Action Science

Concepts, Methods, and Skills for Research and Intervention

Part One

Designing a Science of Human Action

> To proceed beyond the limitations of a given level of knowledge, the researcher, as a rule, has to break down methodological taboos which condemn as "unscientific" or "illogical" the very methods or concepts which later on prove to be basic for the next major progress.
>
> —Kurt Lewin (1949)

The idea of an action science raises thorny philosophical and conceptual issues. Action and science are central concepts in Western thought that are more often contrasted than conjoined. We are accustomed to distinguishing between theory and practice, between thought and action, between science and common sense. Action science proposes to bridge these conceptual chasms. At this point, to be sure, our bridges are more like the slender ropes of explorers than concrete and steel cables. But we hope they help to identify the barriers to an action science and to suggest how those barriers might be overcome.

In Chapter One we place action science in the context of

contemporary debates in the philosophy of science. We ask what the essential features of scientific deliberation are. In our review of the mainstream account of science, we identify hard data, explicit inferences, empirically disconfirmable propositions, and systematic theory as the core features, and we emphasize the role of a community of inquirers who can rationally criticize each others' claims. These are also core features of action science. We then discuss the hermeneutic approach to the human sciences to identify the problems of interpretation that may inhibit rigorous testing. We turn to a third account of science, that associated with the work of Kuhn, for its views on the role that judgment and interpretation play in debates among scientific groups. Drawing from these three accounts of science, we suggest that the features of rational deliberation in science may also come to characterize deliberation in practical affairs. Such is the thrust of action science.

In Chapter Two we discuss the conceptual underpinnings of action science. As a science that hopes to produce knowledge that can inform action, action science requires a conception of practical knowledge that goes beyond the common conception of choosing means to achieve predetermined ends. Following Schön (1983), we emphasize the role of the agent in setting problems as well as in solving them and the importance of reflecting on action to discover the tacit knowledge embedded in it. We then describe how action science makes it possible to test competing interpretations in the action context. Just as the scientific community of inquiry is the basis of scientific rationality, so the norms and rules of inquiry in the behavioral world of a client system are the basis of deliberation in practical affairs; and action science addresses itself to those norms and rules of inquiry. We conclude the chapter by suggesting that action science is an exemplar of critical theory as formulated by the Frankfurt School. A critical theory seeks to engage human agents in public self-reflection in order to transform their world.

In Chapter Three we present the theoretical orientation that informs our work, the theory of action approach (Argyris and Schön, 1974, 1978). This is not the only conceivable ap-

proach to action science, but it is the one that has enabled us to envision an action science and to specify its features. The particulars of our approach are also a necessary preparation for our critique of other research methodologies in Part Two and our discussion of the process of learning skills with which to practice action science in Part Three.

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Philosophical and Methodological Issues

Action science is an inquiry into how human beings design and implement action in relation to one another. Hence it is a science of practice, whether the professional practice of administrators, educators, and psychotherapists or the everyday practice of people as members of families and organizations. Action science calls for basic research and theory building that are intimately related to social intervention. Clients are participants in a process of public reflection that attempts both to comprehend the concrete details of particular cases and to discover and test propositions of a general theory.

In the following chapters we will discuss these key features of action science: (1) empirically disconfirmable propositions that are organized into a theory; (2) knowledge that human beings can implement in an action context; and (3) alternatives to the status quo that both illuminate what exists and inform fundamental change, in light of values freely chosen by social actors.

What kind of science has these concerns? The first feature, empirically disconfirmable propositions organized into a

theory, is characteristic of so-called mainstream science. In it, scientific theories are seen as hypothetical-deductive systems that explain and predict regularities among events. But there is a traditional counterview that argues that the sciences of action cannot take this form, because the interpretive understanding of meanings cannot be reduced to regularities among events. Instead, human beings in everyday life create meanings and guide their actions accordingly. Clarifying the nature of action science will require that we examine this debate between the mainstream account of science and its counterview.

The second feature, knowledge that can be implemented by human beings in an action context, may suggest that we are speaking of applied science. If "applied" means no more than "intended for use," we can have no objection to this label; and indeed the tradition from which action science springs is commonly referred to as applied behavioral science. But applied science is a term that takes meaning from its contrast to basic or pure science. The dichotomy between basic science and applied science reflects a division of labor embedded in the mainstream account of science: The basic scientist generates fundamental, generalizable knowledge that is then put into practice by the applied scientist. We believe that this division of labor reinforces a pernicious separation of theory and practice. Action science attempts both to inform action in concrete situations and to test general theory. Recasting the concept of applied science will lead us to reflect on the nature of practical knowledge, a form of knowing that is traditionally contrasted to theoretical or scientific knowledge.

The third feature of action science, alternatives to the status quo that illuminate what exists and inform fundamental change in light of values freely chosen by social actors, clashes with mainstream conceptions of science. The action scientist takes a normative position. Mainstream science has sharply separated empirical theory from normative theory, and has cast doubt on the scientific legitimacy of normative theory. The split between empirical theory and normative theory is related to the split between theory and practice. Practitioners in the applied behavioral sciences have long recognized that their prac-

tice has a normative dimension. From the perspective of the mainstream account, the values of the practitioner must be sharply distinguished from those of science. Many advocates of the counterview, also, have insisted that the theorist must take a disinterested stance. We take a different view, one that we explain by drawing on the idea of critical theory as developed by scholars of the Frankfurt School, a group of German philosophers that includes Horkheimer, Adorno, and Habermas. A critical social science includes aspects of the empirical-analytic sciences (mainstream account) and the historical-hermeneutic sciences (counterview), but goes beyond them to criticize what is from the perspective of what might be. Justification of the normative stance of critical theory is based on internal criticism of the practices of the community to which it is addressed. A critical social science engages human agents in self-reflection in order to change the world.

Roots of Action Science

In proposing an action science, we take as our point of departure our own practice as researchers, educators, and interventionists working with the theory of action approach (Argyris and Schön, 1974, 1978). It is through reflecting on our practice and relating it to other literatures, including those of the philosophy of science and of social inquiry, that we hope to articulate an action science. The present book builds on previous analyses of mainstream social science (Argyris, 1980) and on reflection on the epistemology of practice (Schön, 1983).

Action science is an outgrowth of the traditions of John Dewey and Kurt Lewin. Dewey (1929, 1933) was eloquent in his criticism of the traditional separation of knowledge and action, and he articulated a theory of inquiry that was a model both for scientific method and for social practice. He hoped that the extension of experimental inquiry to social practice would lead to an integration of science and practice. He based this hope on the observation that "science in becoming experimental has itself become a mode of directed practical doing" (1929, p. 24). This observation, that experimentation in science is but a special case of human beings testing their conceptions

in action, is at the core of the pragmatist epistemology. For the most part, however, the modern social sciences have appropriated the model of the natural sciences in ways that have maintained the separation of science and practice that Dewey deplored. Mainstream social science is related to social practice in much the same way that the natural sciences are related to engineering. This contrasts sharply with Dewey's vision of using scientific methods *in* social practice.

