Scientifically Based Research in Education: Epistemology and Ethics

Adult Education Quarterly 2006 56: 239
DOI: 10.1177/0741713606289025

SCIENTIFICALLY BASED RESEARCH IN EDUCATION: EPISTEMOLOGY AND ETHICS

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In this article, the author begins to trace the concept scientifically based research *in federal legislation, in the Department of Education's Institute of Education Sciences, and in the reports of several National Research Council committees. She also discusses how this concept has produced a certain* scientism *that has been deployed to attempt to control the field of educational research. She points out, however, that scientifically based research treats methodology as if it can be separated from epistemology and thus forgets that different bodies of knowledge and thought make different sciences possible. Thus, science is not one thing, as those who support scientifically based research often claim. Finally, the author suggests that our task as education scholars, researchers, and policy makers in this age of accountability is to engage rather than exclude epistemologies not our own that may help us produce different knowledge and produce knowledge differently.*

Keywords: *scientifically based research; National Research Council; evidence; postmodern*

Scientifically based research (SBR) or evidence-based research (EBR) has become a hot, almost blistering, topic for educational researchers at the beginning of the 21st century. The stakes are high, because the very nature of science and scientific evidence and therefore the nature of knowledge itself is being contested by scholars and researchers who think and work from different epistemological, ontological, and methodological positions as well as by those postmodernists who challenge the metaphysical project altogether. If one believes that different theoretical frameworks are grounded in and structured by different and, perhaps, incommensurable assumptions about the nature of knowledge, truth, reality, reason,

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ADULT EDUCATION QUARTERLY, Vol. 56 No. 4, August 2006 239-266
DOI: 10.1177/0741713606289025

power, science, evidence, and so forth, then one can see why educators are taking sides in this debate that is already organizing the limits and possibilities of what we can think and know and, thus, how we can live in the complex and tangled world of educational theory, research, policy, and practice.

Educational research has long been a worrisome and hotly debated topic. Ellen Lagemann (2000) begins her history of educational research by citing an article written in 1891 by Harvard philosopher Josiah Royce titled "Is There a Science of Education?" a question that continues to trouble educational research in 2006. A key question Lagemann's history explores is "Why has this domain of scholarly work always been regarded as something of a stepchild, reluctantly tolerated at the margins of academe and rarely trusted by policy makers, practitioners, or members of the public at large" (p. x). Lagemann cites early concerns such as the following: (a) "problems of status, reputation, and isolation" (p. 232) that emerge from the early and continued feminized nature of the field, (b) "a narrow problematics" (p. 235) that has produced an individualist, technical, and instrumental orientation to scholarship rather than a historical and philosophical orientation, and (c) "problems of governance and regulation" (p. 238) that have impeded the development of a professional community.

In 1993, Carl Kaestle published an article titled "The Awful Reputation of Education Research," much cited in the current debate, that reports his findings of an oral history project with "33 key agency officials and researchers to express historically grounded judgments on pressing issues in the field including the awful reputation of educational research" (p. 23). He summarized his participants' concerns as follows: (a) "that education R&D doesn't pay off" (p. 27), (b) that "the education R&D community is in constant disarray" (p. 28), and (c) that "the field is politicized" (p. 29). Sroufe (1997), responding to Kaestle's article, said policy makers and educators should keep two key issues in mind: "(1) what education research cannot do and (2) what education research does not do. What education research cannot do is compete head to head with the intrinsic glamour of the engineering and biological sciences" (p. 26), which seems to be the desire of some "soft" social scientists trying hard to be "hard."

Lagemann's and Kaestle's different perspectives each represents a particular and increasingly pervasive discourse that describes educational research as historically and presently *broken* in multiple ways and in need of repair. Whether the picture is as drear as it is made out to be is beside the point—today's fix is to make it "scientific," and the federal government has taken the lead in this project by mandating scientific method in law. The fundamental idea is that better science will make better schools—that "quality" science will enable us to finally reengineer schools so they work. However, what Sandra Harding (1991) called "science as usual" (p. 1) is having ripple effects beyond the P-12 venue and is beginning to circulate in all areas of education.

The desire to make educational research and practice "scientific" emerges in the midst of a neoliberal and neopositivist "conservative restoration" (Lather, 2004, p. 15)

that includes what Stephen Ball (1997) calls a "quality revolution" (p. 260) whose rhetoric is reminiscent of the organizational improvement plan total quality management of the 1980s and 1990s, and a corresponding accountability movement spurred on by a culture of surveillance that mandates increased auditing at every level of education to improve quality. There is a distinct rhetoric of blame, shame, and punishment throughout the conversation about quality, with the frontline classroom teacher and his or her students bearing much of the day-to-day brunt of this tactic. It is now common public knowledge that under the No Child Left Behind Law (NCLB; Public Law No. 107-110), teachers must teach to the test whether this is good or not; they have less and less control over the curriculum; and they, their students, and their schools are easily designated as failures if they do not meet annual yearly progress. This blame is now being extended, not surprisingly, to colleges of education who train both teachers and educational researchers. Underlying this rhetoric is a not-so-subtle moral condemnation of teachers and schools, colleges of education, educational researchers, and all educators who have supposedly too long neglected quality, thereby contributing to the so-called sorry state of education and educational research.

This desire clearly has a strong disciplinary impulse in that everyone involved in education is under the gaze of what I call the quality cops and the science storm troopers. We all need to shape up—everyone, that is, except some of the policy makers who, we are often told, are not happy when educators say that teaching and learning are complex and that if there were an easy fix, we would long ago have installed it. Our legislators are portrayed as too busy to learn and therefore are off the hook and not accountable for the quality of their policies even though they increasingly regulate the behavior of populations across the field of education with little scientific evidence on their side. A surface engagement with difficult issues, which one could say is part of the general anti-intellectual and antiscience agenda of the current government, is not surprising in that there is a "rampant ahistoricism" (Ball, 1997, p. 266) in education policy. "Many contemporary problems or crises in education are, in themselves, the surface manifestations of deeper historical, structural and ideological contradictions in education policy" (Grace, 1995, p. 3).

Education policy is often divorced from other social policy, thus isolating education from the rest of society and targeting it as an easy locus of failure in need of reform. For example, reports from other social scientists—for example, sociologists and economists—that poverty is increasing in this country seem to matter little in the current rush-up to fix educational problems with better science. It is as if the intersection of race, class, gender, sexuality, and so forth does not relate to the condition of education. As contradictions and complications pile up, some policy makers and educators look to a particular kind of science, what Patti Lather (2004), and others (e.g., Sorrell, 1991; Stenmark, 2001) call "scientism" and its particular kind of "rationality" and "rigor" to save us. Once again, the redemptive nature of a particular kind of scientific knowledge is hailed as the cure.

