Edited by

Egon G. Guba

THE PARADIGM DIALOG

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About this volume . . .

Is scientific positivism, long the reigning paradigm for research in the social sciences, the "best way" to conduct social research? This is the central question examined in **The Paradigm Dialog.** Recently three key challengers have appeared—postpositivism, critical theory, and constructivism. All three offer researchers new methodological approaches, and all three present fundamental questions that must be addressed. Can research be conducted between paradigms? Are they equally useful in answering questions of applied research? What constitutes good, or ethical, research in each? In this volume, these and other significant questions are examined by a multidisciplinary group of leading figures in qualitative research. Not surprisingly, there is no agreement on the "best" paradigm question, but the dialog offered in this compelling volume deftly explores important issues in selecting the proper paradigm for tackling a variety of research questions.

With a group of contributors that reads like a veritable who's who in qualitative research, **The Paradigm Dialog** is a must for anyone conducting research in the social sciences.

*"**The Paradigm Dialog** is one of those rare books that simultaneously stretches the mind while projecting one into self-reflection. For the applied practitioner, whether teacher, counselor, or consultant, the possibility of gaining further insight into the underlying assumptions which constrain one's pedagogy or practice is highly possible upon a critical reading."*

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PART I

Setting the Stage

Part I, Setting the Stage, consists of but a single chapter. Essentially this chapter reprises the substantive portion of the original conference proposal, setting out what, in my opinion, are the parameters of each of the three paradigms that were considered: postpositivism, critical theory, and constructivism. In that sense Part I sets the stage for all the chapters that follow.

The reader will quickly see, however, that *my* construction of what constituted the paradigm parameters, and of the emergent issues, was not necessarily shared by other participants. It is likely that readers of this volume will not share them either. Nevertheless, this construction did serve as a common point of reference, and, whether it was reinforced or rejected by individual presenters, it served a useful purpose as a kind of moving target.

I invite the reader to compare my constructions with those developed by the three keynoters (Part II), because their task, as proponents of the paradigm each presented, was to flesh out the form and substance of these three alternatives. In the case of the issue papers (Part III), it is interesting to note how the individual speakers differ among themselves, with the keynoters, and with this Part I construction. The differences are instructive and form the basis for any continuing dialectic.

[1]

The Alternative Paradigm Dialog

EGON C. GUBA

It is not surprising that most persons asked to define the term *paradigm* are unable to offer any clear statement of its meaning. I say it is not surprising because Thomas Kuhn, the person most responsible for bringing that concept into our collective awareness, has himself used the term in no fewer than 21 different ways, if Masterman (1970) can be believed. Some persons view that lack of clear definition as an unfortunate state of affairs. But I believe that it is important to leave the term in such a problematic limbo, because it is then possible to reshape it as our understanding of its many implications improves. Having the term *not* cast in stone is intellectually useful. Thus I will use the term in this chapter only in its most common or generic sense: a basic set of beliefs that guides action, whether of the everyday garden variety or action taken in connection with a disciplined inquiry. Refinement of that definition can be made by each reader while progressing through the book.

In this opening chapter I propose to outline what I take to be the salient differences between traditional positivism, on the one hand, and the three paradigms that have emerged to challenge (replace? parallel?) it on the other. Of course, I have my own preference among them; it would be remiss of me not to acknowledge that preference at once. It is *constructivism*. One immediate consequence is that I recognize that what I am about to say is *my own construction*, not necessarily an *objective* (whatever that may be) analysis. Indeed, as we shall see, constructivists not only abjure objectivity but celebrate subjectivity. The reader should not, therefore, read this chapter in the mistaken notion that it represents gospel or even a widely agreed to position. I

offer it as *one* way to understand the paradigm issue. I should also point out that constructivists are relativists (a position that, I contend, can be well defended; see Guba, 1990); hence it is quite possible for me to entertain *any* construction (including, of course, a paradigm) that is proposed by reasonable and well-intentioned persons. The reader should never forget that the only alternative to relativism is absolutism. As a relativist, I will not reject any construction out of hand.

Generating Inquiry Paradigms

There are many paradigms that we use in guiding our actions: the adversarial paradigm that guides the legal system, the judgmental paradigm that guides the selection of Olympic winners, the religious paradigms that guide spiritual and moral life, and many others. Our concern here, however, is with *those paradigms that guide disciplined inquiry*. Historically there have been many such (Guba & Lincoln, 1989; Lincoln & Guba, 1985), but since the time of Descartes (1596-1650), inquirers have tended to focus on what, in its latter-day version, came to be known as positivism. Nevertheless, all these past paradigms, as well as the emergent contenders, can be characterized by the way their proponents respond to three basic questions, which can be characterized as the *ontological*, the *epistemological*, and the *methodological* questions. The questions are these:

(1) *Ontological*: What is the nature of the "knowable"? Or, what is the nature of "reality"?

(2) *Epistemological*: What is the nature of the relationship between the knower (the inquirer) and the known (or knowable)?

(3) *Methodological*: How should the inquirer go about finding out knowledge?

The answers that are given to these questions may be termed, as sets, the basic belief systems or *paradigms* that might be adopted. They are the starting points or givens that determine what inquiry is and how it is to be practiced. They cannot be proven or disproven in any foundational sense; if that *were* possible there would be no doubt about how to practice inquiry. But all such belief systems or para-

digms are *human constructions*, and hence subject to all the errors and foibles that inevitably accompany human endeavors.

There are certainly many different ways to answer these questions. Descartes, obsessed with the idea that he might be gulled into believing something not true, searched for a sure foundation. (Indeed, his legendary pronouncement, "I think, therefore I am," was the only proposition that he felt that he could propose without himself immediately doubting it.) His overriding concern for certain knowledge has come to be called *Cartesian anxiety*, a *dis*-ease that is still reflected in the positivist (and postpositivist) search to find out "how things really are" and "how things really work."

The Basic Beliefs of Positivism

The phrases "how things *really* are" and "how things *really* work" are ontological creeds. The basic belief system of positivism is rooted in a *realist* ontology, that is, the belief that there exists a reality *out there*, driven by immutable natural laws. The business of science is to discover the "true" nature of reality and how it "truly" works. The ultimate aim of science is to *predict and control* natural phenomena.

Once committed to a realist ontology, the positivist is constrained to practice an *objectivist* epistemology. If there is a real world operating according to natural laws, then the inquirer must behave in ways that put questions directly to nature and allow nature to answer back directly. The inquirer, so to speak, must stand behind a thick wall of one-way glass, observing nature as "she does her thing." Objectivity is the "Archimedean point" (Archimedes is said to have boasted that, given a long enough lever and a place whereon to stand, he could move the earth) that permits the inquirer to wrest nature's secrets without altering them in any way.

But how can that be done, given the possibility of inquirer bias, on the one hand, and nature's propensity to confound, on the other? The positivist's answer: by the use of a manipulative methodology that controls for both, and empirical methods that place the point of decision with nature rather than with the inquirer. The most appropriate methodology is thus *empirical experimentalism*, or as close an approximation thereto as can be managed.

The basic belief system (paradigm) of conventional (positivist) inquiry can thus be summarized as follows:

Ontology: *Realist*—reality exists "out there" and is driven by immutable natural laws and mechanisms. Knowledge of these entities, laws, and mechanisms is conventionally summarized in the form of time- and context-free generalizations. Some of these latter generalizations take the form of cause-effect laws.

Epistemology: *Dualist/objectivist*—it is both possible and essential for the inquirer to adopt a distant, noninteractive posture. Values and other biasing and confounding factors are thereby automatically excluded from influencing the outcomes.

Methodology: *Experimental/manipulative*—questions and/or hypotheses are stated in advance in propositional form and subjected to empirical tests (falsification) under carefully controlled conditions.

There are many ways in which this belief system can be undermined. Each of the three emergent paradigms raises its own objections and proposes its own solutions. I will examine each in turn.

The Basic Beliefs of Postpositivism

Postpositivism is best characterized as a modified version of positivism. Having assessed the damage that positivism has incurred, postpositivists struggle to limit that damage as well as to adjust to it. Prediction and control continue to be the aim.

Ontologically, postpositivism moves from what is now recognized as a "naive" realist posture to one often termed *critical realism*. The essence of this position is that, although a real world driven by real natural causes exists, it is impossible for humans truly to perceive it with their imperfect sensory and intellective mechanisms (Cook & Campbell, 1979, p. 29). Inquirers need to be critical about their work precisely because of those human frailties. But, although one can never be sure that ultimate truth has been uncovered, there can be no doubt that reality is "out there." Realism remains the central concept.

Epistemologically, postpositivism recognizes the absurdity of assuming that it is possible for a human inquirer to step outside the pale of humanness while conducting inquiry. Work in the "hard" sciences has aptly demonstrated that "findings" emerge from the *interaction* of inquirer and inquired into, as shown by, say, the Heisenberg Uncer-

tainty Principle and the Bohr Complementarity Principle (Hesse, 1980; Zukav, 1979). To overcome these problems postpositivists counsel a modified objectivity, hewing to objectivity as a "regulatory ideal" but recognizing that it cannot be achieved in any absolute sense. It *can* be achieved *reasonably closely*, by striving to be as neutral as possible; by "coming clean" about one's own predispositions (as did I in the early paragraphs of this chapter) so that the reader can make whatever adjustments to the proffered interpretations of findings that seem appropriate; by relying on "critical tradition," that is, requiring the reports of any inquiry to be consistent with the existing scholarly tradition of the field; and by subjecting every inquiry to the judgment of peers in the "critical community," that is, the editors and referees of journals as well as their readers. Of course, the latter two requirements also make it virtually impossible for new paradigms to assert themselves, an advantage not lost on the power brokers who protect and defend the (new) hegemony of postpositivism.

Methodologically, postpositivism provides two responses to emergent challenges. First, in the interest of conforming to the commitment to critical realism and modified subjectivity, emphasis is placed on *critical multiplism* (Cook, 1985), which might most usefully be thought of as a form of elaborated triangulation (Denzin, 1978). If human sensory and intellective mechanisms cannot be relied upon, it is essential that the "findings" of an inquiry be based on as many sources—of data, investigators, theories, and methods—as possible. Further, if objectivity can never be entirely attained, relying on many different sources makes it less likely that distorted interpretations will be made.

Second, and perhaps more important, postpositivism recognizes that many imbalances have been allowed to emerge in the zeal for achieving realistic, objective inquiry. A major part of the postpositivist agenda has been devoted to identifying these imbalances and proposing ways of redressing them. It is believed that, if they can be redressed, positivism, in its new postpositivist clothes, can be made useful once again. There are four imbalances; of course, not all postpositivists would agree that all exist and certainly not that they are equally critical.

(1) *The imbalance between rigor and relevance.* In more traditional terms this is the inescapable trade-off between internal and external validity. The greater the control established to achieve internal validity, the less the generalizability of the findings, for, in the final analysis,

laboratory results are generalizable only to another laboratory. The imbalance, created by excessive emphasis on context-stripping controls, is redressed by carrying out inquiry in more *natural* settings. The reader should note that the term *naturalistic* inquiry, often used in the past to denote what, in this book, is called *constructivist* inquiry, is *not* equivalent to this postpositivist proposal; the term *naturalistic* is identified with a *paradigm*, while the term *natural* is identified with a *methodology*, the *doing* part of a paradigm.

(2) *The imbalance between precision and richness*. Precision is critical to a science that defines its major goal to be prediction and control. That the press for precision should lead to an overemphasis on quantitative methods—that epitome of precision—is not surprising, particularly in view of the impressive array of mathematical and statistical methods that are available. This imbalance is redressed by including more qualitative methods. The reader should again note the confusion engendered by this use of the term *qualitative methods* (or, if one chooses, ethnographic, phenomenological, or case study methods). The term *qualitative* is a methods-level term, not a paradigm-level term. The call for qualitative methods is by itself *not* a call for a paradigm shift.

(3) *The imbalance between elegance and applicability*. The press to predict and control places great emphasis on the statement of formal theories—and preferably, broadly based, reductionistic ("grand") theories. The development and testing of these theories characterize much of scientific activity. But such grand theories, while abetting generalizability, often are not found to "fit" or "work" (Glaser & Strauss, 1967) in local contexts. Locality and specificity are incommensurable with generalizability. This imbalance is redressed by "grounding" theory in local circumstances, that is, conducting the inquiry so that theory is the *product* rather than the *precursor* of the inquiry.

(4) *The imbalance between discovery and verification*. Discovery, that is, the process by which a priori theories and their implied questions and hypotheses emerge, is not a formal part of the conventional paradigm. Discovery is merely a precursor rather than an integral part of the scientific process, whose purpose is solely *verification* (falsification). But this position is immediately seen to be absurd when one considers that most of the important advances of science have been made via the creative discovery route rather than by the more mundane and plodding verification route. Clearly both processes are necessary; it is not only unfair but also extremely shortsighted to reserve the mantle

of science only for verifiers. This imbalance is redressed by defining a *continuum* of inquiry, which ranges from "pure" discovery at one end to "pure" verification at the other. The reader should note that the earlier tendency to relegate paradigms other than postpositivism to the discovery end has been replaced with a more ecumenical stance that seems to recognize that both processes can go on in all paradigms. But it should be clear that making this adjustment has nothing to do with paradigm differences; it simply recognizes that positivism, if not postpositivism, made an error in its earlier assessment.

We may note then that the basic belief system of postpositivism differs very little from that of positivism. We may summarize the stances as follows:

Ontology: *Critical realist*—reality exists but can never be fully apprehended. It is driven by natural laws that can be only incompletely understood.

Epistemology: *Modified objectivist*—objectivity remains a regulatory ideal, but it can only be approximated, with special emphasis placed on external guardians such as the critical tradition and the critical community.

Methodology: *Modified experimental/manipulative*—emphasize critical multiplism. Redress imbalances by doing inquiry in more natural settings, using more qualitative methods, depending more on grounded theory, and reintroducing discovery into the inquiry process.

The Basic Beliefs of Critical Theory

The label *critical theory* is no doubt inadequate to encompass all the alternatives that can be swept into this category of paradigm. A more appropriate label would be "ideologically oriented inquiry," including neo-Marxism, materialism, feminism, Freireism, participatory inquiry, and other similar movements as well as critical theory itself. These perspectives are properly placed together, however, because they converge in rejecting the claim of value freedom made by positivists (and largely continuing to be made by postpositivists).

Because they are human constructions, paradigms inevitably reflect the values of their human constructors. They enter into inquiry at choice points such as the problem selected for study, the paradigm within which to study it, the instruments and the analytic modes used, and the interpretations, conclusions, and recommendations made.

Nature cannot be seen as it "really is" or "really works" except through a value window.

If values *do* enter into every inquiry, then the question immediately arises as to what values and whose values shall govern. If the findings of studies can vary depending on the values chosen, then the choice of a *particular* value system tends to empower and enfranchise certain persons while disempowering and disenfranchising others. Inquiry thereby becomes a *political act.*

Given that counterclaim, one might expect critical theorists (ideologists) to reject a realist posture. For if there is a real state of affairs, then it seems unreasonable to argue that value positions that inquirers might take could influence it. Moreover, a *real* reality requires an objective epistemological approach to uncover it—as positivists and postpositivists have claimed all along. But, for whatever reason, critical theorists (ideologists) have elected to believe in an objective reality—as the phrase commonly used by them, "false consciousness," readily demonstrates (because it implies that there is a "true consciousness" somewhere "out there," or, more likely, possessed by the inquirer or some better-informed elite). The task of inquiry is, by definition, to raise people (the oppressed) to a level of "true consciousness." Once they appreciate how oppressed they are, they can act to *transform* the world. The close parallel between *transforming* the world and *predicting and controlling* it should not be lost.

Thus there appears to be a logical disjunction: a *realist* (but probably with the postpositivists, a critical realist) ontology coupled with a *subjectivist* epistemology—subjectivist because inquiry acts are intimately related to the values of the inquirer. The move to a subjectivist epistemology no doubt represents a forward step, but, so long as that epistemology is enlisted in the service of a realist ontology, it seems to lose much of its force.

At the *methodological* level, critical theorists (ideologists) seem more consistent. If the aim of inquiry is to transform the (real) world by raising the consciousness of participants so that they are energized and facilitated toward transformation, then something other than a manipulative, interventionist methodology is required. Critical theorists (ideologists) take a dialogic approach that seeks to eliminate false consciousness and rally participants around a common (true?) point of view. In this process, features of the real world are apprehended and judgments are made about which of them can be altered. The result of effective, concerted action is transformation.

Given this view, we may summarize the basic belief system of the critical theory (ideological) paradigm as follows:

Ontology: *critical realist*, as in the case of postpositivism
Epistemology: *subjectivist*, in the sense that values mediate inquiry
Methodology: *dialogic, transformative*; eliminate false consciousness and energize and facilitate transformation

The Basic Beliefs of Constructivism

It is my belief that proponents of both the postpositivist and the critical theory (ideological) paradigms feel that there can be an accommodation between their positions and, indeed, with conventional positivism. Constructivists, on the other hand, feel that the positivist (and postpositivist) paradigms are badly flawed and must be entirely replaced. Among the more telling arguments are these (Guba & Lincoln, 1989; Lincoln & Guba, 1985):

(1) *The theory ladenness of facts*. If empirical tests are to be valid as arbiters of propositions (hypotheses and questions) put to nature by inquirers, then it is essential that theoretical and observational languages be independent. The "facts" that are collected must be *independent* of the propositional (theoretical) statements. But philosophers of science now uniformly believe that facts *are* facts only *within* some theoretical framework (Hesse, 1980). Thus the basis for discovering "how things really are" and "really work" is lost. "Reality" exists only in the context of a mental framework (construct) for thinking about it.

(2) *The underdetermination of theory*. No theory can ever be fully tested because of the problem of induction. Observing one million white swans does not provide indisputable evidence for the assertion, "All swans are white." There are always a large number of theories that can, in principle, "explain" a given body of "facts." Thus no unequivocal explanation is ever possible. There can be many constructions, and there is no foundational way to choose among them. "Reality" can be "seen" only through a window of theory, whether implicit or explicit.

(3) *The value ladenness of facts*. Constructivists concur with the ideological argument that inquiry cannot be value free. If "reality" can be seen only through a theory window, it can equally be seen only through a value window. Many constructions are possible.

(4) *The interactive nature of the inquirer/inquired-into dyad.* Even postpositivists have conceded that objectivity is not possible; the results of an inquiry are always shaped by the *interaction* of inquirer and inquired into. There is no Archimedean point. And if there is such an intimate interconnectedness in the physical sciences, how much more likely is it that the results of social inquiry are similarly shaped? This problem of interaction is devastating to both positivism and postpositivism. First, it renders the distinction between ontology and epistemology obsolete; what can be known and the individual who comes to know it are fused into a coherent whole. Further, it makes the findings of an inquiry not a report of what is "out there" but the residue of a process that *literally creates them.* Finally, it depicts knowledge as the outcome or consequence of *human* activity; knowledge is a *human construction,* never certifiable as ultimately true but problematic and ever changing.

Given this critique, it is apparent why constructivists feel that an entirely new paradigm is needed. *Ontologically,* if there are always many interpretations that can be made in any inquiry, and if there is no foundational process by which the ultimate truth or falsity of these several constructions can be determined, there is no alternative but to take a position of *relativism.* Relativism is the key to openness and the continuing search for ever more informed and sophisticated constructions. Realities are multiple, and they exist in people's minds.

Epistemologically, the constructivist chooses to take a *subjectivist* position. Subjectivity is not only forced on us by the human condition (as the postpositivist might admit) but because it is the only means of unlocking the constructions held by individuals. If realities exist only in respondents' minds, subjective interaction seems to be the only way to access them.

Methodologically, the constructivist proceeds in ways that aim to identify the variety of constructions that exist and bring them into as much consensus as possible. This process has two aspects: hermeneutics and dialectics. The hermeneutic aspect consists in depicting individual constructions as accurately as possible, while the dialectic aspect consists of comparing and contrasting these existing individual (including the inquirer's) constructions so that each respondent must confront the constructions of others and come to terms with them. The hermeneutic/dialectic methodology aims to produce as informed and sophisticated a construction (or, more likely, constructions) as possible. Simultaneously the methodology aims to keep

channels of communication open so that information and sophistication can be continuously improved. Constructivism thus intends neither to predict and control the "real" world nor to transform it but to *reconstruct* the "world" at the only point at which it exists: in the minds of constructors. It is the mind that is to be transformed, not the "real" world.

We may thus summarize the constructivist belief system as follows (retaining the threefold organization for the sake of contrast despite having argued that, in constructivism, the ontology/epistemology distinction is obliterated):

Ontology: *Relativist*—realities exist in the form of multiple mental constructions, socially and experientially based, local and specific, dependent for their form and content on the persons who hold them.

Epistemology: *Subjectivist*—inquirer and inquired into are fused into a single (monistic) entity. Findings are literally the creation of the process of interaction between the two.

Methodology: *Hermeneutic, dialectic*—individual constructions are elicited and refined hermeneutically, and compared and contrasted dialectically, with the aim of generating one (or a few) constructions on which there is substantial consensus.

What is the Paradigm Dialog About?

I must stress again that what have been outlined on the preceding pages are *my* constructions about the nature of four paradigms—conventional positivism and three contenders for its "crown": postpositivism, critical theory (ideology), and constructivism. We are, nationally and internationally, engaged in a major debate about which of these is to be preferred. It is my own position that a struggle for primacy is irrelevant. As a constructivist I can confidently assert that *none* of these four is *the* paradigm of choice. Each is an alternative that deserves, on its merits (and I have no doubt that all are meritorious), to be considered. The dialog is not to determine which paradigm is, finally, to win out. Rather, it is to take us to another level at which *all* of these paradigms will be replaced by yet another paradigm whose outlines we can see now but dimly, if at all. That new paradigm will not be a closer approximation to truth; it will simply be more informed and sophisticated than those we are now entertaining. The reader is invited to enter into that dialog as she or he reads the following pages.

PART II

Points of View

Part II, Points of View, sets the stage for the dialog that follows in the remainder of the book. Four papers are included: the three keynote addresses and the dinner address.

The keynoters described in ways each thought most appropriate the three alternative paradigms that have emerged in recent decades. Denis Phillips sets out the *postpositivist* position, electing to deal first with an assessment of the current states of affairs that brought positivism into question, and following with a discussion of certain "myths" that he feels represent misunderstandings of postpositivism. Thomas Popkewitz begins with a discussion of certain principles underlying the *critical* position and follows with six questions that he believes spell out the implications of that position for inquiry. Yvonna Lincoln ends the paradigm presentation with a Proustian retrospective of her intellectual journey through *constructivism*, pointing out what she believes are its implications for selected problem areas.

Elliot Eisner's address did not come at the point at which I have placed it in this volume, that is, as part of the plenary session devoted to delineating the alternative paradigms. I inserted it here, nevertheless, because I believe it is a superlative statement about what it means to make a paradigm shift, and it creates a splendid context for what follows.

[2]

Postpositivistic Science
Myths and Realities

DENIS C. PHILLIPS

It is arguable that recent advances in the philosophical understanding of science have vindicated many of John Dewey's views on the matter. Scientific reason is not marked off from other forms of human intellectual endeavor as a sort of model of perfection that these lesser activities must always strive (unsuccessfully) to mimic. Rather, science embodies exactly the same types of fallible reasoning as is found elsewhere—it is just that scientists do, a little more self-consciously and in a more controlled way, what all effective thinkers do. As Dewey pointed out, he believed strongly that intellectual inquiry,

> in spite of the diverse subjects to which it applies, and the consequent diversity of its special techniques has a common structure or pattern: that this common structure is applied both in common sense and science. (Dewey, 1966, p. 101)

Recent work has shown that scientists, like workers in other areas, are in the business of providing reasonable justifications for their assertions, but nothing they do can make these assertions absolutely safe from criticism and potential overthrow. (There are no absolute justifications, hence the somewhat misleading name sometimes given to recent epistemology—"nonjustificationist." This is misleading because it suggests that, if there are no *absolute* justifications, there are no justifications at all!) It is salutary to remember that Dewey pre-

AUTHOR'S NOTE: Helpful comments have been provided by Harvey Siegel and Debby Kerdeman.

ferred not to use the term *truth* but, instead, the term *warranted assertibility*, and he recognized that different types of assertions required different warrants. Furthermore, this change of language highlighted the fact that a warrant is not forever; today's warrant can be rescinded tomorrow, following further inquiry.

None of this means that science is *unbelievable*, or that "anything goes" or "anything may be accepted," or that "there is no justification at all for scientific claims," or that "there are no standards by which the truth or adequacy (or both) of a piece of science can be judged." It simply means that no longer can it be claimed there are any *absolutely* authoritative foundations upon which scientific knowledge is based (hence the other title often given to contemporary epistemology—"nonfoundationalistic"). The fact is that many of our beliefs are warranted by rather weighty bodies of evidence and argument, and so we are justified in holding them; but they are not *absolutely* unchallengeable.

This view of science fits comfortably with what every experienced action researcher and evaluator of social programs has come to understand about his or her own work; these are, par excellence, fields of "the believable," of building the "good case," but where even the best of cases can be challenged or reanalyzed or reinterpreted. Nothing is more suspicious in the field of evaluation than a report that is presented with the implication that it has the status of "holy writ." Researchers in the "pure" sciences, and in the more laboratory-oriented of the social and human sciences, now have to accept that good science is a blood brother if not a sibling to what transpires in these messier and more open-ended fields of endeavor.

What happened in philosophy of science to build this new and modest view? Or, alternatively, what destroyed the older view?

An Outline of Recent Developments

The new view of science could not get off the ground until the foundations of the dominant older view, positivism, had been shown to be untenable. The role that had been ascribed to observation—that it was both the rock-bottom foundation of science and, at the same time, the final arbiter of what could be believed—was reevaluated; and the relation between scientific theories and evidence was shown

to be more complex than had been thought. The related view that science grows by steady accumulation of findings and theories was challenged by the work of Thomas Kuhn and subsequent scholars such as Lakatos and Feyerabend. Obviously these matters are too complex to discuss in full, but a few of the crucial issues can be highlighted.

Observation

It is clear (to all except some mystics) that, if the aim of science is to establish bodies of knowledge about the world, then somewhere in the process of doing science the world must be studied or observed. But it has been recognized for many decades that the positivistic and operationalistic view that all theoretical terms of science must be reducible to (i.e., definable in terms of) observational language is quixotic. The status of operationalism in the behavioral sciences was a hot issue in the decade immediately following World War II, and there were international symposia on the matter. A consensus was reached (except, of course, for a few diehards—an old story): If the positivist/operationalist view were to be accepted, it would have a chilling effect on theorizing about unobservable mechanisms such as the subatomic events that have won Nobel prizes for so many physicists. Carl Hempel, a somewhat "lapsed" logical positivist, drew (in his postpositivist years) the following enticing picture that makes absurd the operationalist notion that concepts can each be reduced to a set of observation statements:

> Scientific systematization requires the establishment of diverse connections, by laws or theoretical principles, between different aspects of the empirical world, which are characterized by scientific concepts. Thus, the concepts of science are the knots in a network of systematic interrelationships in which laws and theoretical principles form the threads. . . . The more threads that converge upon, or issue from, a conceptual knot, the stronger will be its systematizing role, or its systematic import. (Hempel, 1966, p. 94)

Thus the point was driven home that the theoretical concepts of science have meanings that transcend definition in observational terms, and it was realized that, if this were not the case, science would have trouble in growing and extending into new areas.

There is another issue about the role of observation. It has often been held that observation is the "neutral court" that adjudicates between rival scientific claims; together with this has usually gone the belief that science is actually built upon the foundation of indubitable observation. (The operationalist thesis discussed before concerned the *status* of theoretical concepts, not their *origin*. That is, according to the operationalist view, theoretical concepts had the status of being shorthand summaries of observation statements, no matter how these theoretical concepts happened to have originated.) The crucial work that challenged the view that observation is the "theory-neutral" basis on which science is erected was that of N. R. Hanson, where *Patterns of Discovery* (1958) has become a classic. Hanson was not the first to say the things that he said; Wittgenstein used the key illustration that Hanson used, and even Dewey made much the same point. But it was Hanson's work that fired most imaginations.

Hanson's theories may be stated in one sentence: "The theory, hypothesis, or background knowledge held by an observer can influence in a major way what is observed." Or, as he put it in a nice aphorism, "There is more to seeing than meets the eyeball" (Hanson, 1958, p. 7). In other words, observation is theory laden—it is not a theory-neutral foundation. Thus, in a famous psychological experiment, sliders were made from cards selected from a normal deck, and these were projected for very short periods onto a screen in front of observers. All were correctly identified, except for a trick slide that had the color altered (for example, it might be a black four of diamonds). Most commonly this slide was *seen* as a blur or else as a black suit (spades or clubs). A Hansonian interpretation is that there is an interaction between the visual stimulus and the observers' background knowledge ("diamonds are red"), so the final result is that a blur is observed.

