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EDUCATIONAL RESEARCHER 2009; 38; 501
DOI: 10.3102/0013189X09348351

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Learning From Our Differences: A Dialogue Across Perspectives on Quality in Education Research

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The dialogue re-presented in this article is intended to foster mutual engagement—and opportunity for learning—across different perspectives on research within the education research community. Participants in the dialogue each addressed the following questions: (1) What are the touchstones by which you judge quality or rigor in education research (for a single study, a set of studies, or a “field” or community of researchers in dialogue)? What is your chief concern or fear that the touchstones guard against? (2) Where do you see challenges to your perspective in the perspectives of other members of the panel? How might your perspective evolve to respond to those challenges? Given all of this, what are the implications for the preparation of education researchers? Opening and closing comments set the dialogue in historical context, highlight issues raised, and suggest next steps for collaborative learning from the diversity of perspectives in our field.

Keywords: epistemology; philosophy of social science; research methodology

The state of discourse in the field of education research has been likened to the cacophony in the Tower of Babel (Phillips, 2006a). Not only is there a breakdown in communication due to the “multitude of tongues” within the field—the multitude of theoretical and methodological approaches, all with their own specialist terminologies that make mutual comprehension difficult—but, in addition, proponents of different perspectives often hold strikingly different assumptions about the nature of the enterprise in which they are engaged. These include different assumptions about the ends of education research, about its epistemology, and relatedly, about whether it is or should be (or even could be) value free. There are concomitant and often charged disagreements about conceptions of quality or rigor and about the effects of different choices on the educational community. As a result, researchers working in different frameworks often “talk past” each other, if they try to talk at all.

The dialogue re-presented in this article is intended to foster the sort of mutual engagement—and opportunity for learning (Moss, 2005b)—across perspectives that has too often been lacking. The

points of view expressed here were shared first in an interactive symposium, co-organized by Pamela Moss and Denis Phillips, that was presented at the 2009 annual meeting of the American Educational Research Association (AERA), although our text has been refined and revised for publication. Participating in the dialogue were Frederick D. Erickson, Robert E. Floden, Patti A. Lather, and Barbara L. Schneider. Taken together, the remarks of the participants illustrate some of the substantial variation in perspectives on research that our field encompasses, which Phillips (2006a) has characterized as ranging in a rough continuum from right (those researchers who take the natural sciences as a model for the social sciences) to left (those researchers who, for a variety of reasons, believe the natural science model is insufficient or inappropriate for the social sciences). In preparation for this dialogue, the participants agreed to study at least one article from each member of the panel that focused on quality in education research¹ and then to address the following set of questions:

1. What are the touchstones by which you judge quality or rigor in education research (for a single study, a set of studies, or a “field” or community of researchers in dialogue)? What is your chief concern or fear that the touchstones guard against?
2. Where do you see challenges to your perspective in the perspectives of other members of the panel? How might your perspective evolve to respond to those challenges? Given all of this, what are the implications for the preparation of education researchers?

In the sections that follow, Moss and Phillips first provide a brief historical frame for the dialogue and a friendly “guide” to support readers in learning about and from our differences. Then Erickson, Floden, Lather, and Schneider respond to the guiding questions in two rounds of dialogue that address the two sets of questions listed above, the first round focusing on their own perspectives on quality and the second round on the challenges raised by the others’ perspectives. Finally, Phillips and Moss offer some comments about what we have accomplished and the implications for continuing efforts to learn from our differences.

Given the origin of this article in oral dialogue and the subsequent page limits for publication, readers will note that no single coauthor has had space to build an argument with the extensive citations typical of a journal article. We note, however, that the

dialogue format not only serves some of the same purpose of situating each perspective within a range of alternatives but also offers the unique advantage of direct and immediate exchange of ideas in the voices of their original authors. Readers interested in the extended arguments can pursue them through citations to each of the six coauthors' works appearing in the reference list. And, in closing comments, we suggest additional readings that highlight productive disjunctions and complementarities in perspectives on education research.²

Framing the Issues

Pamela Moss

As suggested in our opening comments, while the field of education research is diverse, it is also fragmented. The diversity of perspectives provides a natural resource for learning. The fragmentation, however, makes it difficult to take advantage of that resource. It also leads to perceptions, fair or not, about the weakness of our collective enterprise from both outside and inside the field of education research.

One approach to addressing concerns about fragmentation has been to try to articulate what it is we have in common—while respecting our diversity, what are the standards or principles to which we can all hold ourselves accountable? This approach has led to the publication of various organization-sponsored position statements like the National Research Council's (NRC; 2002) *Scientific Research in Education* (hereafter, *SRE*) or AERA's (2006) *Standards for Reporting on Empirical Social Science Research in AERA Publications*.³ In fact, all the members of this panel have participated, in one way or another, in dialogues about these and other such documents—some of us as members of the panels that produced them, some as external reviewers and critics, some as researchers of the responses to the documents, and some of us in multiple roles at different times.

It is safe to say that many members of the broad research community do not see their conceptions of good research adequately represented in these documents.⁴ These differences signal fault lines within the field, lines that the membership of our panel spans. For instance, at the heart of a number of differences lies the fundamental question of whether the social sciences should approach the study of social phenomena in the same way the natural sciences have approached the study of natural phenomena (e.g., Martin & McIntyre, 1994).

Those who adopt a "unified" conception of science tend to argue that the primary goals of the social sciences, like the natural sciences, should be generalizable explanations and predictions. In contrast, those who take what is often called an "interpretive" (Flyvbjerg, 2001; Rabinow & Sullivan, 1987) approach to social science argue that social phenomena differ from natural phenomena because they are meaningful to the actors involved. Furthermore, meanings are embedded in complex social contexts that shape what can be understood in ways that the actors involved may not perceive, something argued to be equally true of researchers as of the people they study. From this perspective, a primary aim of social science is to understand what people mean and intend by what they say and do and to locate those understandings within the historical, cultural, institutional, and immediate situational contexts that shape them.

These different perspectives on the aims of social science can entail substantially different perspectives on quality and rigor. If you believe, for instance, that good scientists should be objective in the sense of producing knowledge that is epistemologically independent of their personal values and sociopolitical beliefs, then you are likely to privilege as rigorous those methods that demonstrate agreement (replication or reproducibility) among independent observers. If, instead, you believe knowledge is unavoidably shaped by the preconceptions of the knowers (and that independent agreement may simply be a manifestation of a shared bias⁵ among the members of a research community), then you are likely to privilege as rigorous those methods that illuminate the nature of the bias and the social, cultural, and political forces that shaped it.

We are unlikely to agree on issues like these in the short or the long run. Many of us, on all sides of the debate, have come to feel angry about the way we are represented (or ignored) in the discourse of others. There are, for all of us, important ethical issues at stake that are inseparable from our perspectives on the nature and quality of education research. The important question for us is what to do about disagreements like these—how to cope with or, better yet, take advantage of them for mutual learning. As Richard Bernstein (1985) suggests, in one of my favorite quotes, "It is precisely in and through an understanding of alien cultures that we can come to a more sensitive and critical understanding of our own culture and of those prejudices that may lie hidden from us" (p. 36).

So, the goal of our dialogue, from my perspective, is not to try to reach agreement but to try to better understand our different perspectives and to grapple with the challenges that others' perspectives raise for our own. This way of conceptualizing dialogue is largely grounded in H. G. Gadamer's philosophical hermeneutics (e.g., Gadamer, 1975/1994) and its (pluralist) interpreters (see Moss, 2005b).⁶ As Gadamer scholar Georgia Warnke (1999) suggests, good interpretations of others' texts

succeed in illuminating the difference a given text makes to what we thought we knew—either by giving added confirmation to our views or by asking us to rethink or expand them, even if we do so in opposition to the text we are trying to understand. We take the possibility seriously that the text can teach us something, and we therefore put our own views and assumptions into play in trying to understand it. (p. 17)

There may well be genuine commonalities here on which we can build. However, genuine commonalities always grow out of respectful understanding of differences. And, whether we achieve agreement or not, the dialogue can, as Bernstein notes, lead to a more critical understanding of our own perspectives.

Denis Phillips

We are attempting to do here what some scholars claim is impossible, namely, to have a genuine discussion across the chasm—or perhaps it is across many chasms—of deep differences about such things as the nature, purpose, methodologies, standards of rigor, and relevance of education research. (Please note the imprecision in this formulation, to which I will return in a moment.) We are engaging in this challenging endeavor because—along with Jerome Bruner (1990)—we decry the tendency of scholars to "seal themselves within their own rhetoric and within their own parish of authorities" (pp. ix–x). I firmly believe that the other participants in the dialogue have

something to learn from me—and so I have to admit that it is likely I have something to learn from them.