But this retrograde science favored by the Department of Education's Institute of Education Sciences (IES), the division that funds educational research, is a science that reinstalls some kind of positivism[1] and elevates randomized experimental trials as the gold standard. Its accompanying calls for a particular and historical kind of reason, an instrumental reason, echo an absolutism that increases the authority of a select few and penalizes those who disagree. History tells us, however, that positivism was found to be inadequate for addressing social and educational problems decades ago to the dismay of many psychologists, educational and otherwise, who found their methods out of favor after the paradigm wars. Those wars occurred during the social movements of the 1960s and 1970s during which an array of epistemologies—critical theories, race theories, feminist theories, queer theories, neo-Marxist theories, and so forth—were developed in order to work for a social justice that might redress the failures of an exhausted liberalism with its false promises of equality and of a science that ignored the voices of the disenfranchised. It is important to point out, as I have in other writing (St.Pierre, 2002), that researchers employing these epistemologies found qualitative methodology particularly useful because it is grounded in face-to-face interactions with particular (not random) people, that is, it was and is important to talk with and observe people in order to find out what they think about their lives. Thus, these epistemologies have ridden the back of and become deeply imbricated in qualitative inquiry. Dismissing qualitative inquiry is often equal to dismissing those epistemologies and the people who thought and continue to think and live them.

Thomas Schwandt (2005) points out, it is interesting, that psychologists and their randomized experimental trials are not only back in the game but touted as the best scientists with the best science even though we have never quite figured out how to do randomized trials in educational settings (see, e.g., National Research Council [NRC], 2004a). Schwandt (2005) reports that

> The American Psychological Association . . . is practically ecstatic at the prospect of an increased role for psychology in establishing the scientific basis for educational interventions in testing, motivation, classroom management, reading instruction, math instruction, preschool curriculum, and character development and socialization of school children. (p. 286)

It is worth noting that Grover J. "Russ" Whitehurst, the director of the IES, is a psychologist quite capable of making the following statement—based on what evidence, I'm not sure:

> Psychologists are more likely than any other professional group working in the schools to have scientific training—and respect and understanding of the role of research and evidence in practice—they should be prepared to play an important role in moving the culture of education toward reliance on evidence. (Whitehurst, as cited in Schwandt, 2005, p. 286)

So the site of this battle is not just P-12 but the entire "culture of education" in this country. The privileging of psychology and its research methods can be found in the key players across this debate—from the director of the government body that funds educational research, Russ Whitehurst, to Richard Shavelson, the chair of the NRC committee (2002) that recently defined "science." It appears that the interpretive turn (see, e.g., Hiley, Bohman, & Shusterman, 1991), the cultural turn (see, e.g., Jameson, 1998), the linguistic turn (see, e.g., Rorty, 1967), and the postmodern turn (see, e.g, Hassan, 1987) may have passed these people by. They also apparently missed the "legitimation crisis" (Habermas, 1975) and the "crisis of representation" (e.g., Jameson, 1979/1984; Marcus & Fischer, 1986), all of which take issue with the scientism that is now held to be the gold standard. Many educational scholars and researchers wonder how this has happened.

It is not the intent of this article to provide a careful genealogy of that process, though no doubt that work needs to be done. But in the following section, I will track, to some extent, the discursive, juridical, and material formation of the truth of the concept SBR, which is often used interchangeably with EBR, a concept well established in the field of medicine. Paul Bové (1990), writing about Foucault's historical method, genealogy, which examines how discourse produces the truth, explains that genealogy tries to locate

> the power to produce statements which alone can be judged "true" or "false" within the knowledge/power system that produces "truth" and its criteria within a culture. It is, in effect, recognizing this effect of power that genealogy does its work. Indeed, genealogy lets us confront how power constructs truth-producing systems in which propositions, concepts, and representations generally assign value and meaning to the objects of the various disciplines that treat them. (p. 57)

SBR has become the "truth" in education, and that truth is being maintained and perpetuated by a whole network of discursive formations and material practices that are increasingly elaborated by a knowledge/power system that may not be in the best interests of education.

SBR OR EBR IN EDUCATION

On January 8, 2002, President George W. Bush signed into law the reauthorization of the Elementary and Secondary Education Act (ESEA) known as the NCLB Act of 2001 that provides billions of dollars in federal aid to education. According to Feuer, Towne, and Shavelson (2002b), NCLB contains "111 references to 'scientifically-based research'" (p. 4).

In their partial genealogy of this concept, Eisenhart and Towne (2003) explain that the definition of SBR used in NCLB is derived from language in the Reading Excellence Act (REA) of 1999 (Public Law 105-277), which was repealed in 2002 with the enactment of NCLB. They write that Robert W. Sweet Jr., a professional staff member for the majority members of the House Education and

Workforce Committee, was asked to write a definition of SBR for REA. Sweet told Eisenhart and Towne that he produced his definition after searching Web sites and talking with

> numerous university-based researchers (primarily with backgrounds in cognitive psychology) and shared drafts of his work with these researchers (he estimates approximately 20-25 of them) for feedback. The language that emerged from the several-months long process was inserted into REA (1999) and passed without fanfare. (Eisenhart & Towne, 2003, p. 32)

This not-so-rational beginning of SBR reflects Michel Foucault's (1975/1979) comment that chance and accident rather than rational deliberation and careful scholarship are often found at the beginning of things. It is not surprising that psychologists defined science for Sweet as a version of positivism, because that is the science they use. If Sweet had consulted a broad range of educational researchers, the current definition of quality science might be quite different. For genealogical purposes, the definition presented in REA is as follows:

> The term "scientifically based reading research" (A) means the application of rigorous, systematic, and objective procedures to obtain valid knowledge relevant to reading development, reading instruction, and reading difficulties, and (B) shall include research that— (i) employs systematic, empirical methods that draw on observation or experiment; (ii) involves rigorous data analysis that are adequate to test the stated hypotheses and justify the general conclusions drawn; (iii) relies on measurements or observational methods that provide valid data across evaluators and observers and across multiple measurements and observations; and (iv) has been accepted by a peer-reviewed journal or approved by a panel of independent experts through a comparably rigorous, objective, and scientific review. (Sweet, 2004, p. 22)

In their article, Eisenhart and Towne (2003) track the concept SBR produced by Sweet and modified somewhat in bills pending to reauthorize the Individuals with Disabilities Education Improvement Act, parts of the reauthorization of the Higher Education Act, and the Education Sciences Reform Act, which abolished the Office of Educational Research and Improvement (OERI) and established IES. It should be noted that in NCLB, the standards for SBR include reference to causal relationships and privilege randomized experimental trials as the gold standard for establishing causal claims. The definition of scientifically based research in NCLB [Section 9101(37)] is as follows:

> The term "scientifically based research"— (A) means research that involves the application of rigorous, systematic, and objective procedures to obtain reliable and valid knowledge relevant to education activities and programs; and (B) includes research that— (i) employs systematic, empirical methods that draw on observation or experiment; (ii) involves rigorous data analyses that are adequate to test the stated hypotheses and justify the general conclusions drawn; (iii) relies on measurements

> or observational methods that provide reliable and valid data across evaluators and observers, across multiple measurements and observations, and across studies by the same or different investigators; (iv) is evaluated using experimental or quasi-experimental designs in which individuals, entities, programs, or activities are assigned to different conditions and with appropriate controls to evaluate the effects of the condition of interest, with a preference for random-assignment experiments, or other designs to the extent that those designs contain within-condition or across-condition controls; (v) ensures that experimental studies are presented in sufficient detail and clarity to allow for replication or, at a minimum, offer the opportunity to build systematically on their findings; and (vi) has been accepted by a peer-reviewed journal or approved by a panel of independent experts through a comparably rigorous, objective, and scientific review.

