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Introduction: Uneasy Social Science

Richard A. Shweder and Donald W. Fiske

Social science research institutions are hotbeds of pluralistic activity, each scientist holding that "progress is being made on the problem on which I am working." At the same time, and perhaps paradoxically, there has been in the social sciences, at least in recent years, a vague sense of unease about the overall rate of progress of the disciplines. A small, but visible, iconoclastic literature has emerged (see Bibliography) either challenging the scientific status of social research or expressing concern about the accomplishments of the social sciences. Some have even talked of a "crisis" in social inquiry. The essays in this volume accept that challenge and examine the concerns of that crisis literature; they appraise the current state of our knowledge or set forth proposals for acquiring valid and useful social and behavioral knowledge.

It is noteworthy that the prevailing positive mood among practicing social scientists is accompanied by an understandable desire not to be disturbed by worries about the foundations of the science or generalized concerns about a purported lack of progress. There is probably a great deal of wisdom implicit in that desire not to be disturbed, for it can be rather hazardous to believe that there are standard criteria for judging the overall progress of a discipline or field of knowledge or to impose those criteria on one's thinking. Of course, the practitioners of a science are always trying to accomplish something or other, and each practitioner knows what progress means for him or her—it means doing even better what he or she is now doing. But beyond that, there may well be no common currency for measuring progress, no way to make summary evaluations across the range of activities; and we are, perhaps, well advised to resist, even by means of indifference, any attempt to mint such a currency. Indeed, such indifference may be justified even when the attempted assessment of overall progress assumes the

rather bland form of a list of all the diverse things going on, for a list, almost by definition, does not add up to something unified or coherent, nor should it. To demand or announce an assessment of the progress of a field of inquiry is to risk forcing each focused but individualistic program of intense activity into a Procrustean bed. No one wants that. When such an act is permitted, it is typically for ideological or political reasons—or in the name of some “higher purpose” like funding.

Moreover, as students of the history of the sciences, we probably have good reason to beware of standardized, explicit, and precise criteria for judging and regulating the development of a science. Clarity of aim is an uncertain virtue in a healthy science: some activities that turn out to be important seemed aimless originally, and scientists who do have clear aims in mind are often aimed in different directions or at each other. Typically, in a healthy, developing science, the work at the growing edge is highly contested. Typically there are prominent scientists who find it impossible to understand each other. Typically there is relatively little consensus about what the next step should be and what the yardstick is for measuring final success. Indeed, one thing we can learn from the history of the sciences is that the criteria of progress in science are task-specific, diverse, ambiguous, and shifting. No one criterion has served as a general standard or a universal ideal. Nor, taken together, do the diverse criteria add up to a unified index of progress. At times in the development of a science, there is a premium on controlled laboratory experimentation or mathematical formulation, but just as often there is not. Knowledge in a science can occasionally be reduced to a small set of universal causal laws, but typically it cannot. At times, it makes sense to try to predict the future, but not always. One cannot even say that it is always advisable to stick to the presumed facts or to insist upon agreement about the meaning of new concepts. There are times in the history of science when ignoring the apparent facts has paid off. And there are times, perhaps now, when pressing for consensus on the meaning of progress can be rigidifying and positively destructive. So it is quite understandable, perhaps even a form of wisdom, that most practicing social scientists do not want to be bothered by the crisis literature in the social sciences or worry about abstract formulations about the possibility of a social science.

Scientists must confront their individual research problems with some optimism. The positive mood that prevails among most social scientists is not merely the blind optimism of narrowly focused