Subsequent writers have drawn a variety of conclusions from Hanson's work; for instance, many have taken it as supporting relativism—"there is no such thing as objective truth, for what observers take to be true depends upon the framework of knowledge and assumptions they bring with them." Sometimes an example is given that comes from Hanson himself: He imagined the astronomers Tycho Brahe and Kepler watching the dawn together; because they had different frameworks, one would see the sun moving above the horizon, while the other would see the earth rotating away to reveal

the sun! However, a closer reading of Hanson provides no succor for such extravagant relativism, for he explicitly acknowledged that *both* astronomers would agree that what they actually *observed* during the dawn was the sun increasing its relative distance above the Earth's eastern horizon (Hanson, 1958, p. 23); but, of course, they would insist on talking about what they had observed in different terms. This acknowledgment is evidence that Hanson realized people with different frameworks nevertheless can have some views—or can hold some data—in common, and these things can serve as the basis for further discussion and clarification of their respective positions. Thus there is little comfort here for relativists.

A less extreme interpretation then is that, while we must be aware of the role played by our preconceptions in influencing our observations, and while we have to abandon the view that observation is "neutral" or theory free, there is nothing in Hanson that forces us to the conclusion that we cannot decide between rival claims and cannot arrive at consensus about which viewpoint (or which observations) seem to be most trustworthy under the prevailing circumstances. Israel Scheffler (1967, p. 44) put it well:

> There is no evidence for a general incapacity to learn from contrary observations, no proof of a pre-established harmony between what we believe and what we see. . . . Our categorizations and expectations guide by orienting us selectively toward the future; they set us, in particular, to perceive in certain ways and not in others. Yet they do not blind us to the unforeseen. They allow us to recognize what fails to match anticipation.

Theory and Evidence

Over the past few decades, it has become increasingly clear that scientific theories are "underdetermined" by nature; that is, whatever evidence is available (or possibly could be available) about nature, it is never sufficient to rule out the possibility that a much better theory might be devised to account for the phenomena that our presently accepted theory also explains. Or, to put it another way, a variety of rival theories or hypotheses can always be constructed that are equally compatible with whatever finite body of evidence is currently available. (An implication of this, of course, is that we can never be

certain that the particular theory we have accepted is the correct one! There are several developments that are worthy of brief comment.[1]

The first point is illustrated by Nelson Goodman's notorious example of "grue and bleen" (Goodman, 1973), although it should be noted that Goodman made slightly different use of this case. A large amount of observational evidence has accumulated over the ages concerning the color of emeralds; all that have been studied have been found to be green. It might be supposed then that this amounts to irrefutable evidence for the hypothesis "all emeralds are green." But the *very same* evidence also supports the hypothesis that "all emeralds are grue" (where *grue* is the name of a property such that an object is green up to a certain date, for instance, the year 2000, and blue thereafter). The fanciful nature of this example is beside the point; it nicely illustrates the underdetermination of theory by available evidence, for it shows that a general theory ("emeralds are green," that is, "always have been, and always will be") necessarily goes beyond the finite evidence that is available ("the finite number of emeralds observed to date have been green"), thus leaving open the possibility that some ingenious scientist will come up with an alternative explanation for the very same finite set of data.

A related issue concerns what happens when new evidence turns up necessitating the making of some accommodatory change in whatever theory is currently the favored one. Postpositivists now generally recognize that there is *no one specific change* that is *necessitated*. Different scientists may change different portions of the theory—they are free to use their professional judgment and their creativity. It would be a mistake to interpret this as indicating that scientific theories are a matter of mere whim or individual taste; to stress that judgment is required is *not* to throw away all standards. Rather, it is to stress that decisions cannot be made using some mechanical procedure.

This point is often made in terms of the "Duhem-Quine" thesis. Scientific theories, indeed vast areas of science, are interrelated; the image of science as a huge fishnet is a predominant one in much recent writing. It is this network as a whole, rather than little portions of it, that has to withstand the test of dealing with whatever evidence is gathered. It might appear that some piece of recalcitrant data offers a serious challenge to one particular section of the net, but the threat cannot be localized in this way—one scientist may react to the data by altering the "obvious" portion of the net, while others might want

to preserve this piece and so might advocate changing some other portion of the net to accommodate the new information. Once again scientists must use their professional judgment; decisions about how to modify theories cannot be made mechanically.

It might even be the case that, when some counterevidence turns up, scientists might decide to make no accommodatory changes at all—a course of action (or, rather, a course of inaction) that receives the blessing of the new philosophy of science. For one thing, it might well be the case that one of the auxiliary assumptions is faulty. Many such assumptions have to be made in any piece of scientific work. For example, in doing laboratory work, the auxiliary assumption is often made that the chemical samples being used were pure, or that there were no unplanned temperature fluctuations, or that the psychological tests being used were reliable, or that an observer was unbiased, and so on. Scientists can blame one or another of these rather than accept the counterevidence at face value and thereby be forced to change their net.

On the other hand, scientists might simply ignore the counterevidence in the hope that "something will eventually turn up to explain it." It was a traditional tenet of methodology that a scientist must abandon a theory, no matter how attractive it might appear, once some counterevidence became available. It turns out, however, that there are good reasons to suppose that it can be quite rational to adhere to the theory even under these adverse conditions. Paul Feyerabend (1970, pp. 21-22) has been the most forceful writer on this and related issues:

> The idea of a method that contains firm, unchanging, and absolutely binding principles for conducting the business of science gets into considerable difficulty when confronted with the results of historical research. We find, then, that there is not a single rule, however plausible, and however firmly grounded in epistemology, that is not violated at some time or other. It becomes evident that such violations are not accidental events. . . . On the contrary, we see they are necessary for progress.

Imre Lakatos (1972) devised his "methodology of scientific research programs" in an attempt to gauge when changes made in an ongoing research tradition are progressive or degenerative.

Scientific Change

Perhaps the most famous feature of the new philosophy of science, however, is its focus upon dynamics. The process of scientific change has come under increasing investigation since Kuhn's work on scientific revolutions popularized the notion of "paradigm clashes." Science is not static. Theories come and theories go, new data accumulate, and old findings are interpreted in new ways. Involved in all this is the question of the *rationality* of change—what justifies scientists in throwing out old ideas and accepting new ones? There has been much debate, but little consensus, among the postpositivists—witness the work of Kuhn (1970), Popper (1968a), Lakatos (1972), Feyerabend (1970), Toulmin (1970a, 1970b), Laudan (1977), and Newton-Smith (1981). It will suffice to quote a brief passage from Popper to illustrate this major theme in the new postpositivist philosophy:

> I assert that continued growth is essential to the rational and empirical character of scientific knowledge; that if science ceases to grow it must lose that character. It is the way of its growth which makes science rational and empirical; the way, that is, in which scientists discriminate between available theories and choose the better one. (Popper, 1968a, p. 215)

Questions and Answers

There are some who have drawn a dangerous moral from the developments just outlined. Science has fallen from its pedestal; if no knowledge can be totally and unchallengeably justified, then nothing can be disbarred. We have embarked on the rocky road to relativism. But it is possible to retain a hopeful outlook, and even to relish the challenge that this new picture of science presents. It is here that we can obtain succor from the fields of evaluation and action research. People here do not lose heart, yet they are faced with a reality that (we now realize) closely parallels that of "pure" scientists; and some even thrive on the uncertainties of their field. Seekers after enlightenment in any field do the best that they can; they honestly seek evidence, they critically scrutinize it, they are open to alternative viewpoints, they take criticism seriously and try to profit from it, they play their hunches, they stick to their guns, but they also have a sense of when

it is time to quit. It may be a dirty, hard, and uncertain game, but it is the only game in town.

Although, to me, this seems a modest, nondoctrinaire, unsurprising, and eminently reasonable position, there are many who feel uneasy and who continue to raise questions about it. So it might be fruitful to grapple with some of these directly.

Question 1. In what sense is the new position, which has been outlined above, "postpositivistic"? Isn't it merely a weaker form of positivism in disguise? (The position certainly shares some features in common with positivism.) It may have come *after* positivism, and that is the chief reason for calling it *post*positivism.

Answer. In no sense is the new philosophy of science—broad and ill defined though it is—closely akin to positivism (or, more especially, to the most notorious form of positivism, logical positivism). Logical positivism became discredited in the years immediately following the end of World War II; few if any philosophers these days subscribe to its core tenet, the "verifiability criterion of meaning," according to which a statement is meaningful only if it is verifiable in terms of sense experience (excepting logico-mathematical propositions).[2] As was pointed out earlier, one of the serious problems associated with the use of this principle in science was that it made theoretical terms meaningless. The fact is that many theoretical entities cannot be verified in terms of sense experience; neither can laws be confirmed absolutely (for they make universal claims that cannot be verified); but there are few today who would want to argue positivistically that the discourse of subatomic particle physicists or of black-hole theorists is meaningless!

A historical note might be helpful here. In the opening sentences of a paper written in 1956, when positivism was in its death throes, the major logical positivist Rudolf Carnap said that one of his main topics was going to be

> the problem of a criterion of significance for the theoretical language, i.e., exact conditions which terms and sentences of the theoretical language must fulfill in order to have a positive function for the explanation and prediction of observable events and thus to be acceptable as empirically meaningful. (Carnap, 1956, p. 38)

Carnap indicated his optimism (not shared by many others in the mid-1950s) that he would still be able to draw the line that "separates

the scientifically meaningful from the meaningless" (Carnap, 1956, p. 40). A few years later, in the same publication series, Grover Maxwell wrote what must be considered the majority antipositivist opinion:

> That anyone today should seriously contend that the entities referred to by scientific theories are only convenient fictions, or that talk about such entities is translatable without remainder into talk about sense contents or everyday physical objects . . . strike(s) me as so incongruous with the scientific and rational attitude and practice that I feel this paper should turn out to be a demolition of straw men. (Maxwell, 1962, p. 3)

Question 2. Aren't contemporary postpositivists clinging to an old and outmoded realist paradigm?

Answer. The question embodies a serious confusion. The *old positivist view* was antirealist; as explained in the previous answer, the logical positivists (on the whole) denied the reality of theoretical entities, and indeed claimed that talk of such entities was literally meaningless. Modern realism is not a carryover from positivism but is a recent postpositivistic development. Furthermore, there is little consensus within the philosophical community; whether or not realism is viable is a hotly debated topic—there are many contemporary philosophers for it, but there are many against it.[3] There is even controversy about the precise definition of realism; Arthur Fine (1987, p. 359) has written:

> Given the diverse array of philosophical positions that have sought the "realist" label, it is probably not possible to give a sketch of realism that will encompass them all. Indeed, it may be hopeless to try, even, to capture the essential features of realism.

Question 3. Well, old or new, many influential postpositivists are realists. Aren't they overlooking the fact that multiple realities exist, and aren't they overlooking the well-known fact that each society *constructs* its own reality? If you accept these two points, you cannot be a realist! Egon Guba has written that educational researchers (if not all social researchers) are studying phenomena that are

> *social* in nature. There is no need to posit a natural state-of-affairs and a natural set of laws for phenomena that are socially invented—I shall say socially constructed—in people's minds. I suggest . . . an ontology that is relativist in nature. It begins with the premise that all social realities

are constructed and shared through well-understood socialization processes. It is this socialized sharing that gives these constructions their apparent reality. (Guba, in press)

Answer. There are several important issues here, some of which were touched upon in the earlier discussion. In the first place, this question seems inspired by an extreme reading of Kuhn—the view that all of us are trapped within a paradigm and that we cannot converse rationally with those in other paradigms because our beliefs are incommensurable. Even the later Kuhn—the Kuhn of *The Essential Tension* (1977) or of the "Postscript" to the second edition of *The Structure of Scientific Revolutions* (1970)—did not accept this extreme relativism. Furthermore, such relativism seems contradicted by everyday experience within science. Freudians do understand—but, of course, disagree with—Skinnerians, and neo-Marxist social scientists understand colleagues of more conservative bent, and vice versa. The point is that paradigms (if one accepts this controversial notion[4]) serve as lenses, not as blinders.

Second, there is a confusion here between, on the one hand, the fact that different people and different societies have different views about what is real (a fact that seems undeniable) and, on the other hand, the issue of whether or not we can know which of these views is the correct one (or, indeed, whether there is a correct one at all). The relativist is committed to the view that *all* such differing (and contradictory) views are correct (or could be correct at one time), whereas the realist is committed to the view that at best only one view can be right (of course, all views might have portions that are sound or all might be wrong.)[5]

To make this a little more precise: Suppose that one social group believes that "X is the case," and another group believes that "not X is the case." The realist holds that *both* of these views cannot *be* correct, although, of course, some people *believe* one or the other of these to be true—it is the case either that X, or that not-X, but not both. (The realist does not have to believe that we can always *settle* which of these views, X or not-X, is true; the issue is whether both or at best only one *can* be true.) The relativist has to hold that there are multiple realities—that reality is (or could be) both X and not-X—for, if the relativist does not hold this position, then his or her position dissolves into the realist position. Stated thus boldly, it can be seen that the relativist case here hinges on obscuring the distinction between "what people believe to

be true" and "what really is true, whether or not we can determine this truth at the moment."[6]

Third, it is important to note that there are several quite different issues concerning realism, which the neophyte tends to run together, causing a great deal of confusion. The issue discussed directly above concerns whether, and in what sense, multiple realities exist; the opponents of realists here can be correctly labeled as relativists. A different issue was discussed earlier: The point in contention was whether or not theoretical entities (such as those postulated in the theories of particle physics, or in Chomskian linguistics, or in theories in cognitive psychology) can be said to be real; here the opponents of realists are properly labeled as antirealists. It is crucial to note that these antirealists are in no sense relativists. Thus it is a serious flaw in scholarship to claim that, because, in contemporary philosophy of science, there is much debate about the viability of realism, relativism thereby takes on more respectable status. The current debates in philosophy of science are between realists and antirealists, not between realists and relativists (Leplin, 1984; Siegel, 1987).

Finally, this third question raises the very important matter of the social construction of reality. Certainly there is nothing in postpositivism per se that requires denying that societies determine many of the things that are *believed* to be real by their members. Thus an "exotic" society may define certain spirits as being real, and the members of that society may accept them as real and act accordingly. A similar thing certainly happens in our own society, and not just with spirits. All a postpositivist would want to insist upon is that these matters can be open to research: We can inquire into the beliefs of a society, how they came about, what their effects are, and what the status is of the evidence that is offered in support of the truth of the beliefs. And we can get these matters right or wrong—we can describe these beliefs correctly or incorrectly, or we can be right or make mistakes about their origins or their effects. It simply does not follow from the fact of the social construction of reality that scientific inquiry becomes impossible or that we have to become relativists. It does not follow from the fact that a tribe of headhunters socially determines its own beliefs (i.e., the things the members of that group *believe* to be real) that we thereby have to *accept* those beliefs as *true*. What is true—if we have done our research properly—is that we have accurately determined that the members of the tribe do *believe* in their realities. But that is a different issue, which raises no problem of principle at all for

postpositivists. (In a similar vein, it is clear that Freudians believe in the reality of the id and superego and the rest, and they act as if these are realities; but their believing in these things does not make them real.)

It is worth noting that, for decades, postpositivists have accepted this notion of "the social construction of reality." Thus Sir Karl Popper, one of the major postpositivists (it is relevant to note that he claimed to have been the person who killed positivism), stressed that his philosophy "assumes a physical world in which we act," although he added that we may not know very much about it. But, crucially, he stressed it was also necessary to "assume a social world, populated by other people, about whose goals we know something (often not very much), and, furthermore, social institutions. These social institutions determine the peculiarly social character of our social environment" (Popper, 1976, p. 103). Popper includes laws and customs among "institutions."

Question 4. Given the acceptance by postpositivists of Hanson's thesis concerning the theory ladenness of perception, and given the general nonfoundationalist tenor that nothing can be considered as absolutely certain, and so forth, does it not follow that postpositivists have to abandon the notion of objectivity? Hasn't it been stripped of any meaning that it might have had?

Answer. Certainly not! The notion of objectivity, like the notion of truth, is a regulative ideal that underlies all inquiry (Phillips, in press). If we abandon such notions, it is not sensible to make inquiries at all. For if a sloppy inquiry is as acceptable as a careful one, and if an inquiry that is careless about evidence is as acceptable as an inquiry that has taken pains to be precise and unbiased, then there is no need to inquire—we might as well accept, without further fuss, any old view that tickles our fancy.

Now, it *is* true that the objectivity of an inquiry does not guarantee its truth—as was shown earlier, *nothing* can guarantee that we have reached the truth. Perhaps an analogy will help to clarify matters: Consider two firms who manufacture radios; one is proud of its workmanship and backs its products with a strong guarantee; the other firm is after a quick profit, practices shoddy workmanship, and does not offer any warranty to the buyer. A consumer would be unwise to purchase the latter's product, but nevertheless it is clearly understood that the first firm's guarantee does not absolutely mean that the radio will not break down. The fact that this situation exists

is not taken by consumers as invalidating the notion of a warranty, nor is it seen as making each purchase equally wise. And the very same situation exists in science.

The Popperian account of objectivity is widely, though not universally, accepted by postpositivists. The following sentences capture the essence of his approach:

> What may be described as scientific objectivity is based solely upon a critical tradition which, despite resistance, often makes it possible to criticize a dominant dogma. To put it another way, the objectivity of science is not a matter of the individual scientists but rather the social result of their mutual criticism, of the friendly-hostile division of labour among scientists, of their co-operation and also of their competition. For this reason, it depends, in part, upon a number of social and political circumstances which make criticism possible. (Popper, 1976, p. 95)

Conclusion

It can be seen from the foregoing discussion that postpositivism is a broad, complex, and dynamic approach to understanding the nature of science. There is little unanimity on important issues among its "adherents" (if people can be said to adhere to so amorphous a position)—but this is a healthy feature and not a weakness. Paul Feyerabend (1968, p. 33) wrote, a quarter- century ago, that unanimity of opinion may be fitting for some church, or for the followers of a tyrant, but it is most unfitting for science.

The danger to postpositivism comes not from internal dissension but from outside—from those who draw false, and often oversimple, conclusions from some of the very same developments that have produced postpositivism itself.

Notes

1. Many of the following issues are discussed at greater length in Phillips (1987b).
2. For more details on the complicated demise of positivism, see Phillips (1983).
3. A leading postpositivist antirealist is Bas van Fraassen (1980). His *grounds* for antirealism are not those of the logical positivists.
4. It is far from clear that the notion of paradigms as developed by Kuhn is sustainable; see the books by Phillips, Newton-Smith, and Siegel quoted elsewhere in this chapter.

[3]

Whose Future? Whose Past?
Notes on Critical Theory and Methodology

THOMAS S. POPKEWITZ

In writing about a critical science, one of the three paradigms explored in this book, I draw upon and clarify particular elements of a broad epistemological field that has developed in Europe and later in the United States since the middle of the nineteenth century. I call this tradition *critical* to focus upon the conceptualization of educational problems as part of the social, political, cultural, and economic patterns by which schooling is formed. A critical science gives reference to schooling as a socially constructed enterprise that contains continual contradictions: There are the noble dreams and hopes about creating a better and more equal society while, at the same time, social differentiations maintain unequal power relations and subtle forms of social regulation. A critical *science*, in turn, gives reference to a systematic inquiry (scholarship, or *wissenshaft*) that focuses upon the contradictions of educational practice. That inquiry, however, is not solely that of empirical investigation. There is a recognition of the interaction between the empirical tasks (that is, attention given to the ongoing events and phenomena of the world in which we live) and the concepts, theories, and insights that form simultaneously through interchanges with philosophy, history, the arts, other disciplines, and social practice.

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To probe the character of a critical science in education is to bring the theme of the socially constructed character of knowledge into questions about methodology. Throughout the discussion, I will consider the rules and standards of educational research as historically formed and tied to particular social values and political relations that are often hidden through the rituals and rhetoric of science itself. I will do this by focusing on two meanings of *critical*, the role of history and values in science and, in the last section, the relation of science to the problem of social improvement and progress.

What Is Critical About a Critical Science?

There are at least two senses to *critical* that are of importance to this discussion. First is the internal criticism that comes from analytical questioning of argument and method. There is a focus upon theoretical reasoning and the proper procedures for selecting, collecting, and evaluating empirical data. It stresses the logical consistency in arguments, procedures, and language. Continual cross-examinations and rigorous scrutiny of data are its hallmarks.

But to say that the rules of argument are important does not imply that rules of argument are always the same in different times and places. In different intellectual traditions, there are different ways of constructing arguments. We could compare the writing of behaviorialism in psychology with the narratives of history and anthropology to consider different rules that can be applied for defining evidence and constructing a reasoned argument. The differences in the rules of writing portray different assumptions about knowledge and truth: The American Psychological Association emphasizes the cumulative quality of knowledge through placing references into the text; the historical community, in contrast, places greater emphasis upon footnotes to locate where information is found and for criticism and discussion of references to occur (see Bazerman, 1987).

These differences in presentation are partly explained by the "nature" of the problem and the data that each discipline considers. The differences are also understood through the social purposes of the discipline as methods and concepts historically develop. The legacy of late nineteenth-century and early twentieth-century psychology, such as found in the work of G. Stanley Hall and Edward Thorndike, is related to the importance given to individualization, the belief in

useful knowledge that is interrelated with institutional developments found in schools, social welfare agencies, and the military. More will be said about this later.

A second meaning of *critical* can be drawn from these differences to reformulate the issue of logic. Logic involves not just a formal organization and internal criteria of scrutiny but also particular forms of reasoning that give focus to skepticism toward social institutions and a conception of reality that ties ideas, thought, and language to social and historical conditions. *Critical*, in this second sense, considers the conditions of social regulation, unequal distribution, and power. It is most visibly articulated in Marx's concern with the alienation produced with the division of labor in capitalism, Weber's focus on rationalization and bureaucratization, and Durkheim's discussion of the breakdown of the collective organization of culture. Recent social criticism has sought, in various ways, to respond to the constitutive questions posed by these nineteenth- and early twentieth-century writers. The Frankfurt School's Critical Theory, the French structuralists and literary poststructuralism, the sociology of knowledge, cultural Marxism, and feminist theory are contemporary tribal views that vie for the authority to speak for a critical science.[1] As this tradition is carried into educational research, its purpose has been to explore the conflict and tensions of schooling as a socially constructed institution. Let me explore this briefly.

The formation of schooling carried a worldwide hope for the school to fulfill the ideals of individualism and democracy for all in society (Boli, 1989; Kaestle, 1983). At the same time, differentiations of race, gender, class, ethnicity, and religion were brought into the everyday interactions and pedagogical patterns of schooling. An early twentieth-century redesigning of American school mathematics or the creation of new subjects such as "social studies" responded, in the first situation, to assumed different destinations of children and, in the second case, to a desire to provide education for Blacks and American Indians that would make their conditions more humane (Lybarger, 1987; Stanic, 1987).

Learning and teaching, as well, have social implications that are more than the measurement of achievement or the mastery of concepts. Schooling is an institution whose pedagogy and patterns of conduct are continually related to larger issues of social production and reproduction.[2] In this context, pedagogical practice is a form of social regulation in which particular social knowledge is selected and

cast for children to guide their everyday lives; yet the social differentiations in the larger society make school knowledge not equally accessible or equally available for all who come to school. Further, the construction of school processes and knowledge contain codes of ethics and notions of civility and discipline that are to govern personal and public lives.

A critical science is concerned with ways in which social, cultural, and economic conditions produce a certain selectivity in the processes of teaching and the organization of curriculum. It involves a continual skepticism toward the commonplaces and socially accepted conventions of schooling, realizing that social practices contain contradictions in which there are continually issues of power domination. The science of schooling is to inquire into the relations of the conditions and organization of schooling, as continually bound to processes of production and reproduction in society. The relationships, however, are not linear in fashion but shaped through debate and struggle as pedagogical practices are constructed.[3] Important to the debates in the critical sciences are different conceptions of power. For some, priority is given to certain structural relations in the constructions of schooling, such as class, ethnicity, or gender. A different position remains sensitive to these concepts but focuses on how power is circulated through the relation of knowledge to the construction of identity.

It is in the contradictions of schooling that we can talk about the work of educational sciences as one of inversion: The construction of more appropriate social conditions involves making history fragile. A science of schooling explores the constraints and restraints of school affairs, thus poking holes in the causality that confronts us in daily life and that limits our possibilities. Human possibility, it is believed, occurs through understanding how the boundaries and structures are formed through struggle rather than as given as an inevitable and unalterable present.

There is a point of debate within those who practice a critical science about the relation of researchers to social movements. One strand argues that the partisan role of science makes it an obligation to pursue those commitments through active participation in political movements (see Ginsburg, 1988; Giroux & McLaren, 1986). Ginsburg, for example, argues that *critical* is not only an intellectual involvement in the production of ideas but entails a direct and explicit involvement in efforts to transform current social relations. A different stance is

taken here; social scientists are partisans in the forming of social agendas through the practices of science, but that involvement is not necessarily the same issue as that of praxis and the making of strategic choices in political contexts (see, e.g., Cherryholmes, 1988). There is also a distinction here between Anglo-Saxon and Continental traditions of social science that has to be historically considered. (I discuss this more fully in Popkewitz, 1984, and in the manuscript on which I am current working.)

History and the Study of School Life

The epistemological commitment to a study of schooling is also a commitment to a certain self-reflection about the rules and standards of the work of science. A critical stance is to reconstruct a science of education by making history an integral part of the study of methodology. Social values, struggles, and interest influence the questions, concepts, and strategies of educational science. Words of scientific standards, such as *adequacy*, *values*, or *rigor*, are not logical artifacts independent of social affairs but are concepts formed and reformed in a dynamic world of institutional arrangements, linguistic conventions, and contested priorities. There is no individuality in science without communal rules. There is no personal knowledge in any absolute sense or practices that are not bound to the cultural conditions and social circumstances.

I use the notion of history not as a chronology of events or in mere deference to context. History is to acknowledge the present as a heritage not only of physical goods but also of social forms and knowledge. The analytic procedures of theory development or the statistical procedures for data analysis, for example, are a part of more general institutional developments that made abstract knowledge a valued form for organizing social affairs in the nineteenth and twentieth centuries. The reason of the present, therefore, should be seen in relation to its past.

Six themes frame the discussion considering the meaning of *critical* in the practices of a science of education: (a) Certain institutional conditions and practices of educational sciences make the distinctions between methodology, methods, and procedures into technical problems, obscuring the historical problem of scientific epistemology; (b) standards and rules of science are a form of reasoning that is always

bound in time and space; (c) the dichotomy between *objective* and *subjective* is misleading and obscuring of research practices; (d) the production of knowledge is the production of values; (e) there are social interests in the claims for disinterest; and (f) when thinking about science as guiding the future, there is the irony that science is inevitably about the past. The limitation of what science can offer also sets limits on what should be sold to people in positions of power and powerlessness.

(1) *Procedures, methods, and methodology of science.* The commonsense practices of educational research tend to remove social and historical concerns from problems of the construction of procedures and rules for research. This separation is found most often in graduate programs that require students to take particular courses in "methods" or methodology that are, at root, programs concerned with the procedures of collecting and analyzing data. These courses are typically concerned with ways of collecting information: constructing survey instruments, interview procedures, observational techniques, and coding procedures and, more recently, the use of computer-generated data analysis. These activities are important to scientific practices, but it would be in error to understand them in a context devoid of the problems of study or the concepts that are employed in research. The history of science has provided strong evidence that the procedures of statistics, interviews, or observation strategies do not stand alone but are part of a matrix of curiosities, questions, and social practices that, in their entirety, constitute the scientific enterprise.

It is in this matrix of research that methods of study can be properly discussed. Methods are formed from exemplars, given in science, but are reworked through the interrelation of questions, concepts, and procedures as curiosities are directed to empirical phenomena. In the physical sciences, one learns how to use the machinery of the laboratory as the part of a whole: The questions at hand, the concepts brought to bear upon the investigation, and the particular contextual variations contain ambiguities and complexities that researchers work through to shape and fashion their methods of inquiry. There is no scientific method, rather methods related to the particular study being undertaken. Methods of science emerge from a complex process in which the conduct of study occurs.

Methodology, in this context, is concerned with the relations of the various parts of study with the production of findings. Methodology is concerned with the moral order (the rules, values, and priorities

given to social conditions and individual action) presupposed in the practices of science. It is the study of what is defined as legitimate knowledge and how that knowledge is obtained and ordered.

Conventional ways of talking about science that conflate methods and procedures provide little understanding of the underlying matrix of assumptions, dispositions, questions, concepts, and procedures that interrelate in the production of knowledge. It is this sense of paradigm that is most provocative in the work of Thomas Kuhn (1970). But to accept Kuhn's notion of science would be to view the production of knowledge as solely one of the conflict of ideas without locating the practices of science within its social conditions (see Lecourt, 1975; Manicas, 1987; Tiles, 1984).

