether adolescents coming from families using physical punishment have disciplinary beliefs in common with each other that are distinct from adolescents coming from families not using physical punishment. The disciplinary beliefs of fifty-eight high school students were measured using 135 true/false questions about the appropriateness of parental disciplinary actions for a series of hypothetical transgressions. Eighteen of the students reported the use of physical punishment in their homes while forty reported that physical punishment was not used in their homes.

An index of similarity among all fifty-eight students was constructed on the basis of the similarity of their responses to the questions. The results were scaled in two dimensions such that students who gave similar responses were close together while students with dissimilar responses were far apart.

Before presenting the actual results we might consider the possible kinds of outcomes. Figures 29.1, 29.2, and 29.3 show some plausible outcomes of scaling similarities among individuals. In Figure 29.1 the individuals are randomly interspersed indicating that there are no special clusterings among the individuals on the basis of the beliefs under consideration. In Figure 29.2 there are two separate and visible clusters of individuals. This is a frequently assumed model for many psychological studies, for example, introverts versus extroverts, abused versus

nonabused, and alcoholic versus nonalcoholic. The assumption is that both groups can be defined by a series of common attributes. In Figure 29.3 there is a single dense cluster with outliers. In this case the individuals represented by the number signs are not clustered with each other but rather are characterized by being different from the central cluster as well as different from each other. The outliers do not share a common set of attributes—they differ as much among themselves as from the common consensus.

In the study on physical punishment the original assumption was that there is a set of beliefs shared by families who use physical punishment. The actual scaling of the fifty-eight students as shown in Figure 29.4 shows that the results correspond much more closely with the pattern in Figure 29.3. Note that only a few of the students from homes reporting physical punishment are in the central area, while over two-thirds of the students from homes that do not report physical punishment are in the central cluster.

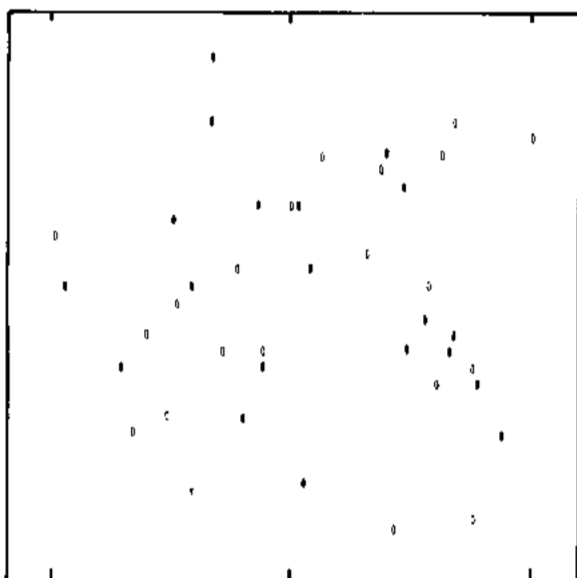
This perspective on patterns of differences among individuals may have wide implications for the study of deviance in general. In studies of child abuse, for example, there may be no common set of cultural or psychosocial beliefs that characterize families with high risk of child abuse. Rather, the families with high risk may be those who deviate too far in any direction from the socially and culturally accepted consensus.

## Survey Research and Structured Interviewing

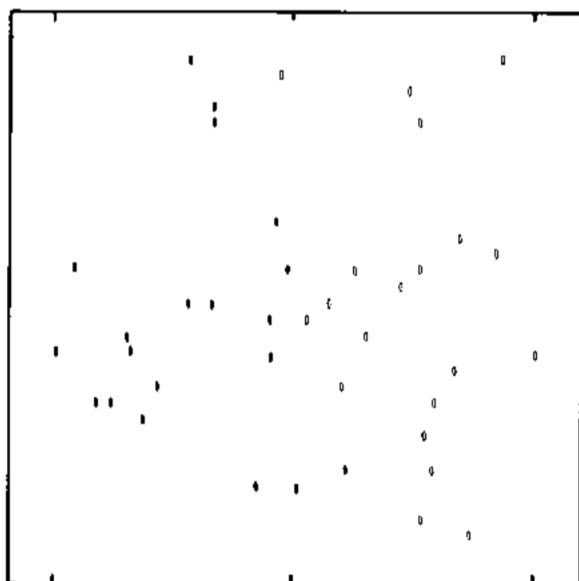
### *Survey Research*

Experienced analysts of data derived from sample surveys, whether from public opinion polls or surveys of behavior, know that small and seemingly innocuous changes in question wording or question order can produce quite large effects on answers. Recently there has been a bringing together of theoretical ideas from cognitive psychology with research on question wording that promises to illuminate some of the long-standing puzzles that have plagued survey research on attitudes.

Questions in surveys are not asked in a vacuum. Even the question that appears first in a questionnaire has been preceded by some introductory material. Questions that are quite closely related, particularly questions that are related to the same attitude object, can work to increase the salience of aspects of the opinion under inquiry. For example,



*Figure 29.1. Simulation of individuals randomly interspersed*



*Figure 29.2. Simulation of individuals clustered into two groups*

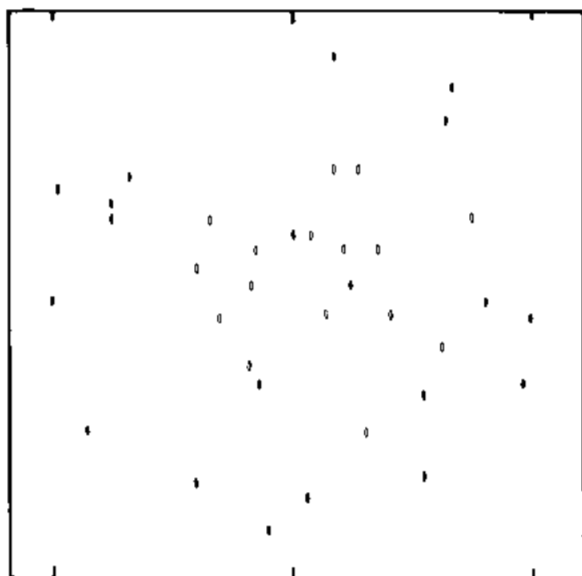


Figure 29.3. Simulation of individuals clustered in one central group with outliers

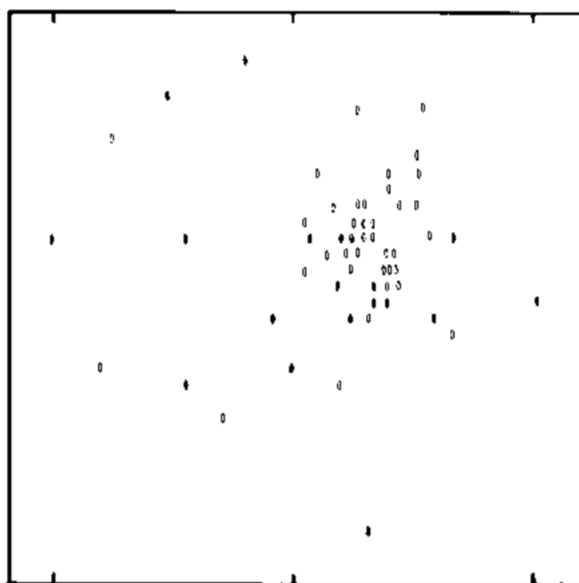


Figure 29.4. Actual data showing physically abused subjects as outliers

in a study exploring the image of three basic foodstuffs it was found that the designation of various foods as particularly "German" was affected by the context in which the questions were asked. In one form of the questionnaire, respondents were asked first about potatoes, then about rice; in another form of the same questionnaire, this order was reversed. When respondents were asked about potatoes first, 30 percent said that potatoes were particularly "German." When respondents were asked about rice first, however, 48 percent said that potatoes were particularly "German." A similar order effect was found for the pair "noodles-rice."

Recently, it has been observed that when a general question and related more specific questions are asked together, the general question can be affected by its position while the more specific question is not. Take, for example, two questions that appeared in the 1980 General Social Survey: "Taking things altogether, how would you describe your marriage? Would you say that your marriage is very happy, pretty happy, or not too happy?" And "taken altogether, how would you say things are these days—would you say you are very happy, pretty happy, or not too happy?" The results of a split ballot experiment in which the order of these questions was rotated indicated that responses to the general question about overall happiness were affected by the order in which the questions were asked, while the question on marriage happiness was not affected by the order.

While it is clear that some questions are affected by their relationship to other questions, many questions are relatively impervious to the context in which they appear. We are still a long way from understanding why contextual effects occur where they do, but there has been a great upsurge in research in recent years. This area of research promises to have considerable practical payoff in the better measurement of public opinion as well as in the more basic understanding of cognitive processes by which people comprehend the meaning of questions.

A related measurement problem concerns the use of memory-based measures in interviewing. A wide range of studies has found that human memory is quite fallible. The problem is made worse by the fact that memory-based data are not just inaccurate—they can be highly biased.

