Positivist Dogmas, Rhetoric, and the Education Science Question
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Explicit versions of positivism were cast off some time ago in philosophy, but a tacit form continues to thrive in education research, exemplified by the “new scientific orthodoxy” codified in the National Research Council’s Scientific Research in Education (2002) and reinforced in the American Educational Research Association’s Standards for Reporting on Empirical Social Science Research in AERA Publications (2006). The author rehearses previous critiques of positivist “dogmas” in education research and applies them to the new orthodoxy. Then, borrowing from the emergent field of the “rhetoric of science,” he explores how pursuit of the education science question has nourished a positivist conception of education research. He concludes by suggesting that the education science question should be reframed and briefly suggests how.

Keywords: philosophy; politics; research methodology

The rumors of positivism’s death have been greatly exaggerated. Although explicitly articulated versions were cast off quite some time ago in philosophy, positivism continues to thrive in tacit form on the broader scene (e.g., Code, 1993; Harding, 2006; Lessl, 2005). A case in point is the “new scientific orthodoxy” (Howe, 2008) in education research, which has been codified in the National Research Council’s (NRC) Scientific Research in Education (2002) and reinforced in its subsequent Advancing Scientific Research in Education (2004) as well as in the American Educational Research Association’s (AERA) Standards for Reporting on Empirical Social Science Research in AERA Publications (2006).

In this article, I briefly rehearse my previous critiques of three positivist dogmas of education research and bring them to bear in arguing that education research should follow philosophy in casting off positivism.1 This review sets the stage for a second form of analysis that borrows from the emergent field sometimes referred to as “rhetoric of science”2 and explores how the tacit form of positivism lives on in education research as an important influence, now resurgent in the new scientific orthodoxy. I contend that the education science question, to which the new orthodoxy has been the response, ought to be abandoned as currently framed because it has been addressed within a broad rhetorical context that presupposes a positivist conception of science. I then briefly suggest how the education science question might be reframed.3

Three Positivist Dogmas and the New Scientific Orthodoxy: A Critique

The Quantitative–Qualitative Dogma

The quantitative–qualitative dogma (or “incompatibility thesis”; Howe, 1988, 2003) holds that quantitative and qualitative research methods are incompatible with one another such that they may be combined “disjunctively” but not “conjunctively” (Howe, 1985). That is, if combined, qualitative methods must be assigned separate roles from quantitative methods, for example, generating causal hypotheses that quantitative methods then test. The source of (conjunctive) incompatibility is that the different epistemologies that putatively underlie qualitative and quantitative methods—interpretivism and positivism, broadly construed—are themselves taken to be incompatible. The fundamental problem with this argument is that it assumes the false premise that positivism is a tenable epistemology with which to align any research methods.

Early- to mid-20th-century positivists elevated scientific knowledge to the uppermost epistemic position and undertook to explicate its features, including how to demarcate it from other kinds of intellectual endeavor, particularly speculative metaphysics. They embraced two central tenets: reductionism and the synthetic–analytic distinction (e.g., Quine, 1970b).

Reductionism is grounded in the verification theory of meaning, which holds that the meaning of a statement is its method of verification. To be (cognitively) meaningful, a statement must be verifiable or falsifiable. The synthetic–analytic distinction divided meaningful statements into two kinds. A synthetic statement was one capable of having its truth-value (truth or falsity) determined (in principle) by observation (e.g., “There is life on Mars”). An analytic statement was one capable of having its truth-value determined by either (a) formal logic (e.g., “All Martians are Martians”) or (b) the meanings of its terms (e.g., “All Martian bachelors are unmarried”). Whereas synthetic statements were a
posteriori (i.e., determining their truth-value requires empirical investigation), analytic statements were a priori (i.e., determining their truth-value is wholly independent of empirical evidence and requires only analysis of their constituent terms).

The synthetic–analytic distinction and reductionism are intimately related. Indeed, according to Quine (1970b), they are “at root, identical” (p. 48). Analytic statements are the limiting case of reductionism in that they are verified or falsified “come what may” (p. 48). Thus reductionism may be identified with requiring scientific statements to be either synthetic or analytic. Statements that fail to meet that requirement lack cognitive significance and thus are not scientific. Among the excluded statements are ones involving unobserved and unobservable metaphysical entities employed in idealist philosophies, for example explaining the trajectory of human history in terms of the evolution of a transcendent Absolute Spirit. Also excluded are less abstruse explanatory entities such as the id, ego, and super-ego.

In his celebrated critique, first published in 1951, Quine (1970b) fundamentally undermined the positivist conception of science, including its demarcation of science from metaphysics, by pointedly criticizing what he labeled the “two dogmas of empiricism”: reductionism and the synthetic–analytic distinction. Quine’s critique was marked by many twists and turns and a good measure of philosophical esoterica, so a detailed exposition would be out of place here.

In very broad strokes, Quine developed two complementary lines of argument. In the first, he challenged the type (b) analytic statements defined earlier, which depend on making sense of concepts such as meaning, synonymy, and definition in other than a posteriori/empirical terms, that is, in terms other than the facts of usage. Were such statements merely about facts of usage, they would be indistinguishable from synthetic statements and thus lack the crucial feature of being a priori. But, Quine argues, the attempts to clarify what makes analytic statements a priori are marked by appeals to “elusive” entities, particularly “meanings,” which are presupposed in the concepts of synonymy and definition. But where do meanings exist? In a transcendent Platonic world? In individuals’ heads? Other attempts to clarify analyticity are marked by circular arguments, which define the crucial concepts in terms of one another, giving the explanation of analyticity an “air of hocus-pocus” (1970b, p. 37). When you strip away the excess verbiage, they amount to arguments along the lines of “The statement ‘All bachelors are unmarried’ is analytically true because it is necessarily true.” And why is it necessarily true? Because it is analytically true.