To distinguish among the three layers of scientific practice helps in understanding that a strictly logical approach to science makes the choices seem procedural. To study scientific methods and intellectual traditions is to consider the relation of rules in historical conditions. A philosophy of science is also its history—how various types of questions are formed in intellectual traditions *and* institutional conditions, that is, how different sets of questions of study emerge as a part of an intellectual tradition. Further, there is a need to explore how questions are determined by, and determine methods in, the production of knowledge. What seems logical about inquiry is made so because of systems of meanings and relations that make "things" seem reasonable and plausible. The practices of statistics or field approaches are not independent skills but exist within general sets of questions and assumptions that provide conditions and purpose to inquiry.

As an example, consider the turmoil in political science during the early 1970s. Two important studies of the problem of governing gave completely different answers about who rules. C. Wright Mills's (1956) *The Power Elite* studied the interrelation of social, business, and governmental elites, identifying how they were connected in subtle but important ways. Mills argued that there was an interlocking institutional arrangement among educational institutions, government, and business in the production of power in society. In contrast, Robert Dahl's (1961) study of *Who Rules* traced the changing background patterns of politicians to changing demography. He defined the process of governing as organized around changing pluralistic interests, arguing that there was no center of power.

Some people asked how two such bright people could come to such different conclusions. The answer had little to do with traditional questions of procedural rigor or bias, for both studies have withstood severe scrutiny. One way to think about an answer is to consider the paradigmatic assumptions by which the studies were conducted. The way in which concepts of power were defined within intellectual traditions (Mills worked within the sociology of knowledge and Dahl from behavioralism) interrelated with the manner in which the techniques of study were brought into and related to the purposes and conceptions of inquiry. Each tradition of inquiry presupposed certain dispositions toward the patterns of the world that were "built into" the methods and concepts of study.

I can summarize the previous discussion in the following manner: Powerful institutional practices support and sustain a belief that educational sciences are only to address procedural problems in defining strategies of research. The organization of science is separated from social movements, historical conditions, or political interests, except in terms of a technical issue to control bias and prejudice. In contrast, if we "see" these procedures as historical constructions, the procedural stance is inadequate. A more adequate approach to purposes and boundaries is explored by considering certain commonplaces of educational research: the separation of objective and subjective in research, the separation of social values from scientific values, and the perception of scientists as disinterested observers. Proceeding in this manner, I intend to reformulate the purpose and rules of a science of schooling related to the critical tradition.

(2) *Redefining the standards and rules of science as a logic in historical context.* I would like to pursue the meaning of *logic* as a way of moving closer to the sets of relations and standards that are important to a critical science. Conventional "wisdom" is that logic is a universally valid process that continually clarifies and redefines what knowledge is, introducing science as a cumulative and progressive development. I want to argue, instead, that the logic of science changes in a manner that is not necessarily cumulative. It occurs as breaks and ruptures in practices and ideas in social transformation. In the fifteenth century, the study of logic was to uncover the rules of the mind through the study of grammar and speech, for the laws of the mind were seen as a microcosm of the larger laws of the world given by God (Durkheim, 1977). Today, logic has a very different set of meanings. Rules of

science give reference to the empirical, rather than to the social, status of the speaker or to the authority of God (Gouldner, 1979). Language is seen as having cross-contextual validity in that its rules transcend the particular and the idiosyncratic.

The changing of logic as scientific practice can be explored in the study of professions. There is no universal concept to assign to the word *profession*. Rather, it entails shifting concepts within different historical settings. In the early nineteenth century, professions were implicitly tied to the American schools' purpose in educating children in the moral precepts of Christianity. Teacher education aimed to have teachers express their sincerity and empathy with Christian values. Teacher seminars were constructed to enable a "professing" of one's religious conviction, to commit to vocations to affirm one's calling. Science was the search for methods to introduce moral and religious values.

Profession in the reform movement of the past decade has very different sets of concepts and epistemologies. Moral and ethical convictions of the 1800s are replaced with knowledge that ties the expert to practices associated with rational planning. In contemporary U.S. society, *profession* assumes an ideological quality that provides occupations with social and cultural authority.

The tying of logic to reason and social history can also be pursued by noting the various roles of psychology in educational research. Although historical considerations could be applied to other disciplines in education, including the development of critical sciences in the United States, my purpose in choosing psychology is its central location in educational research.

Psychology was the only social science to emerge solely within the university, first as a subdiscipline of philosophy and then as a separate discipline in the late nineteenth century. The challenge of psychology was twofold. It was to replace philosophy in a religious crisis about the mind/body split produced by the materialism of Darwin. Psychology was also tied to the emerging schools, with behaviorism dominating through its experimentation, testing, and measurement, which were seen as plausible responses to issues of administering schools.

The invention of behavioral psychology needs to be understood in relation to institutional conditions. The experimentation procedures developed in Leipzig by Wilhelm Wundt were part of the background discussion in the formation of American psychology (O'Donnell,

1985). Although Wundt's experimentation was to focus on human purpose and the objective conditions in which the mind is formed, the American psychologists took a narrower concern with attitudes, attributes, and skills. What Americans saw in the laboratory of Wundt was the study of psychological processes through individual case descriptions about what people thought. American psychology redefined the problem of individualism to consider statistical distributions and group experimentation that would identify population characteristics. (I am reminded here of the first American translation of the Soviet psychologist, Lev Vygotsky. Although Vygotsky sought to develop a Marxist psychology after the Soviet revolution, the English translator left out all references to Marx because they were considered irrelevant.)

The shift in procedures had little to do with the issue of scientific progress or knowledge accumulation. The approaches to American psychology were related to demands for collective data about aggregates of students going to schools. The "audiences" who provided resources were instrumental to the paths taken as science rather than to any "inherent" quality in either the knowledge gained or the scientific approaches used (Danziger, 1987). Historians of psychology have suggested that the particular methods were more related to the utilitarian pursuits in the field of education than to a more general concern with science (Napoli, 1981; O'Donnell, 1985).

The relation of methods of psychology to institutional conditions can help us focus upon certain issues raised about research practices. First, questions about what procedures exist for data collection/analysis are part of the *social* field of science. The ordering of data in psychology had to do with the types of knowledge that were appropriate for professional development, legitimacy, and resources. Second, ontology and epistemology are undifferentiated in the practical world of science. What is to be known and the means of knowing are intertwined and influence each other. Third, what is deemed as adequate knowledge and its development are not necessarily related to what has been previously accumulated through empirical investigations. I can remember the efforts made in secondary (meta) statistical analysis of empirical research, which only result in so much variation in the concepts, sampling, and analysis that straightforward comparisons were not possible.

From this initial discussion, I want to consider the logic of science as a problem of a social epistemology. What is accepted as procedures,

concepts, rules, and investigation is not "natural" or inevitable to research but is made reasonable within institutional settings and social interests. A critical science is concerned with how these rules are socially constructed, responding to and a part of the relations and power arrangements in which science is practiced.

(3) *If I have nice thoughts, will the world become nicer too? The poverty of the dichotomies of subjective and objective.* I will continue the theme of reason and thought as part of their historical and social context but will recast it in relation to discussions about objective or subjective knowledge. I interpret a central debate in philosophy and social science as concerned with this relation.

First, I would accept a modified realist view that there are real objects in the world (see Manicas & Rosenberg, 1985). We do pass through doorways and can hurt our knees if we fall off a bicycle and hit the pavement. Things occupy physical space and time. Yet, once that is said, I still have to take a modified view of realism and, therefore, of objectivity. To say that there are trees is also to recognize that *tree* is an arbitrary name that assumes particular and possibly different meanings as it is placed within symbolic fields. There is a difference in speaking about trees in a school curriculum between a scientist concerned with the environmental issues of tropical forests and ozone layers and a literary description of lovers in a forest. Our categories and distinctions assume significance because of the ways in which they are positioned within language and as that language is made part of the rules and standards of social practice itself. In this sense, there is no essential or "basic" meaning to a word but continual processes by which words are given meaning.

The arbitrary and situationally derived meaning of words has important consequences when approaching a central dualism of research, the separation of *objective* and *subjective*. These two words are made into oppositions that I believe are misleading, obscuring, and often mystifying. *Objective* has nothing to do with external laws or a "nature" to be discovered or verified. Rather, it is to consider the *socially* formed patterns that impinge upon our daily life as unquestioned and seemingly natural boundaries; and, at the same time, because these conditions are historically formed through human struggles, the patterns are dynamic and changing. *Subjective*, in contrast, directs attention to what is occurring in the minds of people, such as the dispositions, sensitivities, and awarenesses that people have in their daily lives.

Once these two words are posed in relation to the phenomena of the world, it is not easy to tease out what is one's individuality and what is formed by unseen and unacknowledged rules that act as horizons for individual reason. Feminist scholarship, among others, has explored the interrelation of family structure, economic transformations, and political forms in defining women, men, and their participation in civic and political affairs (see, e.g., Pateman, 1988b; Weedon, 1987). Although there are reasoned arguments to give value to caring and nurturing in human relations, feminist scholarship has made us aware about the problem of "naturalizing" such characteristics by recognizing that these "womanly" traits were part of the social transformation of the bourgeois home and workplace. Social historians of schooling help us understand that teachers' talk about curriculum, methods, or management of classrooms is not *merely* teachers' talk but words that express complex social relations that are brought into the discursive patterns of schooling through historical processes (see, e.g., Hamilton, 1989). Two collateral issues about research methods emerge from the consideration of the interplay of object/subject. One is the distinction of hard and soft data. We often think that statistics is objective, providing *hard* data that can be trusted. *Soft* data—empathy, open interviews, and introspection—are to be viewed with suspicion. The use of statistics, however, is no better or worse than the questions and methods that underlie the research and the social processes in which the research is used. One needs only to construct a survey to understand how such surveys contain patterns of selection, omission, and dispositions toward the social world. The distinction of *hard* and *soft* also has a gendered quality that cannot be ignored in research and among researchers.

A second collateral issue is the movement to develop field methods to illuminate the actual words, language, and patterns of interactions. Here, the real is considered as the situated thoughts, language, and particular practices of people. The argument is that surveys do not enable researchers to understand the intent and purpose of people as they communicate; it considers qualitative methods as providing an approach that gets to the basic or underlying values, meanings, and interpretations. This second approach naturalizes the present by assuming that there is an essence or reality to be discovered. The view that there is a grounded theory has to be rejected.[4] Reasoning is itself a part of a historical process; the data of perceptions, attitudes and belief need to be continually placed in social and cultural horizons.

In this sense, a critical science assumes that there is no psychology of schooling without a corresponding sociology and concern with history. While talking about voice, empowerment, and personal autonomy is ideologically potent in contemporary rhetoric, the complexity of the interrelation of objective conditions and subjectivity is less straightforward and more profound than can be captured in a language of dichotomies.

(4) *Can there be a disinterested science among interested social scientists?* The rejection of a notion of *objective* as describing the essential properties of a real world also entails a rejection of the belief that the scientist is a disinterested observer. The belief in disinterest is related to the idea that the most appropriate knowledge is "purely" descriptive explanations of how things work. Such knowledge could be used by any groups or interests in society as they seek to understand social affairs (see Easton, 1957). Max Weber, the German sociologist, spoke about a value-free professoriat. He was concerned with university lecturers and their providing different arguments for a problem. He did not assert a value-free science. As the German idea was brought to America, it became the idea of disinterest that contained different meanings and purposes from those found in Europe.

The idea of disinterest became part of the social science debates as the professions were organized during the Progressive era reforms (1880-1920). Disinterest is a political strategy.

The notion of disinterest was given definition in relation to institutional issues. Certain economists, political scientists, and sociologists who were to dominate the professional organizations expressed concern about a reformist scholarship tied to Christian social ethics and socialist causes. It was feared by university presidents that business leaders who supported the university would react against social scientists' involvement in social protests. It was argued that professional debate should be limited to professional meetings and journals. Undue publicity would undermine professional claims of expertise as the internal conflicts about purpose and values were brought into the public discussion. An outcome was a professional ethics that stressed an empiricism without professional involvement in political agitation.

The notion of disinterest emerges in this debate (Furner, 1975; Silva & Slaughter, 1984). It was tied to a particular type of empiricism that was thought necessary to provide social scientists with a secure position in the university, resources for occupational development in an expanding state and economy, and jobs for the new scientists

produced by the emerging graduate schools around the nation. The notion of disinterest was to deny the professoriat legitimacy to act toward social amelioration except in an expert role.

It is interesting to note that the social scientists emerge as part of nineteenth-century movements for social amelioration, and that heritage continues in today's educational reform practices. The appearance of disinterest is easy to maintain when values and dispositions appear to be consensually agreed upon or so dominate that there seems no reason to scrutinize the concepts and theories that are used. Many of the early social scientists took for granted dominant social, cultural, and religious values that they thought should be promulgated. Today, much educational research focuses upon functional relations between teaching and learning and school organizational characteristics while structuring out of consideration the conflict of values in schooling. The functualist obscures the relation between research concepts and strategies and the social values of reform that are being maintained (see Popkewitz, 1984).

At best, disinterest can mean a disciplined and systematic approach to investigation in which one "plays" with different interpretations. But this play always occurs within boundaries, as there are presuppositions, paradigmatic assumptions, and social fields in which practice occurs.[5] Disinterest cannot mean lack of commitment or ideas that have no social location or consequences.

To this point, I have pursued assumptions of a critical science by probing certain beliefs, social values, and interests that are embedded within the practices of educational sciences. I have proceeded in this manner to place the methodological categories and distinctions of educational science in a historical confrontation with those of the social formation of institutions. At the same time, the discussion has sought to reconstruct the rules of research as a problem of engaging in relating history to epistemology. A science of schooling is one that involves complex social, philosophical, political, and historical questions that are intertwined with the practices of science itself.

(5) *The production of knowledge is the production of values.* Social science knowledge is a part of the production, administration, and ideological spheres in society. One way to think about the multiple values that are continually embedded in inquiry is to focus upon two roles of social sciences even though they are contradictory.

One is a civil service function in planning. Often this relates to state practices, such as found in reform movements and evaluations of inputs and outputs of school activities. Much of what can be consid-

ered within positivist research is a part of that structured relation to collect information and rationally guide and administer state programs. Second, and at the same time, social science can provide a critical voice of social consciousness. It takes a skeptical attitude toward the public rhetoric, taken-for-granted patterns, and daily practices found in schools. In considering these different affiliations and social purposes, there has been debate about whether scientists are part of a new technical class in society or whether they are an important stratum in the production/reproduction of class and cultural relations.

In whatever role social scientists adopt, the discourse of social science helps to establish boundaries by which strategies for reform and innovation are to occur. The organization of data posits assumptions about society, such as that the world should be considered as in equilibrium or in conflict or people as rational or irrational. The categories of schooling frame and carry assumptions about what is to be argued;[6] this can be illustrated by juxtaposing the categories of *learning* and *work* as a lens for studying classroom practices. Each word provides a different point of entrance for thinking, seeing, talking, and feeling about schooling.

The legacy of positivism has dulled our senses to these issues of values by focusing upon rules and procedures of science, defining the world as asocial and ahistorical. Taxonomic and hierarchic schemes are created that assume that the sum of the parts equals the whole. Change is a sequential and hierarchic arrangement of discrete elements. A consequence is fragmented notions of causation. The minuscule is made important and social life trivialized.[7] The positivism that I speak about is not necessarily a philosophical position but one concerning the folklore, reconstructions, and training in educational sciences. The development of American behaviorism and empiricism, for example, occurred in ways that were independent of the work being done by the logical positivist; only later was there some interaction.

Another way to consider values is by considering the procedures of inquiry. The use of statistics and case studies can help us focus more clearly on this issue.

We tend to think of statistics as producing a sophisticated science because it is thought that the only values are in the numbers themselves. The discussion, however, ignores the debates in mathematics about what visions are appropriate for exploring both natural and

social worlds (see e.g., Crutchfield, Farmer, Packard, & Shaw, 1986; Ralston, 1986). Further, there is a selective memory that has lost sight of the origin of "statistic" as a science of the state that was to guide modern governments as they began to organize social reforms.[8] Demographic data provided information about social welfare and amelioration.

But more particular to educational sciences are the social values that are in its techniques. The situation is not unidirectional. Statistics have enabled an expansion of phenomena to be examined not previously available, therefore, enlarging the problems studied. At the same time, when statistical measures are constructed, they are always embedded in a system of social values and interests. Let me give two examples.

Factor analysis was derived from factor psychology. It divided the mind into separate spheres that were thought to exist independently of the other but that could be correlated. This view of the mind has long been in disrepute and is no longer given serious thought, yet the technique is still in use. We can consider as well the work of Karl Pearson dealing with group tendencies. It is plausible to relate the direction of his mathematics to his concern with proving the collective superiority of the White race as Britain expanded its colonial empire.

The development of case studies that define social life as a negotiated order provide another set of values that can be read in contradictory ways. Sociology and anthropology introduced approaches to consider the dynamic qualities of people's interactions and language that are not amenable to behavioral methods. But the "deep" description of interactions is also related to the problems of the social management of micro levels in modern states. The methods provide new methods of supervision, observation, and control of individuals as intimate thoughts and feelings are opened to public scrutiny (Foucault, 1978; Martin, Gutman, & Hutton, 1988).

The plausibility of case studies entails a consideration of the American social context in the Depression years of the 1930s and again in the aftermath of the struggles in the 1960s. When other parts of social life become more remote and complex, case studies have a symbolic potency in establishing the importance of one's immediate encounters and power in defining situations (see Popkewitz, 1984, chap. 4). As symbolic canopies, they provide a linguistic structure that emphasizes a negotiated order and participation in a community. In doing so, the prior assumptions and historical conditions in which the interactions occurred are taken for granted.

Although we can identify prior assumptions in the procedures of science, it is not sufficient to talk about values. We have to know how procedures are used in relation to the methods of study. Earlier I spoke of words as having meaning only as they are established in relation to other words and in context. The social implications of techniques also entail rational thinking. Statistics can have different implications: They can be used to produce efficiency and effectiveness in administrative reforms; they can also provide understanding about differentiated cultural dispositions among different class positions and the unequal distribution of wealth. In a similar manner, we need to consider ethnographies. Tom James (1986), a historian, has described how anthropologists worked with progressive educators in Navaho communities. Their task was to create strategies to produce acceptance of federal land use policy during the 1930s. A few of the anthropologists, however, felt moral indignation at the conditions they found and testified in Congress for reform.

From this brief discussion, we can see that procedures and techniques are not the only elements of research subjected to values. Values are in all layers of science. The issue is not to rid science of them or to identify bias for purposes of control. Such control is a chimera that has long been recognized in positivism, hermeneutics, and critical sciences. The problem is to consider the contradictions that interact at all levels of the practice of science. It is also to recognize that the commonsense approach that defines values as distinct and separable from the procedural concerns of science has its own poverty by decontextualizing the way choices are made and the priorities formed.

As part of a critical science concerned with educational research, questions are asked: What social and cultural conditions make plausible the forms of analysis that dominate the educational sciences? How is it that certain forms and styles have come to dominate psychology, sociology, and anthropology in education? What social debates, and by whom, give support to the manner in which the standards and rules of inquiry are adopted and revised? And how have these standards changed over time and in relation to what social transformations?

(6) *Whose future is it anyway? Science as the study of the past.* Let me pose the issue about the usefulness of educational paradigms as a question about what a science can do and cannot do: Any social science is *inevitably about the past*. Epistemologically, social science is

a dialect of language about what *has* happened. Although we like to think of our generalizations as about the present, generalizations are constrained because they are constructions that occur after the events.

There is also a political question when we assume social science is about the present, educational reform, or future social progress. When we adopt a belief that knowledge is about prediction and administration, we have left science and its relation to the empirical world to move into the realm of ideology and social control. The rituals of science become a rhetorical form that is to convince others that what is being done to them is in their own interest.

I say this because I can find no evidence that social science has anything to say qua science about the future, but it does have methods for understanding the boundaries that exist in the past. This is not to say that science cannot help us in the choices we make but that is often in a negative voice. To borrow partially from Karl Popper, science does not verify but refutes. Science can help to understand what choices not to make, such as in eliminating fluorocarbons, controlling the deforestation in the Amazon, or limiting the use of intelligence testing. But in the policy arena, the findings of science are part of a public debate that is rarely that of evidence alone. The determination of futures is not reserved for particular elites and experts who claim a sacred knowledge.

Before ending this discussion about past, futures, and science, there is an important caveat. Science is about the future in an indirect way, but not because it can predict and control. The categories organize phenomena in a manner that sensitizes us toward certain possibilities and, at the same time, filters out others. Implicit in practices then are ways in which people are to challenge the world and locate themselves in its ongoing relations. This role of science and scientists in the ongoing construction of the world needs continual attention.

Some Concluding Thoughts

The major charge of the conference from which the chapters in this book have been drawn was not to determine "who is right about science" but to search for differences and distinctions that can lead to more fruitful practices. In this context, I have considered the values and methodology within an epistemological field that I associate with

"critical." I have argued that our traditional ways of organizing the work of social science as objective/subjective, rigor/relevant, and discovery/verification obscures more than it illuminates.

Central was a placing of debates about methods and truth in a social and historical context. In different ways, I have argued against a notion of knowledge accumulation as a reification of the social and historical conditions in which knowledge is produced and transformed. Although we need to understand what others have said and done before us, it is not just a problem of "adding on" knowledge. It is a complex process of interpretation and analysis that considers how the social forms, knowledge, and struggles of those before us are a part of the present.

One might raise the question, at the end of this short foray into paradigms: What does a critical science have to recommend as a tradition for the study of schooling? I believe that its various strands provide the most elaborate theoretical discussions of the problems of schooling as a socially constructed institution in a world of inequalities. It offers a way to reassert history, value, and ethical choice into the knowledge that we have about social practice. It enables us to understand that freedom and autonomy are never absolutes but always practices within patterns of constraints and restraints, with a purpose of an educational science to poke holes in the seeming causality of social life.

The methods of a critical science are also a cross-checking mechanism on the hubris of intellectuals and power relations that underlie the formation of knowledge itself. For example, much current research accepts the logic and reasoning found in schooling, arguing that researchers and policymakers need to respect teachers' talk. Yet, the style of argument in teaching cannot be taken for granted. It presupposes the particular cultural competence found in schooling, with its interpretive stances and cognitive frames. To evoke images of stages of development, children's "nature," or teachers' reasoning is to engage in conversations that are organized within particular historical settings that are presupposed in the conversations. The words and their assumptions cannot be taken for granted but are part of the problem of the sciences of schooling.

To consider language and thought as existing in historically produced contexts is not to deny that there are agents. Nor is to adopt a philosophical position of extreme relativism, a stance that the earlier discussion of schooling would rule out. Rather, it is to relativize issues

of methodology by making them historical issues that tie the practices of science to those of power and control in society.

One of the questions posed in the formulation of the conference and subsequent book is whether it is possible that the various paradigms discussed here can find any room for accommodation. I have a practical and a "theoretical" answer to this question. Practically, there is accommodation that occurs through the hiring practices in universities and in the scholarly debates.

If we focus upon paradigms as social epistemologies, then we cannot nor do we want to homogenize the distinction and have accommodation. The importance of the divergence is epitomized in the papers from this conference. If we read them not only as giving us information but as ways of expressing relations in the world, we can understand some of the fundamental issues that underlie the "modernity" of the world in which we live. As Habermas (1971) argued, there are different human interests in social science, and these contain different dispositions toward the world and how we challenge it. The argumentation, debate, and cross-fertilization concerning these interests have a dual quality, which makes for a more serious debate about the work and knowledge of science. It also enables a certain humility as we are continually made aware of the precarious quality of our knowledge and agendas.

Yet for me, the practice of science in all the paradigms needs to be reconstructed with a strong sense of its social epistemology, that is, the interrelation of science with the historical conditions in which it works. Without this, science becomes procedural, technical, and one-dimensional. Here, I guess, I leave the pluralism. To include a disciplined sense of history into methodology and methods introduces strong questions about ethics, morality, and politics. It rejects "seeing" the discrete events, whether bound to "qualitative" or "quantitative" techniques, in isolation from the relation of events to historical formations. In that manner, neopositivism and hermeneutic traditions have to be reconstructed. History becomes a part of the analysis and logic of a science as the researched, research, and researcher are interrelated.

A final comment about understanding scientific practices. The three traditions entail a continual debate and struggles about who has the authority to control the signs of science. In the relations among the various traditions, there occur changing boundaries and conceptual relations (see Manicas, 1987; Toulmin, 1972). One cannot adequately consider positivism without also focusing upon the debates on critical

sciences and hermeneutics. At the same time, alternative and oppositional traditions are defined within a horizon of positivism that dominates American culture and science. It is in the interactions and boundaries among the competing traditions that we can locate the practices of science and an adequate study of paradigms.

Notes

1. For those who wish to explore some of the nuances among these different approaches to study, see Beechey and Donald (1985), Bourdieu (1977), Dreyfus and Rabinow (1983), Foucault (1980), Giddens (1987), Habermas (1971, 1984, 1987), Jay (1973), Mannheim (1936), Pateman (1988), Weedon (1987), and Williams (1977).

2. Important to the debates in the critical sciences are different conceptions of power and its implications. For some, priority is given to certain structural relations in the constructions of schooling, such as class, ethnicity, or gender. A different position rejects the argument about privileged concepts and focuses upon how power is circulated to produce subjugation.

3. I think that the reader should be aware that my background is pedagogy; my department is one called "curriculum and instruction." I might take a different position here if I were located in a sociology department or educational foundation, such as British researchers who are interested in similar concerns but housed in sociology departments.

4. In a recent report, a researcher treated a teacher's comments about stages of development and teaching as management as a way of talking about pedagogical reasoning and action. What is ignored is how that language of stages and management came into schooling as a form of social regulation of women teachers, how that language has been incorporated into the "natural" setting of teaching in a manner that makes issues of control and power invisible.

5. I have begged the issue of change only because that will involve another discussion about structured relations.

6. I discuss this in Popkewitz (1984).

7. While a methodological individualism is contained in the empiricism that dominates behaviorism and cognitive psychology and most American sociology, it has been severely critiqued as an untenable philosophical and methodological position (see Udehn, 1987).

8. *Statisik* was initiated in seventeenth-century Germany and as political arithmetic in Britain. In the ancien régime, the French civil service provided information about aspects of society essential to direct the affairs of state. Initially, it did not imply that such information need be quantitative (Clark, 1973, p. 27).

[4]

The Making of a Constructivist
A Remembrance of Transformations Past

YVONNA S. LINCOLN

Ours is a time of crisis and deep ferment—not only politically but intellectually: older school doctrines and entrenched philosophical positions are crumbling or being swept aside and replaced by more flexible and unconventional vistas. In the Anglo-American context, the sway of logical positivism—focused on scientific epistemology—has largely come to an end. (Dallmayr, 1985, p. 411)

As careful readers may have noticed while they perused the history of science literature, there are vastly different definitions of positivism, depending on whom one reads and what his or her original sources might have been (Harre, 1981; Hesse, 1980). I attribute this less to failure to communicate among the positivists (or historians of science) than to something that I shall use as a springboard for this chapter: the highly individual nature of the paradigm-building process and the focus on several elements of a paradigm to the exclusion of others. Since the constructivists have lived with singularly sharp criticism, it has not always been their luxury to be selective about focal elements. Nevertheless, the idea that fooling around with a new paradigm is an intensely personal process, evolving from not only intellectual but also personal, social, and possibly political transformation seems a persuasive and compelling path to take.

In part, I wish to focus on the more recent past, although, of course, the early years were equally important. The early arguments have been available for criticism, for examination, for comparison, and for use by others for some time. That the paradigm revolution is here is a given. Bernstein (1976, p. xii) has observed that

> the initial impression one has in reading through the literature in and about the social disciplines during the past decade or so is that of sheer chaos. Everything appears to be "up for grabs." There is little or no consensus—except by members of the same school or subschool—about what are the well-established results, the proper research procedures, the important results, the important problems, or even the most promising theoretical approaches to the study of society and politics. There are claims and counterclaims.

I have argued Bernstein's point in many other places (Guba & Lincoln, 1981, 1987, 1989; Lincoln, 1985, 1989; Lincoln & Guba, 1985). That the new paradigm's final shape is not yet fixed is also fairly apparent, although I've been attempting to do my part in the hammering process. Critics of constructivist (or naturalistic or ethnographic) inquiry have aided and abetted this cause by pointed criticism of various aspects of the paradigm.

As any good biography begins with the "early years," I'll begin with mine. Please consider what follows next as the childhood reminiscences.