Typically the bias is in the direction of what the respondent believes to be true in general about the world. In some cases what the respondent believes can be the result of long experience, and thereby can give even better results than short-term objective data. However, the problem is that for any particular sample the investigator usually does not know what kinds of bias may obtain on a given memory-based measure. In many cases where data are now collected on the basis of respondent's

recollection, such as health and crime statistics, great improvement could be obtained by shifting to more objective techniques of data collection and by the use of special cognitive techniques of memory elicitation. Thus when it is necessary to use memory-based information, one should use methods that limit memory strain, such as recent, clearly marked, and brief time periods to set a window within which respondents are known to be able to recall certain kinds of events with reasonable accuracy.

### *Structured Interviewing and Multidimensional Scaling*

While the use of structured interviewing to find out what someone did, saw, or heard in the past has proved to be less useful than the early developers of survey methods might have guessed, structured interviewing has been remarkably successful in some areas. The use of various scaling procedures such as multidimensional scaling, when applied to data collected with such techniques as paired comparison, rating scales, etcetera, has proved to be a robust and powerful method of studying human cultures. The study of kinship systems, concepts of disease, color terminologies, ethnobotany, and ethnozoology, for example, have been radically transformed and strengthened by the use of modern measurement and scaling techniques. Recent developments have made it possible to make comparisons between cultures and to identify persons who are most competent and expert within their own culture.

The same kinds of scaling methods have also proved of great value in psychological measurement, involving not only the traditional areas of psychophysics but also the measurement of attitudes and semantic content. Recent work by psychologists in functional measurement has made possible tests of the way in which people combine adjectives to form an evaluative impression and the way in which children intuitively calculate volume from information about height, length, and width.

Another area of great promise concerns the use of various scaling techniques to measure the way in which networks of propositions are organized to form belief systems. There is considerable evidence that most people represent the world around them by means of relatively complex cognitive models which involve interlocking propositions. While to date the techniques of scaling, which are very effective in developing models of how people categorize objects, have not been applied generally to propositions, there is great potential in developing current techniques to apply to such data.

## National Assessment of Education Program as Applied Example

The National Assessment of Education Program (NAEP) is a federally mandated project of the National Institute of Education. The goal of NAEP is to provide accurate information on the proficiency of American children in a wide variety of academic subjects as they progress through the elementary and secondary educational system. NAEP is an assessment rather than an individual testing program because its goal is to provide estimates of the academic achievement of broad classes of American students (by age or grade, region, ethnic background, etc.) rather than to give scores to individual students. Because NAEP is designed to survey accurately the academic achievement of the population of American students in the target ages, it can provide much more valuable information on this topic than can the ad hoc but much publicized studies of self-selected and biased samples such as those who take college entrance exams or those who take exams for entrance into graduate and professional programs.

In order to achieve its purpose, NAEP must make use of some of the most advanced sample survey and psychometric measurement and scaling techniques that currently exist. NAEP uses these techniques to achieve three important goals: (1) to obtain accurate estimates of student proficiency at reasonable cost; (2) to reduce respondent burden; and (3) to establish comparable proficiency estimates over time.

The NAEP sample is a multistage, stratified, cluster sample that covers the entire United States in an efficient and accurate manner. Without the use of this advanced design NAEP simply would not be economically feasible.

Since a high degree of school and student cooperation is essential if the NAEP survey results are to be accurate and representative, it is important to minimize the respondent burden of the NAEP surveys. To overcome the conflict between respondent burden and adequate subject matter coverage, NAEP uses a technique through which each student is asked only a small sample of questions from the entire question-pool for the assessed subject. For example, there may be 300 questions in the total question-pool, but any given student will be asked only forty of these. Different students will be asked different samples of forty questions from the pool.

This type of survey, in which both respondents and questions are sampled, is relatively new and an essential ingredient in the NAEP design. However, because the information collected from each respondent is intentionally incomplete, some method must be used to reconstitute



all of the fragments into an accurate picture of the overall proficiencies of the target populations. This is why NAEP uses modern psychometric models. These models relate responses given to different questions in such a way as to allow the fragmentary information from each respondent to contribute properly to an assessment of the overall proficiency of each target population. The psychometric models are key to the resolution of the respondent-burden/completeness-of-coverage problem.

On reassessment of a given subject area, such as mathematics, it is important to be able to compare results with those obtained in previous mathematics assessments. For example, if the mathematics questions used in one assessment were slightly harder than those used in a subsequent mathematics assessment, we might observe an increase in test scores due entirely to these differences in relative difficulty. As a result, the questions from different assessments are "calibrated" using recently developed psychometric models so that overall assessment scores for a given subject area can be compared across time.

It is important for educational policy that national data be available on the achievement levels of American school children. A national survey such as NAEP is essential for obtaining this information. It would not be feasible without the application of survey and psychometric methods that result from modern research in these fields.

## Recommendations

We feel that the current policies and practices at agencies such as NSF are basically sound. Any recommendations should be viewed as fitting within current policy. There are two different ways to talk about priorities for basic research in the area of measurement and scaling and related subjects in the social sciences. One is to attempt to identify promising substantive areas, and the other is to develop institutional arrangements that develop scientific capabilities in groups and individuals in important areas. An optimal course probably involves the parallel development of both strategies. The two approaches are addressed below.

### *Topics*

Any specific suggestions as to what substantive topics should be selected for special consideration is a most difficult task. There do seem, however, to be two classes of problems that might be considered for special attention. First, problems that require interdisciplinary collaboration for

their solution are sometimes difficult to fit into current categories. It is becoming more and more apparent that many problems require expertise from more than one discipline. Second, problems that have an impact on several fields seem to have special strategic import. Measurement and scaling are clearly examples of fields where new advances are relevant to the whole range of social sciences. We will expand briefly on each of these classes of problems.

One recent example of collaborative interdisciplinary work involves the fields of cognitive science and survey research. A committee under the auspices of the National Research Council wrote a report on "Cognitive Aspects of Survey Methodology: Building a Bridge Between Disciplines," which has led to a series of research projects bridging the contributions of cognitive science in the laboratory and survey research in the field. Studies of this kind should be encouraged.

Several interdisciplinary topics have reached a juncture in which it would be appropriate to bring experts from the relevant fields together to try to synthesize the findings from various perspectives and strive for new levels of integrated knowledge. The nature of shared belief systems, for example, relates to anthropology, cognitive science, sociology, and scaling.

Since measurement and scaling are foundational tools for all sciences it seems appropriate that the research in the important problems in the field receive high priority. The foundational areas in measurement and scaling include mathematical work on general questions concerning the basis of all measurement in terms of numerical and geometrical representations. Examples from numerical representations already being used in a variety of contexts are the theory of subjective expected utility and additive conjoint measurement. The thrust of this work leads us to expect a small finite number of measurement structures. Potentially, dimensions relevant to the social sciences might be measured on scales as well rationalized as those in the physical sciences. There have also been a number of recent developments in the general area of mathematical models and models for geometrical scaling. Such models are used extensively in psychology, anthropology, sociology, and economics. The above topics are foundational for many of the basic research developments in social science.

Multidimensional scaling is used in a wide number of fields so that progress in the field is of immediate applicability. The following general areas appear to be of high promise.

First, there should be major developments in the specification of alternative formal representations appropriate for a variety of different kinds of data. These should include more general spatial models (e.g.,

non-Euclidean models, in which there has already been some work, or more general models for individual differences scaling), more general network models (e.g., overlapping, but nonhierarchically organized, cluster structures, multiple tree-structure models, on both of which some work has already been done, or more general graph theoretic structures, but with some constraint on the nature or number of links allowed in the graph), the further refinement of models for dominance/preference or general multivariate information, hybrid representations with spatial and nonspatial components, and structures that are subject to various constraints based on theoretical or empirical considerations. In these extensions, significant advances would be necessary in the attendant methods of fitting that are combinatorially based and/or directed toward nonlinear optimization under constraints.

Second, most of the work on formal representation has been solely descriptive, with relatively little attention given to developing concomitant statistical inference methods that could help evaluate and interpret the results from fitting a particular model. One would expect a serious body of work to develop that relies on sample reuse strategies (e.g., jackknife and "bootstrap" procedures) and/or nonparametric inference methods. These extensions would help move all of scaling closer to the mainstream of classical statistics, while still enabling the retention of its extremely important exploratory data-analytic potential.

Third, many important real or potential data sets have some type of serious anomaly—too many or too few data and/or data that are missing either in random or in highly systematic patterns. There is an obvious need to modify existing techniques for fitting these scaling representations, or propose alternative ones, to deal effectively with such important but anomalous data sets.

### *Institutional aids*

The field of measurement and scaling is of pivotal importance to the improvement of basic research in the social sciences. At the moment many of the researchers are somewhat isolated in separate schools and departments. Both research and training suffer from the lack of genuine centers of excellence. It is also true that much of the material is highly technical and difficult even for professionals. Consequently, the knowledge involved sometimes diffuses rather slowly. We suggest the establishment of advanced summer research and training institutes on selected topics in measurement and scaling.

These institutes could fulfill both a research and a training function by bringing the best possible faculty together with selected graduate

students in intensive research seminars and training sessions. One model for the organization of such institutes may be found in the Mathematical Social Science Board, which was funded by NSF several years ago and was instrumental in sponsoring a great deal of excellent research and training in mathematically relevant areas of social science. Many of the present generation of researchers were trained or participated in these programs.