In 2003, the Educational Resources Information Center (ERIC) Clearinghouse published an ERIC Digest titled *Scientifically Based Research* (Beghetto, 2003) that attempts to summarize this concept and offers implications for educators. By then, many members of the American Educational Research Association (AERA) were concerned that randomized trials were being privileged by the federal government, and in 2003 AERA published a "Resolution on the Essential Elements of Scientifically-Based Research" that expressed dismay at the position taken by the Department of Education.

> The Council of the AERA reaffirms its commitment to improving the quality of educational research. It reasserts that there are multiple components of quality research, including well-specified theory, sound problem formulation, reliance on appropriate research designs and methods, and integrity in the conduct of research and the communication of research findings. A fundamental premise of scientific inquiry is that research questions should guide the selection of inquiry methods. Council recognizes randomized trials among the sound methodologies to be used in the conduct of educational research and commends increased attention to their use as is particularly appropriate to intervention and evaluation studies. However, the council of the association expresses dismay that the Department of Education, through its public statements and programs of funding, is devoting singular attention to this one tool of science, jeopardizing a broader range of problems best addressed through other scientific methods. The council urges the Department of Education to expand its current conception of SBR. Furthermore, the council directs its staff and officers to take steps with the Department of Education and other associations, organizations, and agencies to achieve a broader understanding of the range of scientific methodologies essential to quality research. Adopted by unanimous resolution on January 26, 2003.

Effective February 24, 2005, then secretary of education, Rod Paige, announced what is called a "priority" that could be used in any appropriate program in order to

> focus Federal financial assistance on expanding the number of programs and projects Department-wide that are evaluated under rigorous scientifically based research methods in accordance with the Elementary and Secondary Education Act of 1965 (ESEA), as reauthorized by the No Child Left Behind Act of 2001 (NCLB).

The proposed priority was published in the *Federal Register* on November 4, 2003 (68 FR 62445) to solicit comments. There were 29 comments supporting it. There were also 183 "respondents who commented that random assignment is not the only method capable of generating understandings of causality," 173 respondents who commented that "the complex nature of causality renders random assignment methods less capable of discovering causality than designs sensitive to local culture and conditions," 186 respondents who commented that "random assignment should sometimes be ruled out for reasons of ethics," and 173 respondents who commented that "although it may be important to examine causality prior to wide implementation, pilot or exploratory programs are often too small in scale to provide reliable conclusions." Other comments objecting to the priority were also submitted, but no changes were made. The priority is as follows:

> Evaluation methods using an experimental design are best for determining project effectiveness. Thus, when feasible, the project must use an experimental design under which participants—e.g., students, teachers, classrooms, or schools—are randomly assigned to participate in the projects activities being evaluated or to a control group that does not participate in the project activities being evaluated. If random assignment is not feasible, the project may use a quasi-experimental design with carefully matched comparison conditions. . . . In cases where random assignment is not possible and participation in the intervention is determined by a specified cutting point on a quantified continuum of scores, regression discontinuity designs may be employed. For projects that are focuses on special populations in which sufficient numbers of participants are not available to support random assignment or matched comparison group designs, single-subject designs such as multiple baseline or treatment-reversal or interrupted time series that are capable of demonstrating causal relationships can be employed. Proposed evaluation strategies that use neither experimental designs with random assignment nor quasi-experimental designs using a matched comparison group nor regression discontinuity designs will be considered responsive to the priority when sufficient numbers of participants are available to support these designs. Evaluation strategies that involve too small a number of participants to support group designs must be capable of demonstrating the causal effects of an intervention or program on those participants. (U.S. Government, 2005)

The language of the federal government documents quoted above illustrates an increasing elaboration, refinement, and solidification of the truth of SBR in public law and federal policy. According to these definitions, much educational research accomplished during the past half century cannot be scientific. It is interesting to note that at a public policy forum presented by the National Academy of Science's Center for Education, the National Educational Knowledge Industry Association, and the Progressive Policy Institute that I attended in Washington, D.C., on March 11, 2004, Robert Sweet said that he realized that his original definition of quality educational research is too narrow and that basing educational policy and practice only on evidence produced by experimental methods might not be such a good idea.

But by 2002, the IES had established the What Works Clearinghouse (WWC) "to provide educators, policymakers, researchers, and the public with a central and trusted source of scientific evidence of what works in education." The following explanation is available on the IES Web site (www.whatworks.edu.gov).

According to the IES, SBR

- employs systematic, empirical methods that draw on observation or experiment; involves data analyses that are adequate to support the general findings; relies on measurements or observational methods that provide reliable data; makes claims of causal relationships only in random-assignment experiments or other designs (to the extent such designs substantially eliminate plausible competing explanations for the obtained results);
- ensures that studies and methods are presented in sufficient detail and clarity to allow for replication or, at a minimum, to offer the opportunity to build systematically on the findings of the research;
- obtains acceptance by a peer-reviewed journal or approval by a panel of independent experts through a comparably rigorous, objective, and scientific review; and
- uses research designs and methods appropriate to the research question posed.

According to the Web site, WWC review teams consisting of a senior content advisor, a methodology consultant, a project coordinator, and research analysts, all of whom hold PhDs, in what fields I am not sure, review research reports using the Study Design and Implementation Assessment (Study DIAD) and the Cumulative Research Evidence Assessment Device (CREAD; available at www.whatworks.edu.gov) to determine whether the reports contain evidence of causal validity according to the WWC evidence standards listed above. The review teams then generate study reports, intervention reports, and topic reports. The general process is described on the Web site as follows:

> The WWC determines the evidence of causal validity of each study according to WWC Evidence Standards [listed above] and gives each study one of three possible ratings: "Meets Evidence Standards" (for randomized controlled trials and regression discontinuity studies that provide the strongest evidence of causal validity), "Meets Evidence Standards with Reservations," (for quasi-experimental studies; randomized controlled trials that have problems with randomization, attrition, or disruption; and regression discontinuity designs that have problems with attrition or disruption), and "Does Not Meet Evidence Screens" (for studies that do not provide strong evidence of causal validity). . . . Studies that "Meet Evidence Standards" and "Meet Evidence Standards with Reservations" are reviewed further to describe and rate other important characteristics. These characteristics include: (a) intervention fidelity, (b) outcome measures, (c) the extent to which relevant people, settings, and measure timings are included in the study, (d) the extent to which the study allowed for testing of the intervention's effect within subgroups, (e) statistical analysis, and (f) statistical reporting.

Lists of individual research reports on various topics that have been reviewed are available on the WWC Web site. At the time this article was written (September 2005),

the only topic on which reviews had been completed was middle school math curricula. Of the 77 studies identified and reviewed to date, 4 meet evidence standards, 5 meet evidence standards with reservations, 66 do not meet evidence screens, and 1 is currently under review. Almost all of those that did not meet evidence standards were rejected because they did not use a strong causal design. Topics that will be reviewed in the future include beginning reading, character education, drop-out prevention, English language learning, elementary school math, early childhood, behavior, and adult literacy.