In a second strand of argument, Quine considered whether the verification theory of meaning might be the way to avoid elusive entities and break out of the circle. Here, he challenges the general presupposition that individual statements may be tested in isolation, which is required if we are to be able to divide statements into the two exclusive categories specified by the synthetic–analytic distinction. In Quine’s alternative view, “our statements about the external world face the tribunal of sense experience not individually but only as a corporate body” (1970b, p. 47). This view is identified with the “Quine–Duhem thesis,” which holds that the conclusions to be drawn from experiences that violate expectations or predictions are not straightforward, as they should be if it is possible to test individual statements. For example, one of the important observations that challenged the belief that the earth is flat was the phenomenon of ships disappearing over the horizon. However, interpreting this as a challenge assumes that light travels in a straight line. One can adjust the “corporate body” of beliefs to maintain belief in a flat earth by assuming that light bends toward the earth, like a kite string—the farther away, the greater the bend. So the observation that ships disappear over the horizon has no power in isolation to verify or falsify the theory that the earth is flat.

Quine referred to the “corporate body” as a “conceptual scheme” or “web of belief” (Quine & Ullian, 1978), which he characterized metaphorically as composed of beliefs that become progressively less subject to revision as the center of the scheme is approached. Toward the center is where so-called analytic truths are found. But these truths are less subject to revision because of their interconnectedness with other beliefs throughout the scheme and the associated ripple effects of altering them, not because they are true “come what may.” Even the laws of logic might be revised with good enough reason. In this vein, scholars have explored the idea of three-valued logics—true, false, and indeterminate—to cope with the uncertainty of quantum mechanics.

The broad and lasting impact of Quine’s critique of positivism is the fundamental challenge he posed to the general idea of isolating the empirical content of science from its humanly contributed conceptual content. In this challenge, his critique is quintessentially pragmatic. The rejection of a pristine empirical foundation for science, shorn of human interests, goals, and activity, is shared by John Dewey, Hilary Putnam, and Richard Rorty, to name only a few of the most prominent pragmatists. And, of course, there is their forerunner William James (1968), who, in his essay “What Pragmatism Means,” remarked:

“Purely objective truth, truth in whose establishment the function of giving human satisfaction in marrying previous parts of experience with newer parts played no role whatever, is nowhere to be found, . . . The trail of the human serpent is thus over everything.” (p. 150)

Thomas Kuhn, who cites Quine approvingly in his seminal Structure of Scientific Revolutions (1962), may be included as a pragmatist as well, although he is typically identified as a historian of science rather than a philosopher. And it is Kuhn who did the most to bring the general pragmatic view into prominence outside philosophical circles. Against positivism, Kuhn also rejected the idea of a pristine empirical foundation for science on which scientific theory is built in a smooth, cumulative way. He emphasized that observation is “theory-laden,” shaped by the humanly constructed “paradigms” that scientists unavoidably bring to observation. Selecting a theory over its competitors requires weighing competing “values” (“pragmatic criteria,” for Quine: 1970a)—accuracy, consistency, scope, simplicity, and fruitfulness (Kuhn, 1977). Such selection cannot rely on an appeal to a straightforward verification-or-falsification horse race, a “crucial experiment,” or the like.

In addition to positivism’s two dogmas, its related doctrine of the “unity of science,” whereby physics served as the model for any endeavor that was to qualify as science, also faltered. This doctrine had led social researchers to attempt to develop their
own objective observational language—*behavior*re. It also supported the view that social research should examine data using the formalized mechanisms for causal inference, explanation, and prediction. Testing of scientific hypotheses was reserved for formalized inference within the “context of justification.” Less formalized, qualitative data and inference were relegated to the “context of discovery,” where hypotheses might be conjectured and tentatively put forth but not verified or falsified.

The collapse of reductionism undermined the justification for *behavior*re, which was also shown to be descriptively inadequate (MacKenzie, 1977; Strike, 1974). There emerged a growing recognition that “intentionalist” explanations were required, suitable for understanding and explaining the norm-regulated behavior in which human beings, unlike molecules in motion, engage. It also became increasingly recognized that exclusive reliance on formalized, quantitative inference was inadequate for social research. The idea that physics should guide a conception of social science had run its course. As Anthony Giddens remarked in 1976, “Those who are still waiting for a Newton of social science aren’t only waiting for a train that won’t come in, they’re waiting in the wrong station altogether” (p. 13).

**The Fact–Value Dogma**

The fact–value dichotomy—the view that facts and values occupy distinct epistemic domains—underpins the value-neutrality doctrine in social science: the admonition to researchers to, as far as possible, purge their work of values and declare any that remain as *biases*. Too little recognized is that the fact–value dichotomy is positivist through and through, being but a corollary of reductionism. Values joined metaphysics in being incapable either of logical demonstration or of verification or falsification by reduction to empirical observation. For this reason, both lacked cognitive significance and thus fell outside the epistemic domain of knowledge. But because positivists were unable to sustain the principle of reductionism against its critics, “the whole argument for the classical fact/value dichotomy was in ruins” (Putnam, 2002, p. 30, italics in original). And so was the argument for value-neutral social research.