There are various strategies for dealing with dialogue across a deep chasm, perhaps the simplest of which is to treat those on the other side as nonnative speakers of one's own language, whereupon their lack of understanding can be overcome by the simple ploy of speaking very loudly! (This is what seems to have happened during the construction at Babel.) A more promising option—although, to be frank, it is one about which I have had (and still do have) strong philosophical reservations—is to treat the views across the chasm as being something like Kuhnian incommensurable paradigms (Kuhn, 1970). So, setting aside the facts that Kuhn's work on scientific revolutions has been overapplied and that there are problems with his notion of "incommensurability" and the way he explained it, I will endeavor to show that his ideas provide a loose framework that can be helpful as we struggle with the issues before us in this dialogue.⁷

According to Kuhn's analysis in *The Structure of Scientific Revolutions* (the most crucial chapters being IX, X, and XII), when a field is inhabited by several rival paradigms, these differ from each other in a number of dimensions, and their technical languages differ; even when apparently the same word is used it has a different meaning in the different paradigms (Kuhn's favorite example was "mass"—the point being that "mass" for Einstein is different from Newtonian "mass"). In fact *all key terms* differ in meaning across paradigms, and this is why the paradigms are incommensurable; the problem of course is not merely that there are differences in word usage (for these could be overcome) but that the different paradigms are quite different conceptual nets that are being cast over nature. Thus, adherents of the different paradigms "live in different worlds" or have been "transported to other planets," as Kuhn famously put it, and this makes cross-paradigm dialogue impossible. (This was Kuhn's original hard-line position, which he later softened and, later still, arguably hardened again.) Inhabitants of such completely different worlds must always talk at "cross-purposes," and choice between their respective positions is a choice between "incompatible modes of community life."

If we accepted Kuhn's hard-line position, the implications for our dialogue would be quite stark: Because we would be dealing with Kuhnian-type chasms, and because cross-paradigm communication is impossible, we would be well advised to abandon this effort to "learn from our differences."

For various technical reasons I think Kuhn's argument that paradigms are incommensurable and present an unbridgeable chasm is faulty (see Newton-Smith, 1981, chaps. V, VII, for a discussion of the key problems), so I reject this stark conclusion—it would be premature to abandon all hope of chasm breaching. Instead, a "softer" interpretation (an interpretation Kuhn himself offered in the Postscript in Kuhn, 1970, but which near the end of his life he seemed to renege) can advance our understanding of the situation we find ourselves in when discussing education research; and with a little latitude this softer approach does not necessitate that we abandon the wonderful images that we are living in different worlds or that we are immersed in incompatible modes of community life. In his Postscript Kuhn softened and said that there was a cross-paradigm "communication breakdown" that made discussion and translation of viewpoints *very difficult but not impossible*.

Here are a few points based on the softer or non-Kuhnian reading of Kuhn:

1. In describing our topic at the outset, I drew attention to the fact that I was being rather imprecise—about the number of chasms and about the precise differences that were at stake about education research—not a promising sign for the start of a meaningful discussion. But of course I was being circumspect, not vague; I was trying hard not to prejudge the forthcoming dialogue or to skew it in my favored direction. For when differences are deep, when there are "incompatible modes of community life," even the ways that the issues are phrased are matters of contention, for the rival positions/paradigms/forms of life will use different terminology, and what a person on one side of the chasm will regard as a crisp and well-focused account of the key issues, a person on the far side will judge to be vague and missing the mark by focusing on irrelevancies and chasing after phantasms. So, as we read on, we need to be alert for differences in the ways each author sets up the key issues. If the authors seem to use different language, does this mean they are conceptualizing the issues differently? Or are they really talking about the same thing, but in a disguised way? Alternatively, if they apparently use the same terminology and apparently are talking about the same thing, is this really the case? (Recall: Newtonians and Einsteinians use the term *mass*, but they are not talking about the same thing—they conceptualize the workings of nature in remarkably different ways.)

2. According to Kuhn's account, different paradigms have different foci—the problems they regard as important are different. Indeed, in moving from one paradigm to another there are "Kuhn gains" and "Kuhn losses" as problems and issues move into focus or become unimportant. So, what are the problems and issues that each of the participants regards as important? And bear in mind that differences in focus, differences in the problems, do not necessarily place the respective positions in opposition—after all, I might not be as concerned about your problem as you are, and vice versa; this may make our work orthogonal, but your pursuit of your problem or interest might not be strictly incompatible with my pursuit of mine. To put it another way: Your participation in the mode of life of your community does not bar me from participating in the life of my community (unless, of course, it is a tenet of your community's mode of life that my community should not be allowed to coexist!). This leads directly to the following point.

3. Kuhn himself stressed that it cannot be said that one paradigm is right and others are wrong, for judgments about such matters are always made from within a paradigm; there is no nonparadigmatic standpoint from which we can judge that Paradigm 1 is right and Paradigm 2 is mistaken. In these respects paradigms are very much like games; indeed Wittgenstein (in a work that Kuhn was familiar with) referred to different "forms of life" as being different "language games," and he said that a language game "has not only rules but also a *point*" (Wittgenstein, 1953/1968, remark 564. It is to be noted

that his “games” are not trivial things but include doing experiments, writing novels, praying, doing arithmetic.) While there is no sense in asking which is the “right” language game or form of life, one can ask what the point of each language game or endeavor is, and you can say that you do not choose to play *that* game because you don’t appreciate its point (although often the point is not fully understood until you actually participate in that game or form of life); or perhaps you don’t play it because you prefer the point of a different game. (Similarly, one can decide that a community’s mode of life is attractive, while that of another is not appealing.)

There is a technique that I have found helpful over the years in clarifying what the point is of various intellectual activities, and which might be helpful as we read the statements by the authors in the dialogue. It is to ask (or to imagine asking), “What is it that the supporters of this game or approach most fear or detest and that they wish to guard against?” For example, the thing I wish most to guard against is the human tendency to indulge in wishful thinking, to grasp at straws, to delude ourselves—to adopt a position or a solution to a problem on weak or spurious grounds simply because we devoutly wish for this problem to be solved.

4. It also follows that *if* you are playing one language game (LG1), you have to play it by the rules of LG1 and not attempt to play LG1 by the rules of LG2. As Wittgenstein (1953/1968) pointed out, “The game is supposed to be defined by the rules” (remark 567). So, are the speakers clear about the game or form of life that they are engaged in, and are they being consistent and not claiming to be engaged in LG1 when they are actually engaged in LG2?

Thus, to give an example, the point of the “natural science game” is to establish bodies of true statements (knowledge) about various aspects of the natural universe, and the rules specify that knowledge claims be accepted on the basis of reliable, tested empirical evidence and well-formed arguments and chains of reasoning about this evidence and its implications (elsewhere I have called this the “platinum standard”; see Phillips, 2006a). So, if you claim to be playing the natural science game, but you cast aside these rules of evidence and adopt other rules, in what sense are you playing this rather than some other game? For the rules of an activity are *constitutive* of that activity (think of how the rules of soccer define the nature of that game), and if you play by different rules you are indeed playing a different game. A separate but closely related issue—one that might arise in the contributions from the authors below—is whether the social science game is a part of the broad natural science game or whether its rules are different enough that it counts as a game in its own right.

5. Another closely related point is that distinctions that are important to make within one paradigm are not necessarily important—or even sensible—within another framework. For example, in the Western analytical philosophy paradigm wherein I work, the distinction between knowledge and belief is vital: We know some things, and we believe other things but don’t know them (i.e., we know some things to be true, but we only believe other things to be true). But in some other paradigms, this distinction is not drawn—to *know* and to *believe* are

synonyms. This is a clear indication that the different paradigms have “different fish to fry,” have different focal purposes. The issues are not whether a distinction is “the correct one” but whether it is well supported and whether it allows the work of that paradigm to proceed fruitfully, and also, if a particular distinction is opposed or ignored, whether it is allowed to creep in surreptitiously. Almost certainly these concerns will be pertinent in the following discussion.

Round I

What are the touchstones by which you judge quality or rigor in education research (for a single study, a set of studies, or a “field” or community of researchers in dialogue)? What is your chief concern or fear that the touchstones guard against?

Frederick Erickson

I have two criteria for quality in a study. First, is the study technically well done? Second, does it show “educational imagination?” (Here I am paraphrasing the title of the book by C. Wright Mills, 1959/2000, *The Sociological Imagination*.)

For qualitative research, *well done* means the study involved a substantial amount of time in fieldwork; careful, repeated sifting through information sources that were collected to identify “data” from them; careful, repeated analysis of data to identify patterns in them (using what some call *analytic induction*); and clear reporting on how the study was done and how conclusions followed from evidence. For qualitative work, *reporting* means narrative reporting that shows not only things that happened in the setting and the meanings of those happenings to participants, but the relative frequency of occurrence of those happenings—so that the reader gets to see rich details and also the broad patterns within which the details fit. The reader comes away both tree-wise and forest-wise—not tree-wise and forest-foolish, or vice versa.

When I say a study has an *educational imagination*, I mean it addresses issues of curriculum, pedagogy, and school organization in ways that shed light on—not *prove* but rather *illuminate*, make us smarter about—the limits and possibilities for what practicing educators might do in making school happen on a daily basis. Such a study also sheds light on which aims of schooling are worth trying to achieve in the first place—it has a critical vision of ends as well as of means toward ends. Educational imagination involves asking research questions that go beyond utilitarian matters of efficiency and effectiveness, as in the discourse of new public management (see Barzelay, 2001), especially going beyond matters of short-term “effects” that are easily and cheaply measured. In the past 40 years (the duration of my career), too many studies have framed questions in the ways that a temporarily visiting social scientist (dare I say, a tourist?) knew how to ask and answer, with “answers” provided to questions that a parent or student or teacher or principal or superintendent would not ask in the same way—yet those local stakeholders are the people who have to live with the answers, and with the questions, concerning their daily work. This way of proceeding is profoundly elitist, making of education researchers social science mandarins—junior philosopher kings—and I think that is wrong. The two questions that dominate current policy deliberations have become “Is this effective?” and “Is this efficient?” In so

many current educational policy matters, however, a prior question lurks: “Do we want to do this in the first place?” (i.e., Is this a means or an end that is worth pursuing?). Such questions can be addressed indirectly by evidence from empirical study, whether quantitative or qualitative, but ultimately they involve also considering value choices directly: whether to teach for basic skills or for understanding; whether children should be able to use calculators in doing arithmetic or use invented spelling in learning literacy; whether racially segregated schools are inherently demeaning and unjust; whether differential spending levels between schools in high- and low-income neighborhoods are wrong or right; whether charter schools are a potential blessing or a curse; whether mother tongue instruction in bilingual education should be encouraged or discouraged. These and many other questions involve issues of value (and of political interest) most fundamentally rather than of simple instrumental utility. In addition to questions concerning value choices, here are some other examples of questions that I consider rich in educational imagination: “What is it like to be a boy in a bottom reading group in an inner-city classroom?” (First, qualitative researchers would consider what is the lifeworld of a single bottom reading group, focusing on a particular child. Then perhaps later we might ask, “Are there lots of groups like this?” But first we would focus on a particular instance.) “Why do the Black kids and White kids at our high school sit at different places in the cafeteria, and should we care about that?” Research that is rich in educational imagination would address both means and ends of educational provision, and it would include practitioner knowledge as well as expert knowledge in the framing and conduct of inquiry. In my judgment, we don’t yet have good examples of research that does this.

