

# This IS Your Father's Paradigm: Government Intrusion and the Case of Qualitative Research in Education

Patti Lather  
*Ohio State University*

*This talk was the Egon Guba Invited Lecture at the American Educational Research Association annual conference, April, 2003, Chicago. This article mobilizes three counterdiscourses to critique the federal government's incursion into legislating scientific method in the realm of educational research via the "evidence-based" movement of the No Child Left Behind Act of 2001. Foucauldian policy analysis, feminism via Luce Irigaray, and postcolonialism via Stuart Hall are used to situate such scientism as a racialized masculinist backlash against the proliferation of research approaches that characterize the past 20 years of social inquiry. Congressional disdain for educational research is addressed within a context of the Science Wars and the needs of neoliberal states, including conservative restoration.*

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Could quantification settle important issues of public policy? Experience was often disappointing, but hope sprang eternal.

—Porter, 1995, p. 152

Science as I have known it and practiced it over the years has had little to do with Washington, D.C. I learned early in my academic career that the Reagan administration was not interested in funding my feminist critical qualitative research. I have stayed away from grants and grant culture ever since, doing what might be termed "little science" with little money and getting by just fine. But during the past year or so, I have found myself sucked into an alphabet soup of OERI, NRC, DOE, NSF, Senate subcommittees on this or that, even something dubbed "web scrubbing" where the U.S. Department of Education is deleting research, including ERIC digests, that it deems unsupportive of Bush administrative agendas.<sup>1</sup> This article is an effort to make sense of the federal government's incursion into legislating scientific method in the realm of educational research via the "evidence-based" move-

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ment of the past few years. Although I will attend some to the consequent effort to address congressional disdain regarding educational research via the National Research Council's (NRC) (2002) report *Scientific Research in Education*,<sup>2</sup> my primary interest is in the structure of the situation. Hence, in what follows, I address the many factors at play including the Science Wars and the needs of neoliberal states in a time of proliferating insurgent "special interests," including that of conservative restoration. Also at play are academic capitalism, entrepreneurship, and ambition, and with a nod to Adorno and for me, always, Walter Benjamin, the traditions of critical theory in terms of the critique of instrumental reason.

In taking on these latest twists and turns in governmental efforts to effect educational research, the reductionisms of positivism, empiricism, and objectivism are assumed. I do not want to rehearse the various critiques of scientism that have arisen in the 30-plus years since Thomas B. Kuhn's (1970) *The Structure of Scientific Revolutions*. Instead, I ask three questions about what I find to be a profoundly troubling situation. First, what is happening to make me willing to return to the scene of my doctoral training in evaluation methods some 20 years ago, to immerse myself in the language of "treatment homogeneity," "setting invariance," the "promiscuous" use of quasi experiments (Cook & Payne, 2002, p. 173), and my favorite, "inadvertent treatment crossovers," in this case of a principal in the treatment condition married to someone in the control school (Cook & Payne, 2002, p. 163)? Secondly, what are the implications for qualitative research of the NRC report, a report that intended a "catholic view toward research methods" in delineating "high quality science" (Shavelson, Phillips, Towne, & Feuer, 2003, p. 25)? Finally, how might the federal effort to legislate scientific method be read as a backlash against the proliferation of research approaches of the past 20 years out of cultural studies, feminist methodology, radical environmentalists, ethnic studies, and social studies of science, a backlash where in the guise of objectivity and good science, "colonial, Western, masculine, white and other biases" are smuggled in (Canclini, 2001, p. 12)? In surveying a variety of ways this topic could be approached, I will particularly call on discourse practices of Foucauldian policy analysis, feminism via Luce Irigaray, and post-colonialism via Stuart Hall.

### LEGISLATING METHOD: SCIENCE FOR POLICY OR POLICY FOR SCIENCE?

Education research is broken in our country . . . and Congress must work to make it more useful. . . . Research needs to be conducted on a more scientific basis. Educators and policy makers need objective, reliable research.

—Michael Castle, U.S. representative,  
quoted in NRC, 2002, p. 28

It is, of course, an old argument that the social sciences are not to be subsumed under a natural science model. In my first encounter with this argument as a doctoral student, Marcia Westkott (1979) argued against what she termed "the first positivist assumption": that the methods appropriate for studying the natural world are equally appropriate for the study of human experience. What is new in what I have to say here is that in this moment of what Foucault (1981/1991) termed "our contemporaneity" (p. 40), this old argument against a unified idea of science (Galison & Stump, 1996) is being disavowed via nakedly political and self-aggrandizing moves.

John Willinsky's (2001) call to broaden and deepen federal major policy statements regarding the translation of educational research into practice provided a wake-up call for me regarding movements at the federal level to legislate method. Rather ingeniously, Willinsky attached a critical agenda to one that is decidedly instrumentalist and even shocking in its lack of attention to the past 20 years of "utilization" research on why "'top-down linear' R&D models of the 1950s and 1960s" did not work (p. 7). Arguing for democratic forms of collaboration and exchange rather than "heavy-handed intentions of driving educational practice" (p. 7), Willinsky foregrounded the "productive tensions and radical challenges that mark this play of interpretations within social science research" (p. 7).