technicians who would rather not be disturbed by esoteric questions about science and progress. If pressed, most social scientists can, without too much difficulty, point proudly to knowledge that has accumulated about many particular topics, to technical advances in research methods, to success at actuarial prediction and assessment of skills, and to various counterintuitive research findings, documenting that certain prominent folk beliefs are contrary to empirical evidence. For a comprehensive and generally upbeat assessment of things, social scientists can turn to the report of the Committee on Basic Research in the Behavioral and Social Sciences, formed by the National Academy of Sciences–National Research Council. Part of the charge of that committee was to identify “illustrative areas of basic research in the social sciences that have developed analytic frameworks of high social utility” and areas “that are likely to be of high value, significance and/or social utility in the near future” (Adams et al. 1982, 1). An upbeat tone can also be found in more restricted contexts, where, for example, the Behavioral Sciences Research Review Panel identified for the National Institute of Mental Health research topics in the social sciences that, on the basis of significant recent developments, “appear to be especially promising for their potential contributions to the understanding of mental illness and health” (Clausen et al. 1983, 1). The gloom-and-doom message that sometimes emerges in the crisis literature is not that difficult to oppose.

There is, however, an alternative approach to the crisis literature in the social sciences, namely, to take seriously the issues it raises. If that literature were merely the imposition of misconceived criteria for assessing progress, it would deserve to be ignored. If that literature were merely a one-sided pessimistic enumeration of failures, it would deserve to be dismissed. But despite its shortcomings, it is more than that. The crisis literature raises to consciousness many of the assumptions that we as practicing social scientists take for granted about the nature of our science and our subject matter and the relationship between them. Those assumptions have a decisive influence on what problems we select for study and how we go about conceptualizing and investigating them. It is the view of the editors of this book that those assumptions are too important to be taken for granted and too much a part of our ongoing research enterprises to be left only to philosophers to think about (but compare the views of Meehl and Rosenberg, chaps. 14 and 15). Thus, instead of ignoring or dismissing the crisis literature, we have decided to address it and offer a variety of re-

sponses to it. The Bibliography which sampled the social science crisis literature, was shared by all conference participants a year prior to our meeting (see p. 371).

Several sources of uneasiness disturb those who are troubled about the social sciences. Whether or not one is troubled by those things, they are the kinds of things with which one must come to terms, especially if one is concerned with the potentialities for the growth of knowledge in the social sciences. For one thing, the typical or modal social science generalization is rather restricted in scope, bound to a particular population studied at a particular historical time in a particular culture, often even bounded by the particular methodology (so-called method effect) used in the investigation. The discovery that social science generalizations are typically narrow in scope is not new. Nagel makes the point in 1961 in his book *The Structure of Science* and he cites a reference from 1934, which makes the same point. Although in 1984 it is not impossible to generate at least a short list of valid universal generalizations in the social sciences, what Nagel had to say in 1961 still seems apt: "[The] conclusions reached by controlled study of sample data drawn from one society are not likely to be valid for a sample obtained from another society. Unlike the laws of physics and chemistry, generalizations in the social sciences therefore have at best only a severely restricted scope, limited to social phenomena occurring during a relatively brief historical epoch within special institutional settings" (p. 459). Acknowledging that human action is mediated by existing "technologies and traditions," Nagel suggests that "the possibility must certainly be admitted that nontrivial but reliably established laws about social phenomena will always have only a narrowly restricted generality" (p. 460). A law can be "universal" only for a homogenous set of objects or events.

The scene today is not unlike that described by Nagel. While several universals have been discovered—Berlin and Kay's generalizations (1969) about the evolution of basic color terms, Barker's generalizations (1971) about the influence of group size on responsible social behavior, Ekman's work (1973) on universal facial expressions for emotion, the discovery of a universal schema underlying the perception of personality (White 1980), several noteworthy findings concerning language universals (Greenberg 1966; Dixon 1977)—they are the occasional exceptions to the rule that social science generalizations are not very general. Indeed, over the past twenty years, the restrictions that bound social science generalizations seem to have increased, as the results of laboratory

experiments did not generalize well outside the lab, as experimental research was shown to be vulnerable to expectancy effects, setting effects, even paid subject effects, and as method effects proved to be so substantial that slight changes in research method produced major alterations in the patterning of one's findings.