One tradition that has pursued the integration of science and practice is that exemplified by Lewin, a pioneer in group dynamics and action research. Lewin is considered the founder of the cognitive tradition within social psychology in America (Nisbett and Ross, 1980, p. 5). Citing the classic Lewinian studies of democratic and authoritarian group climates, Festinger suggests that it is because Lewin showed how complex social phenomena could be studied experimentally that many regard him as the founder of modern experimental social psychology (1980, p. viii). This is not to say, however, that each of the many research programs that can trace their core ideas to some aspect of Lewin's work are also consistent with action science. We consider Lewin himself to have been an action scientist. But since his time there has been a tendency to divorce his contributions to science from those to practice. Research in social psychology has relied on experimental methods for testing hypothesized relationships among a few variables, and it has become distant from practice. Practitioners in the applied behavioral sciences, with some exceptions, have focused on helping clients and have given little attention to testing scientific generalizations.

The Lewinian tradition of action science, in contrast, is that of scholar-practitioners in group dynamics and organizational science who have sought to integrate science and practice (for example, Argyris, 1957, 1962, 1964, 1970; Bennis and others, 1976; Bennis and others, 1973; Bradford, Gibb, and Benne, 1964; Blake and Mouton, 1964; Jaques, 1951; Likert, 1961; McGregor, 1960; Susman, 1983; Trist, 1981). Members of this tradition have emphasized the continuities between the activities of science and the activities of learning in the action context, the mutually reinforcing values of science, democracy, and

education, and the benefits of combining science and social practice.

Lewin produced several conceptual maps that showed how it was possible to bridge the tensions between science and practice. As Gordon Allport noted, "Lewin's concepts are arresting because they serve equally well in depicting concrete situations, and in the task of making scientific generalizations" (Lewin, 1948b, p. viii). These conceptual maps have proven extraordinarily fruitful, both in stimulating subsequent research and in informing behavioral science intervention. They include the idea that social processes are "quasi-stationary equilibria" maintained by a balancing of driving and restraining forces, with the related heuristic that change is better accomplished by reducing restraining forces than by increasing driving forces (Lewin, 1951). The technique of force field analysis continues to be widely used by behavioral science interventionists. A second set of concepts is found in Lewin's three-step model of change as unfreezing, moving, and freezing (Lewin, 1964; Schein, 1979; Hackman and Suttle, 1977). A third set of concepts relates aspiration level and psychological success, which we will discuss in Chapter Nine. Other ideas developed by Lewin include those of "gatekeeper" and "space of free movement," which were used to explain the results of the Lewin, Lippett, and White (1939) experiments on authoritarian and democratic group climates. Such concepts may serve as exemplars for theory development in action science.

Lewin was committed to the kind of science that would improve social practice. His early concepts of action research, an activity that involves studying social systems by changing them, were the seeds of action science. Although Lewin never wrote a systematic statement of his views on action research, several themes stand out (Lewin, 1948a, 1948b, 1951; Lewin and Grabbe, 1948; Marrow, 1969; Benne, 1976; Joiner, 1983; Peters and Robinson, 1984):

1. Action research involves change experiments on real problems in social systems. It focuses on a particular problem and seeks to provide assistance to the client system.

2. Action research, like social management more generally, involves iterative cycles of identifying a problem, planning, acting, and evaluating.
3. The intended change typically involves *reeducation,* a term that refers to changing patterns of thinking and acting that are presently well established in individuals and groups. The intended change is typically at the level of norms and values expressed in action. Effective reeducation depends on participation by clients in diagnosis and fact finding and on free choice to engage in new kinds of action.
4. Action research challenges the status quo from a perspective of democratic values. This value orientation is congruent with the requirements of effective reeducation (participation and free choice).
5. Action research is intended to contribute simultaneously to basic knowledge in social science and to social action in everyday life. High standards for developing theory and empirically testing propositions organized by theory are not to be sacrificed, nor is the relation to practice to be lost.

Philosophies of Action and Science

Any claim to knowledge can be challenged by asking, "How do you know what you think you know?" Answering this question is the domain of epistemology, the theory of knowledge. It has been argued that epistemology has been the central concern of philosophy since Descartes (Rorty, 1979). And at least since the time of Newton, it has seemed that science has been the preeminent way in which human beings have generated reliable, cumulative knowledge. Hence it is not surprising that much of modern philosophy has been concerned with distinguishing science from nonscience and with specifying the conditions of scientific knowledge, an enterprise known as the philosophy of science.

There has been a second approach to the problem of epistemology, that of the analysis of ordinary or commonsense knowledge (Popper, 1959, p. 18). This approach is favored by later analytic philosophy, as practiced, for example, by the later

Wittgenstein, Strawson, Ryle, Hampshire, and Austin (Bernstein, 1971, p. 260). These philosophers have concentrated on the analysis of concepts pertaining to action. The tradition of Continental phenomenology, which has been concerned with the world of everyday life, has also preferred the second approach to epistemology.

These two approaches have collided in the philosophy of social science. According to the mainstream account of science, a view whose origins can be traced to the empiricism of Francis Bacon, Thomas Hobbes, David Hume, and John Stuart Mill, the epistemology of the social sciences is (or should be) essentially the same as that of the natural sciences. According to the traditional counterview, which arose in the nineteenth century to oppose the extension of the methods of the natural sciences to the human sciences, understanding the meanings that are the essence of social action is fundamentally different from explaining events of the natural world. The debate between these two viewpoints has continued for the past century. It is reflected, for example, in Burrell and Morgan's (1979) analysis of the more or less tacit sociological paradigms that underlie organizational theory and research. The debate has become increasingly vigorous in recent years as interpretive approaches to social inquiry press their claims against the mainstream, which they sometimes label "positivism."

In the rest of this chapter we will discuss these and other themes in the philosophy of science in order to clarify the idea of an action science. In our discussion of the mainstream account of science we will identify the core features of science that also characterize our approach. Our discussion of the counterview will identify the problems of interpretation that face the sciences of action and that are often said to render the core features of mainstream science inapplicable to the sciences of action. We believe that it is possible to implement the core features of science in the action context, and we make this argument in the following chapter.

Our argument will be that different accounts of science can be understood in terms of their construal of the relation between science and community. This approach is congruent

with recent work in the philosophy and history of science. Bernstein argues that there is growing agreement that "the significant epistemological unit for coming to grips with problems of the rationality of science" is the scientific community, "an ongoing historical tradition constituted by social practices" (1983, p. 24). This view implies that the standards by which beliefs are criticized, evaluated, and justified are embedded in such social practices as forms of argument. Knowledge is community based, as it were. Indeed, all contemporary accounts of science agree that science is a social enterprise, carried on within communities of inquiry according to practices or rules for distinguishing valid from invalid claims. There is deep disagreement, however, about the characteristics of these communities and their practices.