Worried about "research-wielding technocrats" (p. 9), his article so scared me to death that at the American Educational Research Association's (AERA) 2002 conference, I broke my usual rule of no 8:00 a.m. sessions to attend Ellen Lagemann's talk about her 2000 book on the history of educational research, *An Elusive Science: The Troubling History of Educational Research*. Although her talk was lovely, the discussion afterwards was not. It turned to the attempts of the NRC (2002) report to negotiate between the federal government and the educational research community what it means to do scientific educational research.<sup>3</sup> In spite of the efforts of the NRC report toward a "big tent" of legitimate methods in educational research (Feuer, Towne, & Shavelson, 2002), Lagemann seemed adrift in addressing how calls for generalizability, objectivity, replicability, and a unified theory of science reinscribe a science under duress for some 30 years. Made aware of an afternoon session where what I call "the suits" would be on the podium in force, I went to that session and became even more aghast at the framing statement from a representative from the Office of Educational Research and Improvement about the need for policy research that supported the present administration's initiatives.<sup>4</sup> This sort of nakedness was either strategic or naïve, and these folks did not look naïve. I began to think that maybe I was the naïve one, that I would think the past 30 years of the social critique of science might actually shape contemporary thought about policy driven research. And I began to plot this article as I asked what is happening when at the very time there is a philosophical trend against certainty in the social sciences "this continual and noisy legislative activity" (Elden, 2002, p. 146), with all of its normalizing authority, is working

at the federal level to discipline educational research to a narrowly defined sense of science-based evidence.<sup>5</sup>

When Andy Porter, past AERA president, visited Ohio State University after the 2002 AERA conference, I asked him how scared I should be. "Do I look scared?" he asked back. "No," I replied. "Do I look young?" he asked. "No," I replied, as he stated his view that trends come and go in Washington while the rest of the country gets on with its business. I do not share Dr. Porter's sanguine outlook on this matter. Irrespective of whether the 15-year timeline of the Strategic Education Research Program of the NRC will change the face of educational research, this seems about much more than the latest trend in D.C. Elizabeth Atkinson (2004 [this issue]), for example, asks who loses when "a nation of researchers is locked into a government policy agenda" (p. 117) and urges a sort of heresy against "serving policy" to the point where we collude in our own oppression. How can we take Atkinson's charge to heart of "thinking outside the box" in a time when educational research is being told what science is by bureaucrats and Congress at the very time that an expansive definition of science is being urged in the more high-status areas of science (e.g., Goenka, 2002)?<sup>6</sup>

As the latest wave of the conservative attack on education, this kind of "activist interventionism and expansion of the scope of government" (Shaker, 2002) gives the lie to the rhetoric of decreased federalism in the conservative restoration. Paul Shaker (2002) wrote of the Reading Wars: "This is not a fair fight, it is not what it seems on the surface, and the stakes are high" (p. 11). Learning lessons from its effort to gain control of reading research, the government has targeted math, science, professional development, and comprehensive school reform as its next objects of "high scientific standards." With random field trials (RFTs) now specified by Congress ever more frequently in effectiveness studies of federally funded programs, the design and application of educational research has become a partisan tool, much like standardized tests have functioned for almost two decades now.

What work does the NRC (2002) report do in challenging governmental manipulation of science? The NRC, serving as scientific advisor to the government since 1863, has issued five reports on educational research since 1958. This latest one is trying to speak against the narrow scientism of the Elementary and Secondary Education Act (No Child Left Behind Act of 2001) that was signed into legislation in January 2002. According to Marilyn Cochran-Smith (2002), in an editorial in the *Journal of Teacher Education*, this act "virtually mandates" that to be funded, educational research must be evaluated "'using experimental or quasi-experimental designs . . . with a preference for random-assignment experiments'" (p. 188). By 2004, 75% of funded research is targeted to be RFTs. Rather than a focus on randomized experimental trials as the gold standard, the NRC report attempts inclusivity regarding a range of approaches to educational research, both "quantitative" and "qualitative."

In delineating the scientificity of science, although the NRC (2002) report tries to walk a fine line, it is, ultimately, what Foucault (1981/1991) termed "a kind of tribunal of reason" (p. 60). Given the report's oft-repeated intentions of balance across multiple methods, it took an article by several committee members on the scientificity of design studies in a later issue of *Educational Researcher* (Shavelson et al., 2003) for it to become clear how objectivity is enshrined and prediction, explanation, and verification override description, interpretation, and discovery. Although the contested nature of science is much evoked in the report, an epistemological sovereignty is assumed in delineating and applying principles in the doing of "high quality science." The exclusionary force of its "guiding principles" is striking in its disavowal of different views of evidence, analysis, and purpose. Rationality's domesticating power is particularly fascinating in that the chapter on the specificities of educational research lists all that gets in the way of an engineering approach to science. Values and politics, human volition and program variability, cultural diversity, multiple disciplinary perspectives, the import of partnerships with practitioners, even the ethical considerations of random designs: all are swept away in a unified theory of scientific advancement with its mantra of "science is science is science" across the physical, life, and social sciences. Although one expects to sort through several voices in a committee prepared document, in the end, its efforts to provide guidelines for rigor and enhance a "vibrant federal presence" (NRC, 2002, p. 129) are complicit with the federal government's move to evidence-based knowledge as much more about policy for science than science for policy.