There are several ways one might react to this state of affairs. For example, assuming that any science worthy of its name must uncover universal generalizations about its subject matter, Nagel (1961) rose to the defense of the idea of a social science by attacking the view that universal laws of social phenomena are in principle impossible and by convincing his readers that, despite the record of narrowly restricted generalizations, the possibility could never be totally ruled out that there exist social laws valid for all societies and for all time. The assumption that any science worthy of its name must uncover generalizations of broad scope is compatible, as well, with other diverse reactions: deep concern over the fact that "generalizations decay" (Cronbach 1975); a renunciation of the very idea of a social science, or at least a renunciation of the idea that human subjectivity, identified with free will and the human spirit, can be studied scientifically (Gergen 1973; Rosenberg 1983); hope for the future growth of a young science accompanied by special feelings of pride in those universal generalizations that have been discovered (Converse 1982). Another option is to deny the assumption, to argue instead that universal generalizations are not the *sine qua non* of science, that "not all hypotheses of interest to a scientist are universal generalizations" (Edelson 1984, 28). One particular variant of this view is that social scientists should pride themselves in "thick description," knowing a lot about each case, documenting local events accurately, and generalizing within cases, not across cases (Geertz 1973). Each of these options receives expression in this book.

A second cause for uneasiness among those who are troubled about the social sciences is the persistence, even entrenchment, of multiple paradigms or schools of thought and the lack of convergence over time in the theories and concepts that guide research and are used to interpret evidence. There is a healthy empirical tradition in the social sciences and a richness of rigorously produced research findings; nevertheless, no reduction seems to be occurring in the diversity of conceptualizations and higher-order theories, and many basic issues never seem to get resolved. Perhaps that is why some critics see the social sciences as a game in which the players generate interpretations within the grammar of some unas-

sailable paradigm (Freudian, Darwinian, Marxist, behaviorist, etc.) for events that have already happened, a practice reminiscent of Geertz's proverbial Mexican peasant, the one who first shoots the hole in the fence and then paints the bullseye around it (1973, 26). One reaction to the endemic diversity of theories, claims, and approaches is to view social science as myth, religion, or ideology. Another possible reaction is to argue that the social organization of the mature sciences favors a winnowing out of theories and that it is time for the social sciences to attend to the social system of science and to follow that example accordingly. One might also react by eschewing an interest in theory and attending instead to the accumulation of "facts," although in this book and elsewhere, some argue that social science has difficulty reaching agreement on just what the "facts" are. Yet another reaction is to question the assumption that convergence of belief is a criterion for maturity in science and to examine the alternative assumption that diversity of viewpoint is compatible with rationality and objectivity. Each of these options is explored in this book.

A third thing that bothers those who are troubled about the social sciences concerns the relationship between social science research and the practical demands of society. Confronted with policy questions about such topics as the effects of forced busing, the effects of educational or therapeutic interventions, the direction of social revolutions and religious movements in foreign lands, the impact of tax cuts on savings behavior, and so on, there seem to be, in the eye of the critics, only two types of social scientists: those who are frequently wrong but never in doubt, and those who say "Well, it's all very complex" and then hang up the phone. The truth, of course, probably lies somewhere in between, and perhaps a bit off to the side, for there are many areas in which social science research has yielded socially useful knowledge. Think, for example, of IQ and educational testing, opinion polling, economic indicators, the Bureau of the Census, psychosomatic medicine, the strategic use of game theory, and behavior therapy. If we broaden our notion of useful knowledge to include ideas and theories that alter our consciousness, self-consciousness, values, and goals, then the impact of social science conceptualizations—for example, Freudian, Marxist, neoutilitarian, sociobiological, and relativistic—must be judged to be enormous.

A fourth and final source of uneasiness for those who are troubled about the social sciences is its distinctive subject matter—human subjectivity. Unlike the physical sciences, social science

knowledge is designed to help us understand and explain subjective experience and such "things" as meanings, intentions, ideas, values, and emotions—non-things that Descartes long ago placed beyond the reach of the mechanistic sciences. The questions are still with us whether a science of subjectivity is possible, what that science of subjectivity might look like, and how it is similar to and different from, on the one hand, the physical sciences, and on the other hand, the humanities. Lurking behind such questions is an even broader issue: What are the criteria, if any, that distinguish (good) science from (bad) protoscience from non-science from nonsense?