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# 30

## Statistical Analysis

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Statistics develops and investigates ways of gathering and interpreting naturally variable data. It elucidates almost every field of human endeavor, from gambling and sports to the census and opinion surveys, from the social sciences to biology and physics. It encompasses the intellectually self-conscious study of making inferences and reaching conclusions about regularity and causality from erratic observations. The necessity of statistical thinking arises whenever inherent variation or measurement fluctuations make it difficult to understand data or judge the import of observed relationships. For example, is a sharp monthly or yearly increase in the juvenile delinquency or unemployment rate in a particular area a matter for alarm, an ordinary fluctuation, or the result of a change or quirk in reporting method? Is a decrease in auto injuries due to a new seat-belt law?

The domains of statistics include designing, executing, and analyzing sample surveys, controlled experiments, and other observational programs. Theoretical statistics investigates general methods, usually based on probabilistic mathematical structures, including the behavior of the methods in diverse situations, the limited flexibility of the structures, and the effective presentation of both results and uncertainties. Applied statistics concerns bringing general methods to bear on substantive problems.

### Examples

In the United States, hospital care for those under 65 cost about \$100 billion in 1983; personal medical care services approximate 10 percent

NOTE: The Study Group on Statistical Analysis aimed to convey to a broad audience some notion of the substance and direction of the field of statistics in relation to social science. We avoided names except for a few historical figures. The original report appears here essentially unchanged except for the addition of examples to the introduction and a bibliography at the end. Neither the text nor the bibliography attempts to be comprehensive or to approximate a Who's Who or What's What in statistics. The introduction draws heavily, sometimes without change, from two NRC reports, Kruskal et al., 1970, and Adams et al., 1982. The term "social sciences" includes the behavioral sciences throughout this report.

of Gross National Product. How much patients should pay for medical care, and how medical services should be organized, are problems with which virtually every country continues to wrestle. For years this topic has been debated at an ideological level. A large, lengthy, and expensive experiment on alternative systems of health care and insurance was recently finished, shedding much new light on the financial and health consequences of various alternatives. If the information obtained helps significantly to improve the financing and organization of medical services, the benefits in one year can far outweigh the cost of the experiment.

For example, with alternative health insurance plans assigned randomly to the families in the experiment, the plans paying all or most medical costs produced measurable improvements in certain conditions such as high blood pressure, but no detectable improvement overall in the health of a typical person as a consequence of the increase in medical services used. Thus the data suggest that some specially targeted subsidization of care is a cost-effective way to improve health, but making all medical services free is not. Again, with individuals assigned randomly to a Health Maintenance Organization (HMO) or to a traditional fee-for-service system (and neither group paying for services), the HMO group had 40 percent fewer hospital admissions over a 5-year period, which translated into dollar savings of 28 percent. The health consequences of the less hospital-intensive style of medicine practiced by the HMO, and the satisfaction of its users, are now being analyzed.

A good experiment has great advantages. We can be relatively confident about the findings. For example, we can be confident that the HMO's lower use of hospital services was not due to healthier enrollees, since the design of the experiment made this explanation very unlikely. We can also study some issues of cause and effect that cannot be studied otherwise. For without a carefully designed experiment, it would be virtually impossible to assess how cost sharing would affect health. Experiments can raise the level of debate surrounding an issue by giving all parties a better understanding of the relevant facts.

In the experiment just described, and others, the discipline of statistics plays a vital role in all aspects of the design, including selection of individuals and treatments to be studied and allocation of individuals to treatments, and in the collection, analysis, and interpretation of the data. The statistical concepts and methods applied come from some of the discipline's oldest and most central—yet still very active—fields of study: sampling design, experimental design, and descriptive and inferential aspects of data analysis.

An example of a valuable large-scale study that is not an experiment is the Panel Study of Income Dynamics (PSID). Much of the empirical information that forms the base for research on the well-being of the population comes from studies that gather information on a cross-section of the population at a single point in time. Cross-sectional studies provide "freeze-frame" pictures of the condition of the population at the time of the study, but they lack reliable information on *changes* in the behavior, status, attitudes, or experience of individuals or families. Studies that do measure these changes by following the *same* individuals or families over time are called longitudinal or panel studies. The PSID is such a study.

Analysis of the PSID data shows that cross-sectional freeze-frame pictures can leave a distorted impression, missing the truly dynamic character of changes occurring in the population. For example, over a ten-year period of the PSID, although the percentage of people in families that fell below the official poverty line in any particular year was relatively stable (about 7 percent), nearly one quarter of the people fell below that level during one or more years, about 5 percent fell below during five or more of those years, and about 3 percent during eight or more years. The usual freeze-frame picture does not reveal that a relatively large proportion of American families falls below the poverty line at one time or another or provide a measure of this proportion. The PSID is able to distinguish between the temporarily poor and the persistently poor, and to assess the size and character of each of these groups. With respect to the dynamics of economic inequality over the full range of income, the PSID showed for example that only about half of the Americans in the top or bottom quintile of family income were in the same quintile seven years later. The relatively large degree of movement within the American income distribution cannot be found or its magnitude assessed from the usual freeze-frame picture of the income distribution. Cross-sectional statistics, which show that the distribution of income changed little from year to year, might appear at first sight to provide support for a common view that a relatively large group of individuals is more or less permanently consigned to be disadvantaged, and a relatively large group trapped in dead-end jobs; but the presumed support for this view is misleading. The PSID is able to provide pertinent information on these and many related concerns that enriches our understanding of life in the United States.

In this and other observational studies in the social sciences, the discipline of statistics again plays a major role in the design and in the collection, analysis, and interpretation of the data. Here, too, both well-established and recent methodological advances come to bear.

An example of recent research relates to a substantial change in occupation coding between 1970 and 1980 in the United States census. This change makes it difficult to address a variety of substantively important issues, such as the occupational status of women and minorities. There is broad agreement that the 1980 coding scheme is superior, so the wish is that the 1980 codes were on the 1970 data in addition to (or instead of) the existing 1970 codes. It is prohibitively expensive to re-code the 1970 occupation data, and any simple translation of the 1970 code into the 1980 code would distort the data. Recent progress in statistical methodology for missing information will, however, allow the Census Bureau to provide social science researchers with valid data for comparing 1970 and 1980 occupational distributions, both expressed in terms of the 1980 code.

## Overview

What are the basic roles of statistics in social science research?

*Analysis* of data is a central part of statistics. How can we estimate quantities and draw conclusions of interest? How uncertain are the estimates and conclusions? How sensitive to assumptions?

Another important field of statistics is the efficient *design* of investigations before they are begun. How large should the sample be? How should it be split among various subsamples? What variables should be measured?

A further role of statistics is the construction of useful *descriptive* measures or summaries. For example, cost of living is a vague notion. Statisticians and economists have long struggled to make it precise yet practical to estimate, and they must continually modify it to reflect changing circumstances.

Statistics differs in kind from substantive social sciences in focusing on *methodology* and in applying to and aiding not only all social science but also much other science and public and private activities as well. The importance of statistics to the social sciences has been clear from their beginnings. The reasons include some facts of life about social variability and measurements. People differ greatly from one another, in complex ways. Sampling people is more complicated than sampling iron ingots. Many kinds of measurements on people or social institutions are extremely difficult to make. Even the concepts may be elusive; the intelligence of a person is a much more ambiguous characteristic than the density or even the grain structure of iron. And inferences also tend to be elusive and complex, because social science data are usually nonex-

perimental, and samples are often incomplete or nonrandom. Statistics has become so important in the social sciences that sizable groups of social scientists concentrate on advancing the understanding and use of statistics in particular fields, such as economics, psychology, and sociology, and even in specialties, such as labor, education, and consumer behavior. In fact, most social science needs statistics to be scientific. And much research by statisticians has been motivated by social science problems.

In United States *history*, social statistics began to be collected systematically when the Constitution established the Census, and in the early 1800s the statistician was presumably a social statistician. But modern statistics has grown from needs in all sciences. For example, in the nineteenth century, K. F. Gauss developed the method of least squares to deal with errors in astronomical measurements; Quetelet studied human diversity; F. Galton and K. Pearson, biological variability; and Edgeworth, economic fluctuations. Descendants of their methods are still of central importance in statistics today.

The major developments and enormous growth of statistics have occurred in the twentieth century. R. A. Fisher's ground-breaking discoveries in experimental design and many kinds of analysis were greatly stimulated by problems in agriculture and genetics. Countless others have so expanded the theory and application of statistics that statistical methods are now an important part of most areas of science, technology, and management. For example, statistical quality control began to play a significant role in World War II and is now receiving renewed attention under new conditions of technology, labor, and international competition. Randomized experiments have been crucial to testing drugs and other medical treatments. The chemical industry has been an impetus and beneficiary of advances in experimental design. The formulation and implementation of social policies depend on information obtained from probability samples.

A brief overview of diverse *uses* that have been made of statistics and probability in the social sciences appears in a 1982 NRC report (pp. 19–21). Among other things, it mentions:

*In economics*, large-scale models of the U.S. economy; employment and unemployment; economic indices; effects of taxation, money supply, and other government policies; economic time series and forecasts; theories of duopoly, oligopoly, and rational expectations; economic effects of slavery.

*In psychology*, test calibration; learning models; the formation of subjective probabilities and their revision in the light of new information and use in decision making; research in psychiatry and mental health.