In addition, the WWC Web site contains a "registry of outcome evaluators," researchers it does not endorse but "who conduct research on the effects of replicable educational interventions. This resource is designed to help schools, school districts, and educational program developers identify potential evaluators to conduct studies on educational outcomes" using randomized experimental trial. As an aside, it is not surprising that a new research industry, unrelated to academia, is springing up to do the kind of evaluation work the federal government wants. In any case, the WWC is doing exactly what it intended to do and that is to advise those interested in particular educational programs—for example, principals, teachers, parents—which of those are grounded in high-quality evidence and thus "work" and which are not and thus do not work—as if what works holds across space and time, across schools, across teachers, and across children.

It was clear in a 2004 AERA presentation by the WWC (Session 55.010) that at that time, qualitative research was not acceptable to the WWC—because it does not use experimental designs, it would be immediately rejected—nor were qualitative methodologists included in the WWC review process, although qualitative inquiry can address, though differently, issues of causality (see, e.g., Maxwell, 2004a). When I asked a staff member after the session about the place of qualitative inquiry in the work of the WWC, he said, "You qualitative people should get your own What Works Clearinghouse." I try to be optimistic and hope that the attitude toward qualitative inquiry at the WWC and the IES will change. The concern here, as I wrote earlier, is not the qualitative-quantitative divide but that qualitative inquiry has carried the work of certain epistemologies for decades. Dismissing qualitative inquiry dismisses the questions those epistemologies allow us to ask and therefore the knowledge we might produce.

I should note at this point that a similar effort to evaluate and synthesize educational research has been under way in England for some time by the Centre for Evidence-Informed Policy of the Department of Education and Skills and that there has been a lively conversation (see, e.g., Davies, 2000; Evans & Benefield, 2001; Hammersley, 2001; MacLure, 2005) about the process by which "systematic reviews" of existing research are produced. I have not yet read a critique of the reviewing process in the United States, perhaps because we are considerably behind England in getting our effort under way and scholars do not yet have a sufficient body of reviews to study.

In summary, the definition of SBR first used in the REA of 1999 has been written into federal law, refined by the U.S. Department of Education, and enforced as the IES reviews and classifies past research and funds future research according to standards described above in order "to improve student outcomes" (http://www.whatworks.ed.gov). The goal of this work is to focus on the "impact" of educational research, on what works.

The federal government's focus on SBR is moving through all aspects of education, from extending NCLB to high schools, to making plans to require teacher certification programs to confirm that their work is based on scientific evidence, to making plans to ensure that colleges of education train researchers who can conduct research that is "scientific," to attempting to discipline the educational research community and thereby all areas of education in a multitude of ways. It seems that at the beginning of the 21st century, notions of a particular kind of science and a particular kind of evidence have been put into play regardless of continuing critiques from educators, many of whom have lived this battle before.

NRC, The National Academy of Sciences

In his 2001 president's address, Bruce Alberts, then president of the National Academy of Sciences,[2] asked, "How can the National Academies make a science of education?" (http://www.nas.edu), and in that speech he made it clear that doing so was a priority of the academies. The mission of the new president, Ralph Cicerone, elected by the academies in February 2005, in regard to education is unclear at this point. But the academies' interest in education is long lived, and in its role as science advisor to the government since 1863, the NRC, the operating arm of the national academies has, since 1958, issued several reports on educational research. More recently, the NRC took up Alberts's mission and assumed a significant role in the SBR movement. One of the NRC's most recent reports on educational research, *Scientific Research in Education* (2002), like many projects undertaken by the NRC, originated in the political and policy arenas, in this case in response to some of the federal laws discussed above.

As SBR in education began to be defined in a certain way and installed in federal legislation, it became clear that this concept would significantly affect the kind of research the federal government would fund and could significantly change the entire field of education. In an attempt to bring educational researchers into the conversation, Kenji Hakuta, the chair of the National Educational Research Policy and Priorities Board (NERPPB),

> turned to the NRC to inject the voice of researchers into policy initiatives of this kind. Thus, in early fall 2000, the NRC was formally asked to assemble a committee of education researchers to investigate what constitutes scientific research in education. Its work seemed urgent. In December 2000, the NRC Committee on Scientific Principles for Educational Research (SRE) began its work. (Eisenhart & Towne, 2003, pp. 32-33)

At the 2004 AERA annual meeting in San Diego, in a presidential invited session (Session 25.012) featuring Russ Whitehurst (2004), Richard Shavelson (2004), the SRE committee chair, said in his role as discussant that "the report was done rapidly" in about 6 months, because the committee was concerned that the federal government would legislate what counts as science and scientific method.

Thus, one of the aims of the SRE committee was to respond quickly to and temper the narrow scientism of REA, NCLB, and other federal legislation. Feuer et al. (2002b) wrote that the committee worried that the

> SBR movement will go awry, that narrow definitions of *research* or *science* might trivialize rather than enrich our understanding of education policy and practice, and that the splendors of unfettered scholarship will be eroded by creeping tides of conformity and methodological zealotry.

The report claims that it describes "what constitutes good science" (NRC, 2002, p. vii) and defines six scientific principles that it says hold across all epistemologies. According to the report, these are "norms enforced by the community of researchers that shape scientific understanding. We conclude that six guiding principles underlie all scientific inquiry, including education research:" (a) Pose significant questions that can be investigated empirically; (b) link research to relevant theory; (c) use methods that permit direct investigation of the question; (d) provide a coherent and explicit chain of reasoning; (e) replicate and generalize across studies; (f) disclose research to encourage professional scrutiny and critique (NRC, 2002, pp. 2-5).

The report also defines five core assumptions about science (see NRC, 2002, pp. 24-26). "These principles help define the domain of scientific research in education, roughly delineating what is in the domain and what is not" (p. 240). I find it most interesting that the committee believed it had the authority to exclude certain work from the field of scientific research—that it believed it had a mandate to proclaim, for example, that some work is scientific and some is not. Yet, this exclusionary impulse is a characteristic of positivism because, as Lyotard (1979/1984) wrote, "the system can only function by reducing complexity" (p. 61) and eliminating that which works against its assumptions. The assumption that has most concerned me (see, e.g., St.Pierre, 2002, 2004) is the third:

> We assume that it is possible to describe the physical and social world scientifically so that, for example, multiple observers can agree on what they see. Consequently, we reject the postmodernist school of thought when it posits that social science research can never generate objective or trustworthy knowledge. (NRC, 2002, p. 25)

The footnote for this statement follows:

> This description applies to an extreme epistemological perspective that questions the rationality of the scientific enterprise altogether, and instead believes that all knowledge is based on sociological factors like power, influence, and economic factors (Phillips & Burbules, 2000). (NRC, 2002, p. 25)