Each chapter in this volume addresses one or more of these four problem areas for the social sciences: the problem of general versus not-so-general generalizations, the problem of pluralism or multiple paradigms, the problem of useful versus not so useful knowledge, and the problem of science versus non-science.

D'Andrade's essay (chap. 1) presents an ethnographic account of the diversity of models and ideals of science among practicing scientists. The essay advances several arguments which, quoting D'Andrade, can be summarized as follows:

First, that the sciences contain at least three very different world views, that of the physical sciences, that of the natural sciences, and that of the semiotic sciences; second, that the pursuit of "general laws" is characteristic primarily of the physical sciences; third, that some of the natural sciences, such as biology, have done well despite that fact that they have not found general laws; fourth, that in the social sciences there is considerable division between the natural science approach and the semiotic approach without a reasonable synthesis in view; and fifth, that the emphasis on predicting or explaining individual events as a scientific activity is misguided because of the difficulties of establishing the appropriate boundary conditions.

D'Andrade argues that Hempel's famous "covering law" model is not the ideal form for all scientific thinking, and he examines the foundations of those semiotic or semantic sciences that eschew an interest in lawlike generalizations and seek, instead, to understand "imposed order."

In chapter 2, Converse argues that "a model of science that suggests that either we can, in T. S. Eliot's word, 'roll the universe into a ball' with one grand summary expression like $E = mc^2$, or we are not engaging in science is a false model." Arguing that what you think about, your subject matter, is decisive for how you think,

Converse introduces the notion that each science has its own "texture." He asks, What model of science should a social psychologist choose? He notes that the texture of the biological sciences has become distinct from the texture of the physical sciences and looks forward to the development of a mature social science with a texture of its own. What is distinctive of the subject matter of the social sciences, Converse argues, is its complexity. What makes it a science is that it is aimed at the "systematic decoding of observed regularities and the reduction of the regularities to more parsimonious and general principles that account for wide ranges of phenotypic detail." What should not rule it out as a science, any more than it rules out geology, is that it is "firmly wedded to historical circumstance."

In chapter 3, Fiske examines the problem of method variance in the psychological sciences and the incommensurability and lack of convergence of findings across methods. He likens the production of knowledge in the social sciences to a daily newspaper: "Just as in one day's newspaper, the sports page, the neighborhood news section, and the music reviews report quite unrelated events, so do the various methods in social science. . . . Even when the same event is the object to which several methods are applied, the resulting sets of data can be expected to have little or no covariation." His assessment of the current scene is that knowledge in the social sciences is highly fragmentary, composed of "multiple discrete parcels," and that the specificity of knowledge is related to variations in the types of protocols used to generate data, the size scale of the objects investigated, the time scale over which objects have been studied, and the extent to which data production relies on words and the inferential judgments of observers. Fiske argues that progress will come in the social sciences by discovering new types of protocols, by identifying basic behavioral processes, by more exactly defining the objects of inquiry, and by introducing more controls over the process of data production.

Cronbach (chap. 4) identifies several false ideals of science that he believes have had a repressive effect on the psychological sciences. Among the false ideals is the quest for timeless knowledge and the desire to view the world from a position of transcendent objectivity. "When we ask our theories to 'cut nature closer to the joint,' we ask too much. . . . Many a realist wants concepts to name entities that exist in nature quite apart from man's construing. Social inquiry, I think, would be better off without that aspiration." Cronbach argues that the development of a social physics is highly unlikely and that in the near future we will not witness explanatory

theories of broad scope. He suggests that "social science is cumulative, not in possessing ever-more-refined answers about fixed questions, but in possessing an ever-richer repertoire of questions." He believes that, in the short run at least, development will come from adopting a more pluralistic approach in which there is greater respect for the accurate and reliable description of local events and for historical documentation and in which it is recognized that progress in science can come even from poorly formed, vague, and untestable ideas. As a model of effective, generative research, Cronbach analyzes the Hawthorne research on the social psychology of the workplace at Western Electric Company and identifies several research strategies that he recommends to future researchers.