What I fear most is that education research will come to be done primarily by social science specialists prepared in academic disciplines who know little about everyday life in schools and thus are unable to pursue questions of genuine educational imagination. I fear that they would act as social science mandarins with the power to dictate detailed behaviors to teachers, further constraining the professional discretionary authority of frontline school practitioners—teachers and principals—rather than supporting that authority and informing everyday practice in ways that make sense to practitioners. Given the extremely regressive relationship between management and frontline service provision in schools—as I will describe in my second-round comments—I see the trend toward outsourcing education research to a social scientific mandarin as elitist, dangerous, and wrong.

Robert Floden

Discussions about research quality often cast the issues, for rhetorical reasons, in an either/or framework. A scholar, for example, either is interested in understanding the meanings participants attach to particular events or is interested in generalizations about associations among variables. I think making such sharp dichotomous divisions is not helpful. Scholars often have interests in mixtures of meaning, events, behaviors, actions, and causes. The quality of research encompasses multiple dimensions, with gradations of quality, rather than classifications as good or bad.

The phrasing of the first question posed in this dialogue uses *quality* and *rigor* as though they were interchangeable terms. But

they point to distinct dimensions of evaluation. A judgment of quality, for example, may include assessing whether or not a study addresses a question of broad interest and social significance. A study might be rigorous in the sense that it uses a design that guards against many threats to validity yet be of low quality because the question it addresses is trivial.

My first touchstone for the quality of *individual* studies is the clarity of the central terms used in describing the focus of the research and its conclusions. This touchstone echoes what Moss and Phillips have said about the importance of listening and trying to understand one another. Ambiguity or unclarity about central terms leads to reduction in the quality of a study. One example of the way that unclarity erodes quality comes from research on alternate routes in teacher education. What do we make of the expression *alternate routes*? Studies that make general claims about the impact of alternate routes on teacher quality mislead when they fail to specify what counts as an alternate route. That label might be attached to anything from changing the schedule of a university’s well-established program (e.g., to offer courses as intensive weekend workshops) to eliminating the requirement for any form of practice teaching, as in certification through the American Board for Certification of Teacher Excellence (www.abcte.org).

A second touchstone is that the research must address a question or issue that is important to scholarly knowledge or to policy and practice, or, preferably, both. (For a general discussion of the importance of research that makes both practical and theoretical contributions, see Stokes, 1997. For a historical account of the importance of addressing both in education research, see Lagemann, 2000.) If the results of a study will neither make a substantial contribution to the body of scholarship nor support improvements in policy or practice, the study is of low quality. I do not mean to imply that all questions can be divided into those that are important and those that are not; the importance of questions varies on a continuum. My point is that judgments of research quality should consider the topic addressed, rather than looking only at the rigor of methods used.

A third touchstone is that the study’s report must make a strong chain of reasoning from previous literature and new empirical results to the study’s conclusions. Once again, the strength of a chain is a matter of degree rather than a dichotomous judgment. Although this criterion for quality is important, giving it too much weight may restrict the significance of the question addressed. In the discussion section of a journal article, scholars often describe the ways in which their study addresses issues of major importance. When conclusions are extended to broader populations and contexts, the significance of the question increases, but the strength of support for the claims weakens (Cronbach, 1982).

As scholars move from *individual* studies to *sets* of studies, it is important to make explicit the way each study extends the reach of the work to new populations, new measures, different times. For the research community, it is important, for clarity, to work toward sharing instruments across studies. The quality of research done by a research *community* is enhanced when that community agrees on how it will measure the features or variables central to the work.

The three touchstones I describe correspond to three concerns that I have about many studies in education research. First,

research reports use key terms without making clear how their usage compares with the other ways in which the terms are being used. I am especially concerned when a study uses newly devised methods of measurement, with little basis for understanding how the new method compares with methods used in other studies. Such proliferation of idiosyncratic measures makes it difficult to look across studies and think about how they fit together. Second, reports often do too little to show the reader why the question or problem addressed is important. An aspect of the quality of research reporting is communicating how and why the reported research matters. Third, many reports provide insufficient evidence to allow readers to evaluate the chain between the evidence and the conclusions. Without such evidence, readers cannot judge this aspect of research quality.

Patti Lather

My interest is in empirical work that challenges the orthodoxies of what it means to do research of use in policy arenas, work that interrupts standard ideas of expertise and fosters democratic practices. Such work questions the sort of rationality, common sense, and ideas of causality and evidence that underwrite both the gold-standard proponents of experimental research and those who position themselves as the rational center, bridging extremes of both right and left. Exemplified by the 2002 *SRE* report, such folks had seen their role as providing a service by intervening against the very narrow governmental definition of science (which had been arrived at by a congressional staffer after consultation with a few websites and cognitive psychologists, as Margaret Eisenhart and Lisa Towne [2003] tracked down in their report on the genesis of this definition of science).

The umbrage taken at the umbrage taken on the part of qualitative researchers who felt marginalized by calls for a “common language” and “unity of purpose” disallows the incommensurability at work in this struggle over science, which has to be seen for what it is: power struggles over who gets to set the terms of debate and what it means to court interruption/counternarratives as a move toward better work all the way around. This is about difference, not sameness, and surely endorses the sort of “epistemic reflexivity” that Pamela Moss has long called for (Moss, 2005a, citing Bourdieu & Wacquant, 1992). Qualitative and quantitative researchers, or positivist and interpretive/critical/post, or *exact* and *conjectural*, to use Husserl’s (1970) terms, all would need to figure out how to work together in ways more numerous than, and different from, the descriptive/causal relation that *SRE* articulates.

Given the multiplicities and proliferations of ways of doing education research in tension with both a resurgent positivism and calls for a set of principles to unite scientific practices, my quality touchstones are based in my conception of validity in education research. Validity has to do with how scientific knowledge is made credible. My argument is that the very calculus of credibility has shifted across time, place, and various fields where the traditional foundations of knowledge are challenged by an epistemological indeterminacy that weakens any “one best way approach” to claims of quality and rigor (Lather, 2001; Lincoln, 1995).

That the “science” or “validity” or even “usefulness” generated from this position is hardly recognizable as such by those from positions of “the received view of science” is no surprise. The key is that what is being contested is not science or rigor or even evidence-based

practice but, rather, orthodox views of such matters. Postepistemic, socially grounded practices, rather than exhausting the problem, exemplify how any criteria are situated, relational, temporal/historical. Hence validity is being used to further change the terms of the legitimation of knowledge by loosening positivism and suggesting the critical potential of validity to put under theoretic pressure the claims of a narrow scientism.

Given this positioning, my quality touchstones are three. The first one is that regardless of paradigm I look for research that has some sense of the history, sociology, philosophy, ethics of inquiry, and what might be called a rigor of reflective competence. I’m very interested in redefining rigor so that it includes a certain sense of the history and philosophy of research, so that people can situate what they are doing within these more foundational discourses. Second, as opposed to assuming a unified idea of science and homogeneous standards, I’m interested in quality indicators that fit paradigmatic assumptions, across the spectrum from earlier naturalistic and constructivist paradigms to discourse theory, ethnographic authority, critical, feminist, and race-based paradigms and more recent poststructuralisms. Validity in qualitative research ranges from correspondence models of truth and assumptions of transparent narration to practices that take into account the crisis of representation. Some scholars call for new imaginaries altogether, where validity is as much about the play of difference as the repetition of sameness (Scheurich, 1996). From such a perspective, validity is recognized as part of the power and political dimensions of the demarcation of what is and is not science, what is “good” science, and who gets to say. As such, validity is far more about deep theoretical and political issues than about a technical issue or an issue of allegiance to correct procedure.

Third, I’m interested in practices toward quality that move us toward a science more accountable to complexity that might result in a less comfortable, less imperialist social science that courts unknowingness, fluidity, and becoming: the science possible after the critiques of science of the past 40 years. Is it imaginable that a more complicated view of science might become widely disseminated as part of a broad-based and fundamental questioning of foundations? If Wall Street expertise seems to have clay feet, can scientifically based practice be far behind? If climate scientists understand the enormous uncertainty of nonlinear systems within which decisions must be made and urge what they call “caustic honesty” about what we know and don’t know (Revkin, 2009), can the Institute of Education Sciences (IES) continue to argue that its “strategic” success depends on its “yield” of “research proven interventions” of the one-size-fits-all model (Whitehurst, 2008)?

