

Scientism and Scientificity in the Rage for Accountability: A Feminist Deconstruction

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My work? . . . There is beneath that which science knows of itself something that it does not know. . . I have tried to extricate . . . the unconscious of knowledge. (Foucault in Davidson, 1997, p. 7)

This paper began as I was editing my in-process book, Getting Lost: Feminist Efforts Toward a Double(d) Science, and noticed slippage between the two terms scientism and scientificity. Curious as to what, exactly, was my object of critique, I began my journey with the internet, for the first time in my scholarly career, to see how the two terms shook down. Plunged into old and difficult questions, my struggle was to find a way to shift the terms of the debate away from rather tired epistemological contests and toward something useful in the very political contest over scientific research in education that is our situation today.

In March of 2004, architect Zaha Hadid gave a lecture where she used terms such as soft grid, fluid city, nesting, layering, flood of spaces, imposed complexity toward multiple uses.¹ Around that same time, a newspaper article called for a different kind of thinking in “grasping [the] true nature of [the] enemy” in the case of Al-Qaida’s intentions. With “the enemy’s” decentralized structure constantly changing, the West needs “a new way of thinking.” A defense analyst, explains, “[i]t’s like a bacteria. Every time we find an antidote to it, it evolves into a new form.” A senior lecturer on terrorism and defense studies at King’s College in London, says, “[i]t’s more complex than we are giving it credit for.” He goes on, “[w]hen we are talking about al-Quaida, we are creating an organization in our minds. . . I think there is a danger of terrorizing ourselves better than the terrorists do.”²

Against such calls for a different form of thinking, those of us in educational research have been facing demands for a very traditional form of thinking in terms of research methodology. “This IS Your Father’s Paradigm,” I termed it in an AERA 03 address, as I gave various framings to recent governmental efforts to hold experimental design up as the “gold standard” for educational research (Lather, 2004a,b, Lather and Moss, 2005).

What is this disconnect between a world that, on the one hand, calls out for fluid layerings, multiplicities, non-linearities, and proliferations of complexities constructed at

¹ DeeDee and Herb Glimcher Inaugural Lecture, Wexner Center for the Arts, Ohio State University, March 9, 2004.

² "Al-Qaida’s intentions difficult to discern," by Tod Robberson. Centre Daily Times (State College PA), March 27, 2004, A3.

least in part by the way we understand them and, on the other hand, attempts to legislate one-best-way approaches to scientific research? What does the future look like for qualitative research in education in such times?

This paper is situated within such questions and addresses the possibilities and limits of scientificity in educational research. In what follows, I do a sort of archaeology of the term “scientism” as a way to clear the ground for a look at the less used but arguably more important term of scientificity. I conclude with some thoughts on the implications for qualitative research in education in a time of what might be termed the “rage for accountability.” But first, a moment on what the “feminist deconstruction” of my title might be made to mean.

In her new book, *Undoing Gender*, Judith Butler writes that “feminism is a mess” (2004, p. 175) given the melt-down of its central terms, now situated, in best practice, as historical and performative in their continued rearticulation out of engaged political practices. As such, the feminism I am endorsing demonstrates how putting one’s necessary categories in crisis can help us see how such categories work across time and what they exclude and how rearticulating begins when “the excluded speak to and from such a category, . . . opening up the category to a different future” by contesting the power that works in and through it (Butler, 2004, p. 13). This is a kind of “double(d) movement” I am much interested in these days, using and troubling a category simultaneously, operationalizing the classic move of deconstruction: under erasure. This reinscribed feminism is, then, again in Butler’s words, “to assert an entitlement to conditions of life that affirm the constitutive role of gender and sexuality in political life while also subjecting our categories to scrutiny for their exclusions” (p. 37). This is a sort of loss, a disorientation where openness and unknowingness are part of the process, a self-reflexive, non-dogmatic feminism that relishes conflicting interpretations without domesticating them, a sort of permanent unsettlement in what might be termed a post-foundational feminism. Here feminism is both freed from the demand to be one thing in its task of challenging how knowledge is constrained by a patriarchal symbolic and lost, “ruined by the improper use of its proper name” (Butler, p. 233, speaking of philosophy), a self-loss that is the beginning of a post-idealist community.