Taken together, the chapters by Converse, Fiske, and Cronbach provide alternative assessments of the social and psychological sciences, more or less within the broad framework of the physical science and natural science models as described by D'Andrade. By "physical sciences," D'Andrade means those concerned with stating laws of behavior. By "natural sciences," he means those concerned with what things are made of and how they work, that is to say, mechanisms. In chapter 5, Campbell holds to the unity of scientific method but also offers a bridge to the chapters by Gergen and Shweder, which examine some of the implications and possibilities of D'Andrade's third model of science, the semiotic model.

Under the basic proposition that there are requirements for being scientific that hold for both physical and social sciences, Campbell argues that many of the problems of the social sciences are due to the lack of effective "disputatious" communities of scholars, who challenge each other's specific truth claims with cross-validation and critical argument. Too many schools of thought are engaged in producing "illustrations" rather than "evidence" for assertions of descriptive facts and theoretical interpretations. For many reasons, including shortage of scholars, these assertions go unchallenged. The difficulty of producing convincing "demonstrations" is a part of the problem. While arguing for a unity of scientific method, to date best exemplified in the physical and biological sciences, Campbell's paper provides transitions to later chapters denying that unity. His acceptance of epistemological relativism (as distinguished from ontological nihilism) offers a dialectic with Gergen's more radical relativism. Campbell's parenthetical comments on validity-seeking hermeneutics (atypical of the hermeneutics most frequently invoked by postpositivist social scientists) and the overlap it might have with postpositivist theory of

physical science methods of validity-enhancing belief change provide a bridge to the "interpretive" (or semiotic) models of D'Andrade, Gergen, Shweder, and Cicourel. The chapter can be taken as a rigorous program for a scientific hermeneutics.

In chapter 6, Gergen advances the provocative claim that no psychological theory has ever been abandoned "for reasons of clear observational failure." His essay is a lucid articulation of a radical relativist sociology of science, but it is far more than just that. The essay examines and makes use of key principles of contextualism, constructivism, deconstructionist hermeneutics, and principles from the analysis of the language of action. The central concern of the essay is the relationship, or lack of relationship, between descriptive language and the world of action that language is designed to represent. Gergen rejects the correspondence theory of the relationship between language and reality and, along with that rejection, raises serious doubts about the objectivity and rationality of social science. Identifying a series of principled reasons for believing that there is an inherent indeterminacy to scientific interpretations, Gergen argues that the descriptive and explanatory constructions of social science are "fundamentally free to vary across context of usage." The implication is that virtually any theory—Freudian, Skinnerian, social-learning, role-rule—can be used to describe and explain any action, and "should be capable of absorbing all empirical outcomes." The constraints on interpretation are not empirical or logical but reside primarily in a social process whereby communities of scholars "negotiate" the way in which language will be applied across diverse contexts. Gergen argues that the particular modes of discourse of science that result from the process of negotiation are never value free or ideologically neutral, and he views science as a force for or against social transformation.

Shweder (chap. 7) examines what he believes to be the false contrasts between objectivity and subjectivity, positivism and hermeneutics, science and religion, rationality and superstition, with special reference to the role of those contrasts in discussions about the nature of the social sciences. Drawing on historical and cross-cultural materials, Shweder tries to show that ideas about witchcraft, reincarnation, and the reality of dreams are of a genus not unlike that of our own scientific theories, and adopting the "native point of view," he examines the rationality and explanatory power of those theories. Promoting the concept of "divergent rationalities" and the idea that not everything that is rational must be universal, he introduces an alternative position into current debates in

anthropology about whether so-called religious doctrines are "symbolic" or "irrational." Shweder argues that the contrast between subjectivity and objectivity, science and religion, and so forth, has been overdrawn, one unfortunate result of which is to rule out the possibility of a genuine "science of subjectivity" while forcing upon the social scientist the false choice of treating the social sciences as either a physical science or a humanity. As a plausible alternative, Shweder describes a world of subject-dependent objects (nothing in particular exists apart from our theoretical attempt to understand it) and objectlike subjectivity. Such a world requires a broadened concept of "rationality" and "meaning" in which convergence of belief is not the sine qua non of objectivity. Divergent rationalities are possible, for example, cults, cultures, and the diverse social science schools of thought.