*In other fields*, surveys of social characteristics; victimization and crime rates; effects of incarceration and sentencing policies; deployment of police and fire-fighting forces; jury selection; discrimination, antitrust, and regulatory cases; social networks; population growth and forecasting; decennial census data and estimation of undercount; voting behavior; and decision analysis.

Even such an abridged listing makes clear that sound statistical data, methods, and thinking are essential to almost all empirical research in social science and policy.

*Randomized experiments* on social policies, though rare and expensive, have produced vital information. For example, the Negative Income Tax Experiments were significant in policy debates even before completion, and they provided the most solid evidence we have on the effects of tax programs and schedules on work incentives for the poor. Analysis has found little effect on labor supply and both positive and negative effects on marital stability, suggesting that suitable negative income tax programs could enhance marital stability and maintain labor supply. Major experiments have also been carried out on other economic programs, delinquency and criminal reform, court procedures, rehabilitative programs in mental health, family planning, health insurance and medical providers, and special educational programs, among others. (For description and references, see Riecken et al., 1974, and Hausman and Wise, 1985.) Smaller randomized experiments are important in some other areas of social science, especially in psychology.

Most social science research must be based on data where interventions were not randomized but just "grew" like Topsy. Even so, the step from sample to population can be made secure by random sampling. *Sample surveys* are common and often revealing. In particular, our national information system includes volumes of extremely valuable data, heavily based on samples. It thus provides much information about what is out there, as measured. Its usefulness and potential are tremendously enhanced by the facility of modern computers to store, disseminate, and analyze data. Its maintenance and improvement need continual attention and resources. It is a mainstay of much social science, for without it, much theorizing can never be empirically verified or based. Its importance for this purpose and many others needs continual emphasis.

After making a secure inference to a population and learning how the measured variables are associated, what then? Dealing with measurement error requires sophisticated statistical methods even in a complete census, indeed, even in the decennial census counts, where we know that more people are missed in some groups than in others, but how

many are missed in each location is shrouded in mystery. Much more fundamentally, disentangling *causation* from nonexperimental data, and judging how well they have been disentangled, takes complex and subtle statistical investigation and analysis, even when the data constitute a complete census or probability sample. But it is often the only or best way we can afford to learn about many of the most important social phenomena.

The development of *high-speed computing and data handling* has fundamentally changed statistical analysis. Formerly the question was, can we be clever enough to find a way to condense and analyze the data that is both useful and computationally feasible? Now the question commonly is, what do we want to compute and what is a good way to compute it? Regression analysis, the workhorse for describing the statistical dependence of one variable on others, was possible only for a continuous dependent variable and either a small number of explanatory variables or a carefully selected pattern (greatly constraining experimental designs). Now methodologies for all kinds of situations are rapidly being developed and made available for use, even interactively, in powerful packages, and in time will be combined into superpowerful expert systems. Much has been done, and social scientists have already made tremendous use of it, but much more remains to be done.

The shift of focus just mentioned has brought to the fore problems of *modeling* of a conceptual and even philosophical sort stemming from the possibility of including far more unknowns (parameters) than the data can supply firm evidence about. It may also have contributed to increased recognition of the variety of different kinds of purpose data analyses may have and to increased development of different approaches for different purposes, including exploratory data analysis, description, scientific inference about hypotheses and parameters of well-structured processes or populations, personal decision making, forecasting what will happen in the absence of intervention, and forecasting the effects of intervention (causality). Along with struggles to assess causality in situations other than randomized experiments—themselves a great contribution of statisticians of not so long ago—have come emerging understanding of what causality is and the importance of judgments about assumptions in any causal inference based on purely observational data.

Statistics is a very broad and rapidly developing field. Given limited space, time, and resources, we cannot possibly cover all the important areas and prospects in statistics, nor can we organize it tightly. What follows should be viewed as illustrative but not encyclopedic, even in the interfaces of statistics with the social sciences. And it does not specifically address the interfaces with agriculture, engineering, medicine,



mathematics, and the natural sciences, although all these interfaces are interlaced because similar methodological problems arise in many places.

### **Advances in Collecting and Modeling Data**

The social sciences are better equipped now than ever before to formulate theories as statistical models, to test and refine those theories and models with appropriate data, and to use theories, models, and data to suggest effective interventions or to evaluate policy options. Advances in data collection, in measurement procedures, and in statistical models and their analysis are interdependent and often occur together. After noting some major types of data that are increasingly important in modern social sciences and some special statistical questions they raise, this section discusses some recent and prospective advances in modeling relevant to many kinds of data. The next section will focus on analysis.

#### ***Some Major Types of Data***

Repeated cross-sectional data were established with the census. Such efforts intensified in the late 1960s. An example would be two independent samples measuring opinions in the same way, one taken in 1980 and the other in 1985. The General Social Survey, collected by the National Opinion Research Center with NSF support, is a repeated cross-sectional data base that began in 1972. One methodological question of particular salience in such data is how to adjust for nonresponse and "don't know" responses. Another is how to deal with self-selection bias. For instance, to compare earnings of women and men in the labor force, we have to recognize that many factors determine who will be in the labor force, and these cannot be assumed away by supposing that the two samples of labor force participants were randomly selected in the same way.

Longitudinal surveys and panel data are a special legacy of the 1970s to empirical social science. A longitudinal survey draws a sample at one point in time and measures relevant variables at this and subsequent points in time for the same sample. Some fraction of each panel may be replaced at each time. Examples of panel data are the Current Population Survey, the National Crime Survey, and the Panel Study of Income Dynamics (PSID). The PSID, conducted by the University of Michigan, was begun in the 1960s, and in the 1980s children of the original sample members will be included in the survey, making it the first intergener-

ational panel study of its kind. (The value of longitudinal surveys has already been illustrated and is further described in an appendix.) Special statistical models need to be formulated for panel data, to capture the process of change. Another major issue is adjusting for attrition from the sample: some persons initially sampled might be available for follow-up, and observations taken in the second or later waves might no longer be representative, even if the observations at the first wave were representative. How much of the information provided by a longitudinal survey can be obtained how well from repeated cross-sections is an important question: repeated cross-sections are cheaper but do not provide or take advantage of information on changes by individuals. Close cooperation of social scientists and statisticians on these problems is called for.

Event-history data give the sequence and the timing of events for sample members throughout a fixed period of time. For example, the timing of marriage, childbearing, and labor force participation might be collected for a sample of young people over a period of several years. Generally, one seeks to estimate effects of variables on rates of change from one state to another. Event-history data have been used to study learning processes in the classroom, demographic processes (migration, fertility, and mortality), labor market behavior (mentioned earlier), and even riots, strikes, and revolutions. Special statistical complications arise in event-history data because data collection ordinarily stops before all units have experienced the relevant events under study (e.g., divorce or death), because units might experience a given event more than once in the period of observation (e.g., enter and leave the labor force or the hospital several times), and because units differ in experiences before observation begins. Modeling event histories will be discussed later.

There are, of course, many other important types of data, some of which will be mentioned below. It is important to realize that major data bases of the above types came into prominence in empirical social science mainly in the 1970s. The best studies of these data have used statistical models and methods developed in about the same period of time. Many social scientists believe that such data hold great promise for further development of theory and for evaluation of public or other policies.

We next summarize some of the most important advances in statistical modeling and indicate some areas where important research can be done.

### *Models for Categorical and Limited Variables*

Many of the variables studied in the social sciences are categorical rather than continuous. Examples include: (1) whether a young person is in

school, employed in a job, or unemployed; (2) a person's occupation; (3) identification with political parties or political ideology; (4) attitudes, typically measured with survey questions allowing only categorical response options. Most of the new models recently developed for the analysis of categorical variables can be subsumed under the general heading of discrete choice models or the still more general log-linear models.

Many important advances in understanding have resulted from application of log-linear models and related models for categorical data. Economists have integrated classical utility theory with these models to study consumer decision making. These results have been used, for example, to suggest improvements in public transportation in large cities. Sociologists studying social mobility have been able to separate factors of occupational supply and demand from other factors that affect movement up and down the social hierarchy. Log-linear analysis of social mobility tables reveals a striking similarity in patterns across time and place, after allowing for differences in the distributions of occupations (even when we compare occupational mobility patterns in nations like the United States with many of the Eastern European socialist countries). Log-linear models have also made it possible to locate and analyze systematic differences in mobility among nations and across time. Psychologists and others have used log-linear models to analyze attitudes and their determinants and to link attitudes to behavior. For example, log-linear analyses of youthful drug abuse and attitudes about drug use have yielded many insights into the prevalence and duration of drug use that could not be obtained from linear models applied to the categorical variable involved. Although the new models for categorical data have a secure position in the social sciences, some statistical problems remain whose solution would enhance analyses of many data bases used in the social sciences. When more than a few categorical variables are considered together in the same analysis, statistical methods currently in use are difficult to justify because the data might be sparsely distributed over the set of possible combinations of levels of those variables. Investing in larger samples is often too costly or impossible. Methods for analysis of sparse data need further development. Basic research is needed on the validity of existing methods when the data are sparse, and on the modification of statistical techniques so that valid and useful inferences can be obtained. Some of the solutions likely to be proposed will be computationally intensive.