## EVIDENCE-BASED PRACTICE AND SCIENCE, MONEY, AND POLITICS

With the NRC (2002) report under my belt and with little time for Derrida and Deleuze, I buried myself in the updates on "Bush Science" from *Education Week*, tried to keep up with policy analyses of these twists and turns, and even developed some Web access skills. I learned three things from all of this.

### The British Scene

The first is that Britain has been going through this extremely interventionist regulatory climate policed by statutory bodies for more than a decade. In a "taking stock" edited book published in 2000, *Evidence-Based Practice: A Critical Appraisal*, the focus is largely on health care policy (Trinder & Reynolds, 2000). Appraising strengths and weaknesses across both "champions and critics" (Trinder, 2000a, p. 3), its appeal and, hence, rapid influence is

theorized as rooted in the needs of posttraditional societies for ways of managing risk in the face of a paradoxical dependence on and suspicion of experts and expert knowledge. Combined with the push to value for money, the rise of managerialism, consumerism, and political discourses of accountability and performance, neoliberal ideologies of the neutrality via proceduralism of such practices prevail in an "explosion of auditable management control systems" (Trinder, 2000a, p. 9). Here, at last, is a way to manage quality issues by displacing professional judgment with promised effectiveness via the procedural production of evidence. Although "a product of its time" (Trinder, 2000a, p. 5), the problem is that there is little evidence that evidence-based practice actually works (Trinder, 2000a, p. 2).<sup>7</sup>

In terms of education, Hammersley's (2000) chapter on the British scene notes that in medicine, the focus was on quality of practice whereas in education, the focus has been on the quality of research (p. 163).<sup>8</sup> Hammersley also noted the focus on teaching as opposed to administration and management and how, in spite of the claims of evidence-based practice of being a "radically new venture" (p. 164), research-based teaching has a long history, including a long critique. The shift to qualitative methods in the 1970s was related to the difficulties of measuring what is educationally significant and the limits of causal models given the preponderance of interaction effects. As a result, according to Hammersley, educational research became "embroiled in philosophical and methodological disputes" (p. 167) that cannot be simply overcome. Replicability, for example, is no answer given the "complex web of relationships" (Hammersley, 2000, p. 168). The degree to which the kinds of problems that teachers face are open to solution by research is precisely the question. The importance of contextual judgment mandates a great caution in adapting the medical model. Formulas for transparent accountability are more about politics than about quality of service. Teachers are not as powerful as doctors, so it is worrisome that educational managers can more likely force narrow definitions of effective practice (Trinder, 2000b, p. 238).<sup>9</sup>

For the purposes of this article, it is the mutations of the classic approach in the British scene that are particularly instructive. The introduction of qualitative research, the interruption of the top-down approach, and the pluralistic interpretations of what is evidence: this is a sort of translation in diffusion. Calls for effectiveness studies of evidence-based practice displace the hegemony of meta-analysis and randomized clinical trials by capitalizing on the move in focus from advocacy to implementation. Here qualitative or mixed methods are *de rigueur*. In nursing research for example, given displacement of the empiricism of the natural sciences by phenomenology and its rejection of objectivism, the uneasy fit of qualitative, its lack of a sense of certainty, and its eclecticism require considerable adaptation and, hence, might work as a counterforce to prevailing narrow ideas of what constitutes evidence.<sup>10</sup>

### From Back in the Day

The second thing I learned is that there is a handful of advocates well positioned to push for this. Out of a 1999 conference named, ironically, for Donald Campbell who, of course, championed the case study in later life,<sup>11</sup> emerged a 2002 publication by the Brookings Institution. This book, *Evidence Matters: Randomized Trials in Education Research*, is coedited by Frederick Mosteller, professor emeritus of mathematics at Harvard and early 1970s architect of randomized clinical trials as the gold standard in medical research, and Robert Boruch of the Campbell Collaboration (see Note 15). In their introduction, the coeditors laud the government's serious interest in the quality of education research. Permeating the text are terms such as "standards of evidence" and "scientific rigor," with a nod toward "other kinds of research" as "augmentation" to controlled studies, provided "scientific standards" can be delineated. Driven by "worry about ideology parading as intellectual inquiry" (Boruch & Mosteller, 2002, p. 2), their task was to persuade sponsoring agencies "that there is no easier way to get the answers to the right question" than RFTs (Boruch & Mosteller, 2002, p. 3). Shocked by the paucity of "good studies" (Boruch & Mosteller, 2002, p. 4), they called for political and administrative support for rigorous research to address the bad reputation of educational research. Although claiming "refreshed ways of thinking" (Boruch & Mosteller, 2002, p. 4), most of the essays are a response to critics during the past 20 years.<sup>12</sup>

At root is what to do about federal needs for evaluation data on educational initiatives in a time of belt-tightening economies. The good old days of the 1960s are evoked when the federal trough was rich with program evaluation monies as the research budget soared from US\$3 million in 1960 to US\$100 million in 1967 (Vivovskis, 2002, p. 123). Foregrounding an expansive federal role in financial, political, and regulatory environments, they long for something like the Food and Drug Administration to "require good evidence" regarding which educational interventions are safe or effective. It is high time then, for "rigorous evaluation" on the part of "randomizers" to assume important positions at the federal level. "Generating better evidence for better education" (Boruch & Mosteller, 2002, p. 14) is the watchword.<sup>13</sup>