We will discuss four construals of the relation between science and community. The mainstream view establishes, as a logical requirement for the justification of knowledge claims, a community of inquirers who can rationally criticize each other's claims. This notion, that scientific rationality is grounded in a community of inquiry, goes back at least as far as the pragmatist philosopher Charles S. Peirce, whose views on this matter are echoed in the work of Karl Popper.

The counterview of the relation between science and community rests on the observation that the sciences of action take as their domain communities of social practice. These sciences deal in "constructs of the second degree," in Schutz's phrase (1962, p. 59), because the scientist must first grasp the meanings embedded in the community being studied. Theorists of the counterview are concerned with how knowledge of the commonsense understandings of social actors is possible. In this sense the human sciences may be said to be built on an epistemology of practical knowledge.

A third view of the relation between science and community is that associated with the work of Kuhn (1962). Kuhn focuses on the scientific group as a community of practice with a distinctive language that to some degree cuts it off from other groups, and he asks what kind of rationality governs debates among different groups. This perspective can be understood as a

way of seeing the mainstream view through the lens of the counterview. The epistemic principles of science are seen as embedded in the practical knowledge of groups of scientists.

The fourth view is that of action science, which seeks to enact communities of inquiry in communities of social practice. Such inquiry is a form of practical deliberation, one that is guided by norms of science as well as by norms of practice. In action science we build on the practices for coming to agreement in everyday life, in ways that make them more consistent with scientific values such as valid information and public testing.

Mainstream Account of Science

This account, corresponding to what Scheffler (1982) calls the "standard view," is widely accepted both by practicing scientists and by the informed public. While it was designed with the natural sciences in mind (especially physics), proponents argue that it characterizes all sciences insofar as they are scientific; and this has been the predominant opinion among social scientists (Bernstein, 1976). The mainstream account goes under the names of logical empiricism, critical empiricism, or critical rationalism, and is heir to the tradition of logical positivism. It has been discussed by such philosophers as Hempel (1965a, 1966), Popper (1959, 1963), Nagel (1979), and Scheffler (1981, 1982). Among the social scientists who have discussed it are Merton (1967), Campbell and Stanley (1963), and Cook and Campbell (1979).

In the mainstream account, the core features of science are "hard" data (that is, data whose validity can be checked by different observers), explicit inferences connecting data and theory, empirically disconfirmable propositions subject to public testing, and theory that organizes such propositions. Underlying these requirements is the community of inquiry that is basic to science.

Peirce was perhaps the first to argue that scientific knowledge is legitimated by the practices of a community of inquirers. He noted that no single individual should be the absolute judge of truth. No matter how strong one's inner certainty, be-

lief might be based on prejudices that one has not realized could be questioned (Peirce, 1960, pp. 80-81). The test of truth is rather that a community of investigators, beginning with different assumptions and free to criticize any aspect of each other's work, converge on a set of beliefs. They can never be certain that their beliefs are true, but they can approach truth through a self-corrective process of rational criticism in a community of inquiry.

Scheffler has emphasized that the ideal of objectivity, which is central to the mainstream conception of science, implies independent control over assertion. Like Peirce, Scheffler links the notion of community with that of openness to possible error: "To propound one's beliefs in a scientific spirit is to acknowledge that they may turn out wrong under continued examination, that they may fail to sustain themselves critically in an enlarged experience. It is, in effect, to conceive one's self of the here and now as linked through potential converse with a community of others, whose differences of location or opinion yet allow a common discourse and access to a shared world" (1982, p. 1).

The model of scientific explanation that is central to the mainstream conception of science has been formulated by Popper (1959) and Hempel (1965b), although the basic idea goes back to David Hume and John Stuart Mill. Popper writes, "To give a *causal explanation* of an event means to deduce a statement which describes it, using as premises of the deduction one or more *universal laws,* together with certain singular statements, the *initial conditions*" (1959, p. 59).

Scientific theories are deductive systems of universal laws. Particular events are explained by subsuming them under universal laws. The validity of proposed laws can be tested by deducing from them, in conjunction with certain initial conditions, descriptions of events that should be observed. Thus explanation and prediction are symmetrical, differing only with respect to whether the deduction is made before or after the observation of the event explained or predicted.

This model of explanation, which Hempel (1965a) calls the deductive-nomological model, may be modified by allow-

ing the use of laws that are statistical rather than universal. In either case explanation is achieved by subsuming events under laws; hence each may be called a covering-law model. In the mainstream account, the covering-law model is the general form of explanation in all sciences, including the social sciences and history.

Two levels of scientific systematization are distinguished. The first level is that of observational laws, or statements of empirical regularities, as, for example, "water freezes at 32° F." The second level is that of theoretical laws—for example, a theory of molecular structure. Observational laws, consistent with the covering-law model, are explained by proposing theories from which they can be deduced. It is only with the development of theory from which empirical generalizations may be derived that we achieve major advances in scientific systemization.

The mainstream account distinguishes sharply between the context of discovery, which pertains to generating ideas and putting forth theories, and the context of justification (Popper, 1959, p. 31; Nagel, 1979, pp. 12-13). What is distinctive about science is not the process by which theories are proposed, but the systematic testing that they must survive if they are to be regarded as valid. It will help to understand this position if we consider the view of science that mainstream philosophers are concerned to reject. This is the view, common since the time of Francis Bacon, that science is the practice of an inductive method. In this view, the scientist carefully observes without preconceptions and then generalizes from these observations; the warrant of truth is the purity of observation and inductive inference. But, mainstream philosophers point out, for observation to be useful it must begin with some preconceptions of what is important (Popper, 1963, p. 46; Hempel, 1966, p. 11). Furthermore, they insist that there are no rules for inferring theories from observation. The scientist must invent a hypothesis, drawing on whatever sources of inspiration may be fruitful. Since this creative process cannot be systematized, the validity of scientific theories cannot depend on the context of discovery. Rather, it depends on testing what Whewell calls "happy guesses" (Hempel, 1966, p. 15) in the context of justification.

A proposed theory is tested by holding it responsible for the empirical implications that can be deduced from it. If these implications do not correspond to what is actually observed, then the theory (or some of the auxiliary hypotheses involved in the deduction) may be rejected. If a theory has no empirical implications, it cannot be tested, and it is for that reason not an acceptable scientific theory.