Categorical variables often have ordered classifications. For example, an attitude survey might use a five-point scale from high to low without specifying numerical intervals between categories. Social class and educational level are often described by ordered categories. Ignoring the order may waste a lot of information, but replacing the categories by

successive integers or other arbitrary scores may distort results. Regressionlike analysis of ordinal categorical variables is quite well developed, but their multivariate analysis needs further research. New log-bilinear models have been proposed, but to date most deal specifically with only two or three such variables. Additional research extending these models, improving computational algorithms, and integrating the models with such topics as scaling is called for. A substantial fraction of the variables collected in the major data bases used in the social sciences are in fact ordinal categorical variables: further research on statistical models for their analysis has clear justification.

Models for limited dependent variables, especially variables that cannot take on values above or below a certain level such as number of children, weeks unemployed, and years of schooling, have been used profitably in economics and in some other areas. For example, censored normal variables, called Tobits in economics, have been used in studying decisions to continue in school. It will require major research and development to incorporate information about limited ranges of variables fully into our main multivariate methodologies. Progress beyond a single dependent variable has been fragmentary so far.

### ***Test Theory***

Measurement is in several respects more difficult in the social sciences than in the natural sciences. This difficulty stems in part from the complexity of social and psychological phenomena and the resulting complexity and ambiguity of concepts. The extent to which valid and reliable measurements of human characteristics can be obtained is also limited by ethical considerations and by the consciousness and possible reaction of the subjects. For a variety of reasons, researchers have been unable to settle on single measures for given theoretical concepts. Typically, multiple measures (or multiple indicators) have to be used. Sociologists, for example, often rely on two or more variables to measure an individual's socioeconomic position in the work force. Educational psychologists, to take another example, would not ordinarily measure a student's ability with a single test item.

Classical test theory and newer item response theories in psychometrics deal with the extraction of information from an extreme case of multiple measures. Testing is a major source of data in education, psychology, and other areas. Thousands of test items are archived each year for purposes ranging from college admissions to job training programs for industry. One goal of research on such test data is to make comparisons between persons or groups even when different test items are used for

different persons or groups. Thus one seeks sets of items that not only discriminate particular attributes specifically and well but also lead to measures that are in some sense independent of which particular items might be used in a given instance. New theories and models are proving much more useful and general than classical test theory. They will provide a better theoretical basis for studies of individual differences in attributes, and will be extremely important in applied testing. They have already been used successfully to pre-equate different forms of a test, a procedure made necessary in large-scale testing programs by legislation requiring test item disclosure.

Item response theory forms the basis of computerized adaptive testing, currently being implemented by the U.S. Armed Services and under development in many testing organizations. In adaptive tests, a computer program selects successive items for each examinee based upon success with previous items. Generally, each person gets a slightly different set of items; the equivalence of scale scores must be established by item response theory. Adaptive testing can greatly reduce the number of items needed to achieve a given level of measurement accuracy. Lest item response theory be rushed into service before its ramifications have been worked out, concentrated efforts are needed to explore its statistical aspects, including the fit of the model to particular data sets, methods of evaluating subgroup differences, the effect of various adaptive testing strategies, and methods for estimating item difficulty as a function of ability in a variety of conditions.

Factor-analytic and other latent-structure models grew alongside test theory and have recently rejuvenated it in major ways. The theories need extension to multidimensional ability measurement, and to personality and attitude measurement, where the item-scale relationships are generally nonmonotonic. New models are needed, the inferential basis for both old and new models will need to be refined, and measurement theory itself will have to change as we come to understand and reflect in our models the difference between survey measures of attitudes, where no answer is wrong, and test-based measures of ability.

### *Models for Event Histories*

Social and behavioral research has focused increasingly on processes of change or dynamics. The major current thrusts in data collection indicate this in a dramatic way: repeated cross-section, panel, and event-history data are all aimed at improving our understanding of change. With repeated cross-section and panel data, many of the existing models (linear models or log-linear models) can be modified to incorporate the

time dimension as a category, but exploiting the richness of event-history data calls for treating time differently and directly as a continuous variable. As interest in these data has grown, many researchers have turned to stochastic-process models of one kind or another to describe when and how individuals move among a set of qualitative states. Such models were important in the analysis of the Negative Income Tax Experiment mentioned earlier.

Much of the recent progress in models for event-history data builds on recent developments in statistics and biostatistics for life-table, failure-time, or hazard models; typical analyses of change in social and behavioral (and biological) states are more complex than the older types of biostatistical analysis. The processes often involve repeated transitions (individuals can make many transitions from unemployment to employment or experience several marriages), more than one time variable (transition rates often depend upon biological age, calendar time, duration in a state, and time exposed to some experimental condition), and latent variables (variables that are modeled explicitly even though not observed). In addition, collection of data on event histories often results in only partial histories (as mentioned earlier), self-selection into samples, nonrandom loss of observations (due to attrition and nonresponse), and measurement error in recalling the sequencing and timing of events.

Much research is required for the further development of event-history models. One problem is that rates of change rather than levels are the dependent variables in such models: flows are almost always more variable and harder to study than stocks. There is also keen interest in modeling joint dependence of qualitative states and quantitative characteristics—employment status and earnings, for instance. Methods are required for validating the models and checking the sensitivity of results to small perturbations in predictor variables. Development of graphical methods that can accommodate complex histories and multiple time scales would be valuable. The partial history form of the data has led to nonstandard methods of inference (such as partial likelihood methods) whose foundations and properties deserve to be better understood. Research on all these topics would be of great benefit to event-history analysis.

### *Other Models*

Finally we comment briefly on two of the other, newer types of statistical models used in the social sciences. In social network data, relationships among units (friendships among persons, trade ties among nations) rather than the units themselves are the primary object of study. A so-

cial psychologist might study friendship ties in a group; a sociologist might study intercorporation relationships as inferred from overlapping membership on boards of directors. Special models for social network data have been developed in the past decade, and they give, among other things, measures of the strength of relationships among units. A major statistical problem with social network data is the complex statistical dependence that arises among the objects of study because the units sampled are related in complex ways.

Models for the analysis of covariance structures (the most famous being LISREL) have been used extensively in the social sciences over the past fifteen years. Research needed before these models should be relied upon includes two main topics: the sensitivity of the models and analyses to the assumptions used (e.g., normality assumptions) and the inferential basis claimed for them (particularly when the assumptions are obviously faulty). Analyses that contain higher-order moments, not just variances and covariances, have received some attention recently.

### Advances in Analysis

Every new model needs new methodology for analysis and computer implementation. Even when general methods such as maximum likelihood are applicable, their application requires skillful methodological development. Numerical algorithms and user interfaces must be designed and computer programs written and debugged. The adequacy of the general methods for the specific models needs to be examined. For example, most general methods are known to provide good approximations when sample sizes are large enough, but their accuracy in actual samples usually depends on the model and even the specific data and requires extensive numerical exploration. Investigating sensitivity of the results to the assumptions of the models is important and requires still more creative thought. And so on. It takes substantial efforts of these kinds to bring any new model on line. They become even more important and difficult as we expand our models toward greater realism, usefulness, and complexity and computerize them for widespread use. Important and extensive as they are, we shall not discuss these aspects of analysis further. We shall, however, discuss several advances in analysis less directly tied to specific advances in modeling.

### *Causal Inference*

One of the most important uses of statistical methodology today in the social sciences is the drawing of inferences for causal effects of treat-

ments, interventions, or controllable factors, such as prenatal exposure to low-level radiation, public versus private schooling, monetary policy, and so on. The randomized experiment, which randomly assigns treatments or factor levels under carefully controlled conditions, is the ideal basis from which to draw such inferences. Advances in the theory and practice of randomized experimentation that have developed over the last half century have contributed substantially to the respect that the field of statistics enjoys today in many parts of the scientific community.

Nevertheless, as implied by the examples just mentioned, in the social sciences randomized experiments are often impossible, and most causal inferences must be drawn from observational studies that, however well designed, are nonrandomized. A central problem in analyzing such data—whether the variables are categorical or continuous—is translating inferences about relationships or associations among observed variables into inferences about the causal effects of the intervention variables on outcome variables. This is often essential to the purpose of the research.

Much work is needed on methods for drawing inferences for causal effects from nonrandomized data. Causal analysis is far trickier than passive prediction. It aims at learning about the same kinds of effects as randomized experiments, and rests on an assumption that randomization would guarantee. In nonrandomized data, this assumption, called *exogeneity* by some econometricians, can be investigated only by a thoughtful mix of statistical analysis and prior judgment. A proper philosophical perspective is now relatively clear, clear enough so that serious efforts are building on earlier works aimed at controlling the bias due to confounding variables, exploring the sensitivity of answers to plausible alternative hypotheses, and displaying the results to scientific audiences in revealing ways.

Continuing research into methods for the design, analysis, and evaluation of observational studies for causal effects is crucial, since so many major decisions affecting our lives are currently based on largely unconvincing evidence.

A treatment-control design, for example, has as its objective the creation of two groups such that: (1) the outcome and background variables are well measured; (2) the treated group is representative of a population of those who potentially may receive the treatment; and (3) the background variables,  $X$ , have nearly the same joint distributions in both groups. The purpose of (3) is to reduce uncorrected biases. Matched sampling is a standard method for assembling groups that are similar in  $X$ , and finds for each treated unit a control unit with nearly the same value of  $X$ , thereby simulating a paired comparison experiment



except, crucially, for randomization. Although matching (case-control) methods have been used for many years, they deserve more systematic statistical study.