With that reinscribing moment in play, let us turn, then, to scientism and scientificity in the context of a feminist effort toward a double(d) science, the science possible outside of the constraints of the patriarchal symbolic that Butler speaks to and against so powerfully (2004, p. 208).

Scientism

Nobody espouses scientism; it is just detected in the writings of others.
(*Oxford Companion to Philosophy*, p. 814)

The problem of scientism runs through the field of science and is enjoying a resurgence, in spite of its aura of disrepute. While it may be true that no field of inquiry is unable to benefit from the methods of the natural sciences, it is quite another thing to hold up as the

“gold standard” a very narrow idea of scientific method. This is, it is important to note, as true for the natural or "exact" sciences as it is for the social or "conjectural" sciences, to use Lacan's terms (Leupin, 1991:4) as both have a wide array of methods in use.

Many logically distinct positions can be called scientism.³ As noted in a recent edited collection from a conference on the "proper ambition of science" (Stone and Wolff, 2000), the questions shift from whether the laws and theories of physics can in principle subsume everything to whether everything should be studied by the methods of the physical sciences. My tack here is to focus on how what we call "science" relates to the nature and scope of philosophy, particularly the inadequacies of naturalist orthodoxy where philosophical rigor is modeled on problematic ideas of scientific work (De Caro and Macarthur, 2004). Hence my critique is, interestingly, directed more toward philosophy of science than it is toward science itself. In this, I am, of course, in the company of Thomas Kuhn about whom a recent book (Sharrock and Read, 2002) argues that this distinction is important in any effort to reduce the vitriol of the science wars and get us back to working across the "hard" and "soft" (or "wet" and "dry") divides toward the science possible after our disappointments in the science so structured by a patriarchal symbolic.

The Defenders of Science in the science wars too often assume an anti-science position on the part of critics whereas the real target is a traditional philosophy of science with its received categories of rationality, progress and the growth and accumulation of knowledge. This "ahistorical and mostly formalistic" (Sharrock and Read, 2002:12) philosophy of science reduces scientificity to a methodological imperative and a quantitative one at that.

In what follows, I divide scientism into "old" and "new" forms in the hope of saying something fresh about such well trod ground.

Old Scientism: Of Objectivism

In a "classical argument against scientism" (Uebel, 2000, 151), the noted economist, Friedrich August Hayek, warned of the efforts to force upon the social sciences the methods of the natural sciences. First published in the early 1940's in *Economica* and then collected under the title The Counter-Revolution of Science: Studies in the Abuse of Reason in 1952, Hayek's polemic ranges across scientism and science, society and collectivism, subjectivism and objectivism, as well as theories of history.⁴ With "the

³ Thomas Sorell whose 1991 book, Scientism, is a general reference, contends that scientism is a relatively new term of abuse laid on those who conceive of science narrowly and hold this narrow view as the one best way to knowing. Hayek "borrows" the term from 1936 French usage (1952, note 7, p. 207).

⁴ The continued importance of this work is evident in volume 2 of *Philosophy and Methodology of the Social Sciences*, edited by Mark Smith (Sage, 2004), where Hayek's "Scientism" is reprinted, along with 2 other chapters, making him among the most reprinted authors, topped only by Karl Popper's 4 reprints in volume 1.

unwarranted and unfortunate extension of the habits of thought” of the physical sciences to the study of society, this narrow definition has led to confusion, discreditation and what Hayek terms the “scientistic” prejudice involving “the mechanical and uncritical application of habits of thought to fields different from those in which they have been formed” (pp. 15-16). This “trespass of scientism” is “injurious” to the progress of the social sciences (p. 270). Just as science had to fight the religious prejudice, now it has to fight a scientistic prejudice that Hayek terms a “decidedly unscientific” attitude.

Hayek became a Cold War figurehead for his "deep agenda" of worries about how scientism bred socialism (Uebel, 2000, 159). But he also produced this "one of two most famous of twentieth century polemics in philosophy of science" (Uebel, 2000, Popper being the other) that both captures and effects the beginning of the loss of authority of a tradition in a way that is prescient of later postmodern critiques (Burczack, 1994).