In chapter 8, Secord proposes a "realist" view of science, which he contrasts with the now standard view of science as influenced by positivism. He argues that "scientific laws are to be conceived as causal principles or tendencies rooted in the nature of the relevant entities and not as reflecting regular concomitance between events." He calls for a science focused on "generative mechanisms," and he rejects both the view that "the main task of the behavioral sciences is to discover the regularities in behavior" and the view that explanation of an event is accomplished "through deduction from universal laws that are applicable to the phenomenon." Secord believes that much of social and psychological theory is vacuous because, unlike physical theory, it is unable to specify the conditions under which relationships hold. This leads him to a discussion of the difference between material objects and living organisms, closed and open systems, and to a critical evaluation of laboratory research in the human sciences. The essay concludes with an examination of the role of ordinary language in scientific social understanding and the potential applicability of social science knowledge to real-world problems.

The two chapters by Kleinman and Cicourel (chaps. 9 and 10) take a look at social science knowledge in the context of real-world institutions and problems. While mental health is brought into some of the other chapters, Kleinman focuses on medicine in general and mental health in particular, and Cicourel considers diagnostic and research procedures for gathering information from patients and other subjects. Kleinman advances the proposition that the social sciences are more relevant to day-to-day problems of health care than are the biological sciences, but the main goal of his

essay is to explore a paradox: "Why do the social sciences remain marginal in medicine in spite of their obvious relevance?" Kleinman identifies numerous domains of relevant social science research in medicine—research on psychosomatic illness and stress, epidemiology, health service delivery, social support networks, social class and illness, affective functioning, psychotherapeutic outcomes, cross-cultural studies on somatization, work in medical anthropology on the distinction between illness and disease, and much more. He attributes the marginality of social science research in medical settings to several factors, including a dominant biomedical paradigm expressive of a cultural epistemology favoring empiricism and materialism, the demands of a preprofessional education, the isolation of social science research papers from the medical science journals, and many other systemic and institutional forces that affect the practice of science. Kleinman identifies potentially important areas of contribution for the social sciences in medicine, not the least of which is the questioning of core assumptions and values and the development of a scientific language for talking about subjective experience, relationships, and meaning. Acknowledging that the cognate fields of social medicine, psychiatry, public health, and bioethics are "passing through conceptual doldrums," he sounds a clarion call for supradisciplinary efforts to develop "bridging ideas" between social science and medicine.

Cicourel examines the process by which the products of mental test instruments, survey questionnaires, and structured or open-ended diagnostic interviews are produced. He notes that the popular use of formal and standardized measuring instruments "has not been accompanied by an explicit effort to study the subjects' or respondents' language, reasoning, and comprehension while being tested, completing a questionnaire, or responding to interview questions that are open-ended." In other words, the nature of individuals' mental processes in producing test responses has been presupposed or taken for granted. Reviewing recent developments in cognitive science and artificial intelligence, with special reference to the distinction between declarative and procedural knowledge and the differences between formal and natural languages, Cicourel discusses some of the tensions between the algorithmic, formalistic, context-free assumptions of the information-processing model implicit in, for example, expert systems for medical diagnosis, psychometric tests, and survey research questionnaires and the nonalgorithmic, context-dependent characteristics of the practical discourse and reasoning processes of interviewees, sub-

jects, and respondents. He calls for a reconsideration of the way we design assessment devices as we learn more about the interaction of informal and formal knowledge processes, and he raises questions about the ecological validity of current measuring instruments.