The analysis phase has as its objective the estimation of the effects of the treatments or interventions. Observed differences on  $X$  can be adjusted for. Specific methods include: (1) grouping all units with similar values of  $X$  together into blocks, possibly of unequal size; and (2) covariance adjustment, which builds a model relating outcomes  $Y$  to  $X$  and essentially predicts the outcomes that would have been observed under treatments not received and compares them with observed outcomes under treatments received. Of course, statistical models for adjustment can become very complicated and incorporate complex structures. A vast amount of statistical work needs to be done on the efficacies of such methods for controlling bias, especially when  $X$  has many components.

A critical problem in observational studies is that after adjusting for observed background variables  $X$ , we generally have no reason to believe that systematic differences due to relevant unmeasured variables are absent. As a result, analyses of how answers might change if such variables were also adjusted for is essential in evaluating an observational study for causal inferences. Systematic statistical work on useful models for such sensitivity analyses is just beginning. Another aspect of the evaluation phase is the formulation and examination of interrelated hypotheses about the behavior of the putative effects. These might involve temporal effects, level of exposure, comparisons with alternative possible control groups, which outcome variables should be more affected, and so on. Once again, useful models, tuned to the scientific context and not driven only by mathematical elegance, need much more development. Finally, although complex models are often necessary, once their output is in hand, it should be translated to simple understandable displays for evaluation. No one should trust answers entirely dependent on the accuracy of some giant multivariate model and associated software without appropriate aid, initially, perhaps, such things as simple graphical displays or comparisons of means within groups. Methods for displaying the results of complex adjustments in transparent ways are also highly in need of development.

### ***Robust Techniques of Analyzing Data***

Many technical assumptions conventionally underlie our analyses of data. Some, like the assumption of independence of sampling, can be weakened when the data are sufficiently structured to allow alternative models involving a few parameters (e.g., simple serial correlation mod-

els). Assumptions about shapes of distributions—normality being the most common, of course—have proved to be particularly important, and important progress has been made in dealing with their consequences.

Classical techniques of analyzing data were based upon specific, detailed assumptions about the distribution of variations from one observed value to another, variations that combine both natural variations in what is being studied and errors of observation and measurement. These narrow assumptions were relied upon for two distinct purposes: (1) for choosing the statistical techniques that make the most efficient possible use of the data—that make the estimators and the discrimination among hypotheses as sharp as possible; and (2) for the validity of probabilities contained in the inference statements, conventionally expressed in terms of significance or confidence levels.

Initially, unease about (2) led to the introduction of nonparametric or distribution-free methods, where statements of significance or confidence could be based, either exactly or somewhat conservatively, on very general assumptions about the distribution of variations. Though these methods have remained useful, especially when data naturally occur as ordered categories, they unfortunately leave us with no basis for choice among potential alternative methods, and no simple, unified, trustworthy approach to analyses of even moderate complexity, such as those using multiple linear regression.

More recently, robust techniques have been designed to make their results nearly exact and nearly as sharply discriminating as possible for each of a sufficiently wide variety of alternative distributions for the variations. It turns out that by giving up, say, 10 percent of the discrimination that could be provided under the rather unrealistic assumption of normality, one can greatly improve performance in more realistic situations, especially when large deviations are relatively more common.

Qualitatively, such improvements must come by paying less attention to observations that appear to correspond to unusually large deviations. In most situations, we would clearly wish to do this whenever these outliers came about from some extraordinary cause that we wished to ignore. An important discovery of robustness was that this is also true when ordinary causes produce a distribution of variations with extended tails. Since these two circumstances are, at best, very difficult to distinguish by looking at the data, it is most fortunate that they call for the same remedy. Of course, separate investigation of the outliers themselves is always an obligation and may prove very important.

It is easy to show that compromising high performance for normal distributions and for more extended tails demands substantially reduced

attention to the occasional extreme deviations. Techniques for doing this, by giving more exotic-appearing observations less weight, can be moderately simple, and can be built into the classical machinery of least squares, including multiple linear regression, by a process of iterative adjustment of weights. Procedures of iteratively reweighted least squares can now offer relatively good performance in the face of each of a variety of shapes of distribution for the underlying variations.

These valuable modifications need to be extended to more general schemes of analysis if we are to capitalize on what we have already learned. In some contexts (notably the most classical, widespread uses of analysis of variance), we can already see that the introduction of adequate robust techniques is going to force the recognition of long-standing discrepancies between textbook descriptions of the procedures and how they are used. In such areas, the result will be even greater changes than the concepts and insights of robustness have produced in fields like multiple linear regression. (What the effects on the analysis of multiple responses will be is uncertain, but they can hardly fail to be extensive.)

Special attention is currently being given to the questions raised when the structure of the data and the structure of the analysis combine to give special importance to single observations or small groups of observations. If such "high-leverage" observations also involve extreme variations, the impact on the results of analysis can be very great. Practical techniques for finding what are the best possible alternatives—and we must expect a whole array of alternative solutions when we are optimizing in the face of two or more situations—are only now being developed. As they become usable, we will be forced to face difficult questions of formulation.

Work in this area will be of great importance to both the realism and the discriminating power of the great majority of analyses of data to be made in the years ahead. We have been developing the relevant underpinnings—in terms of optimum compromise in specific situations—and we have been resolutely extrapolating from them as far as it seems reasonable to go. (As we have learned more, the results of earlier extrapolation have turned out to be very satisfactory.) We have urgent needs for (1) more extensive underpinnings, both for checking on past extrapolations and as a basis for new ones, and (2) much more thought as to how to formalize such questions as those involving the co-occurrence of exotic variation and high leverage. The results we need will, when available, improve the choice of techniques and the quality of results in most of our analyses of data—in particular most of those made by social sci-

entists. In some specific applications they will lead to important data-generated questions requiring attack by subject-matter insight, or by gathering new data, or by both routes.

In addition to these important further developments in methodology, it will be important that workers in all fields come to understand clearly the behavior and advantages of the new techniques and the liabilities of the classical ones, as well as the more obvious limitations of the new and advantages of the old. The amount of re-education required, and the difficulty of the task, are hard to overestimate.

### *Repeated-Data Methods*

It is often difficult to assess the uncertainty associated with complex inference procedures, be they robust, model-based, or a combination. Large-sample theory may be unknown or intractable, its small-sample applicability unknown or poor. Repeated-data methods assess inferential uncertainty by repeatedly generating data sets similar to the observed set. The definition of "similar" is crucial, and many clever and sophisticated methods have been devised. Their tremendous promise lies in the freedom to choose scientifically appropriate procedures without regard to analytical convenience, and often in the replacement of assumed distributions by observed distributions. Flexible and imaginative use of computer simulation is the key to these methods, and thus their development is really in its infancy.

For a simple random sample, for example, the bootstrap method resamples the data repeatedly with replacement to generate a distribution of possible data sets. The distribution of any estimator can thus be simulated, confidence intervals derived, etcetera. The jackknife method repeatedly leaves out a fraction of the data and ingeniously adjusts to reduce bias. Predictive sample reuse or cross-validation calibrates the accuracy of any predictor by using it to predict each observation from the others. Adaptive versions go one step further, selecting a predictor to maximize calibrated accuracy and then calibrating this selection process. Allowing for such iteration clearly requires subtlety even when the data are a simple random sample. Extending the basic steps to more complex data structures is still more important and far from routine. Although determining the boundaries of successful application needs substantially more work, there are indications that under relatively general conditions these methods can measure the uncertainty of inferences more safely than traditional normal-based methods do.

Restricted rerandomization tests are a refinement of a method proposed by statisticians long ago, which current computing makes an ef-

fective tool for inference in randomized experiments. The essential idea is to choose the particular experimental allocation of treatments not from all possible ones under a standard experimental design but from a limited number, say 10,000, that are selected randomly or for their better than typical overall balance. Because the number chosen is limited, any test statistic can be evaluated in all randomizations, thereby generating its rerandomization distribution. Valid null distributions can thus be obtained for scientifically meaningful hypotheses and test statistics whether or not they are mathematically convenient.

Multiple imputation is a method whereby each missing value in a data set is replaced by several values representing a range of possibilities, with statistical dependence among missing values reflected by linkage among their representatives. It is currently being used to handle a major problem of incompatibility between 1980 and previous Bureau of Census public-use tapes with respect to occupation codes. The extension of the techniques to address such problems as nonresponse to income questions in the Current Population Survey has been examined in exploratory applications with great promise.

For all these methods of inference, flexible and appropriately tuned statistical insight is freed from the shackles of mathematical tractability by replacing mathematical analysis with arithmetic and logical computing power. We have yet to discover how best to take advantage of this new freedom, but the prospects are that a scientifically exciting and valuable technology is emerging.

### *Analyses of Many Interrelated Parameters*

The hope when using models with many parameters is that they can better represent the real world; a difficulty with such models is that classical principles of estimation, such as straightforward maximum likelihood, do not yield reliable estimates unless either the number of observations is much larger than the number of parameters or special designs and strong assumptions are possible.