This loss of authority was further exacerbated with the work of Habermas in the 1960's. According to Habermas, scientism is part of the dominance of instrumental reason and his particular concern was the scientization of politics (Outhwaite, 1994). Knowledge and Human Interests, “a systematic critique of scientism” (Critchley, 2001:112), argued in 1972 that it is not obligatory to science to exclude ideas of critique and emancipation. Tapping into the long-running discussions in Germany about the differences between the human sciences and the sciences of nature, Habermas argues that the social sciences are necessarily hermeneutic and require history and sensitivity to situation-specific understandings of meaning.

Habermas’s great contribution was articulating alternative methodological orientations for the social sciences. His insistence on the interplay of the philosophical and the empirical and his focused concern with scientism laid the groundwork for filling out what a critical social science might look like, work to which many of us in this room have contributed. Salvaging a concept of scientific truth has remained a stickier point, given his cognitive-interest model of science that collapses science into politics and power. Epistemology becomes social theory where the self reflection of the knowing subject is central in the development of a critical science that refuses the positivism that seduced Marx.

I will return to this issue of rescuing a concept of scientific truth, but Habermas was key in demonstrating how, in the history of the way positivism has exercised its grip upon the social sciences, scientistic ideas are at the root of instrumental reason. They direct us toward technocratic ideals and “[I]n this respect a critique of scientism also acquires political consequences” (Outhwaite, 1994:12) as it makes room to develop the social sciences in the direction of orienting action rather than serving the demands of economic and political powers (Flyvbjerg, 2001).⁵

⁵ An area that has moved dramatically along this front is environmental science. Amplifying ideas of “democratic accountability” (Behn, 2001), scientific expertise is being refashioned along participatory lines toward a kind of “civic science” that brings policy-makers and citizens together (Backstrand, 2003). At the heart of the argument is

While Kuhn was not very interested in the social sciences, his work as well created space for the development of the historical-hermeneutic or cultural sciences with their different methodological procedures where the purpose is forms of mutual understanding for the possibility of orienting action. Kuhnian post-positivism interrupted the idea of a law like world of predictability leading to technical control. Through contact with practical life, a critical social science became intelligible with its intentions of bringing about changes in the unreflected consciousness of social agents that work toward the abolition of present institutions (Teigas, 1995).

What is produced by the “old scientism” of objectivism that has been so excoriated for over seventy years? If one attends to recent arguments regarding the thingness of the world (eg Latour, 2004), then a thing can become any number of different objects of thought. Under methodological scientism, things become outside of history and structure, decontextualized but measurable, what Hayek termed “of the same logical type as Plato’s determination that a just ruler is 729 times as happy as an unjust one” (1952, p. 51). Naïve realisms of various sorts are supported along with what Whitehead termed “fallacies of misplaced concreteness” (Ibid, p. 54). All sorts of pseudo problems arise when, in spite of much rhetoric to the contrary, it is NOT the nature of the problems that guides the choice of methods in the social sciences but, rather, a scientism where unacknowledged objectivism is the water in which the fish swim.

In the present frenzy for accountability, scientism threatens to engulf us all. As an ideology about science, scientism functions as “a type of scholarly trespassing of pseudo-exactitude, embracing incongruous models of scientific method” (Stenmark, 2001, note 1, p. 2).⁶ Driven by the hope that other areas will share in the success of the natural sciences yet widely refuted in even analytical philosophical arguments these days (Morris, 1992), what is this “new scientism” that is so much with us in the present moment? To what extent does it grow out of interdisciplinary moves into the social context of justification and what happens to philosophy in such moves?

New Scientism: What Happened to Philosophy?

Beck’s (1992) notion of “reflexive scientization” that replaces “traditional objectivist accounts of science by a more inclusive science that institutionalizes self-doubt, self-interrogation and self-reflexivity” (Backstrand, 2003, p. 33). See Weber, 1999, for a tracing of this “sea change in conceptualization of accountability” (p. 453).

⁶ Stenmark (2001) in his book on scientism lists several forms, some of which include the “methodological scientism” of the repeatable, observable and measurable, the “epistemic scientism” where a narrow idea of method is the only road to reliable knowledge, a “rationalistic scientism” where only scientific knowledge is rational knowledge, an “ontological scientism” where everything outside of science is a secondary kind of reality, and, of particular interest, the holding up of science as our best hope for conclusions about sociopolitical values and beliefs. This is a kind of “existential or redemptive scientism” where salvation occurs through science and science alone.