My chief fear is obvious, I think, in what I’ve already said: that one-best-way thinking, especially as endorsed by governmental force, will continue to enact the management, containment, and marginalization of both qualitative research and, more important, research that can make a difference in improving our schools. I’m not overly enthusiastic about what I see coming down from the Obama government in that respect. Diane Ravitch, with whom I am usually not on the same page, has called Obama’s education policy “giving George W. Bush a third term” (Robelen, 2009). The continuation of this sort of instrumentalism, with its decontextualized “what works” and claims of value neutrality, is resulting in a “disappointing yield” in terms of the no-effects of experimental design studies that are

starting to come in, one of which cost \$14.4 million (Viadero, 2009b). But most interesting to me are statements from Whitehurst and others that “more luck” is likely “with the next cohort” of these large-scale studies. This reminds me of the mainstream economists in the *New York Times* who, rather than face the limits of free-market theories, mathematical models, and the rationalities of “Economic Man,” announced no interest in heterodox economic theories (Cohen, 2009). Perhaps if skepticism about “methodolatry” were seen as the better part of rigor, Reading First would not have laid its \$6 billion bomb (Zuckerbrod, 2008). But it is hard to be optimistic that we may, finally, be ready to face what Fred Erickson notes as the “repeated failures to deliver on its own promises” of the “pseudo-science” at the root of the “gold standard” approach. Pseudo-science retains its “silver bullet” hold over our political imaginary in spite of the failures that result when the social sciences fall into the “physics envy” of a mathematized knowledge, making a virtue of the general, the invariant, and the aggregative.

I’m worried also about the triumph of standardization in the name of rigor, and I’m particularly worried about what we in the qualitative community will do to ourselves in that respect. I see a lot of folks who live on the same planet I do who are rushing around trying to create standards because they’re worried that if we don’t do it to ourselves somebody’s going to do it from the outside. And finally, I’m worried that we’ll miss the opportunity to move away from an imperialist science and toward a science capacious and democratic in its recognition of necessary contingency as the horizon of our intelligibility.

Barbara Schneider

It seems to me that one point of consensus within the education research community is a sense that high-quality research can take multiple approaches. This is a positive in that it allows for a wealth of information to help solve pressing problems. On a more negative side, the standards for rigor (beyond some universals for differences in approaches) are varied and not shared. In other disciplines, as Lamont and Mallard (2005) have argued, there is some—although not universal—agreement on the dominant research paradigms and what constitutes quality. This is not the state in education research and the result is considerable diversification and often hostility.

With respect to where I am willing to judge quality, it is on studies where the issues pertain to estimating causal effects and those that seek generalizability. With respect to causality, as I have argued in *Estimating Causal Effects Using Experimental and Observational Designs* (Schneider, Carnoy, Kilpatrick, Schmidt, & Shavelson, 2007), the overriding standard for estimating causal inference is the randomized clinical trial (RCT) where researchers have to be concerned with causal relativity, causal manipulation, temporal ordering, elimination of alternative explanations, sampling imbalances, specific versus average effects, and atypical responses. There are also issues in fielding such a study, such as breakdowns in randomization, treatment noncompliance, and attrition. With respect to generalizability for studies examining associative properties, I consider whether the conclusions deal specifically with population estimates that are defensible against issues of selection and whether there are appropriate caveats with respect to causation. In both instances the underlying issues are

that these are studies of merit, on important issues that have face validity as well as internal validity, and are unlikely to result in Type I or Type II errors. For studies with designs less familiar to me (and as an editor or reviewer on panels this often happens), I rely heavily on my colleagues for their advice with respect to design, and many of the people of this panel.

Round 2

Where do you see challenges to your perspective in the perspectives of other members of the panel? How might your perspective evolve to respond to those challenges? Given all of this, what are the implications for the preparation of education researchers?

Frederick Erickson

I think all the panelists would agree that high-quality education research would be high in technical quality. Schneider’s statement is a fine example of arguing for technical quality in her approach to research, and Floden’s statement also mentions matters of technical quality at various points. Many would agree that research questions should have genuinely educational significance—Floden said so explicitly, and he thinks that a trivial topic studied rigorously is not high-quality research. I doubt that all on the panel would agree with my definition of significance as “educational imagination,” but it’s possible that many on the panel would agree with much of what I said about a need for better, more genuinely educational imagination in education research.

The framing statements by Phillips and Moss, as well as the statements by Floden and Lather, make it clear that there are major differences in kind across the different approaches to education research that we on the panel represent, whether empirical or philosophical. These are not differences about method per se, but about presuppositions. I think the real differences are not most essentially between “left” and “right” (as Phillips characterized them), although I’m happy to be considered left. Nor are the real differences those between quantitative and qualitative methods in empirical research. The differences are much bigger—they do involve paradigms, as Phillips and Moss make especially clear, and something prior even to paradigms. An appropriate metaphor could be the elephant—an entity that is both huge and unspeakable (as in the elephant in the living room in a dysfunctional family).

The first elephant is metaphysical—a matter of ontology (what we believe truly exists in the world) and how two distinctly different approaches to social research follow from such beliefs, which are axiomatic, resulting in discourses that are indeed incommensurable. The second elephant is historical—a matter of the distinctive institutional organization of American schools as it developed at the beginning of the 20th century and how “what works?” inquiry follows from it.

My third point concerns fundamental assumptions about the nature of social life. The reason social science has suffered from physics envy is the assumption that the social world is basically like the natural world. What makes physics and chemistry work is an assumption of the uniformity of nature—a unit of force, or of heat, or a chemical element is the same in Britain as it is in France or on the face of the moon or in the most far-flung galaxy. Empirical evidence appears to bear out this assumption in natural science research, supporting generalization of findings from the

investigation of small samples and the identification of general laws, from which a high degree of prediction and control is sometimes possible—not always but relatively, compared with what we see in social scientific research. Hence the physics envy among social scientists and among social scientific education researchers. Even in biology a natural science model for research works, although not as well as in physics. In the 19th century, as the social sciences were developing (looking over their shoulders at the mathematical physics of Galileo and Newton), there was a serious argument over whether social sciences should model themselves after the natural sciences or try for something else. (An ancestor of the natural science model for social science was Comte, among others, aiming for a social physics—the discovery of general laws and for prediction and control akin to that of physics. For the other approach an ancestor was Dilthey, among others, aiming for understanding [*verstehen*] rather than proof and prediction.) Adherents of what became the meaning-oriented approaches to social inquiry, the hermeneutical approaches described by Moss (2005b), took a position that meaning differences made such a difference between one social setting and another that there was in effect a *nonuniformity of nature in social life* (as I called it in my 1986 article on qualitative research on teaching; see Erickson, 1986). The notion was that it is *local meaning* that is causal in social life, and local meaning varies fundamentally (albeit sometimes subtly) from one setting to another. One of the consequences of this notion is deep distrust of the possibility of any generalization at all in social research, a position that Floden very properly says that I took in my 1986 article. He knows, having been formed in philosophy, that there is a big difference in ontological assumptions between the “natural science” and the “human science” practitioners of social inquiry—and Phillips knows this too—even though I would guess that neither Phillips nor Floden agrees with the line of argument begun by Dilthey. Yet lots of education researchers who work within the dominant natural science paradigm for social science don’t even realize that there ever was a serious, intellectually respectable alternative proposed to a natural science model for social (and education) research. They haven’t read the intellectual history of social inquiry.

A good deal of the talking past one another over methods, for 30 years now, comes from that lack of awareness on both sides of the chasm. Close descriptive study of a setting, based on extended participant observation and interviewing, doesn’t try to generalize directly from that setting to others—and the “replicability” that *SRE* called for as part of a seamless approach to scientific inquiry rides roughshod over what is actually a big seam in assumptions—an arroyo—a great divide. If the meaning-oriented assumption about the nonuniformity of nature in the social world is right, then for education, what happens in Miss Smith’s first grade is fundamentally different as a local ecology (subtly different, despite surface appearances of similarity) from what happens in Miss Jones’s room across the hall in the same school building. (Parents know this—that’s why they fight to get their kids into Miss Smith’s room, away from Miss Jones.) Nor is what happens in Miss Smith’s room quite the same as what happens in Miss Robinson’s room in the next school district. It follows that policy evidence for “scaling up”—trying to get everybody to adopt “best practices”—no matter how well produced technically—just doesn’t tell us what we need to know as educators. Best practices, as specific behaviors, don’t

travel intact across the hall in one school building, let alone across the country.

My fourth point concerns the other elephant in the living room: our American institutional history of mass public schooling. At the turn of the 20th century, school organizations adopted the same asymmetry between administrators and teachers that industrial organizations had done—in separating the work of managers from workers. (This is not to say that schools became factories—that’s too strong. But the labor–management relations in schools became factory-like.) Management made the strategic decisions, the think work, and the assembly line workers did muscle work, carrying out the decisions made by managers. White-collar people got college educations, while blue-collar workers got elementary school educations. If we look at American schools on a worldwide scale of contrast with schools in other economically developed societies, American teachers receive less academic preparation for hiring and, once on the job, are given less discretionary authority over the conduct of their work—pedagogy—than any other teachers in the world. In other words, the labor–management relationship in American schools is incredibly regressive—and what teachers themselves know as daily workers is undervalued. American education research, to the extent that it addresses practitioners at all, addresses the “management” side of American schools—and then addresses the legislative and executive governmental policy makers that govern the schools. Research for determining “what works” and for “scaling up” makes sense for those research audiences. But it is complicit with a system of domination and infantilization of teachers. And a further problem is that no actual person teaches children in general; we teach Sam and Mary, here, now. Some general knowledge (what Flyvbjerg, 2001, in a compelling essay, notes that Aristotle called *episteme*) may in part inform what we ought to do with Sam and Mary, but on a daily local basis, it’s particular knowledge of the situation at hand that we really need in order to act prudently, wisely (what Flyvbjerg notes that Aristotle called *phronesis*). Some of those particulars Miss Smith already knows as practitioner knowledge. Some more of them she could get for herself by doing her own qualitative practitioner research on her specific teaching situation with Sam and Mary. She might also get some insight from knowing how other teachers solved their teaching dilemmas in other local places. But she would not be importing knowledge from other places for the purpose of imitating others—“being scaled up”—since local practice is ultimately inimitable, necessarily reinvented locally in each new occasion of practice.