Since the Reagan years, the growing perception has been of more money chasing after bad research and evaluation. Federal agencies were increasingly under the gun of a Republican House of Representatives that wanted to win elections on pro-school reform platforms while spending the least dollars. The watchwords of "scientifically sound" and "politically objective" captured the widespread "discontent with the state of current knowledge of what works in education" (Cook & Payne, 2002, p. 150). Cook and Payne (2002) saw the rejection of experimental design as "probably a major cause of the impoverished current state of knowledge" (p. 151), blaming Eisner, Guba, Lincoln, Patton, Stake, and Stufflebeam by name.<sup>14</sup>

It is of particular interest how conservative think tanks have ratcheted up their focus on education issues since the late 1980s and how entrepreneurial interests are at work.<sup>15</sup> In *Science, Money and Politics*, Daniel Greenberg (2001) probed the demands for utilitarian science versus scientific autonomy within the National Science Foundation, which he situated as a "little dog" compared to defense, space, and medical research. Greenberg noted that the "politics of the academic pork barrel" (p. 184) work toward a sort of "scientific welfare" (p. 39) within the Enterprise University (p. 356) with its grant economy where the social sciences are insulted by being largely left out. If real science, as Greenberg suggested, is about skepticism, curiosity, and passion (p. 24) where transparency of process is the only agreed-on fundamental, "could science serve us better" (p. 10) if it moved beyond its "capacity for believing it is the victim of neglect and hostility" (p. 60) and its grant chasing?<sup>16</sup>

It appears that science, money, and politics have combined with prepositioned capability and sweetheart contracts on the part of self-described "ambitious researchers" (Burtless, 2002, p. 193) to court the increased federal role in the adoption of experimental methods. As argued by Baez and Boyles (2002) in their lovely analysis of the discourse of grants, it is not that "academic capitalism" has not become our way of life. The deal has already been struck. The question is the extent to which we can promote critical work within such a milieu, "work which challenges the categories that organize [our] existence" (Baez & Boyles, 2002, p. 45) given the "Faustian bargain" of the federal and corporate embrace.

### **Toward a Policy Relevant Counterscience: Fieldwork in Philosophy**

The third thing I have learned is that we need to put our critical theory to work in this moment of our now. In his discussion of how conservative modernization has radically reshaped the common sense of society regarding education, Michael Apple (2001) asked, "If the right can do this, why can't we?" (p. 194). In addressing such a question, I suggest that the Left needs a policy turn (Bennett, 1992; Ferguson & Golding, 1997; McGuigan, 2001; McRobbie, 1997), with a focus on program evaluation as a particularly cogent site where a policy relevant counterscience might be worked out.

Suggestive here is *Making Social Science Matter* by Bent Flyvbjerg (2001), a Danish urban developer who argued for a move from a narrowly defined epistemic science to one that articulates a social science that integrates context dependency with practical deliberation. Here considerations of power are brought to bear in delineating a knowledge adequate to our time. Rather than the self-defeating "physics envy"<sup>17</sup> that underlies the objectivist strands of the social sciences, this is a social science that can hold its own in the Science



Wars by contributing to society's practical rationality in clarifying where we are and where we want to be.

Flyvbjerg's (2001) argument for a practical philosophy of ethics that takes power into account "as a point of departure for praxis" (p. 70) focuses on the context of practice as a disciplining of interpretation. Mandating on the ground empirical work, theories are constantly confronted with praxis toward public deliberation. Here social science becomes a sort of laboratory toward public philosophy, what Bourdieu termed "fieldwork in philosophy" (quoted in Flyvbjerg, 2001, p. 167). Case studies assume prime importance as critical cases, strategically chosen, provide "far better access for policy intervention than the present social science of variables" (Flyvbjerg, 2001, p. 86). In such a laboratory, against a narrow scientism in policy analysis and program evaluation, the urgent questions become: Where are we going with democracy in this project? Who gains and who loses and by which mechanisms of power? Given this analysis, what should be done?

"Simultaneously sociological, political and philosophical" (Flyvbjerg, 2001, p. 64), this is a science that does not divest experience of its rich ambiguity because it stays close to the complexities and contradictions of existence. Focusing on practices as event, detecting forces that make life work, sociality and history are seen as the only foundations we have. Instead of emulating the natural or, in Foucauldian terms, "exact" sciences, the goal is getting people to no longer know what to do so that things might be done differently. This is the yes of the setting-to-work mode of postfoundational theory that faces unanswerable questions, the necessary experience of the impossible, in an effort to foster understanding, reflection, and action instead of a narrow translation of research into practice.

## INTERRUPTING A DISCOURSE ONE FINDS SO PROFOUNDLY TROUBLING

In my final section, I put into play three discourse practices quite scandalous to that of the NRC (2002) report to explore what it might mean to dissolve the continuities of dominant narratives. In short, the uncompromising discourses of Foucauldian policy analysis and feminist and postcolonial science are called on to evoke the science that might be possible after the critique of science.