Just as social research is theory-laden, it is also value-laden, in two fundamental ways. First, the descriptive concepts employed by social researchers are often “two-edged” (Howe, 1985, 2003), meaning that they have both descriptive and evaluative dimensions. Consider the concept of *achievement*. It is used to make value-laden descriptions; achievement carries a positive valence, unlike a *pure* descriptive concept such as the number 5. Because such two-edged concepts are routinely (and unavoidably) incorporated into the descriptive vocabulary of social research, so, too, are the values of researchers, policy makers, and program designers participating in, sponsoring, or using such research.

In addition to being incorporated into the descriptive vocabulary of social research, values, including political values, are incorporated into social research methods. First, political values are assumed in the determination of what factors to take as “fixed” or given (Root, 1993) in the design of research. For example, the distribution of income levels might be taken as a given. How income level co-varies with student performance in reading might then be investigated with an eye toward getting a better understanding of “what works” given the status quo. Alternatively, how and why the distribution of income levels co-varies with student performance in reading might itself be the focus of investigation, with an eye toward evaluating the effectiveness and fairness of school-centered education reform and accountability. These two approaches are *conservative versus progressive*, respectively, and they loosely overlap with experimental/quantitative versus interpretative/qualitative methods.

I do not in any way mean to suggest by the latter observation that the relationship between research methods and political values is straightforward or uniform. There is nothing inherently status quo preserving about randomized experiments, and ethnographies can and often do accept the status quo as the backdrop for their work. My aim here is not to criticize or laud the politics of various methodological approaches. It is rather to make the general point that, whatever the methods employed, decisions about what factors to fix in the design and conduct of social research are unavoidable—and are unavoidably political.

Second, any social research methodology incorporates political values by unavoidably assuming a conception of democratic politics, if only implicitly. Consider a technocratic stance, which endeavors to maintain value-neutrality by refraining from making prescriptive judgments and confining itself to generating factual knowledge to be plugged in to the democratic process. Contrary to its self-portrayal, the technocratic conception is not politically neutral, for it *prescribes* that social researchers follow its dictates as opposed to the dictates of, say, deliberative democracy (House & Howe, 1999).

**The Two Dogmas and the New Scientific Orthodoxy**

As indicated earlier, the 2002 NRC report *Scientific Research in Education* (*SRE*) is the exemplar of the new scientific orthodoxy in education research. *SRE* adopts a version of the principle of the unity of science—“At its core, scientific research is the same in all fields” (p. 2)—which it then fleshes out in terms of “epistemological or fundamental principles that guide the scientific enterprise” (pp. 51–52). It distills the following six principles for scientific research (p. 52):

1. Pose significant questions that can be investigated empirically.
2. Link research to relevant theory.
3. Use methods that permit direct investigation of the question.
4. Provide a coherent and explicit chain of reasoning.
5. Replicate and generalize across studies.
6. Disclose research to encourage professional scrutiny and critique.

Principles 1 and 3 are particularly germane to my analysis. Principle 1 is implicated in the matter of demarcation, for it is required in distinguishing science from other intellectual pursuits. It is clear that philosophy, for instance, which is explicitly excluded from the domain of science by *SRE* (NRC, 2002, p. 131, note), fits the general characterization of scientific research as “a continuous process of rigorous reasoning supported by a dynamic interplay among methods, theories, and findings” (NRC, 2002, p. 2). Empirical testability is the additional requirement that philosophy apparently lacks, according to *SRE*, which, to telegraph my subsequent critique, presupposes the kind of empirical–conceptual dichotomy that Quine’s pragmatic critique undermines. Principle 3 emphasizes the value of employing a variety of methods, properly...
tailoring them to individual research questions. This is an important theme, which permeates SRE and which some of its advocates take as evidence that it is a moderate position in comparison with more extreme experimentalist positions that exemplify a "right pole" in a continuum of positions (Phillips, 2006).

The subsequent NRC report *Advancing Scientific Research in Education* (2004) adopted as its goal "to spur actions that will advance scientific research in education" (p. 1). This report followed directly from the framework for education science articulated in SRE: "Our recommendations for accomplishing this goal build on the National Research Council report *Scientific Research in Education.* That report offered an articulation of what constitutes high-quality scientific inquiry in education; this report recommends ways to promote it" (NRC, 2004, p. 1). "High-quality scientific inquiry in education" was identified with the six principles of scientific research set down in SRE.

In my view, the new orthodoxy, as codified in SRE and promoted in *Advancing Scientific Research in Education*, is pretty much a positivist throwback. SRE is often defended as staking out a moderate position that avoids the more extreme "right pole" views that would (re)establish randomized trials as the methodological gold standard and vastly diminish the role and stature of qualitative research. For example, Denis Phillips (2006), himself a member of the NRC committee that produced SRE, writes:

Careful reading of [SRE] reveals that it traverses the proverbial razor’s edge—one on hand it does hold, against those on the left, that useful, valid scientific inquiry is possible in education; but on the other hand it does suggest that the narrow account of the nature of scientific rigor, given by those on the right in terms of the randomized controlled experiment or field trial and a narrow range of related designs, is quite mistaken. (p. 19)

And two other members of the NRC committee, Margaret Eisenhart and Robert DeHaan (2005), write:

From the outset, the committee agreed that . . . the actual practice of scientific research is more descriptively oriented, more dependent on context, less cumulative, and more intuitive—in other words, more qualitative—than is the idealized model of experimentation, frequently described as the path to producing causal explanations. . . . The committee acknowledged the importance of research outside “science,” including philosophical, historical, and critical scholarship, and its contributions to education as well as to both natural and social science. . . . In the course of our deliberations, we discussed social and cultural context, participant involvement, political considerations, and ethical requirements, considering all of them as inherent features of the study of social and educational phenomena. (pp. 3–4)

Given my reading of SRE (careful, I hope), much of what Phillips, Eisenhart, and DeHaan say in these passages received little or no emphasis in the document or was overshadowed by countervailing claims. (In this vein, Phillips identifies at least one individual on the NRC committee as exemplifying the “right pole.”) In any case, a pretty good case can be made for the view that, whatever the intentions of the NRC committee members (and they likely differed), SRE does not fundamentally depart from a positivist/experimentalist conception of education science.