Current philosophy makes the hypothetico-deductive style of reasoning the essence of science. . . Not all the historicizing of Kuhn and Lakatos has dislodged this opinion one whit. . . [but there is] a changing style of reasoning . . . [where I have] learned much from *The Order of Things*. (Hacking, 2002, p. 89)

Sorell (1991) coined the term, "new scientism," as a reaction against both those who write philosophy in ignorance of science and those who fall into metaphysical excess, what the National Research Council report, *Scientific Research in Education*, terms "extreme postmodernism" (2002, p. 25). As an "occupational hazard in philosophy" (1991), an older scientism insisted on not only philosophy but all of culture being led by science. The resurgence is characterized by striking assertions of the capacity of science to improve the everyday. This is not new, for example, Dewey who looked forward to the extension of science into ethical and social domains (Sorell, 1991, p. 11) and it is important to remember the socialism of the Vienna Circle in assessing the politics of the resurgence. The "new scientism," in short, is both more and different than a vast right wing conspiracy (Jewett, 2003).

If Critchley (2001) is right that scientism equates with what Husserl called objectivism (p. 115), then the values at stake in doubting the objectivity of science are crucial to the self-image of philosophy (25). From Hilary Putnam (1978) to Peter Winch (1990), the cozy relation of analytical philosophy to science has been held to task for its road to nihilism that accompanies a scientism that holds that the model of the natural sciences should hold for philosophy. Since Descartes, the search for certain foundations of knowledge is part of the way epistemological justifications have been articulated. A post-Habermasian move into the social context of justification is now challenged by a resurgent scientism on the one hand and a "return of the thing" on the other (Latour, 1999; Wylie, 2002; Brown, 2001).

The next section will deal with the critique of radical constructivism. In the rest of this section, I probe the ground of the resurgence of scientism by looking at how a unity of science argument works in ways both similar and different from the old scientism. Jewett (2003) traces how the "ongoing struggle over the meaning of modern science for American democracy" (p. 64) sets the stage for the science wars. In the old scientism, Dewey's value-laden take on science as a way of thinking about social, ethical and moral questions is front and center in the shifting fortunes of the naturalist argument for the unification of science and its capacity for leadership in the development of social values. In the new scientism, such arguments are mobilized toward the same effect, but with a twist away from Dewey's focus on "science as builder of participatory democracy and toward what science offers by way of management for purposes of governmentality. It is my argument that what is displaced in this move is philosophy.

This displacement maps on to an earlier displacement of philosophy by the post-Habermasian shift from epistemological to social context justification that so opens up the question of the legitimacy of an engaged social science. This shift, I posit, is both the producer and effect of the loss of authority of philosophy or at least the kind of

philosophy that excludes from science the political, institutional, and rhetorical contexts of knowledge production and reception. This is the philosophy that tried to transform itself into a natural science by holding that truth is only accessible through scientific method.

In Butler's new book, her final chapter is entitled "Can the 'Other' of Philosophy Speak?" Here she is interested in a philosophy that has lost control in patrolling what is philosophy, a philosophy that has found itself doubled, outside itself, "lost" in the other, face to face with the philosophical value of not being so sure of itself (p. 234). With continental philosophy largely performed outside the boundaries of philosophy departments, literary critics, cultural critics and social scientists have created an interdisciplinary "theory/philosophy conflation" (p. 247) that makes "the very notion of philosophy a stranger to itself" (250). Here, Butler argues, philosophy proper becomes "a loner, territorial, protective, increasingly hermetic" (p. 246).

Critchley (2001) argues that obscurantism is the ghost that haunts continental philosophy and scientism is that which haunts analytic philosophy. My argument is that the issue is larger: the displacement of philosophy itself by social theory. To explore this possibility, I turn to the concept of scientificity.

Scientificity: From Method to Object (Against Radical Constructivism)

Whichever way you cut the cake, it looks like scientificity just ain't in the institutions (alone). . . You cannot actually move from a pre-paradigmatic state to one of normal science by trying to force a particular fantasized normal science on your colleagues, even if you have the institutional muscle. (Sharrock and Read, 2002, pp. 8-9.)