In chapter 11, Levine advances a position of "methodological pluralism." He argues that the effort to discover a unitary privileged type of knowledge or essential criteria of "real science" must fail "because of the irreducible variety of values, norms, and motives that organize all kinds of action, contemplative as well as conative or practical." Examining what he refers to as the "Babel of contending intellectual positions in our time," Levine holds that it is possible for each of two or more conflicting approaches of knowledge to attain a status of cognitive privilege. Presenting a classification of cognitive features that are the constitutive components of any approach to social knowledge, Levine breaks down intellectual approaches into seven elements, including, for example, "empirical procedures," "explanatory logics," "epistemic products." In the light of his analysis of the elementary components of any approach to social knowledge, Levine argues that endless confusion results from labeling someone a positivist or a Marxist or an empiricist or a Freudian or a Durkheimian. According to Levine, his analytic scheme "provides a more coherent way for critics to assess the value of different kinds of social knowledge—to indicate what kinds of social knowledge may legitimately claim privileged status, and why."

While chapters 1–11 of the book are revised or expanded versions of materials presented at the conference, the next six chapters present the reactions, commentaries, criticisms, or observations stimulated by the conference papers and discussions. They vary considerably in their tone and in their conception of social science research.

In chapter 12, Richter, a geophysicist, presents his reactions to the conference in a discussion of nonlinear behavior. Richter, whose own research is on problems in plate tectonics and fluid dynamics, argues that complexity is the natural outgrowth of the dynamics governing nonlinear systems and that, to the extent the social sciences are studying nonlinear systems, "complexity need not be a reflection of some deficiency in the modes of discourse or analysis, but may well be an inherent property of the systems being studied." Richter constructs a model of a nonlinear system interrelating fear, guilt, and aggressiveness. He tries to show that in nonlinear systems, generalizations about behavior are quite context

dependent and “may even be contradictory when derived from different sets of observations taken at different times.” Nevertheless, the equations describing the underlying dynamics of the system can be relatively simple; Richter argues that social science research might be aimed “at understanding particular elements of this dynamic, instead of seeking universally valid statements about behavior itself.” His overall view is that there is no single standard for scientific achievement in the physical sciences, that there are notable similarities between areas of the social sciences and the physical sciences, and that one of the “external” political problems for the social sciences is “not so much that it studies man, but that all men judge its success.”

In chapter 13, Wimsatt argues that the phenomenon of unpredictability is quite common throughout all the sciences and is not distinctive of the social sciences. When the covering law model of explanation does hold, suggests Wimsatt, it is because we have been able to isolate systems, treat their properties as context independent, and manipulate them as simple idealizations. The major aim of Wimsatt’s chapter, however, is to examine the role of “heuristics” in scientific and everyday problem solving: the cost-effective advantages of heuristic (vs. algorithmic) procedures as well as the systematic biases engendered by reliance on problem-solving heuristics. Wimsatt, a philosopher of biology and the social sciences, poses the question, Why have most major evolutionary theorists favored explanations in terms of “individual selection” over “group selection”? He argues that extant mathematical models attempting “to assess the relative efficacy of individual and group selection processes” have been systematically biased in favor of individual selection because of the reductionist model-building heuristics that are built into the models. Wimsatt identifies several features that characterize a “heuristic” problem-solving procedure and distinguish it from an algorithmic procedure.

Meehl (chap. 14) takes us on a spirited tour of the conceptual foundations of science. Reacting to themes and issues that came up in conference discussions, he presents his own tripartite division of science into functional-dynamic theories, structural-functional theories, and evolutionary theories; and he addresses issues having to do with quantification, open concepts, realism, the covering law model of explanation, and the imputation of motives or intentions. Critical of idealism, relativism, abstract discussions about the nature of science, and social scientists’ spending too much time on problems best dealt with by philosophers, Meehl says we all know

that logical positivism is dead and that too much time was spent at the conference saying so. He speculates that the function of such discussions is to “relieve scientific guilt feelings or inferiority feelings about our disciplines,” and he believes that an “intellectual gloominess” associated with a relative lack of credentialed knowledge characterizes various parts of the social sciences. “It is as if somebody said ‘Well, maybe clinical psychology isn’t up to the standards of historical geology or medical genetics, let alone theoretical physics, but we needn’t be so fussy about our concepts and the empirical support for them because logical positivism, which was so stringent on that score, is a nefarious doctrine, and we are no longer bound by it.’” Meehl then advances the “controversial thesis” that “owing to the abusive reliance in the sciences upon significance testing—rather than point or interval estimation, curve shape, or ordination—the usual article summarizing the state of the evidence on a theory (such as appears in the *Psychological Bulletin*) is nearly useless.”

