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The Making of a Constructivist
A Remembrance of Transformations Past

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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.

For information address:

SAGE Publications, Inc.
2455 Teller Road
Newbury Park, California 91320

SAGE Publications Ltd.
6 Bonhill Street
London EC2A 4PU
United Kingdom

SAGE Publications India Pvt. Ltd.
M-32 Market
Greater Kailash I
New Delhi 110 048 India

rinted in the United States of America

ibrary of Congress Cataloging-in-Publication Data

he Paradigm dialog / Egon G. Guba, editor.
p. cm.
"Sponsored by Phi Delta Kappa International and the School of Education, Indiana University."
Includes bibliographical references and index.
ISBN 0-8039-3822-5. — ISBN 0-8039-3823-3 (pbk.)
1. Social sciences—Methodology—Congresses. 2. Paradigms (Social sciences)—Congresses. I. Guba, Egon G.
H61.P29 1990
300—dc20 90-43947
CIP

IRST PRINTING, 1990

age Production Editor: Judith L. Hunter