Scientificity has long been at the heart of the demarcation debates. But one example is the "scientificity of psychoanalysis," the seemingly endless adjudications over the scientificity of Freud (Leupin, 1991). Also of note here would be the desperate quest across various fields for scientificity, from economists (Cullenberg, Amariglio and Fuccio, 2001) to the Institute of Educational Sciences and its determination to counter the "explosive growth of qualitative research studies" by funding only those studies that adhere to its "the methodological orthodoxy" (Bryant 2004, p. 5). From Popperian falsifiability to Lakatosian research programmes, from criteria of testability and prediction to more recent pronouncements on reliability and generalizability (NRC, 2002), scientificity is about the constitution of science as science. While the recognition that science is evolving, social and historical is oftentimes spotty, even in philosophy of science where one might expect better, the criteria of scientificity are much debated.

Historically, scientificity in the social sciences has been based on measurability, the degree to which an area resembled inquiry in physics (Rorty, 2001). Two generations of post-Kuhnian work has "done its best to fuzz up the logic-rhetoric and hard-soft distinctions" (Rorty, 2001). While what Nancy Cartwright (1999) terms "scientific fundamentalists" still hold to the task of demarcation, focus has shifted to the general

structure of scientificity with openness to specific disciplinarity. Here scientificity is continuously adapted to new contingencies. Isabelle Stengers (1997), for example, argues that scientificity is a productive constraint. Getting access to the singularity of scientific activity in the drive to address what makes a science a science, the pre-existent, neo-positivist criteria are but one form in "the criteria of scientificity that are currently on the market" (81). Conditions of scientificity can be mutilating; they can construct object and question in a unilateral way, drawing on social power, eliminating a priori anything that does not appear to guarantee an objective approach (146). Most importantly to Stengers, it is "trivial" to solve the problem of what science is by defining science "through its objectivity" (1997, p. 81).

In mapping such territory, Foucault describes the thresholds of positivity, epistemologization, scientificity and formalization in his Archeology of Knowledge (1972, p. 186). His interest is in discourses that have the status of scientificity or pretensions to it, and those that present the formal criteria of a science in how they function as an element of knowledge (p. 184). What he does here is localize science in the framework of more general knowledge. He looks at how a science structures certain of its objects, systematizes parts of it, formalizes, underwrites strategies: here science finds its place (p. 185) where it functions among other practices; here is its ideological function. "Ideology is not exclusive of scientificity," Foucault writes (p. 186), and the role of ideology does not diminish as rigor increases and error is dissipated. For those who know their Althusser, this is quite another cup of tea than the science/ideology distinctions that formerly reigned in the Marxist claim to scientificity. To tackle the ideological functioning of a science is to take on the "system of formation of its objects, its types of enunciation, its concepts, its theoretical choices. It is to treat it as one practice among others" (p. 186).⁷

Foucault's question is "what is it for that science to be a science"? (1972, p. 192) His answer is that each, in its turn has the scientificity proper to it. To focus on demarcation criteria is to miss how "all the density of the disconnections, the dispersion of the ruptures, the shifts in their effects, the play of the interdependence are reduced to the monotonous act of an endlessly repeated foundation" (p. 188).

What Foucault helps us see is how the methodological reductionism that has radically flattened the methods into a single model is being displaced by a sort of situated scientificity that neither constricts "science" to one or two privileged models nor allows anything goes arbitrary concept of science. While a general attitude of and emphasis on rigor and objectivity are part of a "plurality of models and types of scientificity suitable for the requirements of diverse fields," different but compatible models of scientificity are elaborated across disciplinary sites while working to avoid methodological fragmentation. In addition, recent exhortations to scientificity are more

⁷ See pp. 182-187 of *Archeology of Knowledge* for the "thresholds of discursive formations."

rhetorically sophisticated in urging adherence to scientific norms.⁸ Here scientificity becomes a performance, for example the textual display of the absence of the author and/or the veneer of scientificity accomplished by the use of mathematics.⁹ Bourdieu (1990) writes of this as “to simulate scientific rigor” (p. 37), “imitat[ing] the advanced sciences” (p. 39) in his argument against positivist orthodoxy and its “false rigours” (p. 40).

In this context of proliferating, situated, rhetorically inflected scientificities, the judgment criteria for scientificity enacts an on-going crisis. In sum scientificity is an arena of struggle in broadening the definition of science. Given that the human sciences work with a vague concept of data, “the rigor proper to true scientificity” is thwarted, especially epistemological definitions of objectivity. Socio-cultural context matters here, unavoidably. Focus shifts to the proper characterization of the object, not control of the subjectivity of the knower. A science defines its own scientificity by elaboration of the conditions that determine the objects of a science and data about them. These are methodologically built objects.