I will digress here to discuss this rejection of postmodernism. First of all, the postmodern work that I have studied carefully for a number of years does not say that "social science research can never generate objective or trustworthy knowledge" (NRC, 2002, p. 25). That kind of statement is not one that postmodern scholars or researchers would likely think or say. Foucault (1971/1972) says that "knowledge is that of which one can speak in a discursive practice" (p. 182), so knowledge (to say it perhaps too simply, the ideas that can be thought and the statements that can be made) is not transcendental but always situated, located within particular epistemological, discursive, linguistic, social, historical, cultural, and material fields of power relations. Knowledge is not above the level of human activity with all its values, desires, politics, yearnings, machinations, and so forth. How can a human mired in such a stew produce knowledge that is not? What postmodernism does do is offer various analyses (e.g., rhizoanalysis, schizoanalysis, deconstruction, genealogy, archaeology, power/knowledge readings, postfeminist race critiques, queer critiques) that allow one to examine how different systems of thought (e.g., positivism, social constructionism, critical race theory, pragmatism, phenomenology, Marxism) describe differently concepts such as rationality, objectivity, truth, knowledge, science, and so forth and, further, to examine what happens differently (to real people) when those different descriptions are put into play. Furthermore, adjectives such as "never" and "always" are anathema to postmodernism. Finally, I doubt that any group of people believe that if something happened in their midst they would agree on what they saw. In fact, our juridical system is increasingly suspect of eyewitness testimony. In sum, many scholars would say that it is only some form of positivism that maintains that knowledge is *not* based on power, influence, and economic factors. That postmodernism bears the brunt of this ill-informed critique is unfortunate.

Nevertheless, this critique of postmodernism began to circulate and was picked up by Russ Whitehurst (2003), who said in a 2003 AERA session (Session 29.001) that we need less theory and more of what works and, further, that postmodern methods, in particular, will not help us learn what works. I will discuss the implications of those statements and others that point to lack of attention to epistemology later in the article. Finally, the report makes recommendations about the design of scientific research in education as well as recommendations about how the federal government can assist in promoting that research and how the educational research community should discipline itself so that it can become properly scientific.

An interpretation of the 2002 NRC report (Feuer et al., 2002b) quickly followed, written by Michael J. Feuer (executive director of the Behavioral and Social Sciences and Education Division of the NRC of the National Academies of Science with degrees in public policy), Richard Shavelson, (an educational psychologist at Stanford University who chaired the SRE committee), and Lisa Towne (the NRC study director for the SRE report who has a master's in public policy from Georgetown University) and published in a special issue of *Educational Researcher*, *31*(8), 2002. The issue also included four comments on

the paper by David Berliner (2002), Frederick Erickson and Kris Gutierrez (2002), James A. Pellegrino and Susan R. Goldman (2002), and Elizabeth A. St.Pierre (2002) as well as a reply to their comments by Feuer, Towne, and Shavelson (2002a). This round of debate was the beginning of a flurry of conversation in the literature about what counts as science and scientific evidence.

Other education journals followed with special issues—*Qualitative Inquiry*, *10*(1), 2004; *Teachers College Record*, *107*(1); and *Educational Theory*, *55*(3), 2005,[3] and there have been many articles since the 2002 NRC report across the field of education on different aspects of the debate, with *Educational Researcher* providing a paper in almost every issue that is sure to keep the field roiling (e.g., Burkhardt & Schoenfeld, 2003; Eisenhart & DeHann, 2005; Glass, 2004; Mayer, 2000; Olson, 2004; Slavin, 2004; Willinsky, 2001), including articles that try to cross the quantitative-qualitative divide (e.g., Chatterji, 2004; Johnson & Onwuegbuzie, 2004; Maxwell, 2004a; Shaffer & Serlin, 2004), though that issue tends to be a red herring in the larger debate about epistemology.

Immediately following the publication of the 2002 NRC report on scientific research in education, the NRC organized a follow-up committee, the Committee on Research in Education (CORE), whose work builds on and extends the recommendations of SRE, seemingly without attention to the growing critique mentioned above. The NRC's CORE committee published three reports: (a) *Implementing Randomized Field Trials in Education: Report of a Workshop* (NRC, 2004a), (b) *Strengthening Peer Review in Federal Agencies That Support Education Research* (NRC, 2004b), and (c) *Advancing Scientific Research in Education* (ASRE) (NRC, 2005). The third publication is a summary report of the committee and contains 13 recommendations under the major headings (a) promoting quality, (b) building the knowledge base, and (c) enhancing professional development. Some of the recommendations indicate a complete lack of understanding of qualitative inquiry, such as Recommendation 8, "Education research journals should develop and implement policies to require structured abstracts" (NRC, 2005, p. 54). The report cites an article by Mosteller, Nave, and Miech (2004) on this topic and claims their article makes the case for the structured abstract "convincingly, offering a prototype structure for consideration by the education research communities and associated journals" (NRC, 2005, p. 54). The abstract might work for experimental designs but not for many qualitative studies; in fact, the research design field in the structured abstract used as an example by the authors is "randomized-controlled field trial" (Mosteller et al., 2004, p. 32). The authors say that this kind of abstract is used in some journals in England, but the journals they cite are all in psychology, which lends credence to Thomas Schwandt's (2005) comment that psychologists are now considered "the 'real' methodologists" (p. 285).

I find other recommendations of ASRE troubling as well, in particular, Recommendation 5, "Professional associations involved in education research should develop explicit ethical standards for data sharing" (NRC, 2005, p. 39)

and Recommendation 6, "Education research journals should require authors to make relevant data available to other researchers as a condition of publication and to ensure that applicable ethical standards are upheld" (p. 45). Those qualitative researchers with whom I have discussed these recommendations say they are unlikely to share their data, which were collected under conditions of anonymity and confidentiality, with anyone. The rationale for data sharing in ASRE is to encourage the "merging, comparing, combining, reanalyzing, or integrating" (p. 39) of data in order to

> facilitate the verification of results obtained by allowing other researchers to reproduce them, enable replications that test the boundaries of theories and help articulate generalizability, promote the development of validated measures, and provide opportunities (and often cost savings) to pursue new questions and directions. (p. 42)

Statements such as these are positivist and describe experimental data and methods and do not take into account other epistemologies that employ qualitative methods.

A final recommendation I find troubling in ASRE is Recommendation 9, "Schools of education that train doctoral students for careers in education should articulate the competencies their research graduates should know and be able to do and design their programs to enable students to develop them" (NRC, 2005, p. 59). This recommendation sounds just fine, but the discussion of this and another recommendation about training educational researchers encourages a "core" curriculum and seems intended to rein in the diversity of the field, as in the following statement: "Such an articulation is important because the field is so diverse and because its participants need to recognize the role, value, and points of convergence across a range of theoretical ideas, epistemologies, and methods" (p. 62). Again, this statement sounds just fine, but a concern is that "convergence" is part of the same logic as "consensus," and both are often fueled by a Hegelian desire to assimilate Difference into the Same. What science, what methodology, and what knowledge possibilities will be eliminated in the urge to converge?

I doubt that any educator objects to the rigorous preparation of educational researchers, but I have become quite concerned about how the recommendations of these NRC reports are taken up and used by people who have a very narrow view of science and, seemingly, a limited knowledge of the variety of epistemologies that define science differently. As Atkinson (2004) points out, "It would seem that, once they are published, commissioned reports of this type take on an air of incontrovertible truth that no amount of academic debate can dislodge" (p. 112).