### A Foucauldian Reading

In Foucauldian terms, policy is one of the three technologies of governmentality, the others being diplomatic/military and economic.<sup>18</sup> Policy is to regulate behavior and render populations productive via a "biopolitics" that

entails state intervention in and regulation of the everyday lives of citizens in a "liberal" enough manner to minimize resistance and maximize wealth stimulation. Naming, classifying, and analyzing: all work toward disciplining through normalizing. Such governmentality is "as much about what we do to ourselves as what is done to us" (Danaher, Schirato, & Webb, 2000, p. 83). It is, contrary to those who see Foucault as a pessimist and determinist, much about how understanding such processes might raise possibilities for doing otherwise.

In *The Order of Things*, Foucault (1970) turned to the matter of the status of the human sciences. Here Foucault argued that to look at such sciences as "pre-paradigmatic" is to buy into some "maturation" narrative that belies how the human sciences are about "constantly demystifying themselves" rather than making themselves more precise (pp. 356, 364). Locating the human sciences in the interstices of the mathematizable and the philosophical, "this cloudy distribution" (Foucault, 1970, p. 347) is both their privilege and their precariousness. Language, meaning, the limits of consciousness, the role of representations, this is the stuff of human seeking to know. Rather than lacking in exactitude and rigor, the human sciences are more a "'meta-epistemological' position" in being about "finitude, relativity, and perspective" (Foucault, 1970, p. 355). Here their very "haziness, inexactitude and imprecision" (Foucault, 1970, p. 355) is the form of positivity proper to the human sciences: "blurred, intermediary and composite disciplines multiply[ing] endlessly" (Foucault, 1970, p. 358).

Whether this is "truly scientific" or not is a "wearisome" discussion (Foucault, 1970, p. 365). The human sciences do not answer to criteria of objectivity and systematicity, the formal criteria of a scientific form of knowledge, but they are within the positive domain of knowledge as much as any other part of the modern episteme. There is no internal deficiency here; they are not "stranded across the threshold of scientific forms" (Foucault, 1970, p. 366). They are not "false" sciences; "they are not sciences at all" (Foucault, 1970, p. 366). They assume the title to "receive the transference of models borrowed from the sciences" (Foucault, 1970, p. 366). Enacting "a perpetual principle of dissatisfaction, of calling into question, of criticism and contestation" (Foucault, 1970, p. 373), such knowledges are tied to a praxis of unmasking the representations we give to ourselves of ourselves. Here is where we learn to think again, opening ourselves to a future thought of the knowledge of things and their order.

In terms of the recent governing mentality of educational research, the "privilege accorded to . . . 'the sciences of man'" is based on the "'political arithmetic'" (Foucault, 1998, p. 323) that makes particular kinds of discourse both possible and necessary. This is not so much about concepts on their way to formation or even the price paid for scientific pretensions, but rather of understanding claims to scientificity as discursive events. Here the "inexact knowledges" become "a field of strategic possibilities" (Foucault, 1998, p. 320),

a "counterscience" of "indisciplined" policy analysis that troubles what we take for granted as the good in fostering understanding, reflection, and action.

### A Feminist Reading

This militantly empiricist and quantitative movement, this desire for hardness with its claims to produce findings that are verifiable, definitive, and cumulative, is set against a softness where interpretation is central and findings are always subject to debate and reinterpretation (Gherardi & Turner, 1987).

French feminist theory is premised on the idea that the classic structure is splitting and opening to becoming and that this becoming will be initiated primarily by women as men have more to lose and psychic structures more called to the scene of castration (Conley, 2000, p. 25). Irigaray's argument that there are "systems of thought dominated by the logic and linguistics of male sexual organs" is, of course, based on psychoanalytic theory (Olkowski, 2000, p. 91). Her concern is that we have so naturalized such language and logic that we do not see the practical aspects of such domination. Regimes of power and systems of philosophy are designed to "penetrate," interventions are engineered, "we encourage one another to be 'hard' on issues" (Olkowski, 2000, p. 92). In contrast is the sort of "embarrassing emotion-fest" of women's work that can be interpreted only as "excess . . . wild or crazy, bizarre, remote, or meaningless" to the task of social policy (Olkowski, 2000, p. 93). Intelligibility demands that language conform to hegemonic and rigid hierarchies, systems of formulation, standards of truth within a logic of solid mechanics. What Irigaray called "placental economies" of fluid negotiation "make us shudder" within the "order of good sense" (Olkowski, 2000, pp. 96, 99). The structure of oppositions thus set up reads such claims to truth as "false claimants, 'corrupted by dissemblance' . . . made from below, by means of an aggression, an insinuation, a subversion, 'against the father' and without passing through the Idea" (Olkowski, 2000, p. 99). "Disconcerting the erection of the male subject," women's bad copies or fake science are "an abyss in which the Father could no longer recognize himself" (Olkowski, 2000, p. 101). Proceeding by alliances, symbiosis, contagion, and what Irigaray called "mucosity," this is a kind of refusal of recognition and of the proper rather than a scene of good daughters making bad copies via replication studies.

Charges of essentialism are, of course, rife here. Arguing what Deleuze called "becoming-woman" as having a "special introductory power," key to all other becomings (Olkowski, 2000, p. 103), women's insight into multiplicity and difference is held to come from the "assemblages that produce minoritarian groups . . . those outside the rules" (Olkowski, 2000, p. 106) and from an embodiment that is not organized by castration or its threat

(Olkowski, 2000, p. 107). Depathologizing that which is associated with women, "the uteral, the vulvar, the clitoral, the vaginal, the placental" (Olkowski, 2000, p. 107): this would transform the social contract and give purchase to seeing science as a site of contestation, an always already gendered practice.