My fifth point (time for an ecumenical gesture) is a response to the question, How might your perspective “evolve” to deal with the challenges from other approaches? I don’t think that question fits what I’ve said above. It seems to me that the natural science model (*Naturwissenschaft*) and the human science model (*Geisteswissenschaft*) for social and educational inquiry start from such differing presuppositions that neither can “evolve” into the other without losing its distinctive edge, its affordances. (That is why I don’t like the term *mixed methods*, which looks to me like an ice-blended smoothie. I prefer the term *multiple methods*—different tools to do different jobs.) To put it another way, when you are counting apples and oranges it doesn’t make good sense to try to find the mean; it makes more sense to say, “We have two different sets here,” not blaming the apples for being defective oranges, or vice versa—and not trying to throw both into a blender in order to make a smoothie

that would destroy the integrity of both the orange and the apple.

A way to respect the reality of a chasm—if it is truly a line of genuine difference—is to try to figure out how to affirm both sides of it, both extremes. That is what *SRE* and the federal Department of Education did not do recently—at best *SRE* attempted a smoothie; at worst the Department of Education said, “You can only have apples. No oranges need apply.”

But what if Comte and Dilthey were both half right? (I prefer to think that Dilthey was 75% right and Comte 25% right, but you get the point.) Are there some ways in which humans can appropriately be considered to act like atoms or electrons? Today at rush hour I drove through what is supposed to be the busiest intersection in the United States—Wilshire Boulevard and the interchange with the 405 Expressway in Los Angeles. A good study of traffic flow in that place could consider the movement of cars as if they were electrons in a stream. Such a study would need to monitor the occurrence of cars in the intersection accurately and perhaps also monitor the speed of individual cars, analyzing variations in occurrence and relative speed in complicated ways. Such a study would not need to consider the particular subjectivities of the drivers, nor would it need to consider how the dynamic ecology of traffic flow that the drivers produce together is continually shifting from moment to moment. Such a study might try to generalize from Wilshire and the 405 to other places, or it might, as an engineering study, just concentrate on that one location. On the other hand, if I wanted to teach somebody how to drive through that intersection without hitting somebody else or getting hit, the knowledge I would need would be very different from that of the traffic flow analyst—and very relevant for my teaching would be insight into the phenomenology of *being a driver there in rush hour* as well as into the ways in which drivers create an ecological environment for each other within the ongoing course of their driving—and how a driver needs to deal with that.

By temperament, I like affirming both sides of a chasm, taking advantage of contradictory truths without trying to erase the contradictions. It seems to me that education research, like social research more broadly, needs to get smarter about which aspects of social life can be appropriately studied as if people were atoms or quarks, or antimatter, and which need to be studied in ways that are radically different from attempts at a social physics because what must be understood and illuminated through narrative description is the lifeworld of their daily practice. To say, “Is this topic appropriate for study from a natural science approach or a human science approach?” is different from saying “Should we use inferential statistics and a randomized field trial or ethnography?” Focusing on the reality of the chasm foregrounds the questions of ontology that I see as fundamental and prior to choices of methods: What is this piece of the world like that we want to study, and for what uses do we want the knowledge that our study might produce?

Could we teach graduate students and beginning assistant professors to think like that? Maybe, but you have to know a lot—about intellectual history as well as research procedures—in order to do so. And what about our publics—the parents, teachers, superintendents, legislators, and governmental executives? The small *d* democrat in me thinks they are capable of a wider repertoire of

questions than just “what works?”—with answers coming from social scientific mandarins. Can we talk with our publics about this in ways that are clear and persuasive and respect their capacity to think? Or as Rodney King said after the Los Angeles riot, “Can we all get along, can we get along? . . . We’re all stuck here for a while. Let’s try to work it out.”

Robert Floden

I agree with Erickson that education research will seldom support rigid prescriptions for practices. I also agree that policy makers and administrators often create rigid prescriptions, claiming that their policies are based in research. That quick step from a tentative, nuanced research conclusion to a narrow rule for action gives research a bad reputation and may lead to bad practice. The transformation of research results to bad prescriptions is a problem of long standing (e.g., Fenstermacher, 1979; Floden, 1981), which has led some scholars to be overly disparaging of the contribution research can make to practice. (For comments on both sides of this issue, see Floden & Klinzing, 1990; Lampert & Clark, 1990.)

I think, however, that Erickson makes the mistake of moving from noting the problems of rigid policy formation to thinking that researchers should avoid trying to answer questions about cause and effect. Causal questions are a critically important area for education research. Much of education research, though not all of it, is aimed at understanding cause. When educators want to know what aspects of classroom activity affect student learning, they want the answer to a *causal* question. The causal linkages may not be simple or mechanistic, but if there were no causal connections, there would be no way for educators to enhance learning. If a researcher did not think there was some connection between what teachers do and what students learn, you would wonder why they would be interested in studying teaching and learning. Among our dialogue participants, my conviction that causal questions are a central part of education research puts me on the right, in what Lather calls the “rational center.”

But I also am convinced that differences of power, especially between policy makers and education practitioners, play a major, though sometimes hidden, role in determining which conclusions from research come to be used to guide practice. Erickson’s criticisms of current uses of research stem from the role that power differentials play in the uses of research knowledge. My recognition of the role that power plays in linking research and practice keeps me toward the political center of our dialogue.

I see two additional challenges to my perspective in the writings of others on our panel. First, some panel members come close to asserting the impossibility of reaching any conclusions that go beyond the particular case. This position might lead some to say that learning from looking across studies is inherently impossible. I agree that education research will seldom support rigid prescriptions for practice, but I still say that research can support general advice, that some general directions are better than others. If research is of high quality, it will be careful to frame conclusions that are clear about the range of applicability. (This follows from my touchstone about the strength of the chain of reasoning leading to a study’s conclusions.)

Second, some of the panelists give more emphasis than I do to the role of social and political factors in education research. I find

this a helpful corrective, reminding me that such factors influence the questions deemed significant, the ways in which key terms are understood, and the tolerance for some weaknesses, rather than others, in the chain of reasoning leading to conclusions. I think some of our panel members go much too far with this line of criticism, but they remind me to be modest about the achievements and possible achievements of education research.

The preparation of researchers should attend, among other things, to their care in statements of conclusions and their recognition that policy makers and practitioners will often interpret conclusions as more general and prescriptive than the researcher intended or stated. With sufficient effort and understanding of the workings of schools, professional developers can create sophisticated methods for helping teachers thoughtfully use practices from research. The Institute for Learning, for example, has developed sets of materials for helping educators understand principles of “accountable talk,” patterns of classroom discussion in which educators and students bring out the reasoning behind claims rather than learning only the claims themselves. Research supports the impact of accountable talk on student learning (Michaels, O’Connor, & Resnick, 2008). Even with a well-thought-out approach, professional developers must be vigilant that principals and teachers do not slip into thinking that the goal is to use the catch phrases (e.g., “I disagree with what you said because. . .”) a fixed number of times in every lesson.

Thus, as part of their doctoral preparation, education researchers should learn about the ways in which practitioners may, perhaps often do, simplify research conclusions. Researchers should learn that those closer to the world of practice—teachers, principals, policy makers—may take conclusions from research that the investigator had not intended to draw. Teachers and administrators are hungry for suggestions about what they might do that would help their kids learn more. If researchers want to avoid having their work turned into the rigid prescriptions that Erickson describes, they need to figure out ways to communicate what they have learned in ways that take account of what knowledge users want (Floden, 2006).

Patti Lather

SRE aggravated the divisiveness among education researchers by raising the stakes. For me, this was not so much about money or grant funding as about training education researchers; so, in terms of challenges to my own perspective, let me begin there. When teaching education researchers, proliferation is a good thing, in my book; noncontainment is the mark of energy. I’ve termed it “wild profusion” (Lather, 2006) in an article on teaching research methodologies that defy homogenization and standardization—this is where I see whatever hope there is for education research being “accountable to [the] complexity” that is the state of teaching and learning in and out of schools. Incommensurability is a good thing, as is escaping normativity. And I’ve spent no small amount of time articulating “quality criteria” to warrant such “new paradigms” work (Lather, 1993).

But what Elizabeth St. Pierre (2006) calls “conventional interpretive methodology” might play an increasing role if we qualitative researchers can, indeed, learn from Fred Erickson’s cautions regarding not pushing the wild profusion of qualitative methodologies

at the expense of practices more intelligible to received ideas of science. I think here of the National Science Foundation’s (NSF; 2004) *Workshop on Scientific Foundations of Qualitative Research* and its 2009 *Workshop on Interdisciplinary Standards for Systematic Qualitative Research* (Lamont & White, 2009), which I have my students read to “get smart” about the grant writing that I am trying to encourage for qualitative folks. This might be exactly the time to help students learn to negotiate both “your father’s . . . qualitative research” (Lather, 2004) and the more postfoundational variants that have long been my interest. This is what I would hope for in the training of education researchers: to be able to negotiate across standard procedures from many paradigms to engage with the uncertainties of knowledge toward more nuanced thinking.