According to SRE, programs of education research proceed cumulatively: from describing and conjecturing, to establishing causal relationships, to understanding the mechanisms underlying such relationships. Although qualitative methods are touted throughout SRE, they are assigned a subsidiary epistemological and methodological role in the report’s most explicit articulation, in chapter 5, “Designs for the Conduct of Scientific Research in Education.” Qualitative methods are employed in the initial describing and conjecturing phase of scientific research. Qualitative methods can also “illuminate important nuances, identify potential counterhypotheses, and provide additional sources of evidence for supporting causal claims in complex educational settings” (NRC, 2002, pp. 125–126). But “if research is to be used to answer the question about ‘what works’ . . . it must advance to other levels of research” (p. 108). The “other levels,” it turns out, are experimental, the “ideal” of which is “randomized trials . . . for establishing whether one or more factors caused change in an outcome” (p. 110). As in the positivist conception, quantitative/experimental methods do the real work of science in the context of justification; qualitative/interpretive methods are subservient, relegated to the context of discovery.

The high epistemic status that SRE affords experimental methods is rooted in its regularity conception of causation, a rudimentary example of which is the following: “T works to produce O, under C” (where T is some treatment, O is some outcome, and C is some set of conditions). SRE is pretty much silent on the concept of intentionalist causation. It refers to the “volitional” features of human behavior and construes this as a problem for experimental research design: “Education is centrally concerned with people: learners, teachers, parents, citizens and policy makers. The volition, or will, of these individuals decreases the level of control that researchers can have over the process” (NRC, 2002, p. 86). Consistent with positivism’s unity-of-science principle and its exclusion of intentionalist causation, SRE construes the difference between causal inference in physics and in the human realm along one dimension: the precision with which regularities can be determined. Because people behave more erratically than protons do, education research, in comparison with physics research, has to cope with more sources of “noise” and larger “error limits” (NRC, 2002, p. 83).

For SRE the focus of education science is establishing causal relationships (regularities) to determine what works. But largely glossed over in SRE is that the question “What works?” is elliptical for “What works to produce valued education outcomes?” And the valued outcomes of interest—the O’s in “T works to produce O, under C”—are picked out by means of the kind of two-edged concepts described earlier, such as literacy, achievement, citizenship, cooperativeness, and the like. As indicated before, by employing such concepts, education researchers routinely—and unavoidably—introduce the values of certain subcommunities of education researchers, policy makers, curriculum designers, and so on, into the descriptive vocabulary of scientific education research. Value commitments are uniformly woven into the fabric of investigations of what works, however infrequently they may be identified or carefully examined.

SRE mildly protests the ways that education research is often buffeted by changes in political winds and recommends that it be better shielded. The third of its “design principles for fostering science in a federal education research agency” is, "Insulate..."
the agency from inappropriate political interference.” Types of inappropriate political interference include “micromanagement of decision making, the distortion of the research agenda to be solely short-run, and the use of the agency as a tool to promote a particular policy or position” (NRC, 2002, pp. 139–140). Otherwise, SRE has nothing to say about the relationship between education science and democratic politics. This silence, combined with SRE’s generally positivist tenor, suggests that SRE incorporates, if only by default, the technocratic conception briefly described earlier. At the heart of this conception is the idea that education science and democratic politics occupy separate domains. Investigating questions such as whether, to what extent, and at what monetary cost “T works to produce O, under C” is in the domain of education science; deciding which Os should be investigated and pursued in light of the information provided by education researchers is in the domain of democratic politics.

The technocratic conception of the role of education research in democratic politics aligns with aggregative democracy, a theory with a distinct positivist bent in that it formulates policy by aggregating preferences (values) that are held to be beyond the reach of critical examination and deliberation. The aggregative theory has increasingly come under criticism from within education research (Strike, 2006) and as a consequence of the reemergence of deliberative democracy (e.g., Gutmann & Thompson, 2004; Young, 2004). Two general criticisms may be advanced from the deliberative perspective. First, exempting the sought-after outcomes of education (the Os in “T works to produce O”) from criticism and revision serves to entrench status quo distributions of goods and powers that are, by and large, unjust and, in turn, precludes the possibility of effective participation in the education research and policy-making processes on the part of many citizens. Second, the technocratic conception needs to be defended rather than presumed. That such a defense could succeed is unlikely. The possibility—and desirability—of culling political values from education research depends on moribund positivist principles.

I could be mistaken in my attribution of a technocratic conception of social research to SRE and the new orthodoxy, or perhaps a technocratic conception is more defensible than I would have it. Still, it is telling that, consistent with the technocratic conception, addressing the question of political values in specifying what constitutes good scientific education research is not among the new orthodoxy’s concerns.