One of the important developments in statistics in the last decade or so has been the expanded use and acceptance of analyses that treat many parameters as or somewhat like random variables, even when they represent fixed quantities to be estimated. There are many techniques and interpretations (James-Stein estimation, ridge regression, empirical Bayes, Bayesian hyperparameter models, variance components, and so on), but all have the useful property of enriching the statistician's choice of methods by building bridges between more standard models and analyses. As demonstrated by theory and practice, such methods have

the potential to improve greatly upon the simpler fixed-parameter methods from which they evolve, especially when the number of observations is not large relative to the number of parameters, because they substantially reduce the number of parameters that have to be estimated by standard methods. Typically, however, they are far more expensive computationally.

Successful applications include college and graduate school admissions, where quality of previous school is treated as a random parameter when the data are insufficient to estimate it well separately. The development of these methods is still quite young, however, and analyses using them are just beginning to take real advantage of existing computing power. For example, efforts to create truly appropriate models and analyses for small area estimation and undercount adjustment in the census are in their infancy. Not only is support needed for the purely statistical efforts but also careful efforts need to be mounted to find out, and show potential users, how much improvement can be obtained in practice by treating parameters as probabilistically related to each other. Systematic documentation of case studies is needed as is much better description of the correspondence between real world settings and appropriate models. As with most powerful tools, if misused they can do more damage than good.

### *Nonlinear Multivariate Methodologies*

Virtually all models now in use impose a linearity assumption of some kind (perhaps through a transformation of variables). But nonlinear multivariate methodologies are becoming available. (Examples, all to some extent prototypes, include projection pursuit regression, classification and regression trees, and multidimensional dynamic graphical display systems.) They are computer-intensive, and if adopted to any large extent, will require greater support of social science computing. But they avoid the false descriptions and spurious effects produced by imposing linear functional forms on nonlinear relationships, and they hold much promise for empirical work. Extensive use with data from actual problems is likely to force many changes and enhancements, and may stimulate quite different approaches to nonlinear multivariate analysis in the social sciences in the decade to come.

### *Expert Systems*

The inevitable rise of what are currently called "expert systems" will surely have a great impact on the practice of the analysis of data. We

can hope that (1) a large fraction of analyses of data will be made more carefully and more effectively; and (2) better strategies for the analysis of data will move from the practice of expert statisticians, some of whom may never have formulated (or even thought hard about) their own strategy, to both general use and wide discussion. Both of these would be of extreme importance to the field of social science as a whole. Both could occur through the development of expert systems for broad, but not all-encompassing, fields of analysis, such as multiple linear regression, factorial analysis of variance, or categorical data analysis.

But powerful tools are dangerous, as we see too often in dire misuses of existing canned statistical packages. The inadequate availability of adequate statistical judgment in analyzing relatively common sorts of data in empirical social science, as well as in many other fields, has long been clear. Until recently, only two possible remedies seemed at all plausible, namely, train many more statistician-data analysts, and make them somehow available to advise social scientists, or include much more training in data analysis, statistically guided, in the education of social scientists themselves. Inadequate numbers of would-be statistician-data analysts and inadequate time in social science curricula have kept either remedy from being taken seriously enough. But had we tried seriously, we would have found an additional bar to progress: what was needed to be learned, either by advisers or by workers, had never been clearly formulated.

Whatever the reasons for this—and they are many—we have now realized that the difficult part of constructing a moderately broad expert system is the specification—and respecification after trial—of the strategy or strategies to be followed. (Modern computer-science techniques guarantee the constructability of an expert system, given a clear account of the strategy to be used. Only rather routine, but perhaps extensive, efforts are required.) As expert systems are developed in each of several data-analytic contexts, we shall acquire corresponding strategies. Wherever two or more strategies are made available in a single context, they can be tried comparatively on a sequence of specific data-sets. Natural selection can greatly enhance evolutionary progress. These strategies will also need to be made available separately from the expert system for those with inadequate access to it and for independent criticism.

We can visualize an era when strategies at least as good as those adopted by today's best consultants will be widely available. The initial need in the training of social scientists will then be how to use a good expert-system strategy—including understanding the nature and importance of the comments it provides—rather than in how to patch together something on one's own. Almost all social scientists could become

able to conduct any of the more common styles of data analysis more effectively and more trustworthily than all but the most favored few do today.

Whatever happens, we can hope, nay, demand, that an increasing fraction of social scientists will take advantage of the growing methodological opportunities. And that the design of expert systems will reflect the best statistical practice. Yet we can be sure that some of the most significant progress in social science and in statistics will not occur by passing a standard computer routine over a standard kind of data but will require new modeling efforts on important data and problems by live experts in social science and in statistics.

## Advances in Presentation

### *Graphical and Tabular Methods*

One of the primary activities of statistical research is to provide summaries, sometimes formally as with hypothesis tests and confidence intervals (or posterior odds and intervals), sometimes more informally as with the results of an exploratory data analysis or the presentation of a graphical display. Graphical and tabular presentation as a statistical domain is now in active renaissance, stemming partly from brilliant suggestions by statisticians of new kinds of pictures (including hanging histograms and the representation of numerical vectors by schematic faces or combinations of trigonometric curves). Another influence has been the rapidly increasing availability of computational hardware and software, much unfortunately promulgated without reference to existing ideas of good graphical and tabular practice. There have been increasingly frequent and good psychological experiments with substantial statistical motivation and input, and critical appraisals of the field. Nonetheless, criteria of good practice are still too much matters of tradition or dogma, without adequate evidence or theoretical coherence.

Activity has, of course, not been limited to statistics. It has included much research by psychologists and cartographers, and even a project attempting to bring in ideas from grammar, linguistics, industrial and publishing design, aesthetics, and classification studies in library science.

The current advances call for vigorous continued development, especially in real collaboration between statisticians and other scientists, to broaden research outlooks and create sparks by juxtaposing varied



research backgrounds. Two relevant psychological domains are perception and the study of insight and creativity. Graphics and tables can communicate information efficiently, but perhaps emphasis should be on more basic goals: creativity, insight, pattern formation. A major objective is to understand better the visual and cognitive processes relevant to effective use of graphical or tabular approaches. This leads quickly to fundamental psychological questions of attention, memory, understanding, learning, problem solving, Gestalt formation, and perception.

We envisage the encouragement of joint work by bringing psychologists and statisticians together for extended periods at universities, industrial research organizations, special research groupings (e.g., CASBS), etcetera.

### *Combining Evidence*

A major strength of science is its cumulative nature. In the march of science, it is essential to have a continuing process of synthesis of the past. Where do we stand now in knowledge of short-term memory, social organization of marketplaces, effects of class size on education? We need accessible summaries of scientific knowledge as background both for current research in the field and for applications to other sciences or to the world at large. Combining evidence from separate sources is a statistical endeavor, and formal methods for special cases go back thirty years or more. In particular, one thinks of (1) theory and practice of combining hypothesis tests; (2) sequential design and analysis of experiments; (3) intercomparisons of laboratories; (4) combination of high-precision measurements of fundamental constants of nature and discussion of their tendency to differ by far more than the internal estimates of their variability can explain. Not to mention Bayesian and likelihood paradigms for combining information.

Especially in the social sciences, there is now growing interest in more ambitious analytical syntheses. This more recent research front is often called "meta-analysis." One major stimulus has been the appearance of syntheses explicitly combining the existing investigations in particular fields: prison parole policy, size of classes in schools, cooperative studies of coronary heart disease and therapeutic treatments, early childhood education, weather modification, and so on.

In such fields, a serious approach to even the simplest question—how to put together separate estimates of effect size from separate investigations—leads quickly to difficult and interesting issues, including:

1. *Dependence.* The usual approach has been to assume (stochastic) independence among the studies under synthesis. Yet we know that var-

ious sources of dependence and common bias exist. A particularly influential teacher, for example, will have students publishing related papers; influential studies will be replicated.

2. *Selection.* Unpublished or unavailable studies are difficult or impossible for a meta-analyst to look at. How serious is this selection process, and how can we allow for it? Are negative findings suppressed?

3. *Coverage.* How practicably can we know that the field has been adequately covered? Perhaps a relevant corner of the literature has been overlooked because the terminology is different. Modern, increasingly available, bibliographical tools—for example, computerized citation indexes—will be relevant. Insofar as coverage is inevitably incomplete, how can we monitor and minimize tendentious choice?

4. *Quality.* How can we measure and allow for varying quality of the work under synthesis? Is there any way to tap the rich unpublished network of information in all fields about who is to be trusted, how this experiment really was done, etc.?

5. *Varying concepts and designs.* Separate studies use different concepts, measurement modes, choices of which variables are fixed and which float freely, etc. Such differences may well influence the effects in which we are interested, and—although variation in approach is ultimately healthy—are bound to make combining studies more difficult.

Rich, informal syntheses allowing individual appraisal may be better than any catch-all modeling, but the literature of meta-analysis is growing. This may turn out to be an important area of discovery in the next decade, relevant both to statistics per se and to improved syntheses in all the natural and social sciences. But the difficult issues must be seriously addressed or it will remain dangerously superficial for its current purposes and inadequate to its current ambitions.