It has proven quite difficult to specify the appropriate logical relations between theoretical statements and observation (Hempel, 1965a, p. 101; Scheffler, 1981, p. 127). Without recounting the history of such attempts, we may note that an initial step was to conceive of observations as formulated in observation sentences. Observation sentences—or "basic statements," as Popper (1959) called them—are of the form "the cat is on the mat," or "at time *t*, the needle of meter *m* coincided with line *l*." The crucial characteristic of observation sentences, or of a *data-language,* is that under suitable conditions different individuals can come to a high degree of agreement that the sentence is true or false by means of direct observation. And, of course, empirical testing of scientific theories depends on the possibility of intersubjective agreement, at the level of observation, among individuals who may disagree at the level of theory.

An important contribution to the mainstream conception of empirical testing has been Popper's idea of *falsifiability.* Popper was concerned with the problem of demarcation, that is, of finding a criterion to distinguish scientific from nonscientific theories (1963, p. 40). He proposed that a scientific theory must be falsifiable, in the sense that the theory must be incompatible with certain possible results of observation (p. 36). Genuinely scientific theories must make risky predictions—predictions that might turn out to be false. On this basis Popper explained his dissatisfaction with psychoanalytic theories: They were not scientific because "there was no conceivable human behavior which could contradict them" (p. 37).

The criterion of falsifiability accords well with Popper's emphasis on rational criticism. Consistent with his distinction between the context of discovery and the context of justification, he argues that the growth of scientific knowledge occurs

through conjectures that are controlled by critical testing. While we cannot hope to know that proposed theories are true, we can hope to detect and eliminate error, and thereby approach truth, by criticizing the theories and guesses of others. Hence the falsification criterion is an extension of the insight that the possibility of discovering error is central to the generating of reliable knowledge. The possibility of discovering error in proposed theories, furthermore, depends on the possibility of intersubjective agreement at the level of data and on explicit inferences that identify the theoretical implications of particular observations.

Strong tests require that hypotheses and predictions be stated prior to observation, because if observations are made and then explained, hypotheses may be selected to fit the data. We may further distinguish between passive observation and experimentation. In the first case, the researcher predicts what will occur and observes if the prediction is confirmed. In the second case the researcher brings about or prevents certain conditions that, if the hypothesis being tested is true, should lead to the occurrence or nonoccurrence of certain observable events. Experimentation is the most powerful methodology for testing theories because, by manipulating the initial conditions, the researcher can rule out alternative explanations (Campbell and Stanley, 1963; Cook and Campbell, 1979).

Accuracy is a most important criterion in choosing among competing theories. Other relevant criteria include the scope, simplicity, and fruitfulness of a given theory. When a new theory replaces an older one, the observational laws explained by the older theory are subsumed under the newer theory. Hence scientific knowledge is cumulative, as wider ranges of empirical phenomena come to be organized by deductive systems.

We can illustrate the mainstream view of scientific explanation with Merton's reformulation of Durkheim's theory of suicide (Merton, 1967, pp. 150-153). Merton's intention was to clarify the function of sociological theory and its relation to empirical research, and Bernstein (1976, pp. 11-14) takes Merton's account as exemplary of the best thinking of mainstream social scientists on this matter.

The empirical generalization that Durkheim sought to explain was the statistical uniformity that Catholics have a lower suicide rate than Protestants. Consistent with the covering-law model discussed earlier, the theorist's task is to state a set of "universal laws" and "initial conditions" from which this empirical regularity can be derived. Merton (1967, p. 151) restates Durkheim's theoretical analysis as follows:

1. Social cohesion provides psychic support to group members subjected to acute stresses and anxieties.
2. Suicide rates are functions of unrelieved anxieties and stresses to which persons are subjected.
3. Catholics have greater social cohesion than Protestants.
4. Therefore, lower suicide rates should be anticipated among Catholics than among Protestants.

Statements (1) and (2) are proposed scientific laws, while statement (3) serves as an initial condition. Given these three statements, statement (4) can be derived. As Merton notes, this example is highly simplified. We may consider it a theoretical fragment, part of a complex theoretical system that has not been fully articulated.

Merton uses this example to illustrate several functions of theory. It identifies relevant features of an empirical generalization by relating it to concepts at higher levels of abstraction, such as social cohesion. It makes it possible to connect diverse findings, such as suicide rates, divorce rates, and incidence of mental illness, all of which may be related to the degree of social cohesion. And it provides grounds for predictions that can serve to test the theory. For example, if social cohesion among Catholics declines, their suicide rate should increase. Merton also notes that theory can adequately serve these functions only if it is sufficiently precise to be testable. On the one hand, for example, it must be possible to determine if social cohesion has increased or decreased among a particular group. On the other

hand, the appropriate degree of precision depends on the state of the science in question. A premature insistence on precision may inhibit progress by leading scientists to formulate their problem in ways that permit measurement but that have limited relevance to significant features of the problem.

As Merton notes, the generalization that Catholics have a lower suicide rate than Protestants "assumes that education, income, nationality, rural-urban residence, and other factors which might render this finding spurious have been held constant" (1967, p. 150n). This assumption identifies an important feature of mainstream social science, and one that is associated with much of the methodological apparatus of social research: The many variables impinging on the phenomena of interest must be held constant so that particular causal linkages can be identified. Experimental methods achieve this aim either by standardizing the experimental situation or by randomly assigning subjects to conditions. Correlational methods rely on statistical techniques for factoring out the influence of variables other than the focal variable.

Mainstream Science and Action Science

We have said that there are continuities in the core features of mainstream science and action science, including hard data, explicit inferences, public testing, and systematic theory. But there are crucial differences as well, some of which we can highlight by raising the following question of the Merton/Durkheim example: What form must scientific knowledge take in order to help us reduce the incidence of suicide? In terms of the mainstream account, to raise this question is to shift the focus of attention from basic or pure science to applied science. Merton, whose interest was in using the Durkheim example to identify features of theory in the social sciences, apparently did not consider it necessary to consider the relation of theory and practice. It is as if the theoretician need be responsible only to the criteria of pure science, leaving it to the applied scientist to tailor basic knowledge to practical ends. We will argue, in con-

trast, that theory that intends to contribute to practice should have features that differ from those of theory responsible only to the criteria of pure science.

Suppose that a mainstream social scientist was interested in using sociological theory to reduce the incidence of suicide. A common approach to using social science knowledge is to formulate policies intended to affect variables thought to cause social problems. Durkheim's theory suggests that greater social cohesion will lead to lower suicide rates. The question then becomes, How might we increase social cohesion? This points to one of the theoretical requirements of the applied social sciences identified by Gouldner (1961): Theory should identify variables that might be controlled by human beings to bring about change in the problem of interest. Thus the social scientist might suggest that housing policy in urban areas be aimed at fostering neighborhoods, in the belief that this will enhance social cohesion and thereby reduce crime, mental illness, and suicide.

If policies are to have an impact, they must be implemented; and their implementation has not been markedly successful, whether in the realm of urban policy (Pressman and Wildavsky, 1973), or in the realm of strategic planning in organizations (Argyris, 1985). This has rarely been a central concern of social theorists. Implementation has been seen as a problem of application, of practice, perhaps of politics, but not of theoretical science. From the perspective of action science, however, implementation is not separable from crucial theoretical issues.