As an aside, the NSF monographs might be good reading for Grover Whitehurst, whose November 2008 report to Congress situates qualitative research outside the bounds of science and, quite fascinatingly, finds “qualitative research grounded in postmodern philosophy and methodologically weak quantitative research” to be “the dominant forms of education research in the latter half of the 20th century” (p. 5). As St. Pierre (2009) writes, “Who knew” that postmodernism had achieved such “ascendance” (Whitehurst’s term)?

I would hope that the seductions of “being of use” to policy makers will not catch qualitative researchers in the “top-down” directive approaches and state instrumentalism that “make contract workers of us all” (Ozga, 2000). Bob Floden’s points on political theory are well taken: Students need to understand issues of state formations, governing mentalities, or “governmentalities” to use Foucault’s term, and issues of democratic capacity building and citizen engagement as part of their methodological training. This is especially so for those with interests in being of use to policy and practice; and, given the “policy turn” and the push of the “engaged academy,” isn’t that all of us now?

Regarding the war-zone atmosphere of irreconcilable differences on the nature of knowledge formation and its uses, is it science war forever? Is a return to Gage’s “détente” possible after this round of No Child Left Behind and *SRE* inflammation? Is there some more productive space than détente? Amid the anxieties that follow the collapse of foundations, the reassertion of objective truth and value-neutral facts, the imposition of standards, the quite predictable push-back on the part of those marginalized by such, how do we train researchers to find a fruitful place to work? In the constantly changing landscape of education research, the application of technical methods and procedures will hardly suffice, IES notwithstanding. After almost a decade of overblown rhetoric on how scientific rigor will heal our schools, how do we make productive use of being left to work within, against, and across traditions that are all positioned within a crisis of authority and legitimation that goes well beyond the academy?

Words if not worlds apart, as much as Denis Phillips and Barbara Schneider and I seem stuck in our differences, perhaps this is our shared space: now that the “cheerleading for experimental design” (Lather, 2008) seems to be abating, replaced by a focus on “translation” (Brabeck, 2008) and, even more recently, “innovation” and “entrepreneurship” (Viadero, 2009a), what new closures and pieties await us? If, to use St. Pierre’s (2009)

words, we stop trying so hard to be hard, can we change the terms of the debate that we have both inherited and contributed to? How can we keep moving in order to produce and learn from ruptures, failures, breaks, refusals? Can we be of use to one another in this endeavor?

Barbara Schneider

I recently completed a chapter on assessing quality in educational journals, where I show that essentially the education journals of AERA in the ISI databases resemble other natural and social science journals, and their problems are much the same as those of journals in other fields (Schneider, 2008). However, in the field of education research, there is a numbers problem, in that there are more education researchers than researchers in other fields, as well as many more journals. The sheer number of people in the education research enterprise raises questions about its quality. How can so many people all be engaged in high-quality work?

What is unique to education is its eclectic epistemological style, and this may be contributing to the notions of poor quality in educational research. There has been a shift from my perspective on what constitutes quality in education research, and I have argued that the recent calls for more studies seeking causal inference, accountability, and other standards of quality reflect a change in a scientific intellectual movement, defined by Frickel and Gross (2005). As with most shifts in intellectual focus, there is likely to be dissent and resistance. Some scholars have argued quite vehemently that the current emphasis on scientific principles in education research is constraining originality and eliminating certain paradigms as being valuable for understanding problems in education (and many of these arguments have been made by people on this panel in the response to *SRE* in *Qualitative Inquiry* [2004] and *Teachers College Record* [2005]).

I believe that the most reasonable way to deal with the issues of contentiousness within the field is to begin with microlevel solutions, which may lead to a more unified understanding of what is high-quality research in education. What I consider to be microlevel solutions is evident in AERA's initiative to establish standards for publication in its journals, organizing different scholars to serve on the committees and addressing different paradigms. The movement toward structured abstracts in AERA and NSF, and the efforts on the part of professional associations to take on a more directive role in the training of editors and peer reviewers, all help to create a consensus for what constitutes high-quality research and scholarship. Will these microinitiatives dramatically change impressions of the quality of education research? Probably not in the short term, but if such initiatives continue, like this panel, and the field itself continues to take on the roles of questioning, fostering dialogue, and building consensus on small steps, the future may be more promising than some have envisioned.

Issues, Accomplishments, and Implications

As co-organizers of this dialogue, we (Phillips and Moss) worked to frame it in a way we hoped each participant would find congenial, to allow us all to share perspectives in our own terms. Our opening comments reflect this stance—but not the extended e-mail dialogue that produced it! In our closing comments, however, we have chosen to reflect on the preceding dialogue in a way that reclaims more of

our individual perspectives—and friendly disagreements—nevertheless honoring our common goal of learning from our differences.

Denis Phillips

In the opening pages of this “dialogue across differences,” reference was made to my recent attempt (Phillips, 2006a) to locate the adherents of different contemporary education research frameworks or paradigms on a “left–right” continuum. Our discussion has revealed, however, that the situation is much too complicated to be represented in such a one-dimensional way. All of us are close in some respects (but in different respects), and we all differ in a variety of ways. Overall, T. S. Kuhn's account seems to have been borne out—“proponents of competing paradigms must fail to make complete contact with each other's viewpoints,” for they always are “at least slightly at cross-purposes. Neither side will grant all the nonempirical assumptions that the other needs in order to make its case” (Kuhn, 1970, p. 148).

In the following brief remarks I attempt to highlight what I see as the main differences between the positions of the other individuals on this panel and my own, referring back to Kuhn when appropriate. As I find Bob Floden's remarks quite conducive, I will focus on the other three members of the panel. (My co-organizer for this dialogue, Pamela Moss, is exceptionally ecumenical, but basically I see her as holding a position not unlike Fred Erickson's; however, as she was not formally one of the four respondents to our questions, I will not include her in what follows.)

It seems clear that Barbara Schneider is located in a traditional, technical social science framework; her account of rigorous work is one that is familiar to us all. I think it would be unfair to claim that she does not regard it important for education research to be relevant and useful; it is simply that—like many traditionalists—she foregrounds methodological precision as the “sine qua non” (to borrow the expression used by Campbell and Stanley many years ago to characterize the internal validity of an experimental study). I see a gap, but no unbridgeable incommensurability, between our respective views here; there is no reason for me to abandon hope that in the future I will be able to convince her—and like-minded colleagues—that paying specific attention to the social importance and other value dimensions of research projects, when assessing them, can be done without compromising their technical standards. Whether these standards themselves can be broadened or abandoned, as Patti Lather has suggested, is a possibility on which Barbara Schneider herself seems to have closed the door—some Kuhnian “arroyos” are simply too wide to cross.

Turning to Fred Erickson, I must stress that I resonated with everything he wrote in his two statements except for one central point. He characterizes the natural and the social sciences in such a way that it is inevitable that there is a chasm between them, and he deplores those who—suffering from “physics envy”—attempt to produce social science (and more particularly education research) that has the same foundational assumptions as physics. I, too, have been busy attacking physics envy, but I have adopted a different strategy: Instead of seeing an enormous gap and insisting that physics must stay on its own side of the chasm, in essence I have been pointing out that, in one respect of paramount importance, there is really not much difference across the arroyo. For

following John Dewey and many others, I hold the view that *inquirers in all fields*, if they are to be productive and produce results that are *warranted*, must use methods of inquiry that are effective. This is a point that John Dewey (1966) emphasized in his book *Logic: The Theory of Inquiry* and which he illustrated with homely examples; he wrote that “authorized conviction” comes from “competent inquiries” (a thought that Fred Erickson himself echoed in the first of his contributions) and added that “we know that some methods of inquiry are better than others in just the same way in which we know that some methods of surgery, farming, road-making, navigating or what-not are better than others” (pp. 8–9, 104). From this perspective, it is a mistake to suppose that physics has a monopoly on competent, effective inquiry.

While the physical sciences have had a remarkable record of success in producing warranted knowledge about the natural world, to make them the standard-bearer is, first, to denigrate other successful and rigorous realms that use (and *must* use) different methods and quite different explanatory concepts—of course I have in mind the biological sciences, geology, and even at least some portions of cosmology. This is why I am not persuaded by part of Fred Erickson’s argument; certainly the human sciences use concepts that are quite different from those of physics, but so do chemistry, evolutionary biology, and geology. A central place in many social sciences must be given to the values, situational analyses, and cultural understandings of the individuals whose actions are being studied (Phillips & Burbules, 2000). What is crucial is how such studies are carried out—studies using these concepts need to be done competently. But I need to stress: “Science” cannot be defined in terms of the use of certain concepts, for if it were, many domains that now are widely agreed to be sciences would be removed from this category. But neither can “science” be defined in terms of the use of particular specialist methods (such as the use of randomized experiments—which, by the way, were not even used by some of the great contributors to physics such as Galileo, Newton, Einstein, and Hawking). Furthermore, I do not accept that the sciences—natural or social—have a single aim (production of causal knowledge in the form of laws that can be the basis for the making of predictions), and I have pointed to the many aims that science (or scientists) have legitimately pursued (see Phillips, 2006b). In short, then, I do not see the chasm that worries Erickson, and I certainly do not see a chasm existing between our respective positions—the differences between the two of us, I believe, are resolvable.