The Empirical Science–Humanities Dogma

The third dogma of education research is the empirical science–humanities dichotomy. C. P. Snow characterized the wider academic community along this dividing line some 50 years ago in his celebrated book The Two Cultures (1959). The gap between empirical science and the humanities has been lurking not far below the surface in education research for some time, but it has been rendered wider and more visible by the new scientific orthodoxy.

SRE distinguishes scientific research from other kinds of scholarship in education in terms of the criterion of empirical testability, which is interpreted so as to exclude the humanities (philosophy and history are the explicit examples; NRC, 2002, p. 131, note), thus isolating the humanities from the intellectual activity identified as the key to improving education policy and practice. Whereas SRE alludes to the idea that scientific research in education may be informed by the humanities, its failure to elaborate in any way renders the allusion but lip service.

Subsequent to the appearance of SRE, AERA formed a task force to formulate standards for education research, whose final product appeared as Standards for Reporting on Empirical Social Science Research in AERA Publications (AERA, 2006), currently available on the AERA website and in the August/September 2006 issue of Educational Researcher. Over the course of its work, the task force had been prompted to narrow its focus from general standards to standards for “empirical social science research,” thereby excluding humanities and related research.

The first paragraph of the standards (AERA, 2006) is as follows:

The American Educational Research Association (AERA) is pleased to provide guidelines for reporting on empirical social science research in AERA publications. These guidelines apply to reports of education research grounded in the empirical traditions of the social sciences. They cover, but are not limited to, what are commonly called qualitative and quantitative methods. Other forms of scholarship equally important to education research include reviews of research; theoretical, conceptual, or methodological essays; critiques of research traditions and practices; and scholarship more grounded in the humanities (e.g., history, philosophy, literary analysis, arts-based inquiry). The latter forms of scholarship are beyond the scope of this document.

That a line of demarcation exists between “empirical social science research” and “other forms of scholarship” is assumed here at the outset of the document and frames the standards that follow. The standards are well done, in my view, and provide very useful guidelines for the kind of research to which they apply. But the problem is that it is not the domain of empirical research—the exclusive domain of empirical research—to which the standards apply, unless, contra the pragmatic undoing of positivism, empirical knowledge can be isolated.

Having narrowed the scope of its standards to align with a reductionist conception of empirical social science research, AERA formed a second task force to develop standards for humanities-oriented research in education, which were adopted by the AERA Council in January 2009. Standards for Reporting on Humanities-Oriented Research in AERA Publications appears in this issue of Educational Researcher (pp. 481–486). The effect of dividing standards so as to have one set for empirical social science and a distinct set for the humanities serves to reinforce the dichotomy between empirical social science and the humanities, because the two sets of standards map onto the dichotomy. In this vein, one of the issues that most bedeviled the humanities standards task force (of which I was a member) was how to conceive humanities-based research as occupying an epistemological and methodological domain distinct from that occupied by empirical social science. For humanities-oriented and empirically oriented research in education do not constitute a dichotomy. Rather, they differ from one another in degree, their methodological boundaries are blurred, and their uses in research practice often intertwine. The task force eventually conceptualized the relationship between the two sets of standards in terms of overlap and complementarity:
Humanities-oriented research is empirical. Because much education research in the social sciences is also empirical, there is overlap between the two domains, and the standards appropriate for evaluating humanities-oriented research in education complement and sometimes overlap with AERA’s Social Science Standards. (AERA, 2009, p. 482 in this issue of Educational Researcher)

The third dogma is a positivist throwback, just like the first two dogmas. The idea that empirical testability may serve as the criterion to draw a line of demarcation separating science from nonscience is a version of the central tenet of reductionism upon which positivism foundered. In the social sciences, the line of demarcation became especially blurred with the advent of interpretivist methodology, in which the aims, requisite skills, and vocabularies of the humanities and empirical social science significantly overlap.

Research in the humanities is not devoid of empirical content. Nor, for that matter, is research in empirical social science devoid of the normative content characteristic of the humanities. Consistent with Quine’s “web of belief,” there is a continuum here, rather than a dichotomy, and a corresponding difference in focus such that humanities-oriented questions differ from more empirically oriented social science questions largely in terms of the ease with which they may be verified or falsified by relatively straightforward empirical testing, which is associated with the problems defining disciplinary traditions. To illustrate, the question of whether expanded school choice exemplifies a more or less just system of distributing education goods—a question investigated by the humanities, particularly political philosophy—is farther removed from rather straightforward empirical testing than is the question of whether expanded school choice increases racial isolation—a question for empirical social science research. But an answer to the first question is not independent of an answer to the second. Moreover, the second question would lack interest if it were not in some way related to the first.

The New Orthodoxy and the Rhetoric of Science

The conception of education science exemplified by the new orthodoxy does not significantly depart from a positivist conception. It mirrors the epistemic difference between the context of justification and the context of discovery, thereby diminishing the true epistemic import of interpretive/qualitative methods in intentionalist explanation. Its appeal to a reductionist empirical criterion—SRE’s “Scientific Principle 1” (NRC, 2002, pp. 54–55)—renders values beyond the purview of science, to be hammered out in political forums and mused about by humanities-oriented researchers using their special non(un?)scientific methods.

But the problems with the new orthodoxy go deeper than the answer it gives to the education science question per se. They are rooted in how the question is framed in the first place and the method of analysis then employed to pursue the answer.