One such issue concerns the mainstream strategy of "holding other variables constant." Implementation means that human beings must design action in concrete situations. Any particular situation is a complex field of multiple, interdependent, conflicting forces. Theory for practice should help the practitioner to grasp the pattern of forces operative in the situation at hand, what Lewin (1951) called the "social field as a whole." Yet human beings cannot take account of everything; we have limited cognitive capacity (Simon, 1969). This suggests

that theory should try to identify patterns that, suitably combined, will be useful in many situations. It also suggests that theory should lend itself to testing in the action context so that the practitioner can make corrections on-line.

A second issue is that knowledge in the service of action cannot rest solely on the analysis of social statistics. It is necessary to get at the meanings embedded in action, at the logic of action. Social statistics are so abstracted from the action context that they do not provide a reliable guide to action in particular situations. This criticism is explored in depth by Douglas (1967) in a critique of Durkheim's study of suicide. Douglas, who is a representative of the counterview that we will discuss in the next section of this chapter, argues that "it is not possible to study situated social meanings (for example, of suicide), which are most important in the causation of social actions, by any means (such as questionnaires and laboratory experiments) that involve abstracting the communicators from concrete instances of the social action (for example, suicide) in which they are involved" (p. 339).

We do not mean to suggest that social statistics have no place in practical deliberation. But their informed use depends on *interpretation* and *judgment* of their relevance in the situation at hand. These are forms of knowing that are frequently contrasted with scientific knowing, as understood in the mainstream account of science.

A third issue is that practice involves the normative dimension. Action intended to increase social cohesion will, if effective, have an impact on the lives of human beings. Is the kind of increased cohesion that might be brought about a desirable objective, on balance? While reducing the suicide rate, might it limit opportunities for differentiation? Who should decide among the probable trade-offs? Such practical, ethical questions are typically finessed by social scientists, who leave them to be decided in the political arena. But practical concerns should not be regarded as tangential to theoretical social science, in our view. Rather, practice should be regarded as interdependent with the ways that knowledge is generated and with the kinds of theory sought.

Counterview: The Logic of Action

Although social scientists have generally endorsed the mainstream account of science, there has been a traditional counterview arguing that the sciences of social action must take a different form than the natural sciences. Social phenomena are meaningful to the human beings who enact them, whereas the events of the natural world proceed quite independently of subjective meanings. In the mainstream account, this difference does not make a difference for the logic of scientific inquiry. But proponents of the counterview insist that this difference is crucial.

The counterview is not a unified movement, but rather a convergence of approaches that focus on social action and align themselves against the mainstream account of science. Wilhelm Dilthey, a German philosopher and historian with whom accounts of the counterview often begin (Dallmayr and McCarthy, 1977; Howard, 1982), directed his arguments against spokesmen of positivist empiricism such as John Stuart Mill. Whereas Mill had argued in *A System of Logic* that "the backward state of the Moral Sciences can only be remedied by applying to them the methods of Physical Science, duly extended and generalized" (cited by Putnam, 1978, p. 66), Dilthey insisted that generating reliable knowledge in the human sciences depended on understanding meanings and that the appropriate methodological model was hermeneutics, the art of textual interpretation. Contemporary advocates of hermeneutics include philosophers in the phenomenological tradition such as Gadamer and Ricoeur. The most influential offshoot of this tradition for empirical research has been Alfred Schutz's phenomenological sociology. While that is perhaps the more direct line of descent from Dilthey, philosophers in the analytic tradition inspired by the later Wittgenstein, such as von Wright (1971) and Taylor (1977), also advocate a hermeneutic approach to understanding social action. Bernstein (1976) discusses the intersection of these traditions and their critiques of mainstream social science.

The emphasis of the counterview on understanding meanings leads to a second construal of the relation between science

and community, one that has been formulated by Schutz in the phenomenological tradition and by Peter Winch in the analytic tradition (Bernstein, 1976, pp. 67-68, p. 139). It may be stated as follows: Interpretations in the human sciences are second order, in the sense that they are built on (and presuppose some understanding of) the commonsense interpretations of social actors themselves. To be sure, there are procedural rules of scientific inquiry—for example, the methodological principles of sociology or anthropology. In this respect the social scientist is part of a community of inquiry, as emphasized in the mainstream account. What is distinctive to the human sciences, however, is that they must grasp the meanings embedded in another community of practice, that which they are studying. The "otherness" of the community being studied is most obvious in the case of the anthropologist doing fieldwork in an exotic culture, whereas the social scientist's commonsense understanding of his or her own culture is often taken for granted (see Geertz, 1973, pp. 14-15). But a distinction may still be made between the scientific community of inquiry and the community of practice within which the actions being studied make sense. Theorists of the counterview are concerned with how knowledge of the commonsense understandings of social actors is possible. In this sense the human sciences may be said to be built on an epistemology of practical knowledge.

Some of the differences between mainstream and counterview may be illuminated by asking, To what extent are the human sciences based on hard data, as that concept is understood by the mainstream? Recall that in the mainstream account, empirical testing of scientific theories depends on the possibility of intersubjective agreement at the level of data among observers who may differ at the level of theory. The social sciences have developed methodological procedures to ensure that data meet this test. Advocates of the counterview have argued, however, that mainstream methodologies preclude inquiry into the rich layers of meaning constructed by social actors. But the interpretive studies of the counterview seem hopelessly "soft" to mainstream social scientists. Here we will describe some arguments characteristic of the counterview. It is

only when we describe our approach to action science, in the following chapter, that we will show how interpretive accounts may indeed be rigorously tested.

We will begin not with the contemporary mainstream view of what hard data should be, but with its predecessor. Recall that an important step in the development of the mainstream view was to conceive of observations as formulated in observation sentences or in a data-language. The logical positivist Rudolf Carnap, following the early Wittgenstein, proposed constructing a language of science in which all legitimate scientific statements could be expressed and that would exclude all "metaphysical" (or "cognitively meaningless") statements (Bernstein, 1971). Popper says of Carnap's proposal: "Psychology was to become radically behavioristic; every meaningful statement of psychology, whether human or animal, was to be translatable into a statement about the spatio-temporal movements of physical bodies" (1963, p. 265).

Behaviorists sought to implement this program in psychology by banishing cognitive terms from science or at least insisting that they be operationalized in terms of physical movements. This approach had the appeal of seeming to get down to the bedrock of physical movements and avoiding the indeterminacy of interpretation and meaning. This vision of the kind of data that are truly scientific has had enormous influence on the social sciences, especially in the United States. Even today, when the dominant orientations in psychology and social psychology are cognitive, it sometimes seems that the scientific ideal is to design measures that are machine readable, such as reaction times.