I am less sanguine about my chances of resolving differences with Patti Lather, although of course the point of the exchanges in this essay is not to reach agreement but to achieve a level of understanding. It seems clear that we are operating in different, incommensurable paradigms—forms of life that have different points. As I read her contributions, a key point made by Kuhn came to mind that made this abundantly clear: A major cause of interparadigmatic failure of communication is the fact that key terms (such as *mass*) have different meanings in the disputing paradigms because they are part of different conceptual networks. The passage where this thought arose was her account of “validity,” in which the term appeared to me to have taken on a new meaning; I cite a few sentences as a reminder:

Validity is being used to further change the terms of the legitimation of knowledge by loosening positivism and suggesting the critical

potential of validity to put under theoretic pressure the claims of a narrow scientism. . . . From such a perspective, validity is recognized as part of the power and political dimensions of the demarcation of what is and is not science, what is “good” science, and who gets to say. As such, validity is far more about deep theoretical and political issues than about a technical issue.

Perhaps there is a straw here that it will be possible to cling to: Our two paradigms or “games” may be incommensurable, but it might be the case that they are both worth playing; and although the two paradigms conceptualize their central concerns differently, it seems that there might be some overlap here. Surely research or scholarship done within Patti Lather’s “form of life” has to be done competently or carefully (she has never advocated careless or slapdash work), so that the concerns that within my paradigm are categorized as having to do with validity must surely also be concerns within her paradigm (whatever other name they pass under). On the other hand, the concerns that seem paramount in her framework, about the stultifying, undemocratic, manipulative misuses (as I would call them) of scientific research that she seems to label as having to do with “validity” must surely also be of concern to me and my fellow travelers although of course we will label them differently (as “misuses,” for example). Of course, from Lather’s perspective what I have called “misuses” are more properly identified as necessary consequences of the current practices in the scientific community—and this may well be a residual irreconcilable difference.

Finally, I think it needs to be stressed that researchers in all fields—certainly all of us who have contributed to this essay—are occupied with building competent *cases*, or as Dewey put it, *warrants*. Cases or warrants are chains of argument that competently tie together evidence of different types, and other considerations, in order to support (or reject) some conclusion or hypothesis. Whether the evidence used, or the manner in which it was collected, should be accepted cannot be decided on a priori grounds; rather, it is the way the evidence is defended and used to make a case in the chain of argument that is decisive. In this 150th anniversary of the publication of the greatest book in modern biology—Darwin’s (1859) *Origin of Species*—it is appropriate to note that in the final chapter, in which he summarized the book, Darwin began, “This whole volume is one long argument” (p. 499), and proceeded to outline the remarkable diversity of considerations that he had marshaled together to build his earth-shattering case.

So, to conclude, the issue is not whether Barbara Schneider, Patti Lather, Fred Erickson, or Bob Floden have been doing rigorous science. The issue is: Have they been producing careful, competent cases or warrants for the claims they make? And have they been making efforts to listen to each other?

Pamela Moss

Reading Phillips’s remarks highlights, for me, two differences in our perspectives that I’ll use to focus my closing comments: (a) Erickson, Lather, and I tend to see “chasms” dividing our perspectives on the social sciences, and (b) Floden and Phillips see more common ground (at least in terms of the different perspectives on social science that Erickson and I foregrounded). Further, as I read them, Floden, Schneider, and (arguably) Phillips see it as important to seek and articulate common ground, including common standards,

whereas Erickson, Lather, and I tend to highlight our differences and worry, to varying degrees, about the effects of privileging commonality, especially common standards to which researchers are expected to hold themselves accountable. Why?

While the search for commonality across disciplinary perspectives is a worthy goal, diverse social theorists and philosophers of social science argue that it needn't be the only or even the primary goal of dialogue. At least equally important is the goal of illuminating what "we" who share a common disciplinary perspective take for granted—those often unconscious categories of perception, appreciation, and action (Bourdieu, 1996) that both enable and constrain what we can know. While this is reflected prominently in Lather's touchstones, I can also locate it in scholarship as diverse as the measurement validity theory of Samuel Messick (1989), the philosophical hermeneutics of H. G. Gadamer (1975/1994) or Georgia Warnke (1994), the critical theory of Jürgen Habermas (1990), and the reflexive sociology of Pierre Bourdieu (1991; see Moss, 1998, 2005a, 2005b). If we privilege the search for commonality over difference and disjunction, we risk what Bernstein (1992) calls "flabby pluralism"—"assimilating what others are saying to our own categories and language without doing justice to what is genuinely different" about the other (p. 66)—and we lose an important opportunity for learning.

Floden rightly worries that stark framing of differences risks implying that researchers typically choose one or the other: "A scholar, for example, either is interested in understanding the meanings participants attach to particular events or is interested in generalizations about associations among variables. I think making such sharp dichotomous divisions is not helpful." The point, however, is not so much about either/or choices that different discourses might entail or about what practices are *possible* within a given research discourse or paradigm. Rather, it's about understanding what's emphasized, illuminated, or made more likely; what's relegated to the background as unimportant or impractical; and what the impact of these prevailing emphases is on the actual practices of social scientists and the communities they study and serve (Bernstein, 1976).

Erickson, on the other hand, worries that the call for evolution in our framing of questions sounds unrealistic. And I would agree if the goal were solely to achieve commonality. It's not that I expect Erickson's perspective to evolve into something that looks more like Schneider's, or Floden's like Lather's; rather, what I hope is that each of our perspectives will evolve to respond to the challenges others have raised, whether we agree with them or not. The goal of this sort of multidisciplinary practice is not necessarily to overturn our preconceptions but to illuminate them for (collaborative) critical reflection—to help us understand their social origins and effects, their partial and contingent character, their benefits and limitations—so that they may be either reaffirmed with a new self-consciousness or enabled to evolve. As Warnke (1994) notes, "Whether we adopt the insights of the other perspective or not, we have learned through our encounter with it" (pp. 131–132). That is the sort of evolution I had in mind.

To illustrate what I see as the risks of privileging commonality without acknowledging difference, I'll turn briefly to an excerpt from AERA's recently published *Definition of Scientifically Based Research*, which is one of the recent organization-sponsored statements of research principles to which I referred in my opening

comments. I will then point to potential lines of critique in light of the preceding dialogue.

The term "principles of scientific research" means the use of rigorous, systematic, and objective methodologies to obtain reliable and valid knowledge. Specifically, such research requires

- A. development of a logical, evidence-based chain of reasoning;
- B. methods appropriate to the questions posed;
- C. observational or experimental designs and instruments that provide reliable and generalizable findings;
- D. data and analysis adequate to support findings;
- E. explication of procedures and results clearly and in detail, including specification of the population to which the findings can be generalized;
- F. adherence to professional norms of peer review;
- G. dissemination of findings to contribute to scientific knowledge; and
- H. access to data for reanalysis, replication, and the opportunity to build on findings. (AERA, 2008)

Missing from this statement, and seriously so in my judgment, is any attention to local meaning or to the tension involved in making generalizations in light of local variations. These are the issues on which Erickson elaborated, and for which Floden and Phillips acknowledged support. Missing as well is any attention to illuminating the "history, sociology, philosophy, [or] ethics of inquiry" that Lather called for or to the sort of encounters across epistemological perspectives that illuminate what is taken for granted in any particular perspective (which is, I believe, a more general statement of Lather's call for challenging orthodoxies). As I argued briefly above, these are not idiosyncratic touchstones but can be traced across multiple theoretical perspectives. Echoing Phillips's comments on terminology, I note also: Terms that mean different things in different research discourses, like *validity* or *generalization*, are not defined or exemplified. And terms that our dialogue suggests are likely to be contested, including *replication* and *reliability* (reflecting, again, the tension between local meaning and generalizations) are used without comment.

In Erickson's fruit metaphor, I would characterize this as a heavily apple-flavored smoothie. In Bourdieu's terms, I would characterize this as a partial perspective on scientifically based research put forth as if it represented the whole. As Bourdieu (1975) suggests, based on his social studies of science: "Partial theories of science . . . are predisposed to perform ideological functions in the struggles within the scientific field . . . because they universalize the properties attached to particular states of the scientific field" (p. 39).

These are not new criticisms. Very similar criticisms of the NRC's (2002) *SRE*, were raised repeatedly across high-profile journals like *Educational Researcher* and *Teachers College Record*, as well as *Educational Theory* and *Qualitative Inquiry*. And yet here we are again, more than 6 years later, with a document whose (unnamed) authors and sponsoring organization council appear to have ignored these issues as if they didn't exist. (See Moss [2005b] for a more sympathetic acknowledgment of the political circumstances that authors of such statements face.)

Lather's characterization of validity "as part of the power and political dimensions of . . . demarcation" is positioned by Phillips as evidence that they are playing different games. And yet the choices

reflected in organization-sponsored statements of principles have political consequences, empowering proponents of certain perspectives over others, beginning with the decision of who gets a seat at the table where principles like these are produced. As Bourdieu (1975) suggests, the dominant agents in the field are those who manage “to impose the definition of science which says that the most accomplished realization of science consists in having, being, and doing what they have, are, or do” (p. 34). Contra Phillips and Lather, Bourdieu (1975, 1991) seeks to move his readers beyond the antagonistic perspectives that see scientific practice as either purely intellectual or purely political to acknowledge that scientific practices are always overdetermined by both considerations. By illuminating those forces that shape our understandings and practices, we can critically examine them and the institutions through which they work. While I may choose to use a different word than “validity” to describe Lather’s concern, I want her moves, alongside those of Phillips, to be part of the social science game I’m playing.