In chapter 15, Rosenberg, a philosopher of science, argues that philosophy of science has little to contribute to the social sciences, which from his point of view is merely another way of saying that all theoretical discussions should be put aside until a relatively late date in the development of a science. He argues that there is no principled line to be drawn between science and philosophy, no formal way to distinguish, for example, the empirical from the metaphysical. There is, however, a continuum that extends from factual reports and lower-level findings and generalizations to high-level theory; and, Rosenberg argues, progress is more likely to come in the social sciences by piling up findings rather than engaging in theoretical dispute. Presenting the idea that “data never point to a unique theory and are always compatible with mutually inconsistent theories” (compare Gergen’s chapter), Rosenberg is doubtful that theoretical consensus can ever be achieved. Nevertheless, he suggests that agreement can be secured about what we need to know or find out, without recourse to theory or, which amounts to the same thing, philosophy.

In chapter 16, Holzman presents an argument for the unity of the sciences and against the view that “the social sciences are in a domain separate from the natural sciences.” On the one hand, he advances the view that all phenomena are context dependent and that “reasons” can be “causes.” On the other hand, he opposes any attempt to restrict social science inquiry to the study of meaning, since that “would rule out a whole set of regularities that call for

social science investigation with respect to lawful regularities." Holzman goes on to present a gloomy assessment of social science contributions to the field of mental health and mental illness, a failure he attributes in part to an overconcern with methodology and a retreat from nomothetic science (compare Kleinman's chapter). He calls for the development of the institution of supradisciplinary committees to identify "important issues that need study."

In chapter 17, Frankel uses Fiske's essay as a taking off point for a critique of the unity of science position (or "methodological unitarianism") and "neopositivism" characteristic of several chapters in the book. She argues that among the conference participants, "the outlook appeared most dim to those strongly committed to traditional norms of unified science, and least so to those willing to consider that science may not be unifiable." Frankel outlines a position of scientific pluralism or "methodological polytheism." She recommends that we not lament the "noise introduced into social science by the humanness of human beings" and addresses the problem of reductionism and emergentism in science.

A final summary chapter by Fiske and Shweder (chap. 18) identifies several topics concerning pluralism versus monism and subjectivity versus objectivity that recurred in the conference discussion and in various chapters of the book.

Reading these chapters back to back, one is struck by the richness and variety of the formulations, the diversity of the ways in which any two chapters might be said to be alike or different, the shifting quality of the alliances that might be formed among authors, and the numerous points of tension. Some chapters emphasize the similarities among the sciences, others emphasize the differences, and the similarities and differences that are emphasized are not necessarily the same ones from chapter to chapter. Some authors rededicate themselves to a search for lawlike regularities in behavior; others renounce that quest. Hermeneutics has its advocates and its detractors. The achievements of the social sciences and their contributions (for example, to medicine) are variously assessed.

The volume in which these essays have been placed side by side has been entitled *Metatheory in Social Science*. Taken together, the essays are representative of the postpositivist intellectual climate of our times. There was once a time, not so long ago, when the very idea of rationality was equated with the results and findings of positive (i.e., objective) science. The results of positive science were considered worthy of respect because scientists had possession of a unitary method for discovering truths, and they knew how to em-

ploy that method to discover useful knowledge. Over the last several decades, that picture of science and the equation of science and rationality have taken their lumps. The drift in contemporary thinking has been to raise serious doubts about whether there are any standards, canons, or methods definitive of scientific or rational thinking. The idea of objectivity associated with positive science has been under attack from many quarters, and it has been variously defended, clarified, revised, and abandoned. The essays in this volume represent the wide range of alternative positions concerning science and subjectivity-objectivity that one might credibly adopt in a postpositivist world.

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