Given the ease with which the federal government has intruded in the P-12 curriculum during the past few years, it is not hard to imagine a similar intrusion in other educational curricula, including the training of educational researchers. Indeed, this intrusion into research training is already occurring, with constant references in the literature and in policy meetings to the Flexner Reports of 1909

and 1910 (the 1910 report was titled *Medical Education in the United States and Canada*), which were prepared by Abraham Flexner for the Carnegie Foundation for the Advancement for Teaching and which reported a nationwide evaluation of medical training institutions that concluded that the United States had too many unqualified physicians produced by an unregulated system of medical education. According to the report, only Johns Hopkins provided acceptable medical training, and institutions deemed "irregular" were forced to close. The reports did encourage needed reforms in the standards, organization, and curricula of American medical schools. Unfortunately, good health care practices were swept away along with bad as an elite "scientific" foundation for health care—pharmacological and surgical medicine or what some call "modern medicine"—was privileged over what today are recognized by even the federal government (see, e.g., U.S. Government, 2002) as useful alternative forms of medicine such as chiropractic, Native American medicine, and meditation that focus on prevention and wellness and not just on cure through drugs. The U.S. government adopted the findings of the Flexner Report as the standard for quality medical education, and medical schools were required to follow the Johns Hopkins scientific model in training licensed physicians. But this is just another example of how education is being compared to medicine where evidence-based scientific practice, not without its critics, has been the rage for some time.

Having defined what counts as science in its 2002 SRE report and extended the work of that committee in the 2005 ASRE report, the NRC is poised to develop "standards of evidence" in social and behavioral science research. To this end, in 2004 the NRC organized a

> planning committee to oversee the conceptual development of a broad, long-term initiative related to the quality of evidence. The planning committee will map out a range of topics and activities to be pursued over a multi-year period to improve the quality of research in the behavioral and social sciences and education and strengthen the ties between behavioral and social science, public policy, and practice. (http://www.nas.org)

I understand that this is an internal committee so its work does not have to be made public (as required by the Federal Advisory Committee Act Amendments of 1997 federal law) in the same way as the work of an external committee that is expected to write a report for the public. Almost all members of the evidence committee are either members or affiliates of the National Academies or members of current or prior NRC committees, which could lead one to believe that there is only one degree of separation among those making decisions about science and evidence.[4] The only committee member who might be called an educator is Robert Boruch, and his PhD is in psychology, not in education. As far as I can tell, no committee members have experience in the public schools, nor do their research methods fall within the range of what I would call qualitative. Needless to say, I was disturbed by these omissions, because the way in which scientific

evidence will be defined could have a strong impact on all areas of education, educational research, and educational knowledge and practices.

Some of us believe that if it continues to endorse reports such as SRE and ASRE, the NRC will have to rethink its claims of good intentions and inclusiveness because of an increasingly vigorous critique of increasingly skeptical critics who believe the reports are largely positivist. One of the chief frustrations of the 2002 NRC report, *Scientific Research in Education*, is indeed its overall claim of inclusiveness though it belies that claim even in the executive summary that prefaces the report when it privileges "cumulative knowledge," says that "at its core, scientific inquiry is the same in all fields," states that that inquiry "builds understandings in the form of models or theories that can be tested," and claims that all science "is guided by a set of fundamental principles" (pp. 1-2). Those statements can be made only within the assumptions and structure of some form of positivism, which has been critiqued by social scientists for almost half a century. Surely, the NRC is aware of that critique.

Another example that claims of inclusiveness are merely claims is that Feuer et al. (2002b), who say that they worry "that narrow definitions of *research* or *science* might trivialize rather than enrich our understanding of education policy and practice" (p. 4), writing with Phillips (Shavelson, Phillips, Towne, & Feuer, 2003) in an article in the same journal, reject design experiments because they use narrative, which is foundational to many epistemologies including critical race theory and other race theories, feminist theories, gay and lesbian theories, queer theories, neo-Marxist theories, and so on. The authors ask these questions about narrative in order to point out its supposed shortcomings:

> To what extent can rival narrative accounts of the same action be ruled out? [Why should rival accounts be ruled out—to silence disagreement?] To what extent would another narrator replicate the account? [Can human activity ever be replicated?] To what extent does the narrative generalize to other times and places? [Why is generalizability privileged?] There is nothing in the use of narrative form, by itself, that guarantees the veracity of the content of the account or which vitiates the need for the usual epistemic warrants used in science [Science itself is simply another narrative.]. (p. 27)

These questions are possible only within the structure of positivism. Furthermore, in the last sentence of the quote, the authors make a not-so-subtle move to exclude narrative from science, a positivist science, thus excluding epistemologies that employ narrative. Later, they even refer to narrative work as "pre-scientific" (p. 28) and thus out themselves and belie their 2002 claim of inclusiveness. I believe claims of inclusiveness in this discourse are grounded in an imperialist reason masquerading as tolerant and pluralist that is, at best, highly effective propaganda.

I maintain that these examples illustrate a lack of knowledge that is not innocent. I also wonder why the NRC committees believe they have the power to make statements such as the following: "At its core, scientific inquiry is the same

in all fields" (NRC, 2002, p. 2) and professional associations and publishers should "require authors of journal articles to make data available to other researchers" (NRC, 2005, p. 75). I would expect more modest, careful, and circumspect claims regarding science from scientists. Unfortunately, as Schwandt (2005) points out, all this is of a piece with the federal government's initiatives to increasingly discipline, control, and regulate populations. Specifically, Stronach and Hustler (2001) point out that SBR is "the epistemological equivalent of Back to the Basics," and that "there is no substantial evidence to vindicate its application" (p. 524). In fact, there is much evidence to the contrary. This is truly a dangerous state of affairs, but as Foucault (1983/1984) wrote, "if everything is dangerous, then we always have something to do" (p. 343). Indeed, an active resistance is being mounted against this neopositivist resurgent attack on epistemologies once called "subjugated" (Haraway, 1988). Whole knowledge systems and the ways of life they make possible are being threatened at the beginning of the 21st century in the name of science.

EPISTEMOLOGY

I return to Lagemann's (2000) statement at the beginning of this essay that an instrumental orientation to scholarship in education rather than a historical and philosophical orientation has always limited the field. That orientation is highlighted in the SBR movement. Patti Lather (1996, p. 2) explained some time ago that "methodology often diverts attention from more fundamental issues of epistemology," and Brian Fay (1987) reminded us that "metatheories of science are not ontologically neutral" (p. 42). That epistemology and ontology are largely unaddressed in the SBR movement may account for what seems like its willful ignoring of an extensive and readily available critique of the science it promotes. At the intersection of different epistemologies, ontologies, and methodologies, science is redescribed. Without that understanding, science may indeed appear to be "unified," to be only one thing, as positivism suggests. Despite the fact that positivism, with its desire for quantification, was roundly rejected on many fronts almost as soon as it appeared, and despite the fact that almost no one wants to be called a positivist, it is alive and well in various guises in this culture. Tom Bottomore (1991) describes how it took hold as follows:

> Positivism became a more-or-less organized international political and intellectual movement, but its central themes have achieved a diffusion in present-day society immensely wider than the reach of any particular movement. The more vigorous and systematic "logical positivism" or "logical empiricism" of the Vienna Circle [in the 1920s] became the most influential tendency in the philosophy of science in the twentieth century, while the project of extending the methods of the natural sciences (as interpreted by empiricist philosophy) to the social sciences has until recent decades been the dominant tendency of thought in these disciplines. (p. 433)

Bottomore's point that strains of positivism have been dominant *until recently* is perhaps what puzzles so many of us. We wonder where those who now hail positivism as the savior of education have been for the past half century during which it became evident that this particular description of science had lost much of its explanatory power.