Later analytic philosophy, with its focus on concepts pertaining to action, can be understood as a reaction against Carnap's proposal for a physicalistic thing-language. Philosophers of action have argued that descriptions of action necessarily involve claims about the intentions of agents and the meanings of their actions (Taylor, 1964; Bernstein, 1971). They point out, for example, that the same physical movements may occur in different actions and that the same action may be carried out with different movements. They further argue that explanations

of action must take into account the beliefs of actors. It is the environment as understood by the agent, the "intentional environment" in Taylor's phrase, that is associated with action, not simply the environment as a set of physical objects. Contemporary mainstream philosophers of science agree that explanations of action will normally indicate the agent's objectives and beliefs (Hempel, 1965a, p. 469).

The question now becomes, How can the meanings understood by social actors become hard data? A traditional objection to the use of cognitive terms has been that beliefs and desires are subjective rather than objective, "in the heads" of actors rather than publicly observable. Against this view Taylor has pointed out that "it is a fact that we do make and verify statements using psychological concepts in ordinary speech" (1964, p. 88). How is this possible? How is it that the meanings of action are publicly accessible?

An answer that has been associated with early versions of the counterview was that the researcher should use a method of empathic understanding, a kind of imagining of the emotions experienced by another person (Dallmayr and McCarthy, 1977). This notion was ridiculed by the positivist philosopher Otto Neurath, who compared empathic understanding to a cup of coffee that stimulates the researcher's thinking (Howard, 1982, p. 29). But this approach is also rejected by contemporary advocates of the counterview such as Geertz, who writes, "The trick is not to get yourself into some inner correspondence of spirit with your informants. . . . The trick is to figure out what the devil they think they are up to" (1983, p. 58).

The contemporary view is that understanding action is like understanding a language. It depends on intersubjective meanings and shared practices, and it is a matter of knowing rather than feelings. Meanings are not private, in this view; they are publicly accessible. An early advocate of this view was Ryle (1949), who argued that the distinction between subjective and objective (in the sense of private mental events versus public physical events), and the consequent difficulty in understanding how psychological concepts could be verified, was a legacy of the Cartesian "dogma of the Ghost in the Machine." He in-

sisted rather that in using mental predicates, "we are describing the ways in which . . . people conduct parts of their predominantly public behavior" (p. 51). But the question remains, How is it that we can distinguish more and less accurate descriptions and that different observers can come to agreement on such matters? Ryle's answer is suggested in the following passage: "Understanding is a part of knowing *how*. The knowledge that is required for understanding intelligent performances of a specific kind is some degree of competence in performances of that kind" (1949, p. 54).

The competence required to understand action may be compared to the ability to speak a language. Von Wright, in a discussion of how we might verify attributions of intention, suggests, "Intentional behavior, one could say, resembles the use of language. It is a gesture whereby I mean something. Just as the use and the understanding of language presuppose a language community, the understanding of action presupposes a community of institutions and practices and technological equipment into which one has been introduced by learning and training" (1971, p. 114).

Like sentences in a particular language, actions make sense in a particular community of practice. The competence required to understand action is acquired with membership in the relevant community. Or, to shift to one of Ryle's examples, an observer can appreciate the stupidity or cleverness of chess-players only if he knows the game.

Perhaps the most popular way of accounting for social action has been in terms of rules. It would seem that rules are appropriate to the description of competent performances and that they simultaneously account for the possibility of recognizing competence. We can speak of competent performance only in instances where it would be possible to recognize a mistake, and the ability to recognize mistakes depends on knowledge of the appropriate system of rules. This argument is consistent with research procedures in linguistics, anthropology, and interpretive sociology. Thus sociologists may seek to discover rules of interaction by observing how members of a community deal with deviants. Linguists probe the intuitions of native speakers

and may test their understanding of rules by creating new sentences and asking if they are grammatical. Ethnographers query native informants and may seek to identify the rules of interaction that would enable one to pass for a member of the culture. These researchers explain the competent performances of members by specifying rules for generating the performances, and they rely on the tacit knowledge of members to identify rule violations (see Harré and Secord, 1972; Cicourel, 1974; Labov and Fanshel, 1977; O'Keefe, 1979; Van Maanen, 1979).

These arguments indicate that the knowledge required to understand action is embedded in the ordinary language and social practices of the community in which the action occurs. The interpretations of the human sciences are second order in the sense that they must first grasp the point of what actors do, as determined by the local context of rules and practices. But a problem remains: The interpretations even of "insiders," those with a member's grasp of the local language, often differ. This is especially true as we move from describing simple actions such as eating or combing one's hair to explaining complex patterns of action such as childrearing or supervising employees.

Even those familiar with and seemingly sympathetic to the counterview criticize its foremost theorists for not coming to grips with the problem of choosing among competing interpretations (see, for example, Bernstein, 1976). Here we will simply indicate some of the obstacles to coming to agreement on the "best" interpretation. Foremost among them is a feature of reason-explanations: It seems that it is always possible to offer further interpretations by considering more of the context of action and by citing other beliefs and desires that are logically connected with the reasons first stated (Gergen, 1982; Schafer, 1976). Hopkins comments, "We can understand a single action as issuing from a network of reasons which can be traced through in many ways" (1982, p. xiv).

Another aspect of the problem of choosing among interpretations is that actors either may conceal some of the intentions and beliefs that enter into their actions or may be simply unaware of some of these meanings. When a superior asks an employee, "How do you think you did?" he can recognize im-

mediately that she wants him to understand that she is asking how he evaluates his performance. But he may be unsure whether she has already formed an opinion about how he did and whether she fears that he may become defensive if she states that opinion openly. Were we to interview the superior later, we might discover that she was aware only that she wanted to help the employee explore his feelings about his performance. On reflection, however, she may agree that she had doubts about his competence, and did not consider saying so because she assumed he would get upset. It is not only in psychoanalytic therapy that human beings recognize as valid descriptors of their action meanings of which they had been unaware.

Another complexity is that different actors may interpret the same action, in which they are both involved, quite differently. The superior may see herself as open, interested, and helpful; the subordinate may see her as controlling and disapproving. Interpretive sociologists such as Goffman (1959) speak of the "definition of the situation" and how it is negotiated in interaction. Participants indeed often come to define a situation similarly, but it is not unusual for their interpretations of it to diverge.

Analogues of these problems also characterize scientific explanation in the mainstream account. Complete description of the causes of any event is unattainable, multiple factors impinge on particular events, and relevant data may be inaccessible. An important difference in the two realms, however, is that people are self-interpreting beings. Their interpretations enter into their actions. Hence a proffered interpretation can be valid, in the sense of possessing causal explanatory power, only if it was a reason for the agent in question. Davidson (1980) argues that "reasons explain an action only if the reasons are efficacious in the situation" (p. 264). It makes sense for an agent to say, for example, "I can see how that might be a reason for doing what I did, but that wasn't what I was thinking." Such a response may count against the interpretation unless arguments for unawareness or unconscious motivation can be sustained. Moreover, the inaccessibility of relevant data means, in the case

of action, that the best sources of relevant data (the agents involved) may be blind and biased in ways that are only partially predictable.