The Early Years of Naturalism

It is handy to think of my intellectual development in terms of early years, an adolescence, and a more mature period. The early years remind me now of the credo of many small business owners: "If I'd known what I was in for, I'd never have started this!" Egon and I rejected conventional inquiry on three basic grounds: its posture on reality, its stance on the knower-known relationship, and its stance on the possibility of generalization. This seemed to us most appropriate when we considered the special case of evaluation (as one form of disciplined inquiry). On a regular basis, we confronted endless political problems dealing with multiple constructions of the same evaluand. It also became clearer and clearer that knower and known not only could *not* remain distanced and separated in the process of evaluation but probably *should* not. And, finally, we began to doubt seriously the possibility of generalization from one site to the next because of contextual factors (Guba & Lincoln, 1981). We had been "brought up" to believe that

> what is unknown or unusual to us will be explained or accounted for by natural sciences in general (e.g., physics, chemistry and biology) and *by the methods they employ in particular.* This natural scientific approach makes a number of assumptions, the three most crucial . . . being that: (a) the phenomenon under study . . . must be observable . . .; (b) the phenomenon must be measurable . . .; and (c) the phenomenon must be such that it is possible for more than one observer to agree on its existence and characteristics. (Valle & King, 1978, p. 4, emphases added)

In this world that we have nearly all inherited,

> the priority is given to the measurement perspective, and, in order for something to be measured, only its tangible aspects can be apprehended, and thus the *indices itself of a phenomenon become more important than the phenomenon.* (Giorgi, 1970, p. 291, emphases added)

From our rejection of conventional assumptions associated with logical positivism, we derived three axioms, and from them a number of what we first called "derivative postures" (Guba & Lincoln, 1981) and later called "implications," because they were implied by accepting the axioms. They included qualitative rather than quantitative methods as the preferred (though not exclusive) techniques for data collection and analysis; relevance rather than rigor as the quality criterion; grounded rather than a priori theory; changes in the nature of the causal questions asked and thought possible to answer; expansion of the knowledge types utilizable from propositional to propositional *and tacit;* an expansionist rather than reductionist stance toward the inquiry; a presumption of the human inquirer as the major although not necessarily only form of instrumentation; an emergent rather than preordinate design strategy; a selection rather than intervention style as focus for the inquiry; a natural, in situ rather than laboratory context for the research; a variable rather than invariant "treatment" mode; patterns as opposed to variables as the analytic unit (Kaplan, 1964); and invited interference—an invitational and participatory mode—as opposed to control in the exercise of the research (Guba & Lincoln, 1981, p. 65).

But as soon as our first work went to press, we were disturbed by things that we had said, or taken for granted. Most particularly, two things that we had taken for granted began to trouble us and our

critics: First, we began to understand that traditional and conventional assumptions regarding causality were axiomatic statements themselves. Should causality not, therefore, also be a parallel axiom for us? We were, after all, playing the Lobachevskian geometry game: turning conventional axioms on their heads and trying to determine whether or not they made sense. From that moment on, we began to talk about a fourth axiom.

Second, the question of what role values played in inquiry also troubled us. Increasingly, we became attuned to a powerful assumption that we had earlier missed, to wit, that inquiry not only could but should be value free. We began to understand that "science" demanded that scientists stand outside of time and context, and, indeed, outside of themselves as persons, in order to deliver research results that stood apart from human values. The purpose of such a stance was clearly, at least for the social sciences, the rendering of judgments regarding appropriate social strategies for the solution of human ills. Only if research results were free of human values, and, therefore, free of bias, prejudice, or individual stakes, could social action be taken that was neutral with respect to political partisanship. But how could humans stand outside of themselves, even in the research process? We were beginning to understand, especially from the feminists, critical theorists, and neo-Marxists, that the research process itself was a political endeavor, with some groups and research models favored over others, with some definitions of problems more acceptable than others, with avenues to funding and support clearly discriminatory (Keller, 1985).

Given our concerns, we finally realized that two items that we had earlier believed to be merely postures, or implications, of the first three axioms were themselves axioms. When we understood this, we realized that there were five axioms, at least as we originally constructed the paradigm.

A Thorny Problem, a Turbulent Summer

At about that time, a challenge arose from a strange source. The editors of education journals were interested in seeing naturalistic case studies appear in print, but they were at a loss as to how to judge the rigor of those studies, and their reviewers were no better off. After his participation in a conference to deal with these issues, Egon took

up the express intellectual task of the development of a set of criteria for judging the process of naturalistic inquiries. The result of that self-imposed task was a set of criteria for judging whether or not any given inquiry was *methodologically* and *analytically* sound (Guba, 1981). The criteria, called "trustworthiness" criteria to distinguish them from the criteria of "rigor" that were applicable to the conventional paradigm, paralleled the standard criteria of internal validity, external validity, reliability, and objectivity but were framed in a very different manner. These parallel or trustworthiness criteria, criteria of credibility, transferability, dependability, and confirmability, could not establish quality with the confidence and assurance that the older rigor criteria did, Egon said, but were nevertheless useful. Somehow, over the years, we have continued to make modest claims for those criteria, never realizing that uncertainty, flux, and transformation, hallmarks of the paradigm itself, meant that certainty would never be possible and would always preclude the certitude and presumed rectitude of conventional rigor criteria. The two of us discussed this work often, but in retrospect—and this is a retrospective and, therefore, reconstructed logic—we were too modest.

We were also only half right.

What Egon had developed was, in fact, a set of criteria that, as our loyal critic John K. Smith pointed out, were parallel, or *foundational*, criteria. That is, they had their foundation in concerns indigenous to the conventional, or positivist, paradigm. If we did not have the conventional paradigm, would we not develop criteria indigenous to naturalism, to phenomenology, or to constructivism?

Of course, John was right. We asked ourselves, can you ever "forget" what has gone before you, what you knew—stand outside of your historical self? Of course not, but it might well be possible to *imagine* oneself outside of one's own history and at least try to think about the question. The answers took more than two years to develop, and we're not certain that they're finished even now. But Egon and I took two different tacks, finally, and managed to come up with some interesting answers. This is more or less what happened:

We first thought about what might come out of a naturalistic and responsive inquiry that would not, or should not, or could not evolve from a conventional one. These forms of knowing and action we called "authenticity criteria" to distinguish them from the methodological process criteria that we had designated as "trustworthiness"

criteria. They included "states of being," particularly for respondents, participants, and stakeholders, which were not expected (or warranted) in conventional inquiry, and one additional criterion, which recognized and attended to the need for such inquiries to express multiple, socially constructed, and often conflicting realities. The latter we termed *fairness*, and judgments were made on the achievement of this criterion in much the same way that labor negotiators and mediators determine fairness in bargaining sessions.

The "states of being" represented something much more subtle. They related both to (a) levels of understanding and sophistication and to (b) the enhanced ability of participants and stakeholders to take action during and after an inquiry and to negotiate on behalf of themselves and their own interests in the political arena. Those "states-of-being" criteria included *ontological authenticity*, or the heightened awareness of one's own constructions and assumptions, manifest and unspoken; *educative authenticity*, or the increased awareness and appreciation (although not necessarily the acceptance) of the constructions of other stakeholders; *catalytic authenticity*, a criterion that is judged by the prompt to action generated by inquiry efforts; and *tactical authenticity*, the ability to take action, to engage the political arena on behalf of oneself or one's referent stakeholder or participant group (Lincoln & Guba, 1986a).

In that intuitive way in which some intellectual tasks proceed, it was only later that we realized the powerful implications of designing and adopting new criteria for trustworthiness. In particular, the distinction between process and data struck us. For instance, the conventional inquirer's assertion that "the data speak for themselves" was erroneous. In conventional inquiry, actually, the *methods* attest to the strength of the conclusions. And in parallel fashion, in constructivist fashion, the data are what speak for themselves. For example, in evaluation (as one form of disciplined inquiry), data are confirmable, but the "test of the pudding" is in the enhanced sophistication of stakeholders and in the comprehension of avenues of action. Likewise, in research, data are likewise confirmable, by reference to original field notes, and the "test of the pudding" is increased understanding as a form of knowledge. In conventional inquiry, pure process leads to pure results. In constructivist inquiry, process is only one means of determining the utility, responsibility, and fidelity of the inquiry. Action and understanding were other components of the judgments regarding the goodness of any given inquiry.

We weren't finished yet, however.

Increasingly, we had come to understand—largely through our able, curious, and harrying students—that there were other judgments to be made about naturalistic or constructivist inquiries. Baldly put, could the methodological strategy be good, could the inquirer be an honest and faithful servant to the inquiry question and still turn out a *product* that fell short of the mark? The answer, of course, was yes. We needed criteria by which we might judge *products*—most typically, a case study rather than a conventional scientific, technical report. We began again, this time taking as our model the study of fiction as a narrative form, and the work of a student, Nancy Zeller (1987), who had training in this area and who sought to explore what judgments about fiction might tell us about compelling narrative.

Building on Zeller's work, and deriving our own criteria from judgments made about case studies which students prepared in various classes for us, we were able to propose a set of criteria which seemed to us appropriate for naturalistic or constructivist inquiries. These criteria were, like the authenticity criteria, *nonfoundational*, because they rested not on conventional inquiry's requirements for research reports but, instead, grew from the concerns of this particular paradigm. The constructivist paradigm, it should be recalled, had as its central focus not the abstraction (reduction) or the approximation (modeling) of a single reality but the presentation of multiple, holistic, competing, and often conflictual realities of multiple stakeholders and research participants (including the inquirer's). Further, in the presentation of those multiple realities (social constructions), a vicarious, déjà vu experience should be created in the reader. This vicarious experience, in addition to providing certain technical help to other researchers (e.g., in the presentation of thick description, which enables judgments regarding transferability to be made), should aid the reader in understanding the nuances and subtleties of conflict and agreement in *this place and at this time*. Further, the written report should demonstrate the passion, the commitment, and the involvement of the inquirer with his or her coparticipants in the inquiry.

Because those things needed to be apparent from the case study, we developed a set of criteria that were responsive to the paradigm itself (or, more precisely, to the *product* of the paradigm). *Axiomatic criteria* are those criteria that display resonance with constructivist (naturalistic) inquiry. *Rhetorical criteria* are those criteria relating to the "form and structure, or the presentational characteristics" of the written

document issuing from a naturalistic inquiry (Lincoln & Guba, 1988, p. 8), and include power and elegance, creativity, openness and problematic qualities, independence, the writer's emotional and intellectual commitment to the case itself, social courage, and egalitarianism.

Action criteria "mean the ability of the case study to evoke and facilitate action on the part of readers," or the "power of such an inquiry to enable those whom it affects directly or indirectly to take action on their circumstances or environments." This is essentially an *empowerment criterion* (Lincoln & Guba, 1988, p. 19). The *application or transferability criterion* refers to the "extent to which the case study facilitates the drawing of inferences by the reader that may have applicability in his or her own context or situation" (Lincoln & Guba, 1988, pp. 20-21).

Now, nobody—least of all me—would argue that the last word has been written on criteria for adequacy of case studies as reports or on trustworthiness or authenticity issues. But our critics and students had clearly pushed us far beyond where we—or I—ever expected to go.

The Middle Years: Experimentation and Excursions

My own observation has been that those careers that can be read as straight lines reflect a single-mindedness that is more akin to narrowness and parochiality than it is to great determination in purpose. Some of the more interesting academic lives I've observed tend to be those that are, in part, committed to explore a line of inquiry and, at the same time, are open to interesting side-street excursions. I'd like to think I'd been big on side streets, conceptually intriguing tangents, and occasionally swerving down "the road less traveled." Thus, in the middle of what might be termed systematic development of new-paradigm inquiry, I took some side roads. And because they tell the reader something about how "problems" occur to inquirers, they are worth some discussion here.

The first tangent occurred when Egon and I were team-teaching a class in program evaluation at the University of Kansas. During the course of one discussion, Egon asserted that, of course, the flowchart for naturalistic inquiry would be the same as that for conventional inquiry, save that the terms—the labels in the boxes—would be different. That assertion was challenged both by me and by the students

in the class, who were determined that such could not be the case. The group of students retired for two days to figure out how the "flow" of naturalistic inquiry might be pictorially represented to demonstrate its difference and distinction from conventional inquiry. What they "drew" shocked and stunned us into a major intellectual exploration of methodological, or strategic, differences between conventional and naturalistic inquiry, and we elaborated on it extensively. A graphics artist connected with the Center for Public Affairs at the university drew up a fine set of models for us, and we took it upon ourselves to work out the question of whether or not inquiry paradigms imply inquiry methodologies (by which we meant, overall design strategy).

The question was important because a number of our critics had been charging that *procedurally* naturalistic inquiry was *not* different from conventional inquiry and that the major difference between paradigms lay in the rather heavier reliance on qualitative methods demanded by naturalism. We argued, and I think successfully, that switching paradigms meant switching strategy in rather dramatic ways, and we provided the "models" to demonstrate how and in what ways (Guba & Lincoln, 1988a).

It might have been years before we tumbled to that problem without the insistence of my students. Sometimes, problems are presented fortuitously; the point is, you explore them when and where you find them, if you find them interesting.

A second side-street incident will show you what I mean. I'd read a number of classic works in program evaluation and, over the years, had begun to be troubled by the ongoing reference of evaluation experts to "evaluation research," to "policy analysis research," or, worse yet, to "policy analysis evaluation research." My hunch was that this language and terminology took hold because major avenues of funding were opening up in evaluation of social action programs and education, and researchers who went after such money were feeling pressure to justify such work as "research" on their own campuses. Evaluation work has never been as highly regarded as research work, especially with promotion and tenure committees, and those who undertook the former needed to connect their work directly to either basic or applied research. But the careless blending of such terms irritated me.

The more I thought about the problem, the more it occurred to me that there were different *categories* of what Cronbach and Suppes (1969, p. 16) had called "disciplined inquiry," inquiry that "has a texture that displays the raw materials entering the argument and the

logical processes by which they were compressed and rearranged to make the conclusion credible." Further, different forms of inquiry ought to lead to different end products, have different expected outcomes, address different audiences, and perhaps employ different strategies in arriving at outcomes.

It was not until I began to chart out differences between research, evaluation, and policy analysis that I realized someone should have argued much sooner that these three activities were actually different *forms* of disciplined inquiry. Hence it made no sense to refer to "evaluation research," save as research *on* evaluation methods or models. Likewise, it made no sense to talk of "policy analysis evaluation" or of "policy analysis research." Research, evaluation, and policy analysis were different inquiry processes, and sorting them out—an interesting intellectual and practical problem—was one of the more fascinating things I've done in the last several years (Lincoln & Guba, 1986b). The important thing about this work, other than its less-than-apparent centrality to new-paradigm research, is the way in which it occurred: as a nagging irritant, a "something" that was wrong but that resided, until I began to grapple with it explicitly, in the tacit domain.

A third side street will demonstrate another way in which problems occur to inquirers. Egon and I had been commissioned to put together an informal workshop with a highly talented group of special educators at the New England Regional Resource Center (NERRC). We gathered oceanside in Maine to discuss problems they were having in providing services to state departments of education throughout New England. During the course of the conversation, someone asked whether the ethical implications of naturalistic inquiry were the same as for positivist inquiry. I did my usual number when someone asks me a question to which I haven't a clue: I made it up as I went along. No, I said, the ethical implications of naturalistic inquiry went far beyond those of conventional positivist inquiry, which are by and large embodied in our federal laws on privacy, confidentiality, harm to research subjects, and informed consent. And I went on to suggest ways in which I thought contemporary federal law failed to take account of new-paradigm research. Fortunately for me, someone with a lap-top computer took all of this down. That provided me with notes to mull over and a chance to think about what I'd so rashly said.

After the workshop, Egon and I went to work on a proposal for the American Educational Research Association, the purpose of which

was to have a paper accepted that would force us to write on the area of ethical issues in constructivism. The result of acceptance was a paper that not only criticized current law on research ethics (aided and abetted by criticism from the positivist camp) but that also outlined special problems with ethics in naturalistic inquiry (Guba & Lincoln, 1989; Lincoln & Guba, 1987, 1989).

By this time, things were starting to be really fun. Our critics were less and less successful at ruining our days, and we were just beginning to understand that we'd hit on something very, very important, something that was part and parcel of a changing worldview in Western society—something that would change the face of research profoundly over the years. It would have applications throughout the academic disciplines and the formal structure of knowledge (Lincoln, 1989b) and had already altered the face of the hard sciences (Zukav, 1979). I felt profoundly the changes implicit in committing oneself to a radical critique of social science: the sense of being an outlaw, a conscientious objector, a civilly disobedient academic.

Clearly, I still didn't appreciate the extent of the problem.

The Rites of Passage

We began to reformulate the axioms. Rather than stating them as we had, initially, in five parts, we began to talk about the ontology, the epistemology, and the methodology of naturalistic, or constructivist, inquiry. In their new form, they went like this:

(1) The ontological axiom states that reality is a social, and, therefore, multiple, construction; that there is no single tangible, fragmentable reality on to which science can converge; that reality exists rather as a set of holistic and meaning-bounded constructions that are both intra- and interpersonally conflictual and dialectic in nature; that, whereas the positivist construction of reality is realist in orientation, the constructivist is relativist; that, whereas the aim of positivist science is to expose and articulate immutable natural laws (for both the social and the natural world), usually expressed as generalizations, and usually in the form of cause-and-effect relationships, the aim of constructivist science is to create idiographic knowledge, usually expressed in the form of pattern theories, or webs of mutual and plausible influence expressed as working hypotheses, or temporary, time- and place-bound knowledge.

Table 4.1 Contrasts Between the Postpositivist, Critical Theory, and Constructivist Paradigms

Question	Paradigm		
	Postpositivist	*Critical Theory*	*Constructivist*
Ontology	Realist	Realist	Relativist
Epistemology	Dualist, objectivist	Interactive, subjectivist	Interactive, subjectivist
Methodology	Interventionist	Participative	Hermeneutic, dialectic

(2) The epistemological position of constructivist inquiry dictates that the positivist subject-object dualism and objectivism be replaced by an interactive monism; that the interactivity between researcher and researched be recognized and utilized in the teaching and learning process between the two; and that the values that inhere in the research process—in the choice of a problem, the choice of an overall design strategy, the choice of the setting, and the decision to honor and present the values that inhere in the site(s)—be explicated and explored as a part of both initial and final research processes and products.

(3) Methodologically, constructivism demands that inquiry be moved out of the laboratory and into natural contexts, where organizational processes create naturally occurring experiments, dictates that methods designed to capture realities holistically, to discern meaning implicit in human activity, and to be congenial to the human-as-instrument be employed; that such methods are typically, although not exclusively, qualitative rather than quantitative; that designs for such inquiries can never be fully articulated until after the inquiry has been declared complete, because the design must emerge as salient issues emerge from research respondents and coparticipants; that theory must arise from the data rather than preceding them; and that the method must be hermeneutic and dialectic, focusing on the social processes of construction, reconstruction, and elaboration, and must be concerned with conflict as well as consensus.

These two paradigms—positivism (or postpositivism) and constructivism—along with critical theory can best be contrasted in the manner shown in Table 4.1.

My colleagues' chapters discuss the other two traditions more extensively, but I believe this table captures the major domains of difference between the competing paradigms on axiomatic or philosophical grounds.

The table has a number of meanings, all of which are important for the debate surrounding paradigm allegiance.

Implications, Paradigmatic and Personal

The interpretive phenomenon. First and foremost, it means an "interpretive turn" (Bloland, 1989), or what Bernstein (1983, p. 30) called "a recovery of the hermeneutical dimension, with its thematic emphasis on understanding and interpretation." Bernstein (1983, p. 31) notes:

> There is, however, a much stronger and much more consequential sense than Kuhn's notion of a "sensitive reading" in which the hermeneutical dimension of science has been recovered. In the critique of naive and even of sophisticated forms of logical positivism and empiricism; in the questioning of the claims of the primacy of the hypothetical-deductive model of explanation; in the questioning of the sharp dichotomy that has been made between observation and theory (or observation and theoretical language); in the insistence on the underdetermination of theory by fact; and in the exploration of the ways in which all description and observation are theory-impregnated, we find claims and arguments that are consonant with those that have been at the very heart hermeneutics, especially as the discipline has been discussed from the nineteenth century to the present.

The divorce of science from its contemporary raw empiricist base, and its realliance with judgment, discernment, understanding, and interpretation as necessary elements of the scientific process, has been slowly formalized over the twentieth century. Bernstein calls this "the shift from a model of rationality that searches for determinate rules which can serve as necessary and sufficient conditions, to a model of practical rationality that emphasizes the role of *exemplars* and judgmental interpretation" (Bernstein, 1983, p. 57, emphasis in original). The significance of this shift is that it presupposes a reliance on tacit as well as propositional knowledge (a major implication of constructivist inquiry) and acknowledges, with feminist critics of science and philosophers, that *"the teaching of method is nothing other than the*

teaching of a certain kind of history" (MacIntyre, cited in Bernstein, 1983, p. 57, emphasis in original).

Thus science, in returning to the hermeneutical tradition, openly acknowledges its own social construction, its roots as a historically derived and practiced process, not devoid of values but firmly committed to the legitimacy and authority of ruling scientific interests.

The interpretive turn in itself has implications for what we understand and know about the world. The (false) certitude of logical positivism, its quiet determinism, are being replaced by less certain forms of knowing and, therefore, more attendant anxiety about knowledge (Bloland, 1989). The "persistent claim that it is science and science alone that is the measure of reality, knowledge and truth" (Bernstein, 1983, p. 46) has been replaced by the claim that reality is socially constructed (Berger & Luckmann, 1973; Harding & Hintikka, 1983), that knowledge is problematic and contested (Lather, 1988a), and that truth is locally and politically situated (Popkewitz, 1984). The implications of this relativity of knowledge are sufficiently unnerving to provoke even inquirers persuaded to constructivism to ask whether we can't have both—ideographic knowledge and generalizable knowledge—much as British chemists asked Lavoisier, the discoverer of oxygen, how he accounted for phlogiston, the mystical element that oxygen replaced (McCann, 1978).

Giving up certitude has been far more difficult than giving up other aspects of the paradigm. Two other aspects, the switch from rigor to relevance and the adoption of qualitative methods, have proceeded much more rapidly and thoroughly than anyone could have guessed. But empiricists cannot part with that need for definitive, concrete, orderly, and certain knowledge, knowledge of a sort that constructivists believe is impossible to achieve—more about knowledge later.

Paradigm pervasiveness. For me, second, the paradigm shift has meant that a quotation that I used years ago is truer than I ever knew. I cited Michael Patton (1975, p. 9) as having said that a paradigm is

> a world view, a general perspective, a way of breaking down the complexity of the real world [sic] . . . paradigms are deeply embedded in the socialization of adherents and practitioners telling them what is important, what is legitimate, what is reasonable. Paradigms are normative; they tell the practitioner what to do without the necessity of long existential or epistemological considerations.

At the time, I failed to realize just how pervasive, how ineluctable, paradigms really were. It was not until challenges began to come in from the field—challenges on criteria, on more criteria, on ethics, on values—that I realized that laying out the ontological, epistemological, and methodological boundaries was just the easy beginning—there's more.

The adoption of a paradigm literally permeates every act even tangentially associated with inquiry, such that any consideration even remotely attached to inquiry processes demands rethinking to bring decisions into line with the worldview embodied in the paradigm itself.

The immediate realization is that accommodation between paradigms is impossible. The rules for action, for process, for discourse, for what is considered knowledge and truth, are so vastly different that, although procedurally we may appear to be undertaking the same search, in fact, we are led to vastly diverse, disparate, distinctive, and typically antithetical ends.

Although accommodation may be possible in terms of what we will allow to be published and disseminated, accommodation between and among paradigms on axiomatic grounds is simply not possible. The socialization processes associated with each paradigm are sufficiently divergent, and the emotional and political commitments so high, that a mix-and-match strategy, at either the axiomatic or the practical level, is likely to produce little more than internal dissonance in the research process, a form of discursive incoherence that renders the findings useless for both camps.

The thoroughly *universal* nature of any paradigm eventually forces the choice between one view or the other. The intrapsychic need for coherence, order, and logic demands that an individual behave in ways that are as congruent and as nonconflicting as possible. Paradigms are ubiquitous entities, permeating and dictating choices even when we are unconscious of their influence in that process. Thus we have to make a commitment as inquirers to one or the other and behave in a fashion congruent with its dictates until we choose another system. To do otherwise is not only to commit paradigmatic perjury, it is to invite psychological disaster.

Subtheoretical implications. There are other implications just beginning to be explored. Those are what I shall call, using the term *theory* loosely, *subtheoretical implications*. By this, I mean whole arenas of

inquiry that are affected by paradigm choice. The arenas of which I speak form inquiry lines for philosophers and historians of science, and no discussion here could do them justice. But it turns out, as I have discovered to my horror, that each arena is profoundly affected by paradigm, or worldview, or choice, such that rethinking one's paradigm commitment means giving time to thought about these things also. They include values; ethics; knowledge accumulation, or models of knowing; scientific discourse; and training issues (i.e., how do we socialize prospective adherents to a paradigm, particularly one that is not the dominant paradigm?).

Questions regarding these arenas will likely consume my maturity as a researcher, and so I shall cover what little I know about them in order to provide some sense of the ways in which they affect inquiry, legitimacy, and hegemony:

Values. It is now becoming quite clear that inquiry does not have to be openly ideological (Lather, 1988a) in order to be value bound. In fact, some would argue (Beardsley, 1980) that inquiry that purports to be value free is probably the most insidious form of inquiry available, because its inherent but unexamined values influence policy without ever being scrutinized themselves. Increasingly, however, even traditional and conventional scientists are calling for an examination of the values that undergird inquiry (Bahm, 1971; Baumrind, 1979). Other more nonconventional scientists—feminists (Bleier, 1986; Keller, 1985), critical theorists (Popkewitz, 1984), and others (Reason & Rowan, 1981)—have called attention to the role that values, under multiple guises and in varied forms, play in inquiry. It seems clear, given criticism from all quarters, that only the most intransigent or the most naive scientist still clings to the idea that inquiry can, or should, be value free. The tidal wave of criticism of this concept (Bernstein, 1983) places it squarely into the *history* of science, not in its contemporary formulations.

Ethics. To admit that values play a role in inquiry, to abjure the objectivity criterion, is to call into question the entire system of ethics that governs inquiry and researcher-researched relations. In the process, it becomes clear that current regulations, standards, and laws that govern the research enterprise are helpful but wholly inadequate (Lincoln & Guba, 1989). Laws that address informed consent, protection of human subjects, privacy and confidentiality, and the use of deception, particularly, were developed in support of the dominant

paradigm. They rest on assumptions that undergird that paradigm and, therefore, ill serve emergent-paradigm inquiry.

No paradigm is without ethical problems, but the problems that plague constructivism are radically different from those that engage the attention of conventional postpositivist researchers. The emphasis on face-to-face interaction, on faithfully representing multiple, constructed, and often conflicting realities, and on maintaining privacy and anonymity while utilizing extensive word-for-word, natural language quotations in case studies as well as the case studies in general are all problems typically faced by the emergent-paradigm inquirer (Guba & Lincoln, 1989; Lincoln & Guba, 1989).

In addition, questions of *process* become singularly critical in new-paradigm inquiry. By questions of process, I mean questions that direct our attention to just how we behave, both as inquirers and toward our respondents and coparticipants in the inquiry process. Heron (1981) makes the argument exceptionally well. He contends that, if we see ourselves (as scientists) as independent humans who exercise rights and control over our own lives with direction, dignity, freedom, and agency, do we have the right to treat others in a lesser manner? But the granting of rights of dignity, agency, freedom, and independence to our respondents creates a situation where our own, often specialized, knowledge is nevertheless *only one form of knowledge that is available*. Our education puts us in a privileged position with respect to formal knowledge, but it does not grant us rights beyond those that are granted to all free human beings. Thus our demeanor both toward our work and toward those who provide us the means to conduct our work—our respondents—must undergo profound alteration. The *invitational* aspects of this form of inquiry are often considered entirely too ideological to have a place within mainstream science. It's better, such critics would say, to leave such inquiry to liberation theologists, Freierian critical theorists, and neo-Marxists. In fact, however, what we have is not a carbon copy of nonmainstream, or "ideological," social science but a mainstream rethinking of the role the social sciences play in everyday, ordinary life (Baumrind, 1985) and the legitimate roles our respondents should be playing in framing the agendas for social research (Lather, 1988a). Criticism of researcher roles vis-à-vis respondents comes from traditional science as well as from emergent inquiry almost equally often.

We have not yet begun to think through an entire ethical system that supports constructivist inquiry. But its political implications are being felt in many places. Soon gone, it is to be hoped, are the days when a well-known researcher can stand in front of an audience at a major professional association and assert that determining facts is best left to scientists and not to research "subjects," who "don't know a fact from a bag of popcorn" (Boruch, 1986). When the "stuff" of science is constructions of reality, rather than "facts" determined by scientists, we will have moved to a social science in which respondents have as strong a voice as the priesthood of science.

Knowledge accumulation and models of knowledge. The question often is directed either to me or to Egon: "Well, if all we have is social constructions of reality, then how do we do what science demands that we do, and accumulate knowledge about our natural or social world?" I think the answer to that question is one that I keep giving but about which I know less than I should (although, please notice, I don't think anyone knows any more about it than I do).