> Despite proclamations that the interpretive turn in the social sciences is complete, and the claims to victory in the "paradigm wars" in education proffered by a diverse range of "qualitative" inquirers, that news does not seem to have reached those folks who have the ears of the IES. (Schwandt, 2005)

How can that be? I admit that I am truly puzzled at this state of affairs and find I agree with Gayatri Spivak (1993), who wrote that she does not understand why "people who do not have the time to learn should organize the construction of the rest of the world" (p. 187).

But Spivak helps me think that the problem with SBR may be one of learning, or rather, of not learning. I believe, as I have written elsewhere (St.Pierre, 2000), that to a great extent, the problem of SBR lies at the intersection of epistemology and ethics and is much about the unintelligibility of epistemologies that are not one's own. I think that when we are entrenched in a particular way of thinking about the world, one in which we have been trained, one that seems to suit our ends and our dispositions, it is very difficult to hear others, to be *willing* to hear them. But if we are really working any epistemology for all it's worth, we will inevitably come up against what Deborah Britzman (1995) calls the "limits of intelligibility" (p. 155), the boundary "where thought stops what it cannot bear to know, what it must shut out to think as it does" (p. 156). At this boundary, ethics comes into play, because we are not just rejecting another epistemology to shut out critique and keep our own intact, we are also rejecting the people who live that epistemology. I was once asked whether I took personally the rejection of postmodernism in the 2002 NRC report. I was astonished even as I realized that the questioner simply did not get it. Of course I take it personally. It was the same as asking me whether I would take personally an attack on how I live in the world. But that question was possible only within an epistemology that believes methodology is simply instrumental and divorced from epistemology and, by extension, that the knower is separate from the known (knower/known), that the scientist can be "objective" (objective/subjective), or that value can be removed from science (fact/value). A social constructionist or a critical theorist would not have thought or asked that question because those binaries do not hold in those ways of knowing.

Judith Butler (1995) asks an ethical question that is useful at this point:

> For the question of whether or not a position is right, coherent, or interesting is, in this case, less informative than why it is we come to occupy and defend the territory that we do, what it promises us, from what it promises to protect us. (pp. 127-128)

Nietzsche (as cited in Spivak, 1974) warns that "One seeks a picture of the world in *that* philosophy in which we feel freest; i.e., in which our most powerful drive feels free to function" (p. xxvii). What is evident in much of the work of the IES and in the 2002 and 2005 NRC reports is an increasingly unchecked deployment of a particular epistemology and its methodology by people in positions of power who do not understand or, if they do, will not acknowledge the violence of that deployment on real people—for example, "Do you take the rejection of post-modernism in the 2002 NRC report personally?"

Ruccio and Amariglio (2003) describe how exclusion structures modern science:

> If the growth of scientific knowledge is the key accomplishment of the past three centuries in the West, it has been accompanied by an elaborate philosophical defense of a variety of exclusionary practices by which those deemed to be untrained in or unreceptive to such science are shunted aside or even denied opportunities to speak (since they are considered to be the voice of unreason). (p. 42)

Serres (1995) says that science took over reason in the Enlightenment, which

> was very instrumental in categorizing as irrational any reason not formed by science. Now, I maintain that there is as much reason in the works of Montaigne or Verlaine as there is in physics or biochemistry, and reciprocally, that often there is as much unreason scattered through the sciences as there is in certain dreams. Reason is statistically distributed everywhere; no one can claim exclusive rights to it. (p. 50)

Strategies for exclusion abound, and I believe that exclusion is unethical. Furthermore, those in positions of power cannot simply ignore the historical link between "science-as-usual" (Harding, 1991, p. 1) and ethics that moved to center stage in this country at the end of World War II and during the Korean and Vietnamese Wars and the social movements of the 1960s and 1970s when it was no longer ethically acceptable for science to exclude the voices of large populations in this country (women, Blacks, homosexuals, the poor, the old, the disabled, and other minorities) or to presume it could produce pure, uncontaminated "knowledge for knowledge's sake"—the Enlightenment rationale for scientific endeavor. After all, science and its knowledge had been used to support genocide and other (in)human brutalities, racism, sexism, classism, anti-Semitism, homophobia, ageism, and so on.

The idea that science is neutral, even transcendent—above the level of human activity, above politics and power—was long ago debunked. As Habermas (1968/1971), for one, pointed out, knowledge and science are always tied to human interests even if some scientists try to make both seem value free (see Hilary Putnam, 2002, for a critique of the fact/value dichotomy). Habermas explained that "since Kant science has no longer been seriously comprehended by philosophy. Science can only be comprehended epistemologically, which means

as *one* category of possible knowledge" (p. 4). Yet, after Auguste Comte proposed positivism in the 19th century, Habermas believes that

> the heir of the theory of knowledge is *methodology* [italics added] pursued with a scientistic self-understanding of the sciences. "Scientism" means science's belief in itself: that is, the conviction that we can no longer understand science as one form of possible knowledge, but rather must identify knowledge with science. (p. 4)

So positivism's methodology reigns supreme in some circles, and the knowledge it produces with its science is assumed to be the best and the truest. Other knowledge is rejected and classified as "prescientific" or "interesting" but not serious, certainly incapable of producing the powerful warrants that experimental researchers invoke when they use methods they claim will produce unbiased data and valid results. Habermas's description of scientism maps onto the claims of, for example, the IES, with its focus on a positivist methodology as the guarantor of quality and objective knowledge.

Foucault (1980) wrote that

> each society has its regime of truth, its "general politics" of truth; that is, the types of discourse which it accepts and makes function as true; the mechanisms and instances which enable one to distinguish true and false statements, the means by which each is sanctioned; the techniques and procedures accorded value in the acquisition of truth; the status of those who are charged with saying what counts as true. (p. 131)

The federal government is reinstalling an old regime of truth, one that almost caused a revolution in this country 40 years ago, in order to maintain its conservative agenda, and education is an easy target. Foucault (1984/1988) said, "I believe too much in truth not to suppose that there are different truths and different ways of speaking the truth" (p. 51). The science I value acknowledges that there are different truths (but not that "anything goes") and that our task as scientists should be "to produce different knowledge and produce knowledge differently" (St.Pierre, 1997, p. 175) in order to enlarge our understanding of those issues about which we care deeply.