Ian Hacking offers a good piece of advice regarding how to approach definitional controversies: “Don’t first define, ask for the point” (Hacking, 1999, p. 5). As it turns out, there is not just one point to be served by pursuing the education science question but several, among which the following three are paramount:

1. It will make education research better by providing guidance to education researchers in the practice of their craft, as well as providing a set of standards for evaluating research quality.
2. It will provide a means for education research to retain or enhance support, including financial, from its patrons, such as the federal government and private foundations.
3. It will help prevent an alien conception of science from being foisted on the education research community from outside, for example, by the U.S. Congress.

The first rationale is the purest in motive. It exemplifies the aim of explicating scholarly norms internal to the education research community, and it is the ostensive rationale in documents such as SRE and AERA’s standards for empirical social science research. The second and third rationales are matters of professional prudence more than the first rationale and may be seen as responses to external considerations.

The three rationales clearly are not exclusive of one another. But more than being consistent, they are inextricably woven into the rhetoric driving the formulation of the new orthodoxy. Rationales 2 and 3 were the primary motivations for specifying what qualifies as scientific education research, and they quite clearly influenced the shape that the new orthodoxy took. The new orthodoxy speaks to internal and external audiences with one voice in its aim of limiting the range of education research that is to be deemed “scientific.” In this way, the new orthodoxy’s rhetoric is multivalent in its purposes and the audiences to which it appeals.

This multivalence is obscured by the method of analysis employed. The method of SRE assumes that the more general question of the line of demarcation between science and other intellectual endeavors is unproblematic and then identifies the kinds of education research that fall on the science side. SRE’s method assumes a kind of essentialism, in which, reminiscent of Plato, identifying instances of scientific education research amounts to looking for a sufficient resemblance to a preexisting and stable idea.

This method is dubious, indeed. The failure of positivists’ sustained efforts to establish science’s epistemic credentials via the principle of reductionism, which would have drawn the positivist line of demarcation, pretty much ended the positivist program in philosophy. How the demise of positivism undermines the new scientific orthodoxy is a story rehearsed in the first section of this article. Here, I shift the focus to a critique of the new orthodoxy’s essentialism.

The Essentialist Response to Definitional Controversy

I begin with an example of a controversy from the second half of the 20th century—whether to redefine the concept of death—that well illustrates an essentialist approach to an important definitional challenge. I then consider the controversy about the concept of equal educational opportunity.

The concept of death. Advances in drug therapy and other health technologies have created a new class of “brain-dead” patients who can be sustained only by administering treatments such as antibiotics, tube feeding, and, especially, mechanical ventilators.
These patients seem to possess some characteristics of the living, for example, heart beat, digestion, and evacuation, but also some characteristics of the dead, for example, permanent loss of consciousness, of spontaneous respiration, of cranial reflexes, and of responsiveness to pain stimuli. Psychiatrist Willard Gaylin (1974) termed these gray-area patients “neomorts” (newly dead) in a chilling Harper’s magazine article titled “Harvesting the Dead,” in which he suggested that perhaps neomorts should remain on life support to serve as a self-replenishing blood bank, to provide fresh organs for transplant, and so forth. Gaylin’s provocative article was part of an intensifying conversation about whether the existing definition of death—cessation of heartbeat and respiration—should be revised.

Critics of the revisionist stance denied that brain death was the same thing as death and insisted that brain-dead patients should be treated accordingly. Among other things, they held that brain-dead patients should not be pronounced dead and have their organs removed while their hearts are still beating and they are breathing with the assistance of a mechanical ventilator. Revisionists countered that they were not offering a redefinition of death, only new criteria for its identification.7 One variant of revisionism was the whole-brain definition of death as total and irreversible destruction of the brain, including the brain stem. (The brain stem remains intact for patients in a persistent vegetative state, such as Terri Schiavo, who thus are not dead according to the whole-brain definition.) According to the advocates of that definition, total and irreversible destruction of the whole brain is what death has always meant, despite the various ways that death may have been empirically identified. Mechanical ventilators just mask death; the whole-brain conception is not a redefinition of the concept of death but merely the use of new criteria used to identify it. A second variant of revisionism is the personhood (or higher brain) definition, which holds that it is the death of the person that needs to be determined, not the death of organ systems. In this view, the death of a person has always meant the permanent loss of consciousness and of the ability to interact socially; prior to medical advances, permanent loss of consciousness was virtually simultaneous with cessation of respiration and heartbeat. (Terri Schiavo could have been dead in this view while she continued to breathe, depending on all the details and their interpretation.) In the end, the whole-brain definition gained wide acceptance and was codified in the Uniform Determination of Death Act (National Conference of Commissioners on Uniform State Laws, 1980), which has been adopted by 43 states.

The controversy about whether to revise the definition and, if so, which of several competing definitions to adopt was ostensibly about the essential features of the concept of death: about getting at what death is—and always has been—not about how it ought to be reconceivd. But this is not how the debate unfolded in reality. Rather than being about how to clear the brush and uncover what death really is, which allegedly had been obscured by changing knowledge and capabilities in health care, the debate was about whether the definition of death ought to remain the same or be reconstructed in response to changing interests and circumstances. Wide acceptance of the whole-brain definition did not come about because its advocates had correctly identified the essence of death. It was settled, instead, on largely pragmatic grounds. It turned on answers to the following kinds of questions: Do brain-dead individuals have any quality of life in human terms? How reliably can death be identified under a given definition (a serious problem for the personhood definition)? Is care of brain-dead individuals an unnecessary—because futile—medical expense that pointlessly drives up medical costs and reduces assets that could be had by the survivors? Is existing in an unconscious state, unable to interact and look after basic bodily functions, an affront to a patient’s dignity? And over the course of prolonged treatment, does it destroy the patient’s memory in the minds of survivors? Could the demand for viable organs for transplantation cause physicians to declare death too hastily in order to make organs available?