### A Postcolonial Reading

In rereading Stuart Hall (1996) on Gramsci for the introduction to cultural studies class I recently taught, I was struck with how the Right models Gramsci's tactics of a "war of position." Condensing a variety of different relations and practices into a definite system of rules through a series of necessary displacements, the state "plans, urges, incites, solicits, punishes" (Hall, 1996, p. 429).

As a sort of "regressive modernism," this disciplining and normalizing effort to standardize educational research in the name of quality and effectiveness is an attempt to hegemonize and appropriate to a reactionary political agenda deeper tendencies in cultural shifts. These might be termed a "new cultural politics of difference" (Hall, 1996, p. 464) and include the displacement of European high culture, the Americanization of world culture, and the decolonization of the Third World, including the decolonization of First World minorities. Such a politics is marked by unevenness, contradictory outcomes, disjunctures, delays, contingencies, and uncompleted projects.

The danger of the reduction of spaces for the doing of other sorts of research on the part of a cultural dominant is that the decentering of old hierarchies and grand narratives of the past 20 or so years has created new subjects on the political and cultural stage. To try to reinscribe a medical model of the 70s is to set oneself up to be read as an "aggressive resistance to difference" (Hall, 1996, p. 468; West, 1990). This backlash attempt to transfer a canonical model to educational research is an "assault, direct and indirect, on multiculturalism" (Hall, 1996, p. 468). As Hall (1996) noted, "there is no going back" (p. 469).

Overtaken by the carnivalesque, a sort of "low science" has emerged out of this proliferation of difference that challenges the fundamental basis of the mechanisms of ordering and of sense-making of European culture. A rich production of counter-narratives is alive and kicking, from subaltern studies to indigenous research methodologies, from native as anthropologist to Al Jazeera, the Arabic TV channel. This is the end of the innocent notion of knowledge production as value neutral. Efforts by the "top" to reject and eliminate the "bottom" for reasons of prestige and status bite back from a place where White masculinities are no longer at the center of the frame.

Hall's (1996) narrative of the coming of feminism to cultural studies is instructive here. Hall told of being targeted as the enemy, "as the senior patri-

archal figure": "I was checkmated by feminists; I couldn't come to terms with it" (p. 500). By this he meant not personally (he noted he was married to a feminist) but in terms of figuring out how to do useful work. "It was time to go," he said.

## CONCLUSION: INDISCIPLINED KNOWING

Will [man] ever be ready to receive . . . a thought that, freeing him from fascination with unity, for the first time risks summoning him to take the measure of an exteriority that is not divine, of a space entirely in question, and even excluding the possibility of an answer, since every response would necessarily fall anew under the jurisdiction of the figure of figures? This amounts perhaps to asking ourselves: is man capable of radical interrogation?

—Blanchot, quoted in Plotnitsky,  
2002, p. 239

To conclude, I have argued that this move at the federal level is the Science Wars (Plotnitsky, 2002; Ross, 1996) brought to the realm of educational research in a way much marked by the anxieties, rhetorics, and practices of a decentered masculinist and an imperialist regime of truth. In this, I realize that I am an enemy amid talk of *détente* and the end of the Paradigm Wars and the call for mixed methods (Tashakkori & Teddlie, 2002). Rather than *détente*, however, all of this reinforces my interest in what Foucault termed "indiscipline" as a move toward a Nietzschean sort of "unnatural science" that leads to greater health by fostering ways of knowing that escape normativity (Nietzsche, 1974, p. 301). By "indiscipline," Foucault (1994) described a mechanism by which a marginalized population/practice is created to exert pressure that cannot be tolerated by the very process of exclusions and sanctions designed to guard against irregularities and infractions (p. 36).

As an irregular trooper in the Science Wars, I see this latest round of reinscribing the idealized natural science model as an effect of power of a sort of historical amnesia that disavows decades of critique and (re)formulations toward a science after the critique of science. To think about the relation of policy and research in such a place of Foucauldian "indiscipline," what I have offered might be viewed, in a Lacanian register, as "the hysteric's discourse" (Fink, 1995). Here "a truly scientific spirit" is commanded by "that which does not work, by that which does not fit. It does not set out to carefully cover over paradoxes and contradictions" like that of the master's discourse with its imperative to be obeyed within its guise of reason (Fink, 1995, pp. 134-135). The hysteric sees the heart of science as "taking such paradoxes and contradictions as far as they can go" (Fink, 1995, p. 135) rather than endorsing a

monolithic science “based on a set of axiomatic mathematizable propositions, measurable empirical entities, and pure concepts” (Fink, 1995, p. 138).

In short, the Science Wars continue; the line between a narrowly defined scientism and a more capacious scientificity of disciplined inquiry remains very much at issue. In terms of the desirability of degrees of formalization, mathematized and not, generic procedures, and rigorous differentiations, there is virtually no agreement among scientists, philosophers, and historians as to what constitutes science except, increasingly, the view that science is, like all human endeavor, a cultural practice and practice of culture. To operate from a premise of the impossibility of satisfactory solutions means to not assume to resolve but instead, to be prepared to meet the obduracy of the problems and obstacles as the very way toward producing different knowledge and producing knowledge differently. Foucault (1981/1991) termed this “the absolute optimism” of “a thousand things to do” (p. 174) where our constant task is to struggle against the very rules of reason and practice inscribed in the effects of power of the social sciences.