Conventionally, we have operated on an accumulation, or aggregationist, model of knowledge: knowledge as hierarchy, taxonomy, or pyramid. Knowledge is conceived as a series of building blocks, and we are trying to construct a Tower of Babel, which, when done, will lead us to heaven. But this pyramid model of knowledge is simply another construction, and perhaps not the most serviceable one at this period of time. It is quite possible that knowledge is more *circular* or *amoebalike*, or that knowledge exists in *clumps* of understanding, with different kinds of knowledge taking different shapes. We desperately need new models of knowledge and knowledge accumulation.

We simply do not have the metaphors we need yet for conceiving of knowledge in any other way but hierarchic, pyramidal, or taxonomic. But we could use images that enlarge and enrich our understanding of how we know and how we organize what we know. There is no doubt that some of our knowledge may effectively be organized in the way in which conventional science directs, but it is equally clear that other forms of knowledge may be organized and stored in very different patterns. And we do not have a language for talking about those patterns yet.

It may be the case that, if some forms of knowledge exist in clumps, or in nonhierarchic organization, we ought to be talking not about "building blocks of science" but about extended sophistication, or the

artistic and expressive process of creatively conjoining elements in ways that are fresh and new. We ought to think of bridging, as a means of linking two bodies of knowledge or understanding, or of synthesizing, as a way of combining hitherto uncombined elements, or of some other linkage processes. As I said, we have no models for scientific knowledge that account for nonhierarchic learning, and we may have to borrow from the poet, the artist, the madman, the mystic.

Discourse. Slowly but surely, it has dawned on me—as it has dawned on others—that the discourse of science supports and reinforces a way of looking at the world that is antithetical to naturalistic or constructivist inquiry. It is also, parenthetically, destructive of human dignity and agency. The language of science, described by Firestone (1987) as a "stripped-down, cooled-out," value-neutral form of discourse, is itself a model of detachment and presumed objectivity. It separates the knower from the known and places science squarely in the domain of distanced disinterestedness. Its very remoteness and passive voice place a barrier between researcher and researched that strategies for ensuring validity could not achieve alone. Popkewitz noted this in the preface to his *Paradigm and Ideology in Educational Research* (1984, pp. vii-viii) when he observed that one

> social dimension of research . . . is the social and cultural location of our research activities. We can think of social science as *dialects of language which provide heuristic fictions for supposing the world is this way or that way.* These fictions or theories are *made to seem neutral by the conventions of science which decontextualizes language and makes knowledge seem transcendent.* (emphases added)

Popkewitz goes on to observe what linguists and anthropologists have known for some time, but what we have ignored in studies of science (particularly social science) as a historical creation: that "to adopt a language for structuring existence is to give organization to the ways in which the existence is to be changed. . . . The languages of science contain thought, ideas, and values, as well as 'mere' descriptions" (Popkewitz, 1984, pp. 52-53).

To play the same Lobachevskian game with discourse that we played with the earlier axioms of naturalistic inquiry (Lincoln & Guba, 1985), we can turn the assumptions of discourse upside down trying to understand what a reversal of rules might mean. For

instance, leaving behind a language that reflects an intended subject-object dualism, we could search for a language that displays *connectedness*. Leaving behind a (meaningless) objectivity, we could aim for a language that reflects *intense interaction* and *interactivity*. Rather than an uncontested language of "fact," we could begin using a language and linguistic forms that reflect the *dialectical and problematic nature of human existence*, a language that shows power, persuasion, arenas of bias, values, conflict, construction, and reconstruction. We could try to avoid the distancing of conventional science by adopting a language that demonstrates *emotional and social commitment* on the part of the inquirer. We could find a form for our work that avoids the dispassionate tone of traditional, conventional science in favor of the language of *energy and passion*. We could, in short, abandon the role of dispassionate observer in favor of the role of passionate participant.

The tone of our inquiries will change radically. Nor should we be, as I have been, ashamed to be called "passionate" or "polemic" or "argumentative." All of those labels, I now understand, reflect the increasing involvement and passion I find in my work. They should reflect the involvement with and commitment to inquiry experienced by other constructivists. We have deluded ourselves that the discourse of constructivism could resemble the discourse of other science, and I and others were wrong. To array the arguments of emergent-paradigm science in the raiments of conventional science is to do new-paradigm inquiry an injustice. We cannot just change the forms and interactions; we have to alter the way in which we discuss those new forms and relationships. The discourse of constructivist inquiry must be recontextualized in such a way as to make it apparent that science and knowledge are not transcendent but, instead, another set of "heuristic fictions" for meaning-making in our world.

The language of the "rape model" of research (Reinharz, 1978), or of force and violence (Easlea, 1986), needs to be replaced with the language of trust, sharing, cooperation, teaching, and learning—a "lover model" of research (Reinharz, 1978) or the "neighborly" concept of community (Savage, 1988). The *moral dimensions* of social research enterprises are of necessity brought to the fore in this language.

To paraphrase a contemporary television ad, "This is not your father's scientific discourse!" But we do need to know more about it. And we haven't begun to think about such a language or what we might agree it should look like.

Training. I have often told questioners that research training programs should be two-tracked, with training in conventional and emergent-paradigm inquiry models, followed by training in quantitative and qualitative methods both, completed with computer applications for both quantitative and qualitative data.

But with what I have intuitively come to understand about the pervasiveness of the paradigm we use to conduct inquiry, I now think that training in multiple paradigms (at least in more than a historical sense) is training for schizophrenia. If we want to change new researchers' paradigms, we must do more than legitimate those paradigms in the inquiry outlets, such as journals. We have to train people in them, intensively. We probably ought not to be dividing their attention with other than historical accounts of conventional science. We probably ought to recognize the profound commitments people make to worldviews and create centers where such training can go on, much as there are centers where psychologists can train to be Freudians, or Jungians, or Adlerians, or places to train conventional dentists, or Crozat dentists, and the like. Dual training, in retrospect, only diminishes the attention that is focused on the intent of inquiry. I once offered such a "parallel" training program model to the critical conventionalists in my audiences. I wouldn't do so now.

A Retrospective

So where does that leave us now? More specifically, where does that leave me now? Feeling a bit foolish, I suppose, because I thought 1985 and *Naturalistic Inquiry* would do it for positivism, naturalism, and inquiry in general and for good. Clearly, there are areas that have not even occurred to me or to us yet, and much systematic work and thinking has yet to be done.

It looks as though both middle and old age will be spent exploring the questions raised in my mind and the paradigm's early adulthood.

Note

1. With all due respect to Proust, whose madeleines provided such a flood of memories.

[5]

The Meaning of Alternative Paradigms for Practice

ELLIOT W. EISNER

Although it's nice for academics to chew on epistemological questions and to debate normative and methodological issues, the aim of scholarship in education is not disinterested knowledge—even if there were such a creature—but the improvement of schooling. It is appropriate, therefore, to ask about the practical ramifications of the new models of mind, method, and knowledge we are so fond of discussing. I intend to do just that. I will address four areas in which the new paradigms can have significant implications: First, I want to discuss the conceptual implications of alternative paradigms; second, their implications for practice; third, their implications for policy; and, fourth, their normative implications.

I know, as you do, that no set of categories, dimensions, aspects, or features of a world as interactive as schooling can be neatly separated into the areas I have just enumerated. But you also know, as I do, that analysis requires separation, even if the parts are part fiction. Consider, therefore, the analysis that I am going to provide as analysis—a way of highlighting different aspects of a complex whole. I address each aspect separately because language itself is a diachronic, not a synchronic, medium. I bracket in order to illuminate and write in parts because I write rather than paint.

Conceptual Implications

Let's consider first the conceptual implications of alternative paradigms. By *alternative paradigms*, I refer to those ideational structures

that portray humans as beings who generate different forms through which they hope to understand and represent the world they inhabit and who believe that the different forms they use to understand and represent that world should be appraised by criteria appropriate to the form. Further, these paradigms hold that "truth" is ultimately a kind of mirage that in principle cannot be achieved because the worlds we know are those crafted by us and because we cannot uncouple mind and matter to know the world as it "really" is (Goodman, 1978). By *alternative paradigms*, I refer to views of mind and knowledge that reject the idea that there is only one single epistemology and that there is an epistemological supreme court that can be appealed to to settle all issues concerning Truth.

One set of conceptual implications of alternative paradigms is a broader view of knowledge, a cultural view of mind (Cole, 1985), a multiple view of intelligence (Gardner, 1983), and a constructive view of cognition. Let's consider each in its turn.

By definition, the introduction of alternative paradigms for inquiry undermines the tacit but widely held belief that there is only one dependable way to know, something vaguely called "the scientific method." Acquiring a critical consciousness of method or knowledge is unlikely when a particular paradigm is so dominant that it has no competitors. What is pervasive often goes unexamined. When alternatives are suppressed or unavailable, we tend to accept what is accepted. When this occurs, we are in a poor position to know what we have. From this perspective, the emergence of alternative paradigms provides platforms from which to examine unexamined assumptions; in effect, their presence forces us to present our position, to defend it, and, therefore, to understand it better.

If this were the only contribution that alternative paradigms made to our conceptual life, it would be of profound importance. Professionally socialized doctoral students in schools of education are often unable to question the premises upon which accepted research methods rest. We usually do not encourage them to consider alternatives—or haven't until quite recently. The reasons for this neglect are many, but things are changing. The growing interest in alternative paradigms makes problematic the belief that one epistemology fits all or that nonscientific modes of inquiry are permissible only as reconnaissance efforts; if you "really" want to know, you need to conduct an experiment.[1] One conceptual consequence of alternative paradigms in education is their salutary effect on the research community. We are now less parochial than we once were.

The emergence of a broader and pluralistic view of knowledge can also contribute to a less dogmatic view in our schools of what it means "to know." Cognitive pluralism makes it more likely that students will understand that propositions, a necessary feature of scientific inquiry, are not by any means the only forms through which we come to understand the world. Poetry and literature, for example, were invented to say what words can never say and, through what they say, we can come to understand what we cannot state. Science, Dewey reminds us, states meaning (Dewey, 1934). Art expresses it. The meanings we are able to construct are influenced by the cultural tools we know how to use and the materials upon which we act. With the emergence of the new paradigms, "coming to know" in the school curriculum can take on a wider meaning. In the process, we are more likely to recognize the epistemic functions of fields we now dismiss as essentially "affective."[2]

Another conceptual consequence of alternative paradigms is a warming toward the idea that mind itself is a cultural achievement (Cole, 1985; Eisner, 1982). Everyone knows what a culture is—it is a place for growing things, and schools are places for growing minds (Cole, 1985). The curricula we offer and the teaching methods we employ are means for creating minds. It is in this sense that the curriculum is a mind-altering device (Bernstein, 1971) and the school a culture for growing minds. As this conception of mind takes root in our conceptual life, it creates an optimism for education for it emphasizes the possibilities of schooling, its capacity to make a difference in the kind of minds that students can come to own. The kind of culture we create in schools, the forms of thinking we cultivate, the forms of representation we make available (Eisner, 1982), the recognition of the relationship between what we give students an opportunity to learn and the content of their experience is intimately related to a conception of inquiry that regards humans as creators of knowledge and makers of mind. Given this conception, we are more likely to cease seeking a fixed, measurable mental entity given at birth and seek instead to do what we can to grow minds as best we can.

Because alternative paradigms engender a pluralistic conception of knowledge, they share a family resemblance to what Gardner (1983) calls multiple intelligences. Intelligence is often conceptualized as something largely uninfluenced by culture, something biologically given. After all, what self-respecting psychometrician would choose to spend his or her time measuring what is fugitive or fleeting? The

real task is to get at what is basic, what is enduring, what is fixed (Jensen, 1969).

But when one entertains the notions of multiple ways of knowing and a cultural view of mind, it is not difficult to entertain the idea that intelligence itself is not one but many, that people cope with important problems in ways that depend on the kind of problem the problem is. Intelligence is, in this view, related to different kinds of action, which in turn is related to the kind of problem, task, or material one acts upon.

In addition to the implications that alternative paradigms have for our view of knowledge, mind, and intelligence, alternative paradigms also influence our conception of cognition. *Cognition*, a term that refers to the process through which the organism becomes aware (Statt, 1981), has often been identified with linguistically mediated thought. To *cognize* is, for many, to think in language. Indeed, some writers believe no other form of conceptual thought is possible (Schaff, 1973). Thinking and the use of language, for them, are synonymous. As our views of knowledge expand and our conceptions of the varieties of intelligence grow, it becomes increasingly difficult to restrict cognition to linguistically mediated thought. Thinking and knowing are mediated by any kind of experiential content the senses generate. Language, severed from semantics, is without meaning, and although images do not accompany every thought carried by language, our language refers to referents we are able to experience, recall, or imagine. Whether we are talking about unicorns, quarks, infinity, or apples, our cognitive life depends upon experience (Eisner, 1988). Cognition is wider than words, and the forms through which our cognition is given public status are as diverse as the social forms of representation we use in culture to convey meaning. As Polanyi (1962) put it, "We know more than we can tell." Again, once we seek a pluralistic universe, we find differences we previously did not cognize; that is, we *re*-cognize the world and that world includes cognition itself.

Finally, with respect to the conceptual implications of alternative paradigms, I want to reiterate what I only touched upon earlier; the newfound appreciation of the epistemic functions of the subject matters of schooling. I refer here to our growing understanding that the forms of representation used in fields like art, poetry, literature, film, theater, and history, as well as those used in the natural and social sciences, were invented to convey meaning that would not take the impress of forms other than those employed: We are able to exemplify

in art, for example, what words cannot articulate, and we are able to describe in words what we cannot exemplify (Goodman, 1978). We are able to convey through analogy, prosody, innuendo, and metaphor what escapes the precision of literal language (Langer, 1957). Forms of representation are functionally unique resources. The newfound appreciation of their contributions to cognition have potentially profound implications for curriculum, that mind-altering device I described earlier.

Implications for Practice

Given the conceptual shifts I have described, what might be their implications for the second of the four areas I wish to address, their implications for educational practice? One is that there is likely to be greater parity across the fields students study. By *parity*, I mean that literature, the visual arts, music, history, theater, and dance, as well as mathematics and science, would be recognized as cognitive in character, requiring intelligence and providing insight, understanding, and experience worth having. At present, this is clearly not the case, Some fields—the arts, for example—are marginalized in education. Some fields are regarded as "more cognitive" than others. Some fields are acknowledged by college admissions committees who count the grades secured by students in these areas when calculating GPA.[3] Other fields, such as the arts, regardless of the brilliance with which a student works in them, are simply discounted. As our epistemologies widen, the potential for rescuing curriculum from a hierarchy that reflects a more or less Platonic conception of knowledge and cognition increases. In short, the privileged place of a limited array of fields of study in our schools would give way to a more ecumenical and broadly arrayed set of curricular options.

Evidence of such a shift in curriculum would be displayed in that most telling indicator of our educational priorities—the way we allocate time to what we teach. Time allocation would reflect both a parity among fields and what Gardner (1983) calls individual proclivities. There would be less effort to put all children through the narrow eye of the same needle. Its details would also manifest themselves in our notion of what constitutes a core set of studies for all—what I refer to as a kind of *culturally referenced* curriculum balance and what individual students could elect without penalty, a kind of *personally*

referenced curriculum balance. The general point here is that changes that take deep root in our conception of mind, knowledge, and intelligence can have very significant practical implications for what we teach.

What is taught is only one aspect of the practical consequences of new paradigms for education. How we organize what we teach is another.

School curricula, particularly at the middle and high school levels, are organized into what Basil Bernstein (1971) calls a "classification code." Subject fields have strongly bounded contours and are insular and essentially independent of each other. In addition, these subject fields are taught mainly through text and other propositional forms. As our understanding and appreciation for multiple ways of knowing grows, there is greater likelihood that a more synthetic, integrated curriculum will be developed. Within a curricular form that relates field to field, the use of multiple forms of representation is more likely. To illustrate the point, consider how a unit on slavery prior to the Civil War might be taught (Epstein, 1989).

Students could, as they do now, rely mainly on textbooks to learn about the past. Yet, given the assumptions in the new paradigms, the textbook would be replaced or at least enhanced with films like *Roots*, with the music of the slaves, with the reading of their stories, their "folksay," with the food they ate; in short, students would encounter a wide range of curricular resources that serve epistemic ends to help them understand the life and times of the slaves prior to the Civil War. Hopefully, what becomes recognized in research circles will get reflected in curricular practice. The literal text is only one means through which the lives of others can be understood. Indeed, the novel may be a more powerful vehicle for transporting adolescents to Alabama, Mississippi, and Kentucky in the 1850s than a textbook rendition of the facts of the period.

Another implication of alternative paradigms for educational practice pertains to educational research and evaluation. We are already seeing in the field several vivid practical consequences of the appearance of the new kid on the block. We are debating issues and exploring methods that did not show a glimmer 20 years ago. The sacred cow has become a bit more profane. There is greater tolerance, even affection in some circles, for new ways to study educational practice and to assess its outcomes. We have new journals devoted to alternative paradigms,[4] more articles are appearing in learned journals that

push deliberation a bit further, and conferences like the one on which this book is based and like the conference "Qualitative Inquiry in Education" that was held at Stanford in June 1988, are providing further legitimation. We have a qualitative special interest group in AERA that is not only alive and well but growing.[5]

But beyond these concrete practical manifestations of the new paradigms, we are inventing new ways to conduct research and creating new forms and methods with which to do educational evaluation. Whether it's Lincoln and Guba's (1985) naturalistic inquiry, Parlett and Hamilton's (1977) illuminative evaluation, Stake's (1975) responsive evaluation, or my own (Eisner, 1985) educational connoisseurship and criticism, efforts are being made to weave a finer and wider net through which the processes and outcomes of educational practice can be understood and appraised. To be sure, we do not have the technical logic that has been developed for conventional approaches to research and evaluation. Furthermore, I do not believe we will ever create the kind of algorithms that are useful in treating quantitatively rendered data, but we have learned that there is more than one way to parse reality, and, with more refined approaches for describing, interpreting, and appraising the educational worlds we care about, greater confidence in methods that elude the security of rule will, I believe, develop. As the new paradigms really take hold, it will be increasingly recognized that Aristotle was correct when he said in his *Ethics*:

> Our discussion will be adequate if it has as much clearness as the subject-matter admits of, for precision is not to be sought for alike in all discussions, any more than in all the products of the crafts . . . for it is the mark of an educated man to look for precision in each class of things just so far as the nature of the subject admits; it is evidently equally foolish to accept probable reasoning from a mathematician and to demand from a rhetorician scientific proofs. (McKeon, 1941, p. 936)

Although we can argue with Aristotle about the meaning of precision—metaphor, for some things, may be more precise than measurement—the point of his statement is surely on the mark. Different forms require different expectations: Aristotle's biological interests served him well in matters epistemological.

It is difficult to imagine a more potent lever for changing the priorities of schools than the evaluative methods we employ. What

we count counts. What we measure matters. What we test, we teach. After all, adaptation is a primary form of survival, and our appetite for assessment requires forms of adaptation in teaching that make survival possible. That is what it means to be accountable. Teachers and school administrators are expected to provide an account in forms that, for many, miss what they care about the most. The promise of new paradigms resides in their potential to provide methods and approaches that are both more equitable and closer to the values practitioners cherish.

In my experience, very few teachers value the tests they are required to administer to their students. They resent being held accountable through methods that they believe neglect what they feel is most important about their own teaching. With the new epistemology and new methods, we may be able to affect schools through assessment procedures that are more congruent with the educational values that I believe most teachers embrace. Should such consequences occur, it would be no small victory.

Thus far I have addressed two potential implications of the new paradigms for practice. One of these was their conceptual implications—the way they shift our way of thinking about knowledge, mind, intelligence, and cognition. The other was their implications for practice itself. Here I spoke of their consequences for curriculum content, for curricular form, and for the way in which we evaluate practice and we conduct research. Now I want to move up a notch and focus on the implications of the new paradigms for educational policy and then for our educational norms, that is, for what we hope to achieve in our schools.

Implications for Policy

Policy is a set of ideas reflecting certain values and beliefs that are created to guide decision making. The policies we form about education in general and about its components such as teaching and evaluation both constrain and stimulate practice. They constrain practice because policies legitimatize particular directions and values. They stimulate for the same reasons they constrain: Policies tell the educational world the direction decisions should take. For example, a school district policy that expects teachers to prepare a lesson plan each day so that the principal of a school can inspect it tells teachers something

about how teaching is viewed and the importance of intentionality in their work. Policy that requires teachers to specify in behavioral terms the objectives toward which they aim articulates further what "the district" believes competent teachers do. Policy that requires that all teachers be evaluated once a year with an "objective" observation schedule by three appraisers independently observing a single 45-minute lesson conveys to those evaluated a tacit, if not explicit, epistemology and its application to the teaching process. Policy that publishes in local newspapers the achievement test scores of students on standardized tests on a school-by-grade basis reveals a set of values about what really counts in that school district and inevitably influences what teachers are likely to attend to in their classrooms.

The examples are endless and I do not want to perseverate. The point is that educational policy is shaped by beliefs about the kind of knowledge one can trust and the kinds of methods one can use to get such knowledge.

In contrast, consider the ways in which the new paradigms might influence how we think about policy pertaining to teaching. One potential consequence of the new paradigms is the way they encourage us to consider the sources of action. In conventional paradigms, action is idealized as a premeditated, goal-directed, cybernetically driven system. To act rationally, you have to have specific goals; the goals, in turn, determine the means you are to employ; the means you employ are then to be evaluated by their effects to determine the congruence between prespecified goals and the behavior of students. If the fit is not good enough, a new cycle is implemented.

What we have here is a recursive system, a means-ends model of rational behavior. Indeed, we have a very tidy world. As new paradigms have emerged in our educational discourse, our understanding of the sources of action in teaching has become less tidy and the role of intuition and qualitative thinking more salient. Body knowledge, as Johnson (1987) puts it, or reflectivity, as Schoen (1983) describes it, or craft, as Tom (1984) regards it, or artistry, as I think about it (Eisner, 1983), have become a part of the way people think about teaching. The industrial model born of Taylorism (Callahan, 1962) and implemented in new garb in the 1970s and 1980s has become less attractive. There is a greater tendency these days to talk about reflective practitioners and clinical supervision (Sergiovanni, 1983) and collaborative teaching. Although there is still plenty of appetite in the 108,000 American

schools for formulaic approaches to the teaching act—the six steps to successful teaching—the picture today is more approximately complex than it was a few years ago.

The new paradigms have altered our conception of the sources of action, and we are recognizing that goals cannot always be specified; some are even difficult to articulate. We are recognizing that intuition is not some mystical process emanating from some Muse but the immediate grasp of field forces, of being able to read immediately the structure of the field in which one acts (Arnheim, 1985), a feat wonderfully performed by a Larry Bird, or an Isaiah Thomas, and that the teacher's *sense* of what is needed, what is right here and now, are critical aspects of skilled teaching. In short, new paradigms that acknowledge the several ways in which humans think and know have loosened the corset that a narrow conception of human rationality imposes upon our conception of competent teaching.

The new paradigms make it more difficult to entertain the desiderata of teacher-proof curricula, or the use of a check-off observation schedule for evaluating teaching, or a Betty Crocker recipe for advancing teaching effectiveness. The new paradigms, I believe, contribute to more generous and more realistic educational policy affecting how teachers are to function. The longer-term consequences of such a policy are yet to come, but one place they might emerge is in the teacher's role in educational research.

The conventional role for research in education is built upon a paradigm that assigns to the specialist the job of studying teaching and learning in order to identify variables that have predictable effects on students. Once these variables are identified, the results of the research are published in journals and shared through in-service programs for teachers. The idea is that, once teachers learn about these studies, they will act upon their results in their own classrooms, that is, they will use what has been discovered by university researchers to do "what works" in their schools (U.S. Department of Education, 1986).

This model is itself modeled after research in agriculture. The agronomist and the botanist do the basic research, the agricultural-extension agent carries the findings to the farmers, the farmers implement what the extension agent has provided, and, seven months later, a larger crop is harvested, all thanks to basic research. I know that this description is something of a caricature, but I also know that in its

essentials it is the way we have proceeded. The *t* test, invented by William Gosset, was first used to determine the effect of fertilizer on the growth of corn.

What this paradigm has meant for research policy is a top-down orientation: Researchers create knowledge and pass it down to teachers. The knowledge that is transmitted is propositional and statistical in form. From such material the teacher—at least the really professional teacher—is to do things differently, and better.

The new paradigms advance another view. Although there is a place for conventional approaches, there is a difference between the kind of knowledge a teacher needs in a particular context and the abstracted generalizations found in learned journals or provided by in-service programs for teachers.[6] Teachers, some such as John Elliott (1986) in England and Mike Atkin (1989) in the United States argue, need themselves to conduct research. It's called action research. It's important that they do so, they argue, because the kind of knowledge secured by those on the inside, working in local contexts and needing to act upon what they know, differs in fundamental ways from the kind that will get an assent from three referees reviewing a manuscript submitted for publication to a learned journal. Research, given the new paradigms, is not likely to be the sole preserve of the university academic. At the very least, it will be a collaborative effort in which professors and teachers have parity.

Furthermore, what research yields is not to be regarded as dependable prescriptions for action but as analogues to increase the quality of teachers' deliberations. As Cronbach (1975) put it, it's to help practitioners use their heads. This is a significantly different view of the use of research. No longer are researchers in the business of sending to the social world information about cause-and-effect relations that ultimately direct action, instead, they provide ideas that can be creatively shaped by teachers in their own situations. This aspiration is at once more modest and more complex. It is more modest because it relieves researchers from the burden of finding the Holy Grail. It is more complex because it recognizes the need for creative rationality in teaching. It expands teaching rather than reducing it to rule. In the process, it confers professional status to the teacher. Behavioral prescriptions might work for bank tellers and airline attendants but they cannot work for teachers in schools concerned with education.

Alternative Paradigms and Educational Aims

I turn now to the fourth and final consideration on my agenda: the implications of the new paradigms for what we hope our schools will achieve. I suspect that the educational values implicit in the features of the new paradigms that I have described are not especially difficult to discern, but, to make them explicit, I address them here and now.

When one operates on the belief that there is one way to validate knowledge, it is not a long step to the belief that students should learn that knowledge. In other words, the primary mission of the school is to see to it that the transmission of knowledge occurs and that students get it right.

Knowledge transmission also means that knowledge not only can be discovered, it can be packaged and stored and transported and tested. In short, it has a life of its own. Furthermore, if there is a canonized body of knowledge, it seems reasonable that it be specified and transmitted to all students (except perhaps to those thought to be incapable of assimilating it; those unfortunates can always work with their hands). Because the same body of knowledge is to be transmitted to all, the same standards should be applied to all and the same criteria should be used to determine who graduates and who does not. The aim, whether intended or not, explicit or implicit, is to standardize curriculum and assessment and to diminish variability among students. Everyone is to have an equal (more or less) share in the same cultural legacy.

It takes no huge imagination to recognize that the recent efforts to specify the content of cultural literacy (Hirsch, 1987), to develop a common curriculum, and to apply standardized "quality indicators" in schools participates mightily in the paradigm I have just described.

The new paradigms, yet again, provide more complex views of educational ends and make educational evaluation a much more daunting enterprise. You will recall that, when I was discussing the meaning of the new paradigms for practice, particularly for curriculum, I said that there were two kinds of curricular balance, a culturally referenced balance and an individually referenced balance. Culturally referenced balance encourages a common array of curriculum content for all students. I do believe that virtually all students ought to have *some* common program of education. An individually referenced

balance fosters the development of those aptitudes, proclivities, and interests that individual students wish to pursue; in short, it fosters productive idiosyncracy. Given the new paradigms' acknowledgment of multiple intelligences and its recognition of parity across subject matters, it would be inconsistent to hold that all students should have nothing but a common educational diet and be assessed by the same set of standardized measures. The good school, given the values that permeate the new paradigms, would aim at increasing individual differences, not reducing them. The good school would seek to increase variance in performance, not to attenuate it.[7]

Such ambitions are, of course, at odds with prevailing lore about effective schools. Yet what the new paradigms imply for educational ends is productive diversity. They acknowledge and value different ways of learning and diverse forms of thinking. Once schools liberate themselves from the idea that the course to be run must be the same for all, and that the goals of that course should be, in the name of equity, common, schools become free to recognize differences as social as well as personal virtues. Educational equity should not be confused with a one-size-fits-all model of practice.

I said earlier that the problem of assessing such a program is daunting. It is. Commensurability simplifies life. One set of goals operationalized within a state or district examination that can be hermetically sealed and optically scored to yield numbers from which stanines can be computed really does simplify educational life. I know, after sitting on admissions committees at Stanford's School of Education for over 20 years, how seductively simple it is to focus on GRE scores and how difficult and time-consuming it is to interpret a student's statement of purpose or even transcripts. When we seriously promote individual differences, we will find it difficult to use the same set of measures to determine what has been achieved. When we care about the journey and the students' experience, as well as the destinations at which they arrive, a fixed multiple-choice test is unlikely to be particularly relevant. When we recognize that learning about culturally rich periods of life requires multiple sources of data, multiple forms of representation, and the use of multiple intelligences, we are inclined to eschew single outcomes. Statistical comparisons may be relevant for some outcomes but surely not for the ones we are likely to care about the most.