### *Wider Modes of Thinking*

The statistics of the middle half of the twentieth century focused on confirmation; what reaches the mystic 5 percent in significance? How short are the appropriate 95 percent confidence intervals? More recently, an exploratory data analysis that is overtly quantitative detective work overtly looking for quantitative appearances, whether or not statistically significant, has received increasing attention. Its techniques must be flexible; as a consequence most include a graphical component, a component that is often of major importance. To date, these techniques, which are proving more and more useful, have evolved for single responses in situations of lesser complexity. Extension to multiple responses—and to single responses in more complex situations—is now

badly needed. The general road is clear but many detailed inputs will be required.

For many years, the frequentist and the Bayesian paradigms have been standard statistical modes for formally summarizing evidence. Heated debate between the schools has pinpointed the theoretical and philosophical issues in simplified examples, but there has been relatively little systematic work on adequately flexible strategies for the applied researcher using one mode or the other to attack a real-world problem with its multiplicity of objectives, sources of evidence, and so forth. This is an important area for research. The direct investigation of strategies for inference using standard statistical techniques in real-world problems would necessarily create a bridge to the areas of exploratory data analysis, expert systems, and sensitivity analysis of the types referred to earlier, and perhaps even as used in the currently popular spreadsheet programs.

Another direction for research into practical modes for formally summarizing evidence is to admit that for certain classes of inferential problems our Bayesian and frequentist tools are too limited, and to consider new or extended modes of inference. Such new modes of statistical thinking may be more appropriate for problems in law, artificial intelligence, and expert systems, where the objective is to combine highly disparate types of evidence, not just scientific data.

### **Bringing Theoreticians to Applications in Statistics**

The great men mentioned in the introduction knew the applied fields they worked in. Statisticians and other metricians need continual involvement with application, to stimulate research on the real problems of data analysis and to avoid overspecialization, insularity, and excess mathematics. They need contact with one another for similar reasons. Impediments include specialized talents, communication difficulties, inadequate incentives, the natural tendency of narrow and hard criteria to drive out broad, amorphous, or soft ones, the natural evolution of academic departments and other perceived peer groups, the exponential difficulty of meeting multiple criteria, the self-reinforcing pressures to refer applied problems to already experienced people and nationally recognizable problems to nationally recognized people, the limited number and kinds of people the nation can recognize, and so on. Many have noted the needs and deplored their unfulfillment. The impediments get less press, perhaps because they are embarrassing and intractable.

Changing human nature does not seem feasible, but there are programmatic measures that would help bring theoreticians into applied work and thus benefit research. We shall sketch some for statistics, urging the parent committee to extend them to other fields and develop them.

Graduate education should be modified. Proposals range from mild (more applied work in courses, departmental seminars, and theses; applied papers; consistent advising; theoretical faculty overtly interested in applications) to mildly heretical (require 50 percent actual data analysis, computation, Monte Carlo simulation, etcetera, and enough substantive science to get a sense of what science really is). Though inexpensive in direct dollars, such changes are expensive in planning efforts and are unlikely in the absence of outside stimuli and other changes.

Part-time or summer jobs or internships in other university departments, government, or business would give students education in how statistical theory is modified in real applications and, if fortunate, the thrill of productive collaborative research, of using or even inventing statistical methods to help make substantive discoveries. Special programs and funding would be required to overcome financial and institutional barriers and support new positions for students and supervisory and administrative efforts by faculty.

Postdoctoral fellowships and internships in substantive areas would get new Ph.D.s out of the narrow ruts otherwise likely and into new ideas and subjects. Though fellows might not work permanently in the areas of their fellowships, many would develop long-range and productive interests in social science and all would gain useful experience while producing research of value to themselves, the substantive areas, and statistics. Well-trained theoretical statisticians should be prepared to move quickly to the heart of a technical area and begin to contribute to the analysis of current problems. Their programs should not prolong studenthood but challenge the student to produce interesting results in the first year and publishable material in the second. Otherwise it unduly impedes career progress. While developing abilities in an applied area and in applying statistics to it, fellows must also digest their training, sustain the research momentum of their theses, maintain professional status in their cohorts, and perhaps teach. These are heavy demands.

Experienced and productive theoretical statisticians also need attractive opportunities for doing applied work, to sustain applied interests, enthusiasm, and stimulation. If a university loses this spirit, it can easily turn out a few generations of irrelevant theoreticians. Arrangements are ever more constrained by tightened finances, competitive pressures, administrative burdens, and working spouses.

Because circumstances vary so widely, a wide range of opportunities

is needed for both junior and senior faculty. Already successful or promising possibilities include temporary positions for statisticians in social science departments, major research projects, research centers and institutes, government agencies, and business. The NSF-supported American Statistical Association programs placing fellows in the Census Bureau, in the National Institute of Justice, and soon in the National Center for Education Statistics, have been very successful. NRC projects would provide some good opportunities if deeper involvement than current volunteer work allows could be supported financially.

There is a lot of applied statistical work to be done. Much is now handled, or mishandled, by nonstatisticians. But providing statistical consulting as efficiently as possible in the short term is not the purpose here and would often defeat it. The purpose is to bring theoreticians to applications for the long-term good of their research and the field of statistics. How effective programs might be conceived, organized, and integrated with others is beyond our scope, but to achieve this purpose, they are essential.

## Appendix

### *Longitudinal Survey Analysis*

Individual experience intersects social policy most clearly in the timing and character of major life-course transitions—home-leaving and home-forming, labor force entries and exits, marriage, childbearing, and the accumulation and disbursement of financial and of human resources. Everyone has opinions about how societal mores, economic conditions, and social programs are shaping lives and encouraging and discouraging kinds of behavior. We all generalize from our own case and from the groups we belong to. But in the last few decades social scientists have been able to go beyond personal and group perspectives by drawing on detailed survey data collected in national probability samples. How much it costs to raise a child, how much unemployment contributes to divorce—the effects are so different from group to group that homespun generalizations are little more than myths. But carefully collected national data have been giving social scientists the opportunity for balanced assessment.

Longitudinal surveys, which record life-course changes of the same individuals reinterviewed in successive interviewing “waves” year to year, are the special entree, with their “before” and “after” snapshots, for studying causes and consequences of decisions and transitions. The two-decade-long investment by the National Science Foundation (NSF)

in longitudinal data bases like the Panel Survey of Income Dynamics and the National Longitudinal Survey has now laid the foundation for a unique advance, the advance to analyses that span a generational cycle. The children from the initial waves of these longitudinal surveys are now reaching their own family formation and career initiation. Representative national views of a full generational cycle in fertility, work life, household, and income dynamics are coming within our grasp.

Comparing the fertility or income of different people in different circumstances at the same point in time always leaves a large proportion of the variability unexplained. Common sense tells us that a lot of this unexplained variability is not really random. There are explanations, individual by individual, in each person's past, in parental models, upbringing, and earlier sequences of experiences. We each, of course, remake our views of our past to rationalize our present, so that retrospective data are of questionable validity. But generation-long longitudinal data allow readings on the sequence of past circumstances uncolored by later outcomes. Thus for the first time quantitative analysis is becoming feasible, analyzing the aspects in which the experience of individuals recapitulates their parents' experience and the aspects where they diverge, the aspects where early decisions constrain later opportunities, and where detailed background leaves its imprint on later contributions to society.

These opportunities opened up by longitudinal data resources spanning a generation are at the moment curtailed by the limitations of existing methodology for the statistical analysis of complicated longitudinal sequences of transitions. Good early progress has been made in modeling rates of transition and hazard functions for the timing of change when a limited spectrum of alternatives is being studied. But new statistical methods are needed to exploit the full richness of the data now available. For instance, new methods are needed for clustering complicated sequences of transitions to construct succinct data-based typologies. New methods are needed for separating relevant from irrelevant sets of interaction effects when large numbers of repeated transition variables are present. A key to the development and testing of such new methodologies is computer-based microsimulation. Demographic, social, and economics microsimulation techniques have been developed with NSF support through the 1970s and have arrived at a level of considerable maturity. But their widespread use in tandem with the statistical modeling of longitudinal data series is just beginning.

New resources, in amounts modest in comparison to the investment in data already made and continuing, would greatly accelerate progress in this field. At present, there are huge entry costs for young scientists

seeking to work with longitudinal series and parallel simulation experiments. Computer costs, while diminishing, are not negligible, so that even the early stages of research require grant support. New workers in the area have to learn the complicated data formats, codebooks, and simulation methods before they can write competitive grant proposals, but they need grant support for the hands-on work through which they can gain this expertise. Furthermore, in investing their time, they are gambling that future support will be forthcoming. In the 1970s extended programmatic support grants to established scientists enabled new researchers to enter the field by joining their teams and collaborating with them. But the recent trend toward shorter-term and narrowly targeted support has diminished that career path. Today, similar good effects might be achieved by the NSF program for providing numerous small grants with quick turnaround between application and award exclusively to cover tape acquisition and computer costs for analysis of the data series already created and continued under NSF sponsorship.

The existence of generation-long national longitudinal data series is one of the great legacies that the 1970s and 1980s bequeath to American social science of the 1990s. Analytic methodology and substantive application of this methodology for putting this legacy to best use should have a high priority for expanded emphatic support.

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