Lest this make me sound so ecumenical as to have no bearings of my own, let me say that a key touchstone of high-quality social science for me is openness and responsiveness to challenge from alternative perspectives, where we each make an ethical commitment to understand alternatives in their strongest possible light, to risk our own prejudgments, and to search for differences as well as commonalities among the alternatives (Bernstein, 1992; Gadamer, 1975/1994; Warnke, 1994). We can each hold our own perspectives *and* learn from the challenges others raise, even when we disagree.

And so, that leaves us with a question: For the multidisciplinary field of education research to thrive—to be and to be perceived as consistently engaged in high-quality work—what might we need to hold in common? It seems, at least, premature, and perhaps ultimately counterproductive, to work toward a statement of “principles of scientific quality” intended to apply across diverse communities of research. Perhaps a more prudent task might be conceptualized at this meta-epistemological level: proposing principles, practices, social structures, and incentives that ensure a vigorous educative dialogue. In many respects, this resembles the sort of microinitiatives that Schneider calls for. It also suggests the importance of access to multiple concrete examples of the sort of “careful, competent cases” illustrating different research discourses that Phillips recommends so that we can come to a deeper understanding of our own local meanings. While dialogues like the one engaged here represent small steps in this effort, there is more, I’ve argued, that we as a field can do (Moss, 2005b):

Editorial boards for journals, books, and online publications can invite and encourage cross-disciplinary dialogues, especially dialogues that permit multiple turns of talk focused on particular issues. . . . Funders can seek out and support research agendas that involve complementary or even oppositional approaches to understanding the same educational phenomena. Universities and other local educational agencies can provide structure and incentive for cross-disciplinary dialogue in multiple ways. Each of us can take some responsibility to learn at least enough about a second research discourse to understand the challenges it poses for our own. . . . The opportunities for mutual learning are profound if we can find the means and will to sustain such conversations. (pp. 282–283)

Continuing the Dialogue

It is, perhaps, commonplace to end a journal article with a tidy summary of its lessons. Given the multiple voices in our text, we have chosen not to attempt that. As is likely clear from reading our responses to one another, even conclusions of similarity and difference can vary with perspective. As Bernstein (1992) notes: “There are no algorithms for grasping what is held in common and what is genuinely different. Indeed, commonality and difference are themselves historically conditioned and shifting” (pp. 66–67). What we offer, instead, are suggestions for additional sets of readings that *both* illuminate differences and complementarities in philosophical or methodological perspectives *and* offer more in-depth understandings of particular perspectives. As we’ve noted above, there have been multiple special issues of journals focusing on *SRE* (NRC, 2002)—*Educational Researcher* (2002, Vol. 31, Issue 8), *Qualitative Inquiry* (2004, Vol. 10, Issue 1), *Educational Theory* (2005, Vol. 55, Issue 3), *Teachers College Record* (2005, Vol. 107, Issue 1)—in which we and many others have participated. These symposia offer multiple perspectives focused instructively on a common text intended to have broad relevance.⁸ AERA’s recently published *Handbook of Complementary Methods in Education Research* (Green, Camilli, & Elmore, 2006) provides reviews of literature reflecting different methodological perspectives. Its chapters by Bredo (2006) and Kelly (2006) offer instructive vantage points from which to view the complex territory covered by the volume. Volumes by Flybjerg (2001) and Phillips and Burbules (2000), taken as a set, provide accessible introductions, rich with concrete examples, to some of the philosophical issues underlying our dialogue, each from an instructively different perspective.⁹ An edited volume by Ercikan and Roth (2009), *Generalizing From Educational Research*, tackles the issues of generalization and local meaning from multiple perspectives. Those who want to pursue these philosophical issues with primary source readings can turn to Martin and McIntyre’s (1994) *Readings in the Philosophy of Social Science* and then to the philosophers whose work is excerpted there. We hope these will serve as useful starting places and that our readers will find ways to add their own voices to this important dialogue about the nature—and future—of the field in which we are collectively engaged.

NOTES

¹The set of articles read by the panel members were Erickson, 2009; Erickson and Gutiérrez, 2002; Floden, 2007; Lather, 2004; Moss, 2005a, 2005b; Phillips, 2006a; Schneider, 2004. Readers will find that these articles elaborate many of the perspectives represented more briefly in this symposium.

²A good introduction to the variety of approaches to education research can be found in AERA’s *Handbook of Complementary Methods in Education Research* (Green, Camilli, & Elmore, 2006); in the present essay we do not wish to prejudice the issue of complementarity!

³*Scientific Research in Education* (National Research Council [NRC], 2002), or *SRE*, is referred to multiple times in this article. For readers who are unfamiliar with it, this is a report prepared by the NRC in response to a congressional charge. An executive summary of the report can be downloaded from http://www.nap.edu/catalog.php?record_id=10236, and the full report can be read online at the same address. Most relevant to the current discussion are the report’s guiding principles for scientific inquiry. In abbreviated form, they are as follows:

- Pose significant questions that can be investigated empirically.
- Link research to relevant theory.
- Use methods that permit direct investigation of the question.
- Provide a coherent and explicit chain of reasoning.
- Replicate and generalize across studies.
- Disclose research to encourage professional scrutiny and critique. (p. 52)

⁴One reviewer suggested that AERA's 2006 *Standards for Reporting on Empirical Social Science Research in AERA Publications* (and its recently published companion, the 2009 *Standards for Reporting on Humanities-Oriented Research in AERA Publications*) represented a more inclusive statement than the NRC's (2002) *SRE*. Because three of the authors of this article served on the panel that drafted the first document (Erickson, Moss, and Schneider), I am inclined—with bias acknowledged—to agree. Of course, it is also important to acknowledge that the documents were intended to serve somewhat different purposes. All that said, I know from comments we received that the 2006 AERA standards we drafted nevertheless generated some of the same sort of criticism as *SRE* from researchers of quite different persuasions who do not see their vision of research adequately represented in it.

⁵One reviewer stated that the term *bias* should not be used in characterizing an interpretive perspective, as “bias is only thinkable if one believes in objectivity.” This generalization appears to reflect another instance of the same words taking on different meanings within different discourses, even within the constellation of discourses that might be called “interpretive.” My use of the term is grounded in Gadamer's philosophical hermeneutics, which falls firmly within the interpretivist tradition (Rabinow & Sullivan, 1987). Gadamer (1975/1994) argued that there is no understanding without preconceptions or “prejudices”: “The important thing is to be aware of one's own *bias* [italics added], so that the text [one is reading] can present itself in all its otherness” (p. 269). As frequent Gadamer interpreter Richard Bernstein (1985) noted, the goal is to separate those prejudices that are disabling from those that are enabling.

⁶Kelly (2006) offers an extended perspective on the role of dialogue and its relation to epistemology, based in Jürgen Habermas's (1990) discourse ethics. See Moss and Schutz (2001) and Moss, Girard, and Haniford (2006) for a comparison of Gadamer's and Habermas's models of dialogue.

⁷Kuhn's work has generated an enormous literature, which is still expanding. Two now-classic discussions (selected from many) are Lakatos and Musgrave (1970), and Newton-Smith (1981); a more recent discussion is Margolis (2003).

⁸In the following reference list, readers will find at least one reference to each of the symposia in cited articles for which the present article's six coauthors are first authors. In many cases, these journals have published additional commentary on *SRE* that one can find by searching the journals in later issues.

⁹The recommendation to read Phillips and Burbules (2000) was made by Moss, who persuaded Phillips to let her include it.

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AUTHORS

PAMELA A. MOSS is a professor of education at the University of Michigan, 4220 SEB, Ann Arbor, MI 48109-1259; pamoss@umich.edu. Her research interests are at the intersections of educational assessment, validity theory, and interpretive social science.

D.C. PHILLIPS is professor emeritus of education and (by courtesy) philosophy at Stanford University, 485 Lasuen Mall, Stanford, CA 94305-3096; d.c.phillips@gmail.com. His areas of research interest include philosophical and methodological issues in social science and education research and in constructivist positions in psychology, sociology, and science.

FREDERICK D. ERICKSON is the George F. Kneller Professor of Anthropology of Education in the Graduate School of Education and Information Studies at the University of California, Los Angeles, Box 951521, 1002 MH, Los Angeles, CA 90095-1251; ferickson@gseis.ucla.edu. His research interests include organization and conduct of face-to-face interaction, sociolinguistic discourse analysis, ethnographic research methods, study of social interaction as a learning environment, and anthropology of education.

ROBERT E. FLODEN is the University Distinguished Professor of Education at Michigan State University, 116M Erickson Hall East, Lansing, MI 48824-1034; floden@msu.edu. He has studied teacher education and other influences on teaching and learning, including work on the cultures of teachers, on teacher development, on the character and effects of teacher education, and on how policy is linked to classroom practice.

PATTI A. LATHER is a professor of education at Ohio State University, 101B Ramseyer Hall, 29 West Woodruff, Columbus, OH 43210; lather.1@osu.edu. Her work examines various (post) critical, feminist, and poststructural theories, most recently with a focus on the implications for qualitative inquiry of the call for scientifically based research in education.

BARBARA L. SCHNEIDER is the John A. Hannah Distinguished Professor of Education and a professor of sociology at Michigan State University, 516B Erickson Hall East, Lansing, MI 48824-1034; bschneid@msu.edu. Her research interests include sociology of education, scale-up research, and quasi-experimental designs for estimating causal effects.

Manuscript received June 30, 2009

Revision received July 31, 2009

Accepted August 6, 2009