Resistances to Change

I would like to conclude with the acknowledgment that the implications for practice I have described are riddled with optimism. My private hope is that the thought can be the parent of the deed. The kinds of practices I have described are, on the whole, more a description of aspiration than a description of fact. So I leave you with questions—questions that ratchet the problem up to what might be considered a political level.

What are the resistances to the kinds of changes I have described? What functions are now served by the forms of practice that now pervade our schools?[8] What makes it so difficult to diversify our programs of study, to alter the structure of our schools, and to use the approaches to research and evaluation in our schools that so many of us have pioneered? These questions invite us to examine what I have called the politics of method (Eisner, 1988).

So let me end with another hope. It is the hope that Egon, maybe with our encouragement and help, will be willing to organize another conference next year, one that examines the politics of method and the possibilities of change in our schools. If he does, I know that I, along with all of you, will be among the first in line for tickets.

Notes

1. In some circles, qualitative research is thought to provide no basis for establishing causal relationships. Experiments are considered the paradigm procedure for securing causal knowledge, and qualitative research is considered an essentially exploratory activity until one can secure "hard" data.

2. The distinction between feeling and knowing is deeply ingrained in Western culture. It is also deeply rooted in our educational culture. Relatively few theoreticians dealing with epistemological issues in education underscore the importance of feeling as a source of knowing. The result is a marginalization of subjects deemed "affective." The arts are the first to be assigned to such residual categories. The result, in my view, is a profound misunderstanding of the sources of knowledge.

3. My own institution, Stanford University, does not include grades that students receive in the fine arts in high school when calculating their grade point average for admission to Stanford. This policy is both a symbolic and a practical reminder of the marginality of the arts and the parochial conception of knowledge that still pervades universities.

4. See, for example, the *International Journal for Qualitative Studies in Education.*

5. Membership in the qualitative interest group in AERA has more than doubled since its inception in 1986.

6. In-service programs operate on the assumption that experienced teachers are well served by listening to professors of education and others teach them, in settings removed from the school, how to perform in their own classrooms. This is akin to a basketball coach providing advice to a team he has never seen play.

7. Sir Herbert Read, British aesthetician, poet, and critic, wrote in *Education Through Art* (1943) that there were two principles that could guide education. One was to make children into what they are not. The other was to help children become what they are. He opted for the latter, stating that fascist societies try to do the former. Self-realization, he believed, was a primary educational goal. Furthermore, when individual differences are cultivated and fostered, the quality of the society itself is increased because of productive diversity. Given Read's observation, one that I share, bringing all children to the same place would be a liability, not an asset, in education.

8. This question has, of course, been raised by many critics of schooling. See, for example, Michael Apple, *Education and Power* (1982), and the works of Henry Giroux.

KNOWLEDGE ACCUMULATION

[18]

Three Views on the Nature and Role of Knowledge in Social Science

JENNIFER C. GREENE

This chapter examines the perspectives of postpositivism, interpretivism,[1] and critical theory on issues related to social scientific knowledge accumulation. The discussion is spirited by efforts both to honor the paradigmatic pluralism of this era and, given my own strong pragmatic orientation, to question what it all means for the practical import of our work. For each inquiry framework, in turn, honor is paid via an introductory sketch, both the form and the substance of which are intended to be illustrative; a brief review of the paradigmatic assumptions most germane to knowledge issues; and a focused discussion of the nature of knowledge and its links to the form and function of knowledge accumulation. Then, the challenge is offered via a critique of the implications of each paradigm's view of knowledge accumulation for the purpose and role of science in our world, with an emphasis on the interrelationships of theory, research, and practice.

As a baseline for this discussion, the perspectives of the conventional inquiry framework on these knowledge accumulation issues are offered first. Within our long-standing scientific tradition, knowledge has been equated with theory, where theory comprises a precise,

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testable network of universal, lawlike relationships among clearly defined variables, a network that is determinate, explanatory, predictive, and verifiable. In conventional science, theories are developed, tested, and refined through empirical research. So, research is intentionally cumulative, and hallmarks of good research studies include clearly defined hypotheses derived from existing theory and results that take the form of generalizable theoretical propositions. The task of the scientist is thus to develop theory. Once developed, scientific theories can be used to address problems or advance life quality in the world of practice. In conventional science, that is, there is a "categorical distinction" between research and practice, between the development of scientific theory and applications of this theory to practical problems (Bernstein, 1976, p. 44).

In relationship to this conventional portrayal of knowledge accumulation, three alternative images frame the present discussion. As the paradigm that represents "old uncertainties unthroned, but not abolished" (Cook, 1985, p. 37), postpositivism also embraces a *social engineering* view of the role and purpose of science. Interpretivism, however, seeks not to adjust the conventional framework but to replace it. With its grounding in phenomenology, hermeneutics, and value pluralism, interpretivism's perspective on the role of social science in the world is likened to *storytelling*.

Critical theory rejects both postpositivism and interpretivism as stand-alone paradigms because of their silence on issues of politics, values, and ideology. This critical inquiry framework seeks to make such issues central to science, thereby intertwining the purpose of science with that of *political engagement and action*.

This chapter then concludes by identifying key issues that cut across these diverse images in the spirit of what Gareth Morgan calls "reflective conversation" (Morgan, 1983b, p. 374). In this era of paradigmatic pluralism, Morgan urges such conversation as a way of facilitating more thoughtful research practice, and especially greater responsibility among social scientists, for "their role in making and remaking social science as we know it today" (Morgan, 1983b, p. 376).

Stances

As one additional set of introductory comments, I believe it is important to share my own predispositions regarding the three paradigms and the knowledge accumulation issues to be addressed. These

comprise four main themes. First, regarding my own expertise or my qualifications for this discussion, I can claim modest mastery of the foundations and perspectives of both postpositivism and interpretivism but consider myself more of a novice with respect to critical theory. My discussion of this latter inquiry framework should thus be viewed as more tentative. Second, my paradigmatic loyalties continue to be troublesomely divided. I have substantially rejected the conventional paradigm that initially shaped my identity as a social scientist but, as yet, am unwilling to swear allegiance to a single alternative. I have opinions about various aspects of different paradigms, but, in the main, I remain a learner, intensely curious and eager to continue learning about the multiple inquiry frameworks that abound in this pluralistic era (Lincoln, 1989). Third, I count myself among those "who believe that science is a remarkably different validity-producing social system [say, than the arts or religion] and at the same time are puzzled as to how this can be so" (Campbell, 1988, p. 498). With the nearly universal recognition that values, ideology, and beliefs permeate the very fabric of social science, what then sets the logic and validity of science apart from any other human endeavor? Finally, and perhaps most important, I believe that all of this self-conscious and often rarified discourse about the assumptional bases and coherence of our work really does matter. This is reflected primarily in this chapter's explicit emphasis on the practical significance of social science. Miles and Huberman (1984) have argued that epistemological purity does not get research done. In counterpoint, I would contend that epistemological integrity does get meaningful research done right. The important "evaluation criteria that can be brought to bear on the nature of knowledge . . . relate [primarily] to the way knowledge serves to guide and shape ourselves as human beings—to the consequences of knowledge, in the sense of what knowledge does to and for humans" (Morgan, 1983b, p. 373).

Postpositivism: Social Science as Social Engineering

Sketch

Thomas Cook's *Postpositivist Critical Multiplism* (1985) is a leading example of postpositivist thought. This approach to inquiry aims to "approximate the ultimately unknowable truth through the use of

processes that critically triangulate from a variety of perspectives on what is worth knowing and what is known" (Cook, 1985, p. 57). The multiplism argument is rooted in the classic methodological ideas of multiple operationalism (Campbell & Fiske, 1959) and between-method triangulation (Denzin, 1978; Webb, Campbell, Schwartz, & Sechrest, 1966). But, in direct response to the philosophical attacks on conventional science, Cook proposes such additional forms of *methodological* multiplism as multiple analyses of the same data set. He also extends the triangulation argument to *theory-related* forms of multiplism, including, for example, the testing of multiple explanatory models for a given set of data (rather than assessing the goodness of fit of a single model). Further, to redress the disappointing failure of social science to contribute meaningfully to the reforms of the Great Society era, Cook advances forms of multiplism that acknowledge the *politics and value pluralism* of such policy contexts, for example, the inclusion of multiple and diverse constituencies in formulating the research agenda.

The Nature of Postpositivist Knowledge and Key Underlying Assumptions

Cook's proposal for critical multiplism, in concert with the remarks on postpositivist "myths and realities" by Denis Phillips (this volume) in the present forum, provide a view of the nature of knowledge in postpositivist thought.

Knowledge remains theory in postpositivism, where theory is construed as a "model" (Cook, 1985) or a "huge fishnet" (Phillips, this volume) of complex, mutually interacting casual relationships among specified constructs or variables. That is, postpositivists believe that human phenomena can best be explained in terms of causal relationships. But this causality is assumed to be complex, multiplistic, and interactive. "Human and social relationships are more like pretzels than single-headed arrows from A to B . . . more like convoluted multivariate statistical interactions than simple main effects" (Cook, 1985, p. 25). Moreover, good theories accurately explain and predict human phenomena but may or may not actually correspond to truth. For, given the realist ontological stance of postpositivism—the belief that there is a natural world out there and that our task as scientists is to know and understand it, in order to explain and predict it—truth remains a "regulative ideal" (Phillips, 1987b, in press). However, because "no longer can it be claimed there are any *absolutely authoritative foundations* upon which scientific knowledge is based" (Phillips,

this volume; see also Bernstein, 1983), truth is acknowledged as "ultimately unknowable" (Cook, 1985). Hence, theory in postpositivism is more like *small theory* and knowledge claims are concomitantly more modest. "Any return to grand theory in human sciences . . . is a selective and wishful interpretation of social science research" (Overman, 1988, p. xvi).

In fact, postpositivist knowledge claims or theoretical propositions are viewed, from Dewey, as "warranted assertibility" (Phillips, this volume) or as established regularities or probabilities about human phenomena rather than as universal laws that govern human behavior. Knowledge claims gain warrant when they are supported by carefully marshaled, objective evidence and when their argument is credible, coherent, and consensual, in other words, when they have survived a *critical tradition* of evaluative challenges and unsuccessful refutations (Cook, 1983, 1985; Phillips, in press). This notion of a critical tradition, derived from Popper, constitutes the essence of Cook's multiplism proposal; he advocates multiplism precisely to invite open criticism from diverse and pluralistic perspectives. "So long as ultimate truth is not accessible, the process of assigning validity is social and partly dependent upon a consensus achieved in debate" (Cook, 1983, p. 89).

Survival of the critical tradition is similarly integral to the postpositivist conception of objectivity. For all alternative inquiry frameworks, acceptance of Hanson's insight that no observations are theory or value neutral (Phillips, 1987b, in press) forces either a reformulation or a rejection of the conventional view of objectivity as freedom from bias. Postpositivists have opted for reformulation, arguing for a view of objectivity as "critical intersubjective verifiability across heterogeneous perspectives" (Cook, 1983, pp. 83-84; see also Campbell, 1984). Knowledge claims so verified are more objective and thus more warranted or more likely to be true. This reconstrual of objectivity also shifts its locus from the individual scientist and the context of discovery to the "community of inquirers" and the context of justification (Phillips, in press). "The objectivity of science is not a matter of the individual scientists but rather the social result of their mutual criticism" (Popper, quoted in Phillips, in press).

Knowledge Accumulation in Postpositivism

With a view of knowledge as small but convoluted, pretzel-like theory and a belief in truth as a regulative ideal, postpositivism maintains as the goal of empirical research the development of

generalizable theoretical propositions, yet views such generalizations as attainable only tentatively and probabilistically. "Most scientific results have the character of hypotheses, i.e., sentences for which the evidence is inconclusive . . . [and which are] liable to be superseded in the course of scientific progress" (Popper, quoted in Campbell, 1984, p. 4). Further, with a commitment to an open critical tradition and a concomitantly muted confidence in methodology, postpositivism's empirical quest for knowledge emphasizes replicability across heterogeneous populations, settings, times, perspectives (see, for example, Cronbach, 1982) and deductive, critical refutation. Scientific generalizations gain warrant only through such replication and criticism. Thus knowledge in postpositivism is accumulated or small theory developed not via the single definitive study but from programs or traditions of empirical research, and past research serves less as the foundation and more as the catalyst for future inquiry.

As Howe (1985) and Phillips (this volume) describe this relationship between research and knowledge growth in postpositivism, empirical evidence can either provisionally confirm a theoretical hypothesis or prove inconsistent with it. If the latter, and the evidence is accepted as credible and thus falsifying, then postpositivists can use this evidence in a variety of ways. No one specific change, i.e., rejection of the given hypothesis, is necessitated (Phillips, this volume). This is because the empirical test does not apply to this hypothesis alone but to the entire theory within which it is embedded. So, different scientists may decide to modify different portions of the relevant theory or even to make no theoretical modifications, awaiting further evidence. That is, decisions about how to modify theories and thus contribute to knowledge growth require professional judgment; they cannot be made mechanically (Phillips, this volume). Nonetheless, while acknowledging the role of professional judgment in scientific growth, postpositivists continue to question how such growth can be rationally justified. And on this, Phillips asserts, "there has been much debate, but little consensus" (Phillips, this volume).

So Why Do Social Science? The Postpositivist Response

> The ideology of the experimenting society is a *method* ideology, not a content ideology. That is, it proposes ways of testing and revising theories of optimal political-economic-social organization rather than proposing a specific political and economic system. (Campbell, 1984, p. 16.)

> [The social scientist's job] is to interpret the world, not to change it; he [or she] interprets it by offering and testing theoretical explanations. . . . Therefore, he [or she] endorses a categorical distinction between theory and practice or action. (Bernstein, 1976, p. 44)

These quotes well illustrate the *intended* political and value neutrality of postpositivism and its continued separation from the world of practice. The line demarcating social science from practice is more permeable in postpositivism than in conventional science. For example, Cook argues that social science must interface with the pluralistic politics and values of applied contexts, especially policy contexts, and that social scientists must not just "build the restricted set of assumptions of the powerful into their research" (Cook, 1985, p. 37). Also arguing largely within the context of applied social policy, Campbell (1984, p. 4) quotes Popper as saying, "Practice is not the enemy of theoretical knowledge, but the most valuable incentive to it."

Nonetheless, the postpositivist social scientist's main job is to participate in the critical community of inquirers whose collective task it is to develop warranted scientific knowledge. The individual scientist's participation is marked by his or her own values, theoretical predispositions, and beliefs, thereby generating a critical but not a normative warrant for the community's collective product of theory. This theory then is to be used to enhance or extend the quality of human endeavors in the world of practice. "How people use the theory to guide practice is not a question of science but of politics" (Popkewitz, 1984, p. 39). So, practical action is a potentiality of the theory because the theory contains valued instrumental knowledge about manipulanda (Cook, 1983), but theory and action remain separate. And so, belying its claims for neutrality and consistent with the character of social engineering, postpositivism clearly rests on a value foundation of utilitarianism, efficiency, and instrumentality.

Interpretivism: Social Science as Storytelling[2]

Sketch and Key Interpretivist Assumptions

The constructivist paradigm developed and continuingly nurtured by Yvonna Lincoln and Egon Guba (Lincoln, this volume; see also Guba & Lincoln, 1981, 1987, 1988a: Lincoln, 1988, 1989; Lincoln & Guba, 1985, 1986a) constitutes a major example of interpretivist

thought and a significant influence within contemporary paradigm debates. The following is a brief sketch of this paradigm, drawn largely from Lincoln's chapter in this volume, in a form that approximates its own voice.

The impersonality of the small conference room—its institutional-beige walls absent any adornment and its hard, uncomfortable black chairs arranged in neat precise rows like soldiers on a parade ground—only heightened the drama unfolding with the current speaker at the front of the room. She spoke of a constructivist paradigm for social inquiry, a paradigm erected from the rubble that ensued when the tower of conventional science, besieged by the batterings of the new philosophy of science, finally toppled. Constructivism, she argued, is based on an entirely different, synergistic set of assumptions about the world and the manner in which we can know it.

One such assumption is that "reality is a social, and, therefore, multiple, construction" (Lincoln, this volume). As social, this reality derives from human interactions aimed at meaning making, comprises intersubjective meanings that "exist only by social agreement or consensus among participants in a [given] context" (Eisenhart, 1988, p. 103), and thus is multiplistic as well as ever changing. Moreover, "the ways in which [humans] interpret their own actions and those of others are not externally related to, but constitutive of, those actions" and of human beliefs, practices, and institutions more generally (Bernstein, 1976, p. 156). Other constructivist assumptions are that "knower and known are interactive, inseparable" and that "inquiry is value-bound" (Lincoln & Guba, 1985, p. 37). These represent, the speaker noted, not just acceptance of Hanson's insight but actual celebration of it "as an opportunity to be exploited" (Lincoln & Guba, 1985, p. 101) as in maximizing the power of the dialectical interaction between a cooperating respondent and a human inquiry instrument to generate meaningful understanding.

Beyond these bold strokes of scientific philosophy, the other contribution to this drama was the speaker's integration of the personal with the scientific in her presentation. She spoke of her immersion in constructivism as an "enlightening, curious, idiographic, and piquant voyage" (Lincoln, this volume). She shared her struggles to respond to critics along the way and to make whole and coherent *her* vision of social inquiry. As we share many value stances, my vision of constructivism would be similar. But I can't help but imagine that there are

constructivists with different personal values, and then I wonder, what do their visions of constructivism look like?

The Nature of Interpretivist Knowledge

From Lincoln and others, interpretivist knowledge comprises the reconstruction of intersubjective meanings, the interpretive understanding of the meanings humans construct in a given context and how these meanings interrelate to form a whole. Any given interpretive reconstruction is idiographic, time- and place-bound; multiple reconstructions are pluralistic, divergent, even conflictual. Hence, interpretivist knowledge resembles more context-specific working hypotheses than generalizable propositions warranting certainty or even probability. But what is the character, the form and substance, of these working hypotheses and thus of interpretivist knowledge?

- Interpretivist knowledge is grounded knowledge (Glaser & Strauss, 1967), not developed from armchair speculations or elegant deductive reasoning but both discovered and justified from the field-based, inductive *methodology* (Guba & Lincoln, 1988a) of interpretivist inquiry.
- Interpretivist knowledge represents *emic* knowledge or inside understanding of the perspectives and meanings of those in the setting being studied, and it encompasses both propositional and tacit information (Stake, 1983; though see Phillips, 1987b, pp. 92-94, for a critique of this claim). That is, the understanding communicated in interpretivist knowledge comes not only from its words but also from the broadly shared contexts of natural experience within which it is embedded.
- Interpretivist knowledge constitutes not nomothetic models but holistic "pattern theories or webs of mutual and plausible influence" (Lincoln, this volume), webs that reflect a hermeneutic intertwinement of part and whole and a view of knowledge that is more "circular" or "amoebalike" than hierarchic and pyramidlike (Lincoln, this volume).
- Interpretivist understanding also aims for internal consistency and coherence. "Correspondence theories identify truth with a relationship *between* language and reality; coherence theories identify truth with internal consistency among claims *within* a language" (Howe, 1988, p. 15).
- And interpretivist knowledge is value-bound and hence "conflictual," "problematic and contested . . . locally and politically situated" (Lincoln, this volume). Moreover, "from this [interpretivist] perspective, social inquiry is meaningful only because it does involve values" (J. Smith, 1983, p. 47).

Knowledge Accumulation in Interpretivism

As is evident by this portrayal of interpretivist knowledge, interpretivism denies the possibility of universal social laws and empirical generalizations.[3] If all knowledge is socially constructed, value bound, and indeterminate, "only time- and context-bound working hypotheses (idiographic statements) are possible" (Lincoln & Guba, 1985, p. 37). So, interpretivist research generates working hypotheses that are connected not to a priori theory but to a context-specific, often emergent inquiry problem, which may or may not be informed by existing knowledge.

> The evidence generated by interpretive research is much more likely to be of an evocative rather than a comprehensive kind, to be sustained, rejected, or refined through future studies. The conclusions of one study merely provide a starting point in a continuing cycle of inquiry, which may [or may not] over time serve to generate persuasive patterns of data from which further conclusions can be drawn. (Morgan, 1983c, p. 398)

Yet, if all knowledge is context-specific working hypotheses and if research studies may or may not be connected to one another, how is knowledge accumulated within this inquiry framework? What is the meaning of interpretivist scientific progress? Two forms of response to these questions will be offered.

First, within interpretivist circles, the challenge of knowledge accumulation has been primarily addressed by the general concept of *transferability*. This concept shifts the inquirer's responsibility from one of demonstrating generalizability to one of providing sufficient description of the particular context studied so that others may adequately judge the applicability or fit of the inquiry findings to their own context. The locus of judgment about transferability thus also shifts from the inquirer to potential users. (See Cronbach, 1982, for similar themes presented for evaluative inquiry.)

Robert Stake's (1983) *naturalistic generalization* is one version of this transferability concept. Stake argues that "naturalistic generalizations develop within a person as a product of experience. They derive from tacit knowledge of how things are . . . [and] seldom take the form of predictions but lead regularly to expectation" (Stake, 1983, p. 282). Further, the interpretivist case study can provide a basis for such

generalizations because it vicariously communicates natural experience as well as tacit knowledge. The importance of communicating "vicarious, 'déjà vu' experience" is also emphasized in Lincoln's formulation of criteria for constructivist case studies (Lincoln, 1988, this volume). And Lincoln and Guba offer transferability (to replace generalizability) as one of their four trustworthiness criteria for the constructivist inquiry process (Lincoln, this volume; Lincoln & Guba, 1985, 1986a). Regarding the latter, Lincoln and Guba contend that the inquirer must provide, at minimum, a thick description of the inquiry context and of the transactions or processes observed in that context that are relevant to the inquiry problem (Lincoln & Guba, 1985, p. 362), though they also acknowledge that "it is by no means clear how 'thick' a thick description needs to be" (Lincoln & Guba, 1986a, p. 77). Then, "the final judgment [about transferability] . . . is vested in the person seeking to make the transfer" (Lincoln & Guba, 1985, p. 217). Such persons may be interested readers, other researchers, or practitioners, lending multiple meanings to the transferability concept. In short, interpretivist inquirers must provide for the possibility of transferability, but its actualization—in the form of scientific knowledge accumulation or enhanced practice—depends on the interests of potential users.

Second, Lincoln's comments in the present forum openly invite further work on these issues of knowledge accumulation within constructivism. Arguing that we do not yet have a language for talking about forms of knowledge that are not hierarchic or taxonomic, neither do we have a language for conceptualizing connections between nonhierarchic knowledge forms. Maybe, she argues,

> we ought to be talking not about "building blocks of science" but about extended sophistication, or the artistic and expressive process of creatively conjoining elements in ways that are fresh and new. We ought to think of bridging, . . . or of synthesizing, . . . or of some other linkage processes. . . . we have no models for scientific knowledge that account for nonhierarchic learning, and we may have to borrow from the poet, the artist, the madman, the mystic. (Lincoln, this volume)

This importance of this challenge is underscored by the problematic character of the relationship of interpretivist knowledge to the world of practice, as discussed next.

So Why Do Social Science? The Interpretivist Response

As grounded knowledge, interpretivist knowledge is embedded within the world of practice. Being value laden, interpretivist knowledge is not neutral or even critically neutral but *interested* knowledge, embued with the normative pluralism of the world of practice. Being value-laden, interpretivist knowledge is also permeated by the values and interests of the inquirer. Constructivism does aim to monitor and minimize the intrusion of inquirer biases into the inquiry process. When such reflexivity is successful, the inquiry findings represent primarily the meanings and values of respondents, and the inquirer's role becomes one of translator or intermediary among differing communities (Bredo & Feinberg, 1982b, pp. 430-431). Yet, any efforts to mute inquirer interests can be only partially successful at best. As Lincoln observed, the "research process itself [is] a political endeavor" (Lincoln, this volume).

Lincoln's interests as an inquirer are oriented around those of inquiry stakeholders and include fairness, action, and empowerment. She is seeking "a mainstream rethinking of the role the social sciences play in everyday, ordinary life" (Lincoln, this volume), a role that includes stakeholders as collaborators in inquiry, that fairly presents the constructions and values of all stakeholders in a setting, and that enhances the ability of stakeholders "to take action, to engage the political arena on behalf of oneself or one's referent stakeholder or participant group" (Lincoln, this volume). I believe these interests reflect Lincoln's vital immersions in the domains of social policy and program evaluation. And, as noted earlier, I share some of this experience and many of these values. But I believe they are our values as inquirers and not inherently those of the interpretivist inquiry paradigm.

Rather, the interpretivist paradigm must be characterized as value relative. Interpretivist knowledge inevitably reflects the values of the inquirer, even as it seeks to reconstruct others' sense of meaning and supporting beliefs. Further, as argued previously, uses of this knowledge depend on the interests of potential users, whether other researchers, policymakers, practitioners, or social program beneficiaries. So, even though interpretivist knowledge is embedded within the normative, pluralistic world of practice, interpretivist inquiry "is not directly [or necessarily] concerned with judging, evaluating, or

condemning existing forms of social and political reality, or with changing the world" but with describing and understanding its constitutive meaning (Bernstein, 1976, p. 169). And so, given its value relativity, common goals of interpretivist inquiry can only be to enrich human discourse, "to bring us in touch with the lives of strangers . . . to converse with them" (Geertz, quoted in Rabinow, 1983, p. 66), "to enlarge the conversation" (J. Smith, 1984, p. 390) with our own understandings and our own stories. That is all?

Critical Science: Social Science as Political Engagement

Sketch

The sketch for the third inquiry framework, critical social science, is presented as a conversation, illustrating the communication and dialog essential to critical science. The setting is a community housing agency that seeks adequate housing for homeless and other low-income individuals in the community. The participants are Elena, an agency staff member for the past five years since her graduation from college, and Bill, a middle-aged, unemployed, temporarily homeless steel worker who is one of the agency's more active and outspoken clients.

Elena: Hi Bill. You wanted to talk with me as soon as possible. What's up?

Bill: Hi Elena. How's the bum-and-crazy business these days? Just kidding. Actually, I wanted to know if you heard Marcia Wilcox's talk last night at the YWCA about her research on housing in this town.

Elena: No, I didn't go. I'm really sick of researchers and their so-called facts and figures.

Bill: Well, Marcia was different. She started with history, saying that since the Depression days in this country, federal policy on low-income housing has never been more than an empty promise, or at most a half-hearted one. Oh sure, there have been some good guys—and gals—and some good intentions in the government all along. But, these intentions never really had much of chance, because they were opposed by the development interests of business and industry.

Elena: We all know that, that's nothing new. And, besides, these intentions you mentioned—they're not empty or half-hearted at all. They represent the fundamental ideals and values in this county.

Bill: Yes, I know, and Marcia agreed, too. She talked about these values as underlying the intent of federal housing policy over the years. But, as I was saying, this intent has always been opposed by the development interests. *And*, the way our government is set up automatically favors these interests over our ideals. She said something like, the political structure inherently contradicts the values of social policy intent.

Then, Marcia got local—and here is where you should be interested. She said that the same thing happens at the local level, and that in this town, agencies like yours are part of the problem.

Elena: Part of the problem! I don't understand! Our whole reason for being is affordable housing for low-income people. We also have a good working relationship with the Downtown Business Association. And I've always thought that was good political strategy, you know, like the lamb lying down with the lion.

Bill: Yeah, but by lying down with them, you're doing a whole lot more than just resting. As Marcia said, you're buying into what they represent. And you're therefore reinforcing a local political situation that, just like national politics, favors growth and development even without trying to do so. These priorities are built into the whole structure of the political system. So, what's really needed are some challenges to this structure. Without them, low-income housing will always remain but a quadrennial campaign promise.

Elena: Like what kinds of challenges?

Bill: Marcia gave us some good leads on this. I've made some phone calls and a group of us are meeting tonight to talk more about her ideas. Want to join us?

On Critical Social Science

This sketch is intended to illustrate three key knowledge-related attributes of critical social science: its embeddedness in history and ideology; its own ideology, as revealed in the meaning of *critical*; and its dialectical synthesis of historical dualism. (Critiques of these and other tenets of critical social science are offered by Bredo & Feinberg, 1982a, 1982b; Fay, 1987.)

According to Popkewitz (this volume) the rules, standards, and logic of science do not have constant meanings, but embody different concepts that are historically constructed and tied to social agendas

(see also Popkewitz, 1984). So, varying views of science, as represented by alternative inquiry frameworks, reflect different intellectual traditions that both arise from and embody different interplays of history and ideology. The assumptions, value dispositions, and methodologies of each tradition coherently interrelate to generate its definition of what counts as legitimate scientific knowledge.