AN ETHICAL TURN

So what are scientists to do when their science is, at best, proclaimed by the powerful to be prescientific and, at worst, rejected? What are we to do when we find a description of science that has been found so lacking overtaking other descriptions and intruding into all areas of education? SBR is a fine example of Foucault's (1978/1991) *governmentality*, an analysis of liberalism and neoliberalism that describes a mode of power by which state and complicit nonstate institutions and discourses produce subjects that satisfy the aims of government policy, in this

case a conservative restoration. This is not a pretty picture. "It is the kind of government mentality that expands its reach into all aspects of the lives of its citizens; it is the kind of governance that counts, describes, defines, that brings everything under its gaze" (Scheurich, 1994, p. 306). Stephen Ball (2001) says SBR serves

> the attempt to monitor, control and instrumentalize all and every facet of educational experience. To make us all to think about ourselves as individuals who calculate about ourselves, "add value" to ourselves, improve our productivity, live an existence of calculation, make ourselves relevant. (p. 266)

This impulse sees "teaching as a form of technical control [engineering] over the production of learning outcomes, thereby rendering them increasingly predictable" (Elliott, 2001, p. 558) in order to serve a market-driven economy.

I joined this fray naively, thinking that the 2002 NRC report and the article by Feuer et. al (2002b) in *Educational Researcher* to which I was invited to respond were somehow aberrations, but as I have tracked this movement for the past 4 years in this country and in Britain, where SBR has been in place longer and its critique more sustained and damning, I have lost that innocence. I have experienced, variously, astonishment, disbelief, outrage, and despair at how SBR is being deployed as the truth about science and at the insidious and stealthy way it deposits that truth throughout the network of education. I have struggled to compose an ethical approach to those who support SBR, because I know as well as anyone that any epistemological position, including my own, can be dangerous.

I situate my work and my life in bodies of knowledge called postmodernism and feminism, both of which demand that we question the truths we hold sacred, our necessary fictions. Butler's (1995) caution that I should not forget that my attachments to postmodernism and feminism are protecting me from thinking certain things, and Spivak's (1993) caution that "what I cannot imagine stands guard over everything that I must/can do, think, live" (p. 22) insist that I engage, rather than exclude, other epistemologies and the Other who claims them in order to move toward the unthought. Thomas Keenan (1997) has complicated my understanding of ethics with his comments about reading, which he says is

> our exposure to the singularity of a text, something that cannot be organized in advance, whose complexities cannot be settled or decided by "theories" or the application of more or less mechanical programs. Reading, in this sense is what happens when we cannot apply the rules. (p. 1)

I suggest that ethics, too, is what happens when the simplistic rules and moral codes our culture provides become inadequate in the face of the difference of the Other. I suggest that science is what happens when we give up the simplistic definitions and procedures our culture provides in the face of inquiry in medias res. Thus, I will always be unprepared to be ethical, and I will never know what science is. This is deconstruction at its finest, this breakdown of certainty, a willingness

to be unsure and to learn to thrive in the restless, rigorous confusion that is learning—*inquiry.*

In the current political climate, reversing the binary and putting in charge of truth a different set of experts with an oppositional view is not an answer; rather, as Derrida (1967/1974) explained, the task is to overturn the structure that allows oppositions to exist. But how we do this? Mouffe (1996) reminds us that "any modern democratic project must come to terms with pluralism. This means discarding the dangerous dream of a perfect consensus, of a harmonious collective will, and accepting the permanence of conflicts and antagonism" (p. 20). I doubt that I will ever be convinced that SBR is a good thing, but I have good friends (e.g., Feuer & St.Pierre, 2005) who are involved in that work, and I doubt that they are fools. It seems to me that habits of generosity, which can exist alongside frustration, conflict, and even anger, might move us toward a different space in which radically different readings of the world (and of science) might function side by side, not necessarily happily or easily, but learningly. I am very interested in how this conflict can be a fruitful site of learning and especially interested in the ethics we will have to invent to learn from each other. I intend to keep showing up in sites where SBR is discussed, as part of the opposition at this point, and hope to find others who might be willing to learn from me as I am from them. As Goldfarb (as cited in Schwandt, 2005) says, the "success of the opposition is not required. Its persistent appearance is" (p. 305).

NOTES

1. Positivism is the theory developed by Auguste Comte in the 19th century that attempted to extend the methods of the natural sciences to the study of society. Patti Lather (1991) provides the following summary of positivism:

> The basic assumptions of positivism are four: 1) the aims, concepts, and methods of the natural sciences are applicable to the social sciences; 2) the correspondence theory of truth which holds that reality is knowable through correct measurement methods is adequate for the social sciences; 3) the goal of social research is to create universal laws of human behavior which transcend culture and history; and 4) the fact/value dichotomy, the denial of both the theory-laden dimensions of observation and the value-laden dimensions of theory create the grounds for an "objective" social science. (p. 172)

I include postpositivism within positivism when it is used to indicate a slight correction of positivism in an attempt to address some of its most obvious inadequacies but still maintain its foundationalist structure.

2. Information about the National Academy of Sciences (NAS), the Division of Behavioral and Social Sciences and Education (DBASSE), and DBASSE's Scientific Principles for Educational Research, Committee on Research in Education, and evidence committees, their members and their biographies, records of workshops and meetings, and other information are available at the NAS Web site, http://www.nas.edu. Note that the NAS regularly reports the work of its committees at the Current Projects section of its Web site.

3. The articles in issue *10*(1) of *Qualitative Inquiry* are by Yvonna S. Lincoln and Gaile S. Canella (2004), Patti Lather (2004), Joseph P. Maxwell (2004b), Kenneth R. Howe (2004), Thomas S. Popkewitz (2004), Katherine E. Ryan & Lisa K. Hood (2004), Marianne Bloch (2004), Elizabeth Atkinson (2004), and Elizabeth A. St.Pierre (2004).

The articles in issue *107*(1) of *Teachers College Record* are based on a 2003 American Educational Research Association (AERA) session titled "Yes, But Is It Science? Implications of the 2002 NRC [National Research Council] *Scientific Research in Education* Report for Qualitative Inquiry" (Session 22.012). The chairs of the session were Pamela A. Moss and Patti Lather, and the participants were Courtney P. Cazden, Frederick Erickson, James Paul Gee, John Willinsky, and Vanessa Siddle Walker, with Michael J. Feuer as discussant (not Lisa Towne as listed in the conference program). The articles in the special issue are by Frederick Erickson (2005), Margaret Eisenhart (2005b), James Paul Gee (2005), Patti Lather and Pamela A. Moss (2005), Pamela A. Moss (2005a), Vanesssa Siddle Walker (2005), and John Willinsky (2005).

The articles in issue *55*(3) of *Educational Theory* are by Margaret Eisenhart (2005a), Kenneth Howe (2005a, 2005b), Pamela Moss (2005b), and Thomas A. Schwandt (2005).

4. Norman Bradburn, the evidence committee chair, is a statistician whose interest is survey methodology; Cynthia Beall is a physical anthropologist; Lawrence Bobo is a sociologist who does measurement studies on issues of race; Robert Boruch is a professor of educational psychology and statistics whose work features randomized experimental trials; Nancy Cartwright is a philosopher whose research interests include causal inference and objectivity in science; Mark Chassin is a physician who is interested in developing measures of the quality of health care; Ron Haskins is an economist; Susan Mayer is a sociologist interested in the measurement of poverty; Kenneth Prewitt is a political scientist who was director of the U.S. Census Bureau; Timothy Smeeding is an economist; and Ewart Thomas is a psychologist and statistician.

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