The concept of equal educational opportunity. A process similar to that in the controversy about the concept of death took place in the controversy about the concept of equal educational opportunity (EEO). James Coleman (1968), a nonessentialist in this story, proposed conceiving of EEO in terms of equality of outcomes, which various essentialists subsequently labeled as a conceptual confusion (e.g., Burbules & Sherman, 1979; Jencks, 1988; O’Neill, 1976). The meaning of opportunity, they argued, is such that providing an opportunity cannot be providing a desired outcome. People are free to exercise opportunities or not; and when opportunities are exercised, people may nonetheless fail to achieve the desired outcome.

The nonessentialist response is that linguistic intuitions about the concept of EEO, which are based on a history of usage, may have to be modified, depending on what is at stake (Gutmann, 1999; Howe, 1997). In the case of EEO, opportunities must be “effective” (Coleman, 1968) or “worth wanting” (Howe, 1997), not “bare” in the sense of being impossible to exercise (as in the argument of the San Francisco school board in Lau v. Nichols [1974] that Chinese-speaking children enjoyed the same opportunities as their English-speaking counterparts: same rooms, books, teachers, etc.) or “costly” in the sense of oppressing group members as the price they pay for their success (as in schools forcing GLBT youth into the closet). Making sense of opportunities that are effective or worth wanting cannot be separated from the question of their capacity to produce desired outcomes.

The real issue, then, is not what the essence of EEO is but how EEO ought to be conceived to serve the purposes of providing it. It is a pragmatic-cum-political question that turns on the degree to which intervention in the lives of students to better equalize their life chances, both in and outside schools, is to be embraced (Coleman, 1968).

So what does all of this have to do with the education science question? That question, too, has been approached in an ostensibly essentialist manner that obscures its underlying rhetorical features. The controversies on brain death, EEO, and education science share the general essentialist presumption that the controversy is (should be) about what X is. This approach is typical of definitional controversies (Schiappa, 2003). The twist is that rather than seeking to uncover the essence of education science directly, the new orthodoxy presupposes that a set of general criteria for science is unproblematic and then seeks to specify the varieties of education research that fit them.
The Pragmatic Response to Definitional Controversy

The pragmatic alternative to essentialism—the antagonist in the brain death and EEO illustrations—sees definitional controversies as involving “ought” questions rather than “is” questions. It asks how X ought to be (re)conceived rather than what X is—and sees (re)definitions as “rhetorically induced” (Schiappa, 2003). The inducement can take at least two forms: (a) controversies about action or policy, leading to “definitional ruptures” (illustrated by the brain death and EEO examples); and (b) controversies about legitimacy or authority (illustrated by the education science example). These two forms can go hand-in-hand and probably often do, but the second is clearly primary with respect to the education science question.

As indicated previously, the new orthodoxy frames the education science question in terms of an “is” proposition rather than an “ought” proposition and assumes that what science is, generally speaking, is unproblematic. In the process, it tacitly elevates science above the rhetorical fray that characterizes disputes about what ought to be and then ascribes this status to education science as well. But what about the conception of science that the new orthodoxy builds on? Is it really above the rhetorical fray?

Although not the sort of thing one is likely to encounter in standard accounts of scientific method in textbooks and courses, science historically has had to cultivate patrons for its survival. To do so, it painted itself as more than merely a certain kind of persuasive argument. Francis Bacon, an early champion of science, proclaimed that scientific thinking exemplified “minds washed clean from opinions” (quoted in Lessl, 2005, para. 15). When done right, science is (essentially) objective and provides the most secure basis for knowledge. It is free of self-interest and politics; it is a purified way to seek truth, not tethered to how its results may be interpreted and used; it is the key to solving problems and to making social and technological progress. In this, says Thomas Lessl (2005), science is a “master of disguises” (para. 15). It has sought to “construct a priestly ethos”—by suggesting that it is the singular mediator of knowledge, or at least of whatever knowledge has real value . . . and should therefore enjoy a commensurate authority” (para. 27).

The problem is that much of what priestly science claims to be has been called into serious question. Thomas Kuhn penetrated science’s (positivist) disguise in a variety of ways, going so far as to suggest an analogy between scientific revolutions and political revolutions to emphasize the rhetorical rough-and-tumble that goes into establishing a scientific theory. As indicated previously, various values, such as accuracy, scope, simplicity, consistency, and comprehensiveness, are applied and weighed. A form of judgment is employed in which there are no formal rules—and no “is” propositions to be uncovered—to determine what theory ought to win out. This occurs through a process of persuasive argumentation.

Kuhn’s focus was natural science. Positivist social science was criticized as well, and more fundamentally, with the advent of what Rabinow and Sullivan (1979) refer to as the “interpretive turn.” Of particular interest here is criticism of the objective-cum-value-neutral pair, tersely joined in Bacon’s ideal of scientific minds as “washed clean from opinions.”

Feminist “standpoint” theorists have effectively challenged the objective side of this pairing by arguing that achieving objectivity in social research requires more than just paying close heed to the canons of traditional positivist social research regarding design, sampling, analysis, and so forth. For avoiding these sources of bias does not address the threat to objectivity that results from the historical dominance of certain groups in social science, particularly White men, which limits what can be perceived and hypothesized. Harding (1993) turns on its head the claim that positivist social research is too objectivist: “It is not that [positivism] is too rigorous or too ‘objectifying’ . . . it is not rigorous or objectifying enough” (pp. 50–51).