## NOTES

1. *Education Week on the Web* (Davis, 2002) reported that the U.S. Department of Education plans to overhaul its Web site and in the process delete thousands of files (including ERIC digests) of non-Bush era educational research as well as that which does not support the current administration’s views. Government document librarians and education librarians all over the country are quite concerned about the archival implications of this plan, political considerations notwithstanding (see also <http://www.lib.msu.edu/corby/ebss/accesseric.htm>).

It is important to note that such efforts are going on across other areas, including health Web sites where 14 House Democrats have charged that the “web scrubbing” of the Centers for Disease Control site in regards to abortion and breast cancer “distorts and suppresses scientific information for ideological purposes” (Clymer, 2002). In addition, the Pentagon is pressuring CIA analysts to tailor their assessments of the Iraqi threat to build a case against Saddam Hussein in a “politicization of intelligence” (Miller & Droglin, 2002). One journalist surmised that this “illustrates how, far below the political radar screen, the Bush administration can satisfy conservative constituents with relatively little exposure to the kind of attack that a legislative proposal or a White House statement would invite” (Clymer, 2002, p. A3).

2. This disdain is quite evident in a 1998 report, *Education at a Crossroads: What Works and What’s Wasted in Education Today* (1998), that delineates efforts since the Reagan administration to codify proper scientific method in assessing the outcomes of educational programs.

3. According to Greenberg (2001), the National Academy lives off of the production of “generally dour studies” (p. 297), most of which are ignored. Such studies, Greenberg observed, are produced largely by staff members who know well “the report industry . . . [where] much is written but little is read in Washington” (p. 299) while fronted by “overscheduled, part-time committee members” (p. 393). The NRC (2002)

report was produced in a particularly quick schedule of 6 months to inform Office of Educational Research and Improvement reauthorization.

4. Davies (2003), writing from Australia, which is further down the road of accountability culture, pointed out that it is typical of new managerialism that "the objectives will come first and that the 'experimental research evidence' will be generated to justify them" (p. 100).

5. See Friedman, Dunwoody, and Rogers (1999) for a discussion of scientific representations of uncertainty and media and public responses.

6. See Katz and Mishler (2003) for the growing interest in qualitative research in medical science. The authors caution that such work not be assigned a supplemental role of secondary and limited value but rather, both quantitative and qualitative need to serve as "a critical vantage point for assessing the other so as to provide a fuller and dynamic understanding of the problem" (p. 49).

7. The British experience becomes most interesting where "a number of cracks are beginning to show" (Trinder, 2000b, p. 236). There is a notable paucity of high quality evidence evenly distributed; there is little focus on application; the cost of assembling an evidence base may outweigh benefits; and doubts abound about the exclusion of bias, the ethics of randomized clinical trials, and how scientism constrains the answers it can supply. In the British scene of health care policy, given the well-known trade-off between internal and external validity in randomized clinical trials, clinical judgment remained important as did continual evaluation via effectiveness studies in real-world settings versus the efficacy studies of randomized clinical trials (Reynolds, 2000, p. 30). The problems of transfer to noncontrolled clinical settings were not minor. Given worries about the way such efforts might be used to ration health care, limit professional autonomy, and endorse a distorted view of science, impact has been "remarkable" at the policy level but "patchy" in terms of practice (Reynolds, 2000, p. 33).

8. Such talk in education usually disallows the controversy within the medical field regarding the quality of medical research and the uses to which doctors put it in the face of experience-based knowledge (Hammersley, 2000; Trinder & Reynolds, 2000).

9. Rising out of medicine with its strong scientific research tradition, the authors posit that the less scientifically driven areas of social policy will be less effected given the clash of their long-developed qualitative and nonexperimental quantitative research traditions with the ontological, epistemological, and methodological tents of evidence-based practice. Such prognosis did not take into account Bush Science.

10. One note of hope is from the nursing field where the journal *Evidence-Based Nursing*, founded in 1998, publishes structured summaries of both quantitative and qualitative research. Although "soft" forms of research remain marginal (Blomfield & Hardy, 2000, pp. 121, 123), it is the only British journal of the movement to include qualitative research due to the significant interpretative component of nursing-led research (Blomfield & Hardy, 2000, p. 131). If the evidence-based practice movement is to be embraced by nurses, "it must embrace a broader definition of evidence than is currently allowed" (Blomfield & Hardy, 2000, p. 130).

As an example of this, the Cochrane Collaboration, set up by the British National Health Service in 1992, prepares and disseminates systematic reviews of health care research (Reynolds, 2000, p. 21), with centers in the United Kingdom, Europe, North and South America, Africa, Asia, and Australasia (Trinder, 2000a, p. 1). Trinder (2000b) noted that a Cochrane qualitative methods group is forming, although she cautioned

that such moves must be accepted and valued on their own terms (p. 237) (see <http://www.salford.ac.uk/ihr.cochrane/homepage.htm>).