Hermeneutic methods for arriving at correct interpretations have been discussed by many writers. Apel speaks of "canonical methods, as for instance, grammatical interpretation, interpretation in the light of literary genre or topic, interpretation of single utterances of a work by the whole of it, and vice versa, historical interpretation, [and] psychological-biographical interpretation" (1977, p. 302). But it is a feature of all such methods that they refine interpretations by other interpretations. They do not provide ways of breaking out of what has been called the *hermeneutical circle.* For example, Taylor points out that if someone disagrees with our interpretation, we may point to other passages in the text, or other features of the context of action, that support our reading. But support for any reading can only be by means of other readings (1977, p. 103). We must always appeal to an understanding of the language involved. Taylor suggests a criterion for superior interpretations: "From the more adequate position one can understand one's own stand and that of one's opponent, but not the other way around." He adds, "It goes without saying that this argument can only have weight for those in the superior position" (p. 127).

Providing multiple perspectives, each of which is a redescription of the action, seems almost a methodological principle of the counterview. Geertz (1973), for example, appropriates Ryle's notion of "thick" description to characterize the ethnographer's task of representing multiple layers of meaning. This notion fits Taylor's criterion of adequacy, and it is a way of dealing with the circumstance that different actors may hold different interpretations of the same action. But we do not share what seems to be Taylor's pessimism about the possibility of coming to agreement on the more adequate interpretation. Open discussion among members of a community of practice can lead to agreement that one interpretation is more adequate than another, even in the opinion of those who originally held the less adequate interpretation. We will describe guidelines for such discussions when we consider action science.

Scientific Rationality as Practical Knowledge

The mainstream account, while designed with the natural sciences in mind, has been widely accepted as appropriate to the social sciences as well. The traditional counterview, while disputing this claim in respect to the social sciences, has conceded the natural sciences to the mainstream. But in recent years there have been increasing challenges to the mainstream view as an adequate account of the natural sciences. The most widely discussed of these challenges has been that of Kuhn (1962). Kuhn's argument is part of a larger movement that Bernstein (1983) calls postempiricist philosophy and history of science, a movement that includes a number of philosophers who have vociferously disagreed with Kuhn. Even this wider perspective will not enable us to take account of some lines of argument in contemporary philosophy of science—for example, the "realist" theory of science (Harré and Secord, 1972; Manicas and Secord, 1983; Outhwaite, 1983).

Our organizing device is the way in which different accounts of science construe the relation of science and community. The several lines of argument that may (rather awkwardly) be called *postempiricist* share a view of the scientific community of inquiry as a community of practice. What this means is that, in reflecting on the nature of scientific rationality, postempiricist philosophers argue that it shares the features of practical deliberation. Criteria for coming to agreement are embedded in the social practices of groups of scientists as members of particular traditions. This argument is developed in detail in Bernstein (1983). We will illustrate it here with reference to Kuhn. The discussion will serve as a bridge to action science, in which we build on the features of practical deliberation to enact norms of scientific rationality.

Kuhn (1962) has argued that the history of science does not support the image of science presented by the mainstream account. He has proposed, rather, that the growth of knowledge must be understood in terms of the community structure of science. The unit of scientific knowledge is the group of specialists who are "bound together by common elements in their education and apprenticeship, aware of each other's work, and

characterized by the relative fullness of their professional communication and the relative unanimity of their professional judgment" (1970a, p. 253).

Members of such a group share a "paradigm," or a set of assumptions about what problems are important and how one might go about solving them. Members of a group who work within an accepted paradigm engage in "normal science," a puzzle-solving activity that extends the shared paradigm. When normal science leads to anomalies, or when the shared paradigm no longer supports puzzle solving, the scientific group enters a period of crisis or "revolutionary science" in which one paradigm may be replaced by another.

The controversy over Kuhn's account centers on his claim that paradigms are "incommensurable." By this he means that those who work within different paradigms do not share a set of premises on which an algorithm for theory choice may be constructed (1970b, pp. 199-200). Observation is theory laden, to the point that those who hold different theories may be said to see different worlds. This contention strikes at the heart of the mainstream conception of objectivity (Scheffler, 1982). Recall that in the mainstream account empirical testing of scientific theories depends on the possibility of intersubjective agreement at the level of observation among individuals who differ at the level of theory. Kuhn argues, in contrast, that the proponents of competing theories do not share a neutral language adequate to the comparison of observation reports (1970a, p. 266).

What is it that a group of specialists shares that enables them to engage in normal science, and the lack of which makes different paradigms incommensurable? Beyond saying that they share a "paradigm," which has become a notoriously fuzzy concept (Masterman, 1970), what are the features of the necessary disciplinary matrix? Kuhn suggests that they include shared symbolic generalizations, models, values, and exemplary problem-solutions (1970a, p. 271). We might say that members of a scientific community share a language of practice that they have learned in the course of their education and apprenticeship. Use of the language, which is to say competence in practicing science within any given community, develops through exposure

to concrete problem-solutions. Practitioners learn what counts as the right kind of problem and what counts as a solution. The knowledge that is built into the language is acquired as the language is learned, by processes that are not well understood. A community of specialists is like a language community, and paradigms are incommensurable for the same reasons that translation is problematic.

Kuhn's initial (1962) account of the process of theory choice emphasized the concepts of persuasion, gestalt shifts, revolution, and conversion experiences. This led critics to complain that he portrayed theory choice as irrational (Scheffler, 1982; Lakatos and Musgrave, 1970). In his replies, Kuhn has insisted that this is a misunderstanding; rather, he is arguing that theory choice proceeds according to a different kind of rationality than that embedded in the mainstream account. Scientists do indeed debate on the basis of good reasons, including the standard list: accuracy, scope, simplicity, and fruitfulness (1970a, p. 261). But, Kuhn argues, "such reasons constitute values to be used in making choices rather than rules of choice" (p. 262). Theory choice is thus a matter of value conflict rather than of logical proof.