The values explicitly promoted by critical social science are well articulated by its concept of critical,[4] and Popkewitz (this volume) offers two senses of what is critical about critical social science: (a) an analytic posture by which the logical consistency of arguments, procedures, and language receive continual cross-examination and scrutiny (not unlike the critical tradition of postpositivism), and (b) a lens for this posture that "give[s] focus to skepticism toward social institutions and . . . considers the conditions of social regulation, unequal distribution, and power" (Popkewitz, this volume). A critical social scientist would ask, for example, whether observed patterns of relationship "reveal invariant regularities of social action" or "express ideologically frozen relations of dependence" (Bernstein, 1976, pp. 230-231). Critical science also embodies an action-oriented commitment to the common welfare. It "has a [fundamental] practical interest in the fate and quality of social and political life . . . in radically 'improving human existence'" (Bernstein, 1976, pp. 174, 180).

Finally, Popkewitz (this volume) describes critical social science as a tradition that exposes the ideological bases and thus the poverty of such dualisms as objectivity and subjectivity, rigor and relevance, discovery and verification, and even ontology and epistemology. For example, "[objectivity and] relativity [are] issues only within the context of foundationalist epistemologies which search for a privileged standpoint as the guarantee of certainty" (Lather, 1988c, p. 10). In short, "phenomenology negates positivism, and philosophies of praxis [or practical action] are concerned with negating the dualism thus created" (Morgan, 1983b, p. 372; see also Bernstein, 1983).

The Nature of Critical Knowledge

Following directly from these attributes, knowledge in critical social science is, substantively, nonfoundational knowledge about the historical, structural, and value bases of social phenomena as well as about contradictions and distortions therein. Knowledge in critical science is also interested knowledge or knowledge that reflects the

values and priorities of a particular intellectual-cultural-social tradition. In the critical theory of the contemporary Frankfurt school, advanced most notably by Jürgen Habermas (1971), legitimate interests include the technical-instrumental and practical-communicative knowledge claims of postpositivism and interpretivism, respectively. But, in part because neither of these informs us how to tell good from bad, their inquiry paradigms are supplemented and superseded in critical theory by one that takes a third emancipatory, action-constitutive interest as fundamental (Bredo & Feinberg, 1982a, p. 275). "An empirical statement [or critical knowledge claim] must be judged by its intentions for the good and true life" (Fischer, 1985, p. 251, from Aristotle via Habermas).

So, critical knowledge is also practical, action-oriented knowledge that enlightens and thereby catalyzes political and social change. Critical knowledge enlightens an audience by revealing the structural conditions of their existence, specifically, how these conditions came about and what distortions or injustices they currently represent. Such enlightenment carries within it an enabling, motivating force to stimulate action, a catalyst for self-reflection toward greater autonomy and responsibility and for strategic political action toward emancipation (Bernstein, 1976). Critical knowledge does not prescribe such action, for that would be action in its merely technical sense. Rather, critical knowledge represents "a genuine unity of theory and revolutionary praxis where the theoretical understanding of the contradictions inherent in existing society, when appropriated by those who are exploited, becomes constitutive of their very activity to transform society" (Bernstein, 1976, p. 182).

Knowledge Accumulation in Critical Social Science

So, how does knowledge—as an interested, emancipatory account of the history and values underlying social phenomena—accumulate in critical social science? As I understand this inquiry framework, the short answer is that it doesn't. Nor is it intended to. As Popkewitz (this volume) argues, the belief that scientific knowledge is a progressive development in which evidence continually clarifies and modifies what is known is erroneous. The logic of science is historically formed and changes in a manner that is not necessarily cumulative. From a critical perspective, knowledge accumulation in its building-

block sense reifies "the social and historical conditions in which knowledge is produced and transformed" (Popkewitz, this volume) rather than respects the ideological and dynamic character of such conditions. With such respect, what counts as knowledge, including critical knowledge, changes with the times. Moreover, knowledge itself, as practical and action oriented, changes the social-political conditions in which it is produced. So, to endeavor to build on prior work is to estrange it from its own social-historical context, to deny, in turn, its impacts on that context and thereby to strip it of meaning.

Popkewitz (this volume) does say that "we need to understand what others have said and done before us" and that this involves a complex process of interpretation and analysis. Just what this means, however, is not entirely clear.

So Why Do Social Science? The Critical Science Response

The practical import of critical social science, its role and function in the world of practice, *is* entirely clear and, moreover, is vitally integral to this inquiry framework. Critical social science denies the distinction between *is* and *ought*, between science as theory and research, and practice as normative, ideologically based action. Critical science seeks to reclaim the critical function of theory (Bernstein, 1976); to reassert the scientist's role as an interested observer who speaks with "a critical voice of social consciousness" (Popkewitz, this volume); to have a "practical political impact" (Fay, 1987, p. 2); "to change the world, not to describe it" (Popkewitz, 1984, p. 45).

Critical social science strives to meet these aims via the action-constitutive nature of its knowledge, its "unity of theory and revolutionary praxis" (Bernstein, 1976, p. 182).

> Critical social science is an attempt to understand in a rationally responsible manner the oppressive features of a society such that this understanding stimulates its audience to transform their society and thereby liberate themselves. (Fay, 1987, p. 4)

Causing some disquiet here is my difficulty in giving concrete form to knowledge that inherently but nonprescriptively catalyzes political action. Just what does such knowledge look like?

Concluding Comments

This discussion about the nature and role of social scientific knowledge in postpositivism, interpretivism, and critical science has been with intention minimally comparative and largely descriptive. I endeavored primarily to present the arguments of each inquiry framework with some measure of internal integrity rather than in reference to selected concerns or criteria. I hoped thereby to invite broad participation in the identification of important issues for further conversation. Some people may be most interested, for example, in issues related to causality, others in questions about subjectivity. My own nominations of agenda items for further conversation are reflected in the critiques made of each paradigm's stance on the role and purpose of social science in society. In concluding this discussion, I would like to return to these issues. Their presentation here highlights both the language and the concepts that differentiate the three inquiry frameworks and some of their common challenges.

(1) Given the acknowledged, though varied, complexity of and the contextual and/or historical boundaries on social knowledge, can it serve other than local or micro-level interests? Can and should social scientists aspire to serve the common good, or do we need a social scientist in every community?

(2) Given that all social knowledge is value bound, value laden, or value based, is all social science fundamentally about human values? Can and should social scientists seek to "recapture moral discourse" (Schwandt, 1989b) as our most significant societal role?

(3) As social engineers, as storytellers, or as catalyzers of political action, what moral and ethical responsibilities do social scientists have for the consequences of our work?

(4) Even as social scientists from quite different perspectives share a commitment to the improvement of social life through our work, we diverge in how this commitment is actualized in the world of practice. Is relativism justifiable in this context? Whose interests should be served by social science?

And now, please, let us converse.

Notes

1. Throughout this chapter, the more generic term *interpretivism* is used to include the constructivist inquiry framework set forth by Lincoln (this volume) and the terms *paradigm* and *inquiry framework* are used interchangeably.

2. This discussion will focus on the constructivist view of interpretivism, including similar and related views, but excluding dissimilar qualitative traditions (see Atkinson, Delamont, & Hammersley, 1988; Jacob, 1987, 1988; M. Smith, 1987).

3. Here, there is a sharp break between interpretivism (especially as constructivism) and some other qualitative traditions. Ethnography, for example, does address general theories of culture and does acknowledge the possibility of generalizable knowledge (see the references listed in note 2).

4. Bredo and Feinberg (1982b) criticize other inquiry frameworks for not fully acknowledging or justifying their value positions. Critical theory, they say, at least attempts to do so, notably, Habermas's efforts to define the "universal pragmatics" of a theory of communicative competence (Bredo & Feinberg, 1982b, p. 436).

KNOWLEDGE ACCUMULATION

[18]

Three Views on the Nature and Role of Knowledge in Social Science

JENNIFER C. GREENE

This chapter examines the perspectives of postpositivism, interpretivism,[1] and critical theory on issues related to social scientific knowledge accumulation. The discussion is spirited by efforts both to honor the paradigmatic pluralism of this era and, given my own strong pragmatic orientation, to question what it all means for the practical import of our work. For each inquiry framework, in turn, honor is paid via an introductory sketch, both the form and the substance of which are intended to be illustrative; a brief review of the paradigmatic assumptions most germane to knowledge issues; and a focused discussion of the nature of knowledge and its links to the form and function of knowledge accumulation. Then, the challenge is offered via a critique of the implications of each paradigm's view of knowledge accumulation for the purpose and role of science in our world, with an emphasis on the interrelationships of theory, research, and practice.

As a baseline for this discussion, the perspectives of the conventional inquiry framework on these knowledge accumulation issues are offered first. Within our long-standing scientific tradition, knowledge has been equated with theory, where theory comprises a precise,

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testable network of universal, lawlike relationships among clearly defined variables, a network that is determinate, explanatory, predictive, and verifiable. In conventional science, theories are developed, tested, and refined through empirical research. So, research is intentionally cumulative, and hallmarks of good research studies include clearly defined hypotheses derived from existing theory and results that take the form of generalizable theoretical propositions. The task of the scientist is thus to develop theory. Once developed, scientific theories can be used to address problems or advance life quality in the world of practice. In conventional science, that is, there is a "categorical distinction" between research and practice, between the development of scientific theory and applications of this theory to practical problems (Bernstein, 1976, p. 44).

In relationship to this conventional portrayal of knowledge accumulation, three alternative images frame the present discussion. As the paradigm that represents "old uncertainties unthroned, but not abolished" (Cook, 1985, p. 37), postpositivism also embraces a *social engineering* view of the role and purpose of science. Interpretivism, however, seeks not to adjust the conventional framework but to replace it. With its grounding in phenomenology, hermeneutics, and value pluralism, interpretivism's perspective on the role of social science in the world is likened to *storytelling*.

Critical theory rejects both postpositivism and interpretivism as stand-alone paradigms because of their silence on issues of politics, values, and ideology. This critical inquiry framework seeks to make such issues central to science, thereby intertwining the purpose of science with that of *political engagement and action*.

This chapter then concludes by identifying key issues that cut across these diverse images in the spirit of what Gareth Morgan calls "reflective conversation" (Morgan, 1983b, p. 374). In this era of paradigmatic pluralism, Morgan urges such conversation as a way of facilitating more thoughtful research practice, and especially greater responsibility among social scientists, for "their role in making and remaking social science as we know it today" (Morgan, 1983b, p. 376).

Stances

As one additional set of introductory comments, I believe it is important to share my own predispositions regarding the three paradigms and the knowledge accumulation issues to be addressed. These

comprise four main themes. First, regarding my own expertise or my qualifications for this discussion, I can claim modest mastery of the foundations and perspectives of both postpositivism and interpretivism but consider myself more of a novice with respect to critical theory. My discussion of this latter inquiry framework should thus be viewed as more tentative. Second, my paradigmatic loyalties continue to be troublesomely divided. I have substantially rejected the conventional paradigm that initially shaped my identity as a social scientist but, as yet, am unwilling to swear allegiance to a single alternative. I have opinions about various aspects of different paradigms, but, in the main, I remain a learner, intensely curious and eager to continue learning about the multiple inquiry frameworks that abound in this pluralistic era (Lincoln, 1989). Third, I count myself among those "who believe that science is a remarkably different validity-producing social system [say, than the arts or religion] and at the same time are puzzled as to how this can be so" (Campbell, 1988, p. 498). With the nearly universal recognition that values, ideology, and beliefs permeate the very fabric of social science, what then sets the logic and validity of science apart from any other human endeavor? Finally, and perhaps most important, I believe that all of this self-conscious and often rarified discourse about the assumptional bases and coherence of our work really does matter. This is reflected primarily in this chapter's explicit emphasis on the practical significance of social science. Miles and Huberman (1984) have argued that epistemological purity does not get research done. In counterpoint, I would contend that epistemological integrity does get meaningful research done right. The important "evaluation criteria that can be brought to bear on the nature of knowledge . . . relate [primarily] to the way knowledge serves to guide and shape ourselves as human beings—to the consequences of knowledge, in the sense of what knowledge does to and for humans" (Morgan, 1983b, p. 373).

Postpositivism: Social Science as Social Engineering

Sketch

Thomas Cook's *Postpositivist Critical Multiplism* (1985) is a leading example of postpositivist thought. This approach to inquiry aims to "approximate the ultimately unknowable truth through the use of

processes that critically triangulate from a variety of perspectives on what is worth knowing and what is known" (Cook, 1985, p. 57). The multiplism argument is rooted in the classic methodological ideas of multiple operationalism (Campbell & Fiske, 1959) and between-method triangulation (Denzin, 1978; Webb, Campbell, Schwartz, & Sechrest, 1966). But, in direct response to the philosophical attacks on conventional science, Cook proposes such additional forms of *methodological* multiplism as multiple analyses of the same data set. He also extends the triangulation argument to *theory-related* forms of multiplism, including, for example, the testing of multiple explanatory models for a given set of data (rather than assessing the goodness of fit of a single model). Further, to redress the disappointing failure of social science to contribute meaningfully to the reforms of the Great Society era, Cook advances forms of multiplism that acknowledge the *politics and value pluralism* of such policy contexts, for example, the inclusion of multiple and diverse constituencies in formulating the research agenda.

The Nature of Postpositivist Knowledge and Key Underlying Assumptions

Cook's proposal for critical multiplism, in concert with the remarks on postpositivist "myths and realities" by Denis Phillips (this volume) in the present forum, provide a view of the nature of knowledge in postpositivist thought.

Knowledge remains theory in postpositivism, where theory is construed as a "model" (Cook, 1985) or a "huge fishnet" (Phillips, this volume) of complex, mutually interacting casual relationships among specified constructs or variables. That is, postpositivists believe that human phenomena can best be explained in terms of causal relationships. But this causality is assumed to be complex, multiplistic, and interactive. "Human and social relationships are more like pretzels than single-headed arrows from A to B . . . more like convoluted multivariate statistical interactions than simple main effects" (Cook, 1985, p. 25). Moreover, good theories accurately explain and predict human phenomena but may or may not actually correspond to truth. For, given the realist ontological stance of postpositivism—the belief that there is a natural world out there and that our task as scientists is to know and understand it, in order to explain and predict it—truth remains a "regulative ideal" (Phillips, 1987b, in press). However, because "no longer can it be claimed there are any *absolutely authoritative foundations* upon which scientific knowledge is based" (Phillips,

this volume; see also Bernstein, 1983), truth is acknowledged as "ultimately unknowable" (Cook, 1985). Hence, theory in postpositivism is more like *small theory* and knowledge claims are concomitantly more modest. "Any return to grand theory in human sciences . . . is a selective and wishful interpretation of social science research" (Overman, 1988, p. xvi).

In fact, postpositivist knowledge claims or theoretical propositions are viewed, from Dewey, as "warranted assertibility" (Phillips, this volume) or as established regularities or probabilities about human phenomena rather than as universal laws that govern human behavior. Knowledge claims gain warrant when they are supported by carefully marshaled, objective evidence and when their argument is credible, coherent, and consensual, in other words, when they have survived a *critical tradition* of evaluative challenges and unsuccessful refutations (Cook, 1983, 1985; Phillips, in press). This notion of a critical tradition, derived from Popper, constitutes the essence of Cook's multiplism proposal; he advocates multiplism precisely to invite open criticism from diverse and pluralistic perspectives. "So long as ultimate truth is not accessible, the process of assigning validity is social and partly dependent upon a consensus achieved in debate" (Cook, 1983, p. 89).

Survival of the critical tradition is similarly integral to the postpositivist conception of objectivity. For all alternative inquiry frameworks, acceptance of Hanson's insight that no observations are theory or value neutral (Phillips, 1987b, in press) forces either a reformulation or a rejection of the conventional view of objectivity as freedom from bias. Postpositivists have opted for reformulation, arguing for a view of objectivity as "critical intersubjective verifiability across heterogeneous perspectives" (Cook, 1983, pp. 83-84; see also Campbell, 1984). Knowledge claims so verified are more objective and thus more warranted or more likely to be true. This reconstrual of objectivity also shifts its locus from the individual scientist and the context of discovery to the "community of inquirers" and the context of justification (Phillips, in press). "The objectivity of science is not a matter of the individual scientists but rather the social result of their mutual criticism" (Popper, quoted in Phillips, in press).

Knowledge Accumulation in Postpositivism

With a view of knowledge as small but convoluted, pretzel-like theory and a belief in truth as a regulative ideal, postpositivism maintains as the goal of empirical research the development of

generalizable theoretical propositions, yet views such generalizations as attainable only tentatively and probabilistically. "Most scientific results have the character of hypotheses, i.e., sentences for which the evidence is inconclusive . . . [and which are] liable to be superseded in the course of scientific progress" (Popper, quoted in Campbell, 1984, p. 4). Further, with a commitment to an open critical tradition and a concomitantly muted confidence in methodology, postpositivism's empirical quest for knowledge emphasizes replicability across heterogeneous populations, settings, times, perspectives (see, for example, Cronbach, 1982) and deductive, critical refutation. Scientific generalizations gain warrant only through such replication and criticism. Thus knowledge in postpositivism is accumulated or small theory developed not via the single definitive study but from programs or traditions of empirical research, and past research serves less as the foundation and more as the catalyst for future inquiry.

As Howe (1985) and Phillips (this volume) describe this relationship between research and knowledge growth in postpositivism, empirical evidence can either provisionally confirm a theoretical hypothesis or prove inconsistent with it. If the latter, and the evidence is accepted as credible and thus falsifying, then postpositivists can use this evidence in a variety of ways. No one specific change, i.e., rejection of the given hypothesis, is necessitated (Phillips, this volume). This is because the empirical test does not apply to this hypothesis alone but to the entire theory within which it is embedded. So, different scientists may decide to modify different portions of the relevant theory or even to make no theoretical modifications, awaiting further evidence. That is, decisions about how to modify theories and thus contribute to knowledge growth require professional judgment; they cannot be made mechanically (Phillips, this volume). Nonetheless, while acknowledging the role of professional judgment in scientific growth, postpositivists continue to question how such growth can be rationally justified. And on this, Phillips asserts, "there has been much debate, but little consensus" (Phillips, this volume).

So Why Do Social Science? The Postpositivist Response

> The ideology of the experimenting society is a *method* ideology, not a content ideology. That is, it proposes ways of testing and revising theories of optimal political-economic-social organization rather than proposing a specific political and economic system. (Campbell, 1984, p. 16.)

> [The social scientist's job] is to interpret the world, not to change it; he [or she] interprets it by offering and testing theoretical explanations. . . . Therefore, he [or she] endorses a categorical distinction between theory and practice or action. (Bernstein, 1976, p. 44)

These quotes well illustrate the *intended* political and value neutrality of postpositivism and its continued separation from the world of practice. The line demarcating social science from practice is more permeable in postpositivism than in conventional science. For example, Cook argues that social science must interface with the pluralistic politics and values of applied contexts, especially policy contexts, and that social scientists must not just "build the restricted set of assumptions of the powerful into their research" (Cook, 1985, p. 37). Also arguing largely within the context of applied social policy, Campbell (1984, p. 4) quotes Popper as saying, "Practice is not the enemy of theoretical knowledge, but the most valuable incentive to it."

Nonetheless, the postpositivist social scientist's main job is to participate in the critical community of inquirers whose collective task it is to develop warranted scientific knowledge. The individual scientist's participation is marked by his or her own values, theoretical predispositions, and beliefs, thereby generating a critical but not a normative warrant for the community's collective product of theory. This theory then is to be used to enhance or extend the quality of human endeavors in the world of practice. "How people use the theory to guide practice is not a question of science but of politics" (Popkewitz, 1984, p. 39). So, practical action is a potentiality of the theory because the theory contains valued instrumental knowledge about manipulanda (Cook, 1983), but theory and action remain separate. And so, belying its claims for neutrality and consistent with the character of social engineering, postpositivism clearly rests on a value foundation of utilitarianism, efficiency, and instrumentality.

Interpretivism: Social Science as Storytelling[2]

Sketch and Key Interpretivist Assumptions

The constructivist paradigm developed and continuingly nurtured by Yvonna Lincoln and Egon Guba (Lincoln, this volume; see also Guba & Lincoln, 1981, 1987, 1988a: Lincoln, 1988, 1989; Lincoln & Guba, 1985, 1986a) constitutes a major example of interpretivist

thought and a significant influence within contemporary paradigm debates. The following is a brief sketch of this paradigm, drawn largely from Lincoln's chapter in this volume, in a form that approximates its own voice.

The impersonality of the small conference room—its institutional-beige walls absent any adornment and its hard, uncomfortable black chairs arranged in neat precise rows like soldiers on a parade ground—only heightened the drama unfolding with the current speaker at the front of the room. She spoke of a constructivist paradigm for social inquiry, a paradigm erected from the rubble that ensued when the tower of conventional science, besieged by the batterings of the new philosophy of science, finally toppled. Constructivism, she argued, is based on an entirely different, synergistic set of assumptions about the world and the manner in which we can know it.

One such assumption is that "reality is a social, and, therefore, multiple, construction" (Lincoln, this volume). As social, this reality derives from human interactions aimed at meaning making, comprises intersubjective meanings that "exist only by social agreement or consensus among participants in a [given] context" (Eisenhart, 1988, p. 103), and thus is multiplistic as well as ever changing. Moreover, "the ways in which [humans] interpret their own actions and those of others are not externally related to, but constitutive of, those actions" and of human beliefs, practices, and institutions more generally (Bernstein, 1976, p. 156). Other constructivist assumptions are that "knower and known are interactive, inseparable" and that "inquiry is value-bound" (Lincoln & Guba, 1985, p. 37). These represent, the speaker noted, not just acceptance of Hanson's insight but actual celebration of it "as an opportunity to be exploited" (Lincoln & Guba, 1985, p. 101) as in maximizing the power of the dialectical interaction between a cooperating respondent and a human inquiry instrument to generate meaningful understanding.

Beyond these bold strokes of scientific philosophy, the other contribution to this drama was the speaker's integration of the personal with the scientific in her presentation. She spoke of her immersion in constructivism as an "enlightening, curious, idiographic, and piquant voyage" (Lincoln, this volume). She shared her struggles to respond to critics along the way and to make whole and coherent *her* vision of social inquiry. As we share many value stances, my vision of constructivism would be similar. But I can't help but imagine that there are

constructivists with different personal values, and then I wonder, what do their visions of constructivism look like?

The Nature of Interpretivist Knowledge

From Lincoln and others, interpretivist knowledge comprises the reconstruction of intersubjective meanings, the interpretive understanding of the meanings humans construct in a given context and how these meanings interrelate to form a whole. Any given interpretive reconstruction is idiographic, time- and place-bound; multiple reconstructions are pluralistic, divergent, even conflictual. Hence, interpretivist knowledge resembles more context-specific working hypotheses than generalizable propositions warranting certainty or even probability. But what is the character, the form and substance, of these working hypotheses and thus of interpretivist knowledge?

- Interpretivist knowledge is grounded knowledge (Glaser & Strauss, 1967), not developed from armchair speculations or elegant deductive reasoning but both discovered and justified from the field-based, inductive *methodology* (Guba & Lincoln, 1988a) of interpretivist inquiry.
- Interpretivist knowledge represents *emic* knowledge or inside understanding of the perspectives and meanings of those in the setting being studied, and it encompasses both propositional and tacit information (Stake, 1983; though see Phillips, 1987b, pp. 92-94, for a critique of this claim). That is, the understanding communicated in interpretivist knowledge comes not only from its words but also from the broadly shared contexts of natural experience within which it is embedded.
- Interpretivist knowledge constitutes not nomothetic models but holistic "pattern theories or webs of mutual and plausible influence" (Lincoln, this volume), webs that reflect a hermeneutic intertwinement of part and whole and a view of knowledge that is more "circular" or "amoebalike" than hierarchic and pyramidlike (Lincoln, this volume).
- Interpretivist understanding also aims for internal consistency and coherence. "Correspondence theories identify truth with a relationship *between* language and reality; coherence theories identify truth with internal consistency among claims *within* a language" (Howe, 1988, p. 15).
- And interpretivist knowledge is value-bound and hence "conflictual," "problematic and contested . . . locally and politically situated" (Lincoln, this volume). Moreover, "from this [interpretivist] perspective, social inquiry is meaningful only because it does involve values" (J. Smith, 1983, p. 47).

Knowledge Accumulation in Interpretivism

As is evident by this portrayal of interpretivist knowledge, interpretivism denies the possibility of universal social laws and empirical generalizations.[3] If all knowledge is socially constructed, value bound, and indeterminate, "only time- and context-bound working hypotheses (idiographic statements) are possible" (Lincoln & Guba, 1985, p. 37). So, interpretivist research generates working hypotheses that are connected not to a priori theory but to a context-specific, often emergent inquiry problem, which may or may not be informed by existing knowledge.

> The evidence generated by interpretive research is much more likely to be of an evocative rather than a comprehensive kind, to be sustained, rejected, or refined through future studies. The conclusions of one study merely provide a starting point in a continuing cycle of inquiry, which may [or may not] over time serve to generate persuasive patterns of data from which further conclusions can be drawn. (Morgan, 1983c, p. 398)

Yet, if all knowledge is context-specific working hypotheses and if research studies may or may not be connected to one another, how is knowledge accumulated within this inquiry framework? What is the meaning of interpretivist scientific progress? Two forms of response to these questions will be offered.

First, within interpretivist circles, the challenge of knowledge accumulation has been primarily addressed by the general concept of *transferability*. This concept shifts the inquirer's responsibility from one of demonstrating generalizability to one of providing sufficient description of the particular context studied so that others may adequately judge the applicability or fit of the inquiry findings to their own context. The locus of judgment about transferability thus also shifts from the inquirer to potential users. (See Cronbach, 1982, for similar themes presented for evaluative inquiry.)

Robert Stake's (1983) *naturalistic generalization* is one version of this transferability concept. Stake argues that "naturalistic generalizations develop within a person as a product of experience. They derive from tacit knowledge of how things are . . . [and] seldom take the form of predictions but lead regularly to expectation" (Stake, 1983, p. 282). Further, the interpretivist case study can provide a basis for such

generalizations because it vicariously communicates natural experience as well as tacit knowledge. The importance of communicating "vicarious, 'déjà vu' experience" is also emphasized in Lincoln's formulation of criteria for constructivist case studies (Lincoln, 1988, this volume). And Lincoln and Guba offer transferability (to replace generalizability) as one of their four trustworthiness criteria for the constructivist inquiry process (Lincoln, this volume; Lincoln & Guba, 1985, 1986a). Regarding the latter, Lincoln and Guba contend that the inquirer must provide, at minimum, a thick description of the inquiry context and of the transactions or processes observed in that context that are relevant to the inquiry problem (Lincoln & Guba, 1985, p. 362), though they also acknowledge that "it is by no means clear how 'thick' a thick description needs to be" (Lincoln & Guba, 1986a, p. 77). Then, "the final judgment [about transferability] . . . is vested in the person seeking to make the transfer" (Lincoln & Guba, 1985, p. 217). Such persons may be interested readers, other researchers, or practitioners, lending multiple meanings to the transferability concept. In short, interpretivist inquirers must provide for the possibility of transferability, but its actualization—in the form of scientific knowledge accumulation or enhanced practice—depends on the interests of potential users.

Second, Lincoln's comments in the present forum openly invite further work on these issues of knowledge accumulation within constructivism. Arguing that we do not yet have a language for talking about forms of knowledge that are not hierarchic or taxonomic, neither do we have a language for conceptualizing connections between nonhierarchic knowledge forms. Maybe, she argues,

> we ought to be talking not about "building blocks of science" but about extended sophistication, or the artistic and expressive process of creatively conjoining elements in ways that are fresh and new. We ought to think of bridging, . . . or of synthesizing, . . . or of some other linkage processes. . . . we have no models for scientific knowledge that account for nonhierarchic learning, and we may have to borrow from the poet, the artist, the madman, the mystic. (Lincoln, this volume)

This importance of this challenge is underscored by the problematic character of the relationship of interpretivist knowledge to the world of practice, as discussed next.

So Why Do Social Science? The Interpretivist Response

As grounded knowledge, interpretivist knowledge is embedded within the world of practice. Being value laden, interpretivist knowledge is not neutral or even critically neutral but *interested* knowledge, embued with the normative pluralism of the world of practice. Being value-laden, interpretivist knowledge is also permeated by the values and interests of the inquirer. Constructivism does aim to monitor and minimize the intrusion of inquirer biases into the inquiry process. When such reflexivity is successful, the inquiry findings represent primarily the meanings and values of respondents, and the inquirer's role becomes one of translator or intermediary among differing communities (Bredo & Feinberg, 1982b, pp. 430-431). Yet, any efforts to mute inquirer interests can be only partially successful at best. As Lincoln observed, the "research process itself [is] a political endeavor" (Lincoln, this volume).

Lincoln's interests as an inquirer are oriented around those of inquiry stakeholders and include fairness, action, and empowerment. She is seeking "a mainstream rethinking of the role the social sciences play in everyday, ordinary life" (Lincoln, this volume), a role that includes stakeholders as collaborators in inquiry, that fairly presents the constructions and values of all stakeholders in a setting, and that enhances the ability of stakeholders "to take action, to engage the political arena on behalf of oneself or one's referent stakeholder or participant group" (Lincoln, this volume). I believe these interests reflect Lincoln's vital immersions in the domains of social policy and program evaluation. And, as noted earlier, I share some of this experience and many of these values. But I believe they are our values as inquirers and not inherently those of the interpretivist inquiry paradigm.