What I am endorsing here is a sort of post-methodology that bespeaks an end of grand narratives about research. Fuller (1997) refers to this as the “secularization of science.” While methodology is key to demonstrating scientific credentials, the ground would shift to foregrounding disagreements, ambiguities, uncertainties, contradictions and incoherencies. Political, social, ontological and personal concerns have now invaded what before was thought to be a technical arena. This is the post of post-methodology. Rather than essential features of science offered as a benchmark for assessing scientificity as movement toward “real science” as the NRC defined its task, the move is toward a scientificity defined as how a discipline works toward creating new phenomena (Hacking, 1983; Bachelard, 1949).

Osborne and Rose (1999) give an example of this in their project on the history of empirical social thought where they look at public opinion research. Embracing a scientificity that is not just exactitude, epistemology, and methodology, but material and technical factors as well, they look at how stable methods and instruments enhance a consensus around a sense of objectivity of what has been discovered. In the social sciences characterized by no agreed on research front, that are more about lineages and recycling back to earlier framings, recurrent commentaries, endemic disagreement, no

⁸ See Fuller and Collier, 2003, for an explicitly sophistic and dialectical approach to rhetoric in the social studies of science. See, also, Fuller, 1997.

⁹ In the “science wars” Lacan’s mathemes are often held up as a prime example of the “misuse of mathematics” by the human sciences. For the “aggravation” caused to Sokal et al. see Plotnitsky, 2000. But see, Marini, 1992, for an unpacking of mathemes as “a logic that is not a logic” (p. 68) in attempting a theoretical language “that recognizes its uncertainties and contradictions” (p. 69). Mathemes might be looked at as the formalized transmission of not understanding, the formalizing structure of the relationship between terms in order to communicate in shorthand or metaphor the necessary lack of language. Thanks to conversation with Ian Parker and Eric Burman for this “truth of mathemes.”

substantial consensus and questionable applicability, the use of statistics toward exactitude and calculability is not enough (Hacking, 1991). The central question of discovery is far more interesting than the demarcation debate.

Osborne and Rose argue that the social sciences have brought a great deal into discovery, not only public opinion and the consequent “scientization of politics” that so concerned Habermas, but others abound: juvenile delinquency, mid life crisis, stages of moral development. This is quite Foucauldian, of course, but it is also quite Latourian.

Latour has been a leader in this shift from epistemological, philosophical questions of demarcation to sociological questions of laboratory life and a focus on how truths and phenomena are constructed. His interest is phenomena creation and consensus generation processes. The world is full of facts, he argues. They are the outcome of micro practices that put them into circulation. A reality effect is produced once controversy settles through processes other than simple reference. In short, objectivity is conferred so that research on it can proceed. Candidate facts abound about a phenomena, some get “reality effect” and some don’t. This is where it gets interesting, the “contingent historicity of truth” where some things become true rather than others (Latour, 1999).

What gets in the way of the social science creation of phenomena is “the illusion that the essence of scientificity lies in the formal properties of their representational systems, their theories” (Latour, 1999, p. 12). The cupboard of science gets pretty bare if the bar is set so high. In a Foucauldian vein, the social sciences have been quite successful if we look at how they have reshaped the ways humans think about themselves, how capacity and possibility are activated.

Yet scientificity is destroyed if all the sciences are looked at only socially. The thingness of the thing has to be taken into account. This, Latour says, is the critique of the social studies of science. To recognize the resistance of natural objects to social explanations is to call for a new respect for the adequacy of objects where “Things have become Things again” (2004, p. 236), something more and different than mere projections onto an empty screen.¹⁰ “The old tired theme of social construction” (Stengers, 1997) means the demise of society as an adequate source of explanation. Another type of scientificity is needed for the social sciences, a post-positivist scientificity that is not paralyzed by “physics

¹⁰ The humanities are also seeing a “return of things.” See special issue of Critical Inquiry on Things (Brown, 2001) that traces ideas back to Heidegger’s essay on “The Thing” through recent articulations of “material culture studies” (p. 2). See, also, Daston (2004) for a look at “object studies” that combines physicality and significance across both the sciences and the arts and humanities and Grosz (2001) for a particularly philosophical look at architecture as material culture. Chapter ten is entitled “The Thing” as she traces “the great thinkers of the thing” from Newton, Descartes and Kant to Darwin, Nietzsche, Peirce, James, Bergson, Rorty and Deleuze, with the latter group positing things as both found and made, always caught in flux, in-the-making.

envy.”¹¹ This is a kind of interpretive scientificity that takes into account the ability of the object to object to what is told about it. Here objectivity renders objects capable of resisting social explanation.