11. Flyvbjerg (2001) quoted Campbell:

Qualitative common-sense knowing is not replaced by quantitative knowing. . . . This is not to say that such common-sense naturalistic observation is objective, dependable, or unbiased. But it is all that we have. It is the only route to knowledge—noisy, fallible, and biased though it may be. (p. 73)

See Hamilton (2002) for a discussion of the (mis)uses of Campbell to warrant the “restorationist attempts to re-instate experimental design at the heart of social research” (p. 8). Hamilton’s point, of course, is that Campbell, in his own words, “re-canted” (Campbell, quoted in Hamilton, 2002, p. 6) his negative view of the single case study.

12. It is no new news that practitioners rarely alter their practice on the basis of research findings (Birnbaum, 2000; Trinder, 2000a, p. 3). It is an attractive picture to think of basing practice on the most up-to-date, valid, and reliable research findings. But how feasible are randomized field trials in an environment where, in the Columbus Public Schools in 1999, 45% of students, 53% of teachers, and 75% of principals move schools each year (Bush, 2003)? Claims for objectivity are overstated and mask hidden assumptions and values. Rather than reifying evidence, especially knowledge accumulation, how might social science serve us better than the parade of behaviorism, cognitivism, structuralism, and neopositivism that have all failed to successfully study human activity in a way modeled after the assumedly cumulative, predictive, and stable natural sciences?

13. Chapters cover pointers on how to “market the experiment,” tales are told of “horrific epithets” lobbied at “randomizers” as comparisons are made to the Tuskegee syphilis study (Mosteller & Boruch, 2002, p. 22). The best persuasion is “obviously” to tie funding to willingness to participate in randomized field trials (Mosteller & Boruch, 2002, p. 33), especially given how participants “will eventually figure out how much is involved” (Mosteller & Boruch, 2002, p. 35). Finally, remember “you are not a ‘Nazi’” (Mosteller & Boruch, 2002, p. 38) as you develop a thick skin in the face of press and community group resistance. Admittedly, however, “politics usually trumps research” (Mosteller & Boruch, 2002, p. 43) when the stakes are high and ethical standards “circumscribe the import of RFTs [randomized field trials]” (Mosteller & Boruch, 2002, p. 52).

14. In this return to the center of educational research, what evaluation theorists were “willing to let die” (Cook & Payne, 2002, p. 168), listen to the rhetoric of justification: “policy makers can easily grasp the findings and significance of a simple experiment” that is accepted at “face value” (Burtless, 2002, pp. 183, 184) by news media as well, whereas nonexperimental methodology yields a confusion of differing results. “Many target populations are politically weak. Even if many people in these populations object to random assignment, they may lack the political power to stop randomized trials” (Burtless, 2002, p. 188). Ethical issues are “the biggest practical obstacle,” and “if opponents to experimentation are politically influential, their influence can doom the effort to use random assignment” (Burtless, 2002, p. 194). Unfortunately, “American educators are well organized and politically influential,” (Burtless, 2002, p. 196) and this has hampered the movement.



15. These include efforts such as that of Robert Slavin of Johns Hopkins University, codeveloper of the Success for All improvement program used in more than 1,800 elementary schools, and Robert Boruch of the University of Pennsylvania–based Campbell Collaboration, designed to do for the social sciences what the British-based Cochrane Collaboration does for health care. Formed in 1998 as a nonprofit organization, the international Campbell Collaboration, along with the American Institutes for Research, a Washington, D.C. think tank, was awarded a 5-year US\$18.5 million contract in 2002 to develop the What Works Clearinghouse ([w-w-c.org](http://w-w-c.org)) to summarize effectiveness data from randomized field trials of social and educational policies and practices for policy makers and practitioners (see [www.campbellcollaboration.org](http://www.campbellcollaboration.org)).

See Laitsch, Heilman, and Shaker (2002) for a discussion of the role of think tanks in consolidating conservative modernization. One notable exception to the conservative bent of such centers is the Education Policy Studies Laboratory at Arizona State University, where Alex Molnar and David Berliner, among others, are doing research on the issues concerning high-stakes testing (Viadero, 2002).

16. See Baez and Boyles (2002) for a critical review of how “grant culture” characterizes much of what happens in the name of research at universities. They cited an article by Loren R. Graham (1978), “Concerns About Science and Attempts to Regulate Inquiry,” that makes clear the issues of this article are not new.

17. This phrase, credited to Freud, was used in the *New York Review of Books* (Flyvbjerg, 2001, pp. 26-27). It is, interestingly, used in the NRC (2002, p. 13) report, without attribution.

18. This section is from Patti Lather (in press).

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*Patti Lather is a professor in the Cultural Studies in Education Program, School of Educational Policy and Leadership at Ohio State University where she teaches qualitative research in education and gender and education. She has held visiting positions at the University of British Columbia, Goteborg University, and the Danish Pedagogy University as well as a 1995 sabbatical appointment, Humanities Research Institute, University of California-Irvine, seminar on feminist research methodology. Her book Getting Smart: Feminist Research and Pedagogy With/In the Postmodern received a 1991 Critics Choice Award. Troubling the Angels: Women Living With HIV/AIDS, coauthored with Chris Smithies, received a 1998 CHOICE Award as one of the best academic books of the year. She is presently working on a manuscript titled Getting Lost: Feminist Efforts Toward a Double(d) Science.*