Rather, the interpretivist paradigm must be characterized as value relative. Interpretivist knowledge inevitably reflects the values of the inquirer, even as it seeks to reconstruct others' sense of meaning and supporting beliefs. Further, as argued previously, uses of this knowledge depend on the interests of potential users, whether other researchers, policymakers, practitioners, or social program beneficiaries. So, even though interpretivist knowledge is embedded within the normative, pluralistic world of practice, interpretivist inquiry "is not directly [or necessarily] concerned with judging, evaluating, or

condemning existing forms of social and political reality, or with changing the world" but with describing and understanding its constitutive meaning (Bernstein, 1976, p. 169). And so, given its value relativity, common goals of interpretivist inquiry can only be to enrich human discourse, "to bring us in touch with the lives of strangers . . . to converse with them" (Geertz, quoted in Rabinow, 1983, p. 66), "to enlarge the conversation" (J. Smith, 1984, p. 390) with our own understandings and our own stories. That is all?

Critical Science: Social Science as Political Engagement

Sketch

The sketch for the third inquiry framework, critical social science, is presented as a conversation, illustrating the communication and dialog essential to critical science. The setting is a community housing agency that seeks adequate housing for homeless and other low-income individuals in the community. The participants are Elena, an agency staff member for the past five years since her graduation from college, and Bill, a middle-aged, unemployed, temporarily homeless steel worker who is one of the agency's more active and outspoken clients.

Elena: Hi Bill. You wanted to talk with me as soon as possible. What's up?

Bill: Hi Elena. How's the bum-and-crazy business these days? Just kidding. Actually, I wanted to know if you heard Marcia Wilcox's talk last night at the YWCA about her research on housing in this town.

Elena: No, I didn't go. I'm really sick of researchers and their so-called facts and figures.

Bill: Well, Marcia was different. She started with history, saying that since the Depression days in this country, federal policy on low-income housing has never been more than an empty promise, or at most a half-hearted one. Oh sure, there have been some good guys—and gals—and some good intentions in the government all along. But, these intentions never really had much of chance, because they were opposed by the development interests of business and industry.

Elena: We all know that, that's nothing new. And, besides, these intentions you mentioned—they're not empty or half-hearted at all. They represent the fundamental ideals and values in this county.

Bill: Yes, I know, and Marcia agreed, too. She talked about these values as underlying the intent of federal housing policy over the years. But, as I was saying, this intent has always been opposed by the development interests. *And*, the way our government is set up automatically favors these interests over our ideals. She said something like, the political structure inherently contradicts the values of social policy intent.

Then, Marcia got local—and here is where you should be interested. She said that the same thing happens at the local level, and that in this town, agencies like yours are part of the problem.

Elena: Part of the problem! I don't understand! Our whole reason for being is affordable housing for low-income people. We also have a good working relationship with the Downtown Business Association. And I've always thought that was good political strategy, you know, like the lamb lying down with the lion.

Bill: Yeah, but by lying down with them, you're doing a whole lot more than just resting. As Marcia said, you're buying into what they represent. And you're therefore reinforcing a local political situation that, just like national politics, favors growth and development even without trying to do so. These priorities are built into the whole structure of the political system. So, what's really needed are some challenges to this structure. Without them, low-income housing will always remain but a quadrennial campaign promise.

Elena: Like what kinds of challenges?

Bill: Marcia gave us some good leads on this. I've made some phone calls and a group of us are meeting tonight to talk more about her ideas. Want to join us?

On Critical Social Science

This sketch is intended to illustrate three key knowledge-related attributes of critical social science: its embeddedness in history and ideology; its own ideology, as revealed in the meaning of *critical*; and its dialectical synthesis of historical dualism. (Critiques of these and other tenets of critical social science are offered by Bredo & Feinberg, 1982a, 1982b; Fay, 1987.)

According to Popkewitz (this volume) the rules, standards, and logic of science do not have constant meanings, but embody different concepts that are historically constructed and tied to social agendas

(see also Popkewitz, 1984). So, varying views of science, as represented by alternative inquiry frameworks, reflect different intellectual traditions that both arise from and embody different interplays of history and ideology. The assumptions, value dispositions, and methodologies of each tradition coherently interrelate to generate its definition of what counts as legitimate scientific knowledge.

The values explicitly promoted by critical social science are well articulated by its concept of critical,[4] and Popkewitz (this volume) offers two senses of what is critical about critical social science: (a) an analytic posture by which the logical consistency of arguments, procedures, and language receive continual cross-examination and scrutiny (not unlike the critical tradition of postpositivism), and (b) a lens for this posture that "give[s] focus to skepticism toward social institutions and . . . considers the conditions of social regulation, unequal distribution, and power" (Popkewitz, this volume). A critical social scientist would ask, for example, whether observed patterns of relationship "reveal invariant regularities of social action" or "express ideologically frozen relations of dependence" (Bernstein, 1976, pp. 230-231). Critical science also embodies an action-oriented commitment to the common welfare. It "has a [fundamental] practical interest in the fate and quality of social and political life . . . in radically 'improving human existence'" (Bernstein, 1976, pp. 174, 180).

Finally, Popkewitz (this volume) describes critical social science as a tradition that exposes the ideological bases and thus the poverty of such dualisms as objectivity and subjectivity, rigor and relevance, discovery and verification, and even ontology and epistemology. For example, "[objectivity and] relativity [are] issues only within the context of foundationalist epistemologies which search for a privileged standpoint as the guarantee of certainty" (Lather, 1988c, p. 10). In short, "phenomenology negates positivism, and philosophies of praxis [or practical action] are concerned with negating the dualism thus created" (Morgan, 1983b, p. 372; see also Bernstein, 1983).

The Nature of Critical Knowledge

Following directly from these attributes, knowledge in critical social science is, substantively, nonfoundational knowledge about the historical, structural, and value bases of social phenomena as well as about contradictions and distortions therein. Knowledge in critical science is also interested knowledge or knowledge that reflects the

values and priorities of a particular intellectual-cultural-social tradition. In the critical theory of the contemporary Frankfurt school, advanced most notably by Jürgen Habermas (1971), legitimate interests include the technical-instrumental and practical-communicative knowledge claims of postpositivism and interpretivism, respectively. But, in part because neither of these informs us how to tell good from bad, their inquiry paradigms are supplemented and superseded in critical theory by one that takes a third emancipatory, action-constitutive interest as fundamental (Bredo & Feinberg, 1982a, p. 275). "An empirical statement [or critical knowledge claim] must be judged by its intentions for the good and true life" (Fischer, 1985, p. 251, from Aristotle via Habermas).

So, critical knowledge is also practical, action-oriented knowledge that enlightens and thereby catalyzes political and social change. Critical knowledge enlightens an audience by revealing the structural conditions of their existence, specifically, how these conditions came about and what distortions or injustices they currently represent. Such enlightenment carries within it an enabling, motivating force to stimulate action, a catalyst for self-reflection toward greater autonomy and responsibility and for strategic political action toward emancipation (Bernstein, 1976). Critical knowledge does not prescribe such action, for that would be action in its merely technical sense. Rather, critical knowledge represents "a genuine unity of theory and revolutionary praxis where the theoretical understanding of the contradictions inherent in existing society, when appropriated by those who are exploited, becomes constitutive of their very activity to transform society" (Bernstein, 1976, p. 182).

Knowledge Accumulation in Critical Social Science

So, how does knowledge—as an interested, emancipatory account of the history and values underlying social phenomena—accumulate in critical social science? As I understand this inquiry framework, the short answer is that it doesn't. Nor is it intended to. As Popkewitz (this volume) argues, the belief that scientific knowledge is a progressive development in which evidence continually clarifies and modifies what is known is erroneous. The logic of science is historically formed and changes in a manner that is not necessarily cumulative. From a critical perspective, knowledge accumulation in its building-

block sense reifies "the social and historical conditions in which knowledge is produced and transformed" (Popkewitz, this volume) rather than respects the ideological and dynamic character of such conditions. With such respect, what counts as knowledge, including critical knowledge, changes with the times. Moreover, knowledge itself, as practical and action oriented, changes the social-political conditions in which it is produced. So, to endeavor to build on prior work is to estrange it from its own social-historical context, to deny, in turn, its impacts on that context and thereby to strip it of meaning.

Popkewitz (this volume) does say that "we need to understand what others have said and done before us" and that this involves a complex process of interpretation and analysis. Just what this means, however, is not entirely clear.

So Why Do Social Science? The Critical Science Response

The practical import of critical social science, its role and function in the world of practice, *is* entirely clear and, moreover, is vitally integral to this inquiry framework. Critical social science denies the distinction between *is* and *ought*, between science as theory and research, and practice as normative, ideologically based action. Critical science seeks to reclaim the critical function of theory (Bernstein, 1976); to reassert the scientist's role as an interested observer who speaks with "a critical voice of social consciousness" (Popkewitz, this volume); to have a "practical political impact" (Fay, 1987, p. 2); "to change the world, not to describe it" (Popkewitz, 1984, p. 45).

Critical social science strives to meet these aims via the action-constitutive nature of its knowledge, its "unity of theory and revolutionary praxis" (Bernstein, 1976, p. 182).

> Critical social science is an attempt to understand in a rationally responsible manner the oppressive features of a society such that this understanding stimulates its audience to transform their society and thereby liberate themselves. (Fay, 1987, p. 4)

Causing some disquiet here is my difficulty in giving concrete form to knowledge that inherently but nonprescriptively catalyzes political action. Just what does such knowledge look like?

Concluding Comments

This discussion about the nature and role of social scientific knowledge in postpositivism, interpretivism, and critical science has been with intention minimally comparative and largely descriptive. I endeavored primarily to present the arguments of each inquiry framework with some measure of internal integrity rather than in reference to selected concerns or criteria. I hoped thereby to invite broad participation in the identification of important issues for further conversation. Some people may be most interested, for example, in issues related to causality, others in questions about subjectivity. My own nominations of agenda items for further conversation are reflected in the critiques made of each paradigm's stance on the role and purpose of social science in society. In concluding this discussion, I would like to return to these issues. Their presentation here highlights both the language and the concepts that differentiate the three inquiry frameworks and some of their common challenges.

(1) Given the acknowledged, though varied, complexity of and the contextual and/or historical boundaries on social knowledge, can it serve other than local or micro-level interests? Can and should social scientists aspire to serve the common good, or do we need a social scientist in every community?

(2) Given that all social knowledge is value bound, value laden, or value based, is all social science fundamentally about human values? Can and should social scientists seek to "recapture moral discourse" (Schwandt, 1989b) as our most significant societal role?

(3) As social engineers, as storytellers, or as catalyzers of political action, what moral and ethical responsibilities do social scientists have for the consequences of our work?

(4) Even as social scientists from quite different perspectives share a commitment to the improvement of social life through our work, we diverge in how this commitment is actualized in the world of practice. Is relativism justifiable in this context? Whose interests should be served by social science?

And now, please, let us converse.

Notes

1. Throughout this chapter, the more generic term *interpretivism* is used to include the constructivist inquiry framework set forth by Lincoln (this volume) and the terms *paradigm* and *inquiry framework* are used interchangeably.

2. This discussion will focus on the constructivist view of interpretivism, including similar and related views, but excluding dissimilar qualitative traditions (see Atkinson, Delamont, & Hammersley, 1988; Jacob, 1987, 1988; M. Smith, 1987).

3. Here, there is a sharp break between interpretivism (especially as constructivism) and some other qualitative traditions. Ethnography, for example, does address general theories of culture and does acknowledge the possibility of generalizable knowledge (see the references listed in note 2).

4. Bredo and Feinberg (1982b) criticize other inquiry frameworks for not fully acknowledging or justifying their value positions. Critical theory, they say, at least attempts to do so, notably, Habermas's efforts to define the "universal pragmatics" of a theory of communicative competence (Bredo & Feinberg, 1982b, p. 436).

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Emergent Paradigms
How New? How Necessary?

MARGARET D. LeCOMPTE

I begin this response by suggesting that conflict and incompatibility among paradigms is more at issue in education than it is in the social sciences generally. Of concern is not so much research in the social sciences but research in education, and how debates in science and philosophy do *not* trickle down into educational practice (Popkewitz, this volume). My purpose will be to broaden the discourse somewhat by asking, What are the social sciences? I then shall look at the definitions given for each of the paradigms under consideration—the postpositivistic, constructive, and critical theoretical. Finally, I will talk about what knowledge really is, and the kinds of knowledge that might be additionally sought, given the definitional constraints posited by the keynote addresses for the conference on which this book is based.

The enthusiasm of formerly positivistic researchers for alternative methods for investigating human problems seems to derive from the domination of American pedagogical studies by psychology and especially behavioral psychology, with its attendant commitment to testing, measurement, and experimental research. This has been coupled with a very American fascination with efficiency and causality; these have made educational research and practice vulnerable to the worst excesses and rigidities of logical positivism.

AUTHOR'S NOTE: I am indebted to Kenneth Howe for his insightful assistance with interpretation of philosophical debates and assertions, and to A. Gary Dworkin for background in sociological theory.

Paradigms and Intellectual Ethnocentrism

Firestone (this volume) defines *paradigms* or *disciplines* as cultures that constrain the behavior and beliefs of their members. Further, never having been exposed to alternative ways of doing things leads one naturally to assume that the way one has been taught is the only way to think and operate. A corollary to cultural isolation is lack of awareness of the existence and legitimacy of other cultures—which can lead to ethnocentrism (Atkinson, Delamont, & Hammersley, 1988). The converse is what Warshay (1962) calls possessing a breadth of perspective.

How New, and How Emergent, Are the "New" Paradigms?

The enthusiasm for new paradigms may simply be the excitement one feels when immersed in a foreign and unfamiliar culture—before culture shock sets in. Further, the new paradigms described in this conference really are not new. Postpositivism (Phillips, this volume) resembles pragmatism, especially in its approach to methods and processes of verification. Constructivism, with its emphasis on multiple constructed realities and *emic*, or subject, meaning, is a combination of symbolic interactionism, ethnomethodology, and other phenomenological approaches to inquiry. Critical theory originated in Europe after World War I. In fact, the issue of how problematic conventional science inquiry becomes when empirical realities change has been of concern for a long time in other social science disciplines (Eckberg & Hill, 1979; Friedrichs, 1970; Gouldner, 1970). What *is* new is the use of these perspectives by pedagogues and educational researchers.

Some of the contemporary paradigmatic soul-searching in education may have resulted because educators bought wholeheartedly into a model more appropriate for laboratories or hard sciences—behaviorism and statistical methods adapted from genetics and agricultural economics. It did not fit what they were studying but they nonetheless clung to it like a religion in order to emulate *hard* scientists. Consequently, the use of qualitative methods felt like a breath of fresh air compared with the rigidities of quantification. What I suggest is that the fresh air had existed all the time—in other social science disciplines.

Hence, we may not really need new paradigms; we may only need to use more fully and imaginatively those we already have. We especially should not substitute for science the techniques of literary criticism or *storytelling*—a metaphor for authentic products of interpretive research often ascribed to Geertz (1973, 1988) in what I believe to be a misreading. An example can be drawn from the clinical work of the neurologist Oliver Sacks (1986), who used the often poetic insights and frequently mad alternative realities of his brain-damaged clients to inform and enhance his scholarly research. While he called for a new form of science and treatment of the mind that blended research findings with the "realities of madmen and poets" (see Lincoln, this volume), Sacks never ceased being a scientist.

What Are the Social Sciences?

Weber (cited in Berger, 1977, p. 162) used the terms *social science*, *cultural science*, and *history* synonymously—as, to some extent, does Popkewitz (this volume). In so doing, he emphasized the human, contextual, social, and, hence, interactional nature of what the social sciences investigate. Today, the social sciences usually include sociology, psychology, economics, political science, and anthropology. Each asks its own peculiar questions and has its own mode of inquiry and ways of looking at the world. But history now is grouped with the humanities—literature, poetry, languages—and most university administrations do not consider it to be a social science. Even the credentials of anthropology sometimes are questioned; it may be considered a humanity or lumped with geography. Yet, cultural studies and history *are* the foundations of the social sciences. They are critical to our understanding of educational processes—as are all of the social sciences.

Paradigmatic monotheism derives from intellectual isolation from other scientific traditions. Such isolation is dangerous, because it leads to "intolerance. . . fruitless polemic, [and] hypercriticism. [It also] leads to the belief that new thoughts are revolutionary and [to] neglect of relevant scholarship received to be outside the paradigm simply because it 'belongs to another tradition' " (Atkinson, Delamont, & Hammersley, 1988, p. 233). Instruction in American colleges of education has been typified by this kind of isolation. Textbooks in research design, which really are texts in experimental design, typically devote

only a chapter or two to all other models. Until recently, the majority of colleges of education had no courses in field or qualitative methods; in most, they are still electives. Few colleges of education teach historiography; none teaches it regularly.

Although I by no means wish to denigrate the great contributions psychology has made to our understanding of human life and cognition, the focus of psychology is the isolated individual, decontextualized, and laboratory bound. It provides neither a complete nor in ideal model for studying *social*, *cultural*, and *historical* men and women. The "headlock that educational psychology has had on educational research" (Spindler & Spindler, 1989) has created the so-called conventional science model, which, indeed, is too narrow. It has led to the sort of intractable problems of explanation that have long frustrated educational evaluators struggling with its application to events in the real world (see Eash, 1985; Guba & Lincoln, 1981; LeCompte, 1972; Stake, 1985). It also has imposed significant limits upon the question: What constitutes legitimate knowledge and modes of knowledge accumulation?

The so-called conventional science paradigm generates and legitimates only a particular kind of knowledge—that which can be quantified and measured. However, other paradigms, appropriate to other social science disciplines, inform our investigations in education. And cross-disciplinary approaches not only will help us avoid the danger of creating new, equally rigid and restrictive canons for critical, post-positivistic, and constructive investigation but will prevent us from celebrating the death of an old orthodoxy by creating a new one just as doctrinaire.

What Is the Nature of Knowledge?

My next concern is with the definition of the nature of knowledge. Three assumptions about *emergent* paradigms were implicit in the conference keynote addresses. First, the paradigms are philosophically and operationally incompatible. Second, they are based upon different definitions of reality and, hence, generate substantively different kinds of knowledge. Third, they lead to different versions of the truth. To address these assumptions, I feel that we must define the term *knowledge* more carefully and in terms of a broader definition of the social sciences than that embraced by the conference conveners.

By definition, the various social science disciplines concern themselves with different aspects of human social and cultural life. Economics addresses issues of exchange, distribution of resources, and production. Political science looks at the arrangement of power and control. Sociology and anthropology examine the structure and dynamics of human organization as well as their genesis. Psychology examines the workings of the individual human psyche. Clearly, staying within the confines of one discipline restricts the scope of questions that can be asked, the methods to be used for investigation, and the explanations that inquiry generates. The result is a serious problem for accumulation of knowledge: without a cross-disciplinary perspective, the types of knowledge that can be generated are limited (Cahnman, 1965).

A second problem is that confusion exists over distinctions among *information*, *knowledge*, and *truth*. The question at issue is this: How can we verify the truth of research results we have generated? My response: Although we can generate and accumulate knowledge in any scientific tradition, we will have a very hard time generating and accumulating truth.

Because it must at least be capable of falsification, scientific knowledge refers to information and facts about reality, whether viewed as context specific or not. Truth, however, has three connotations: (a) warrantability, practical value, or utility; (b) a universal and permanent definition of reality; and (c) correctness or rectitude in behavior. The first two clearly involve debate between realists and idealists, which plays itself out both in philosophical ponderings and in scientific investigation and upon which I hold both a pragmatic and agnostic position. The third addresses what I feel are cultural issues governing proper forms of interaction among people. Failure to make these distinctions creates a confusing logic that begins with science and ends with religion: "Having now defined what real or true knowledge is, and having delineated how one goes about acquiring it, it is clear (here making the shift from description, or science, to prescription, or religion) that there is only one right or true way of carrying out investigation!"

Both knowledge and truth deal with understanding, but of different kinds. Knowledge and information fall into the realm of science, while truth is an issue for philosophy and religion. Knowledge and information address what Weber (1968, cited in Heap, 1977, p. 177) called "direct observational understanding," or the sense or identity of acts

or phenomena. Observational understanding allows us to see actions as what they are and is required to locate and perceive patterns of like actions. They also can address explanatory understanding (Weber, 1968, p. 8), or that which tells us the motive or purpose for an act. Explanatory understanding is, however, valid only within a given social or cultural context. When what is valid for a given context is seen as appropriate for, or mistaken for *truth* in, all contexts (as has been the case in American educational research for many decades), the mischief starts.

Knowledge then is a representation, or picture, of empirical reality in the human mind; it is, in fact, an abstraction, because the limitations of the mind dictate that humans choose to attend to those phenomena that are value relevant or culturally significant for them (Berger, 1977). While principles of abstraction, or value relevance, differ, of course, from group to group, the use of the principle of value relevance tells us what is important to look for. It permits us to look for two kinds of information: (a) the ideas that motivate people and (b) the activities that they carry out because of what motivated them—or, as Popkewitz (this volume) calls them, the objective and the subjective. This perspective facilitates the accumulation of information and knowledge. It also permits an examination of truth—at least truth as defined by those who are being studied. It answers the somewhat unanswerable question, "What is Real?" in terms of the pragmatists' notion of warranted assertability. However, it does not resolve the problem of relativity, upon which Lincoln (this volume) holds an uncompromising position. Complete relativism, I think, is dangerous. It leads to complete reductionism; it means that people cannot even talk to one another and that interaction and meaning become random (Long, 1958). I am uncomfortable with that position on political and cultural as well as scientific grounds.

What Knowledge Can be Accumulated in Each of the Postconventional Science Models?

Greene (this volume) uses three metaphors to describe the emergent paradigms. Postpositivists are social engineers, constructivists are storytellers, and critical theorists are social activists. For postpositivists, reality is what works, what can be warranted or verified; knowledge is *small theory*; and truth is a regulative ideal. Knowledge

accumulation then consists of building ever more complex and complete, if probabilistic, explanations of phenomena at the small theory level, facilitating an understanding of specific group mechanics across and within specific, not universal, groups.

For constructivists, reality is a social and multiple construction; knowledge is derived from understanding and consists of a consensus within a given context of individual perceptions, constructions of meaning, and the values that underlie them. It is acquired in the investigation of almost dyadic interaction where the knower and the known are inseparable—or "interactive monism" (Lincoln, this volume). Because individual interpretations are ideographically bounded by time, place, and persons, and multiple reconstructions are pluralistic, divergent, and conflictual, knowledge accumulators become folklorists, devoted to the collection of stories. Truth is completely relative to the context.

Critical theory and constructivism have a great deal in common. Both view reality as context specific and believe that human activity is generated by the motivations and interests—or ideologies—underlying them. However, critical theorists place their analysis squarely on a macro-historical plane, while constructivists appear to be more concerned with microlevel patterns of interaction. For critical theorists, reality is ideological and dependent upon the interests, values, and priorities of the particular intellectual-cultural-social tradition of those who develop it; it becomes "real" to people insofar as the social permeates the consciousness of individuals. Knowledge then consists of histories that are located in society and implicated in shaping the productive, administrative, and structural dynamics of the given society (Popkewitz, this volume). For critical theorists, knowledge is purposive and linked to practice and the promotion of change. It cannot, therefore, be accumulated, because to do so would make it "real" when, in fact, it is context dependent. The implication might seem to be that there is no truth, for it too would have to be dependent upon its historical context.

However, time *does* stop for critical theorists, because the goal of knowledge or practice is what is "good," "right," "responsible," and "empowering for individuals." These concepts are themselves historically bound; they are deeply rooted in western European philosophical traditions dating from the seventeenth and eighteenth centuries. Truth then, for the critical theorist, is whatever leads to the achievement of these conditions.

What Knowledge Can We Accumulate?

The term *accumulate* is an interesting one, with a descriptive valence all its own. It reminds me of cotton pickers dragging behind them their tow sacks, which grow increasingly heavier as the day progresses. It implies a horizontal, rather than hierarchic, accretion. In scientific terms, this means that, while we may be able to add to our knowledge base descriptive information about how our world works, we may not be able to generate causal laws that will hold true regardless of the context. We may be able to accumulate (a) information about the ideas that motivate people and groups of people and (b) information about the activities they carried out because of what motivated them (Weber, 1968). In this chapter, I will address this form of knowledge accumulation in each of the three *emergent* paradigms, accepting for the moment their own terms and definitions of knowledge, and without exciting philosophical debates about truth. I will not discuss postpositivism at length, because, as *neopragmatism*, it is a familiar perspective. Knowledge accumulation simply involves constructing better, if less grand and presumptuous, explanations of phenomena. I will devote my comments to Greene's (this volume) discussion of critical theory and constructivism, beginning with the most troublesome for me—the constructivist paradigm.

Knowledge and the Constructivist Paradigm

Greene (this volume) states that the constructivist paradigm defines knowledge as what people "know" or how they define their situations; as I understand her formulation, the task of the knowledge accumulator in the constructivist paradigm is that of the folklorist or collector of stories. This strikes me as insufficient. It is an overreaction, an attempt to be artful in response to the aridness characteristic of conventional scientific writing. All of a sudden, social scientists have discovered passion, art, portraiture, and the multiple jabberings of a peopled reality. They have jumped from the methods of somewhat reified science to literature. But neither artful portrayal nor thick description necessarily creates knowledge, though they could produce an initial data base. The real issue is this: Of what should the data base consist? Here the lack of sociology in Lincoln's (this volume) description of constructivism is most apparent.

Lincoln's constructivist model could be subject to the same critique leveled at some interpretive, interactionist, and phenomenological approaches to understanding human behavior: it is decontextualized, hanging in air. It posits the existence of a cobweb of multiple negotiated realities but never delineates the interests they reflect, the rules that govern the negotiation of reality, or their historical antecedents. Yet these are the warp and weft of social life—and constitute the realm of culture and society. While human life may be individually *interpreted*, it also is *socially* defined (Ritzer, 1980). It is governed by ideas that people develop concerning desirable or obligatory ways in which their coexistence should be structured. The ideas, norms, or values are not automatic or natural, but, while out*side* of people, they still are " 'artificial' and man-made; they are called norms and values because adherence to them is not merely contemplative, but involves the recognition that the actor who holds them somehow has the duty or task to attempt their practical implementation" (Berger, 1977, p. 168).

General cultural values are ideas that groups of people have developed concerning the regulation of their activity. Thus the concepts of actor, act, role, norm, value, expectation, obligation, and institution are sociological concepts that constitute the building blocks of human social life—the carpet that underlies the design (see Sacks, 1986, p. 176). They also are embedded in the history of the group, not simply created *de novo* in every social interaction. If this historical dimension is not kept clearly in mind, studies of human social life seem to suffer from a kind of collective Korsakov's amnesia, wherein the items and events in the present must constantly be re-created each instant because no past exists to guide, constrain, and connect them to an identity.

The constructivist paradigm concentrates on delineating interaction and meaning *at the moment*; it leaves one with a curious sense of ephemeralness and impermanence. Missing is the *social definition*, or knowledge of the social rules, the framework, that lies *outside* immediate action. Both—the immediate and particular as well as the constant and universal (for the group)—are needed for an authentic portrayal of human social life. Thus there are prior conditions, rules, and sets of circumstances that restrain and guide the identities and expectations that people have; these in turn structure their negotiations as well as their definition of their own situations. Consideration of the writings of sociologists like Peter Blau (1964, 1977), George Homans (1950, 1974), George Herbert Mead (1934/1967), Alfred

Schutz (1932/1967), and others would be helpful. These works set out a framework for the constraints that shape interaction, both at the individual and the group levels, both for researchers and those they study.

Knowledge Accumulation and Critical Theory

I disagree with Greene's (this volume) statement that critical inquiry does not and is not intended to accumulate knowledge. My reading of critical theory is that, although knowledge does not, it is true, accumulate in the sense that it moves hierarchically toward ever more refined and accurate versions of the truth, it can accrue horizontally in time. In other words, the tow sack can be filled, and its contents can inform our actions. I believe that what the critical theorists are saying is that knowledge about people's ideas and their actions can be collected and accumulated; however, it is a knowledge base suffused with a specific analytic posture that moves away from a preoccupation with method, roles, and technique and toward an understanding of issues of value—the tacit and interest-based assumptions that structure contemporary life. In this way, our critical senses, once dulled by overriding concern with procedures, can be awakened to a context-bound form of knowledge and investigatory methods that explicitly are *not* value free or hierarchic.

Rather, they will emphasize accumulating historically based, context-bound description, confronting the values and interests inherent in both the method and the results of the research. In this way, the data basis for continuing, historically informed dialog and analysis can be established.