Imitating the natural sciences won't get us to such a reinscription of objectivity, a post-scientific sort of objectivity. Latour calls for studies where objects are rendered as recalcitrant as possible, “as disobedient as possible to the protocol” (When things strike back, p. 6). This is a post-intentional sort of objectivity that backgrounds the consciousness and reflexivity that have been front and center in efforts toward a different science. But it is also a sort of post the “post-modern eulogy of networks, fluids and fragments” that abandons the quest for a common world. Things have neither the unity of the modernists nor the multiplicity of postmodernists, Latour argues. The social is only one voice in this.

At the least, such arguments lead one to recognize that there are many kinds of objectivities. They include Haraway's formulation of the promise of objectivity where scientificity becomes partial connection and ontological relationism (1991:193) They include Latour who distinguishes between “the polemical kind of objectivity” versus the objectivity created by science in action (1999, p. 20). Perhaps explanations “resorting automatically to power, society, discourse had outlived their usefulness,” he suggests (2004, p. 229). Urging a “second empiricism,” he tracks how many participants are gathered in making a thing a thing. Objects are a gathering and critique needs to move not away from but toward the gathering of the Thing, mediating, assembling, gathering many more folds (p. 246).

One example of this approach that has caught the imagination of my feminist reading group is The Body Multiple (2002) by Annemarie Mol, a Dutch professor of political theory who does social studies of medical knowledge practices. Against the grand generalizing schemes of the What Works Clearinghouse,¹² she documents how context specific medical interventions are in a study of atherosclerosis. Moving beyond perspectivalism and constructivism, she foregrounds materialities and events in practice in order to explore how social order is possible. To give an object a complex present (p. 43) is to move beyond the “turtles all the way down” that has dominated interpretive work. “Something complicated is happening here” (p. 67), she argues, spreading the activity of knowing widely, beyond knowing subjects and objects to be known. In her work that combines ethnography and reflection on the literature in a split-text format, a

¹¹ 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 report, without attribution (p. 13.)

¹² The What Works Clearinghouse, headed by Robert Slavin at Johns Hopkins University, is one of two groups chosen by the US Department of Education (now the Institute of Educational Sciences) in August of 2002 to develop and manage a clearinghouse on scientific evidence of “what works” in education (w-w-c.org/). It is infamous in qualitative circles for its scientific ideas of proper research design as experimental studies using large data bases.

sense of acute situatedness is the key in enacting how ontologies are brought about in practice. Maybe something else is happening, she argues, a sort of coherence in tension where incoherence is not a lack of scientificity. In her work, an ontology of variants in tension contributes to the "rich, adaptable, and yet tenacious character" of good practice (p. 115). The object becomes a conjunction with various distributions where ongoing incompatibilities co-exist. This is a kind of thinking from matter and a speaking to/learning from relationality that has been pioneered by feminist science studies (Haraway, 1991; Wylie, 2002). Now linked to a critique of strong constructivism, a sort of post-realism is a-borne, a new materiality that refuses the ideal/material split. This is a new objectivity about neither results nor detached knowledge, but, rather, what art historian, Steven Melville (1996), terms "objectivity in deconstruction" where the nostalgia for transparent presence is put in tension with a respect for the object's capacity to surprise and exceed us in a way that foregrounds the inadequacy of thought to its object.

To move into my conclusion, the search for a normative philosophy of science has been a wrong road. A philosophy of science that abstracts from practices rather than prescribing them would include the political, institutional, constructive and rhetorical features and contexts as well as the conditions of production and reception. The dream of the social sciences of short-cutting politics via social engineering has led to an "extravagant scientism" that thwarts efforts toward problem solving. Perhaps the way out of our impasse is to use these articulations of a post-sort of objectivity toward increasing the legitimacy of an engaged social science that explores how a complexified social science enriched with contextual factors can be of use in the social policy arena.