Kuhn seems to emphasize the negative argument that theory choice in science, involving as it does value conflict and differences in judgment, does not fit the traditional model of scientific rationality. While he insists that the process of theory choice is nevertheless rational, he has not succeeded in clarifying this new kind of rationality or in distinguishing it from irrational persuasion (Bernstein, 1976, p. 93). Paradoxically, it may be that philosophers who have sought to defend the mainstream account against Kuhn's attack have gone further toward articulating a new model of rationality. This is because, to meet Kuhn's attack, they have reinterpreted the mainstream account. For example, Scheffler (1982) agrees that observation is theory laden; but he argues that the mainstream ideal of objectivity can be preserved, because observation can still conflict with hypothesis and thereby provide independent control over assertion. Scheffler does not disagree with Kuhn's claim that criteria such as accuracy, scope, simplicity, and fruitfulness function as

values rather than as rules; rather he argues that such values can serve as second-order criteria in terms of which rational debate is possible (Scheffler, 1982, p. 130). Thus Scheffler does not think that there is some algorithm for theory choice that, "properly applied, must lead each individual in the group to the same decision" (Kuhn, 1970b, p. 200); instead he provides an interpretation of scientific rationality that does not require such a procedure.

Another philosopher who has contributed to articulating a new model of rationality is Lakatos (1970), who provides a reinterpretation of Popper's falsification criterion. Lakatos is strongly critical of Kuhn's "irrationalism." In his own attempt to identify rational criteria for theory choice in science, however, he accepts several of Kuhn's points. He agrees that the history of science does not bear out the received theory of scientific rationality (p. 115). He also agrees that theories are discarded only when apparently better theories are available to take their place. And he agrees that standards of rationality are embedded in the practices of scientific communities. Extending a point made by Popper, he points out that no disconfirmed prediction "proves" that a theory is false, because the observation itself might be in error. Hence a viable "falsificationism" depends on making certain kinds of decisions—decisions based on conventions adopted by the scientific community. These include judgments that theories or conceptual schemes necessary to observation may be regarded as "unproblematic background knowledge." For example, observations in microbiology presume the validity of the optical theories embedded in microscopes. Also, theories typically predict particular events only on the assumption that other factors do not interfere. The scientific community must have standards for deciding whether this "other things being equal" clause can be regarded as unproblematic in particular cases. Lakatos writes, "The problem of 'controlled experiment' may be said to be nothing else but the problem of arranging experimental conditions in such a way as to minimize the risk involved in such decisions" (1970, p. 111n).

If it is true that the scientific community must decide whether accepted theories will be retained or new theories will

be adopted, then criteria are needed to differentiate what Lakatos calls "progressive" and "degenerating" problem-shifts. A shift to a new theory (more precisely, a revised version of an older one) is progressive if it leads to the discovery of novel facts while *still* explaining facts explained by the older theory (1970, p. 118). It is a series of theories, not an isolated theory, that must be appraised. Thus falsification has a "historical character," and the epistemological unit is the "research program." It is because Lakatos locates criteria of rationality in the historically situated practices of the scientific community that Bernstein (1983) hears him as one of the voices of postempiricism.

Two inferences that may be drawn from the preceding discussion will be important to our later argument. First, scientific rationality can be traced to the social practices of scientific communities. This implies a continuity between science and practical deliberation. Second, there is an important distinction to be made between debate carried on in terms of prevailing standards of decision—what Kuhn calls normal science—and debate over the standards themselves. Rorty (1979, p. 320) has suggested generalizing Kuhn's concepts of normal and revolutionary science to those of normal and abnormal discourse in any area of life. Normal discourse is that which can assume common criteria for reaching agreement, and it is analogous to the idea of an algorithm for theory choice. Abnormal discourse is that in which such criteria are problematic, as is frequently the case in political debate. The deliberative process appropriate to abnormal discourse must deal with value conflict. Let us emphasize once again that such deliberation can be rational. A contribution of the postempiricist philosophy of science is the recognition that abnormal discourse, long accepted as endemic to practical affairs, is also an essential part of science. This recognition may make more palatable the notion that the features of rational deliberation in science—for example, "responsibility to the evidence, openness to argument, commitment to publication, loyalty to logic, and an admission, in principle, that one may turn out to be wrong" (Scheffler, 1982, p. 138)—may also come to characterize deliberation in practical

affairs. Such is the thrust of action science, to which we now turn.

Action Science: Inquiry in Practice

In action science we create communities of inquiry in communities of social practice. To see what this entails, consider the following parallel between scientific inquiry and social practice. A scientific community enacts rules and norms of inquiry that justify its claims to knowledge. A community of social practice also has an interest in justifying such claims. To be sure, in a community of social practice the primary interest is practical, involving questions of the type, "What shall I (we) do?" In contrast, in a scientific community the primary interest is supposedly theoretical, involving questions of the type, "What is the case?" But in the pursuit of practical interests, members of a community of social practice make, challenge, and justify claims to knowledge. In so doing, they enact rules and norms of inquiry that may be more or less appropriate to generating valid information and effective action. The practice of action science involves working with a community to create conditions in which members can engage in public reflection on substantive matters of concern to them and also on the rules and norms of inquiry they customarily enact. Action science builds on the preferences of practitioners for valid information and consistency by creating conditions for public testing and potential disconfirmation of knowledge claims. In these basic respects the normative thrust of action science is the same as that of mainstream science. But the ways in which action science implements these norms frequently differ from the methodologies of mainstream science, because the key threats to validity in the action context differ from those deemed most important to research that aims at knowledge for its own sake.

Action science also enacts a community of inquiry of the kind traditional to science—a community composed of action scientists who communicate through research literature. This book is oriented toward such a community. However, what is distinctive about action science is its mode of engagement with communities of social practice. Research communications

among action scientists will focus on this distinctive type of work, and much of the testing of knowledge claims will occur through engagement with client systems.

An analogy between the scientist and the human being in everyday life has been in good currency among behavioral scientists for several decades (for example, Lewin, 1951; Kelly, 1955; Heider, 1958; Schutz, 1967; Kelley, 1971; also see Argyris, 1980, p. 11n). While united in their emphasis on cognition, those who employ the analogy are of diverse theoretical perspectives. One of the distinctive features of our approach is its focus on the behavioral worlds created by human beings and their impact on generating valid information. This is the dimension of community that we have been emphasizing in our treatment of philosophies of science. Action science is oriented toward public reflection on practice in the interests of learning. A frequent focus of reflection in action science is the reconstruction and criticism of the rules and norms of inquiry customarily enacted in the community of practice, as these determine the system's capacity for learning.

Action science is not alone in advocating that communities of inquiry be enacted in communities of practice. This formulation also seems appropriate to critical theory as articulated by theorists of the Frankfurt School (Habermas, 1971; Geuss, 1981). Habermas speaks of creating conditions that approximate the "ideal speech situation," which would allow human beings to come to a rational consensus about how to conduct their affairs. To our knowledge, however, Habermas has not devoted his energies to creating such conditions in the real world.

Action science is centrally concerned with the practice of intervention. It is by reflecting on this practice that we hope to contribute to an understanding of how knowledge claims can be tested and justified in practice and of how such inquiry is similar to and different from that of mainstream science. In the following chapter we will discuss features of knowledge that can contribute to practice. This will involve discussion of an epistemology of practice for action science, empirical testing in the action context, and the relation of norms and values to knowledge in action science.