Implications for Qualitative Research in Education: From Scientism to a Non-Reductionist Scientificity

As Uebel (2000) points out in his article on scientism, Popper and Hayek, any Left "policy turn" must not overlook that part of the neo-liberal conservative thrust is that policy is ineffectual. Neo-classical economics, for example, argues that citizens are sovereign, rational, and don't need governmental intervention, thereby endorsing privatization. In the case of the US budget deficit, many theorize it is to spend ourselves into ruin quite deliberately as a way of, finally, breaking the back of Big Government. This is part of the background for NCLB and its failure to fund itself that has so upset those who care about schools.

In our time of the "rage for accountability," the long-running remorseless exposure of the social sciences as non-scientific or pre-scientific had led us to experimentalism as the royal road to scientificity. In a very nice piece on the "comeback" of experimentalism, Howe (2004) addresses the NRC report "in the context of the debate about what constitutes scientific research" (p. 43). He charges this resurgence with ignoring the last decades of evolution in social science methodology, providing "no fresh answers to long-standing criticisms" and of "overselling" what it can provide. Campbell and Cronbach's

“recanting” are ignored;¹³ medical research is misappropriated;¹⁴ and, most importantly from the vantage point of this paper, science is essentialized based on “an outmoded philosophy of social science” (p. 50).¹⁵ Arguing for what he terms “mixed-methods interpretivism,” Howe urges democratic versus technocratic moves and a recognition of the power politics at work in present efforts toward standardization. “Methodological fundamentalists” are having their moment; critical researchers are being written off as “ideologues”; it is the science wars/culture wars brought to the arena of educational research.

This is captured in a recent Institute of Education Sciences (IES) call for postdoctoral research training fellowships in the education sciences (release date, July 9, 2004). The call is for “a new generation of methodologically rigorous and educationally relevant scientific research that will provide solutions to pressing problems and challenges facing American education.” Background information argues that while a solid research base is needed, “significant capacity issues within the education research community” stymie efforts to transform education into an evidence-based field. The dominance of qualitative methods is “a clear sign of the mismatch between the focus of the practice community and the current research community.” Psychometrics or “rigorous training in research methodology and statistics” is what is needed if educational research is to “contribute to the solution of education problems and to provide reliable information about the education practices that support learning and improve academic achievement and access to education for all students.”

Simply defending qualitative research will not suffice here. As Carol Steiner (2002) points out, most qualitative work is scientific in its concern with objectivity, generalizability and rationality in “the epoch of technicity” (p. 3) where competence means adhering to the rule and law of a paradigm. Triangulation, reliability, validity, shoulds and musts, even “is” and “are” are suspiciously realist. Computer assisted data analysis feeds the process.

What is at question is the adequacy of standard methods, the desirability of research and policy goals, and the philosophies of science that prescribe narrow views of these issues. The effort by the federal government to legislate scientific method is an attempt to muscle through a “fantasized normal science” toward improving educational practice. It is a kind of bullying that is grounded in the search for a normative philosophy of science

¹³ For a development of this argument with a focus on how Donald Campbell, patron saint of the evidence based movement, championed the case study in later life, see Hamilton, 2002.

¹⁴ In terms of the “evidence” of diffusion in medicine: “It takes, on average, 17 years for the results of clinical trials in to become standard clinical practice” (Porter and Teisberg, 2004). Thanks to Greg Gascon for this cite.

¹⁵ Ian Stronach calls this the “politics of amnesia” (comments made at RCBN Seminar, The Educational Future and Innovative Qualitative Research, International Perspectives, Education and Social Research Institute, Manchester Metropolitan University, UK, February 4-5, 2005).

that disallows the complexity and messiness of practice. The result is impoverishment rather than improvement. That loss is being borne by the children, teachers, and administrators in our schools.

My hope in this essay has been to contribute to an alteration of the terms of the science wars by engaging in a critique of philosophy of science more than of science itself. Urging a healthy respect for the adequacy of the object, I have reinscribed objectivity and articulated an interpretive scientificity that is grounded in the need for an applied social science that can cope with the multiplicity of the social world. For those attentive to the demands of different contexts and different communities, this is our moment to act in the struggle for an applied social science that can engage strategically with the limits and possibilities of the uses of research for social policy toward the improvement of practice.

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