Harding advances the idea of “strong objectivity,” which “requires that the subjects of knowledge be placed in the same critical, causal plane as the objects of knowledge” (1993, p. 69). Accomplishing this level of objectivity requires including a much broader range of researchers and participants than has been the norm in social research, particularly groups that historically have been excluded. Precisely because such groups have been marginalized, inclusion of their perspective—or “standpoint”—promises to improve understanding of the workings of social processes over the kinds of understanding that grow exclusively out of the mainstream, where what is available through alternative standpoints has been largely invisible.

Lorraine Code (1993) focuses on the epistemological schema that has been the focus of much philosophical analysis: “S knows that p,” where S is the knower and p (a proposition) is the object of knowledge. Code objects to the conception of objectivity implicit in the schema in which subjects of knowledge are an abstraction, such that “competent” knowers (Ss) are interchangeable. She also objects to the schema’s abstraction of knowledge such that it fails to distinguish propositions (ps) about human beings from those about physical objects. Given the current historical situation, rather than avoiding the subjectivity associated with distinguishing among knowers, the “S knows that p” schema simply assumes that the subjectivity of White male social scientists applies (or should apply) to all. And the schema also implies that human beings are to be known in the same “unsus- tainable” way that the objects of physics are known. According to Code, researchers’ theoretical and value orientations are not independent of their social position or of the broader sociohistorical context, both of which play an important role in determining what “facts” are thinkable and likely to be found. Also, human beings, unlike the objects of physics, have subjectivities. Thus, she contends, “objectivity requires taking subjectivity into account” (p. 32).

With respect to the value-neutral side of the objective/value-neutral pair, deliberately including marginalized groups in social research (as both participants and researchers) has a rather obvious political dimension vis-à-vis democracy and social justice. This is not something that thinkers such as Harding find problematic—quite the contrary. For Harding (2006), fusing social science with political commitments is unavoidable, however “unconscious” of such commitments researchers may be.

Similarly, in Code’s (1993) view, value orientations are not (should not be) immune from critical scrutiny. But as Code points out (p. 27), the posturing objectivity of positivist social science, invoked by the trump card “Science has proved . . . ,” renders the critical scrutiny of values un(anti)scientific, with the result that “inquiry stops right where it should begin” (p. 30).
Social Research and Political Values

The claim that social science is unavoidably infused with politics has been a recurrent and central theme in my analysis. It could benefit at this juncture from being fleshed out by concrete examples. So I will digress and consider two relatively well-known cases, beginning with the “Lysenko affair,” a popular example among philosophers regarding the relationship between science and politics, often used to illustrate the evils of mixing them (e.g., Phillips & Burbules, 2000).

Trofim Lysenko was a Ukrainian agronomist who, in the 1930s, joined and ultimately won a dispute between Soviet geneticists and Lamarckians vying for acceptance of their hereditary frameworks, particularly as they related to crop production. An important question in the dispute was which kind of theory best fit with Marxist materialist tenets and thus also with the political revolution. Both sides claimed theirs fit best and labeled the competitor bourgeois.

Although not himself a member of the Communist Party, Lysenko very effectively played to the political powers, including Stalin, to promote and win acceptance of his own version of the Lamarckian alternative. Lysenko emphasized the (alleged) evidence of his successes in practice, while suppressing his failures and characterizing geneticists as ivory-tower intellectuals who spent their time manipulating the characteristics of fruit flies. He aggressively attacked geneticists to the point of labeling them class enemies and, with the help of the Soviet government, squashed their opposition, in some cases having them jailed. In the Soviet scientific community, Lysenko's theory ultimately was recognized to be utterly wrong and was abandoned, but not until it had disastrous effects on Soviet agricultural production.

Lysenko succeeded at a particular time in Soviet history when the competition between genetic and Lamarckian theory was considered by Soviet scientists to be a genuine scientific disagreement. The principle of materialism, a tenet of communist political theory, was also an epistemological-cum-metaphysical issue with respect to the ontological status of genes, complicating the meaning of “political” in the rhetorical context. Much more was at issue than just a strategic question on the part of scientists of how to secure support from the political powers for their favored theory. Lysenko was, indeed, championing bad science, and politics was heavily implicated in it. But was the science bad because it was infused with politics per se or because it was infused with bad politics—antidemocratic politics—whereby dissenters were crushed and the evidence as judged by the scientific community ran second to Lysenko’s ability to sell his theory to the political powers?

Consider now the infamous J. Philippe Rushton, a Canadian psychologist whose raison d’être is linking race to intelligence, brain size, a range of moral behavior, and penis size (where the relationship is inverse). Asians rank at the top, Caucasians next, and Africans after that. Like the authors of The Bell Curve (Herrnstein & Murray, 1994) and Arthur Jensen (Rushton has been a coauthor with Jensen), Rushton has amassed mounds of data and statistical analyses in his pursuit, allegedly following objective science wherever it leads.

Setting aside the warrant of Rushton’s claims vis-à-vis his data and methodology, it is difficult to see how his work can be separated from politics, to be evaluated only in terms of its data and analytical methods, much less why it should be. Why, as Code asks, should inquiry stop here? Why shouldn’t we ask why Rushton's work is of any interest, and to whom? Who supports it? What are its implications for policy? For democracy? For those it portrays as inferior?

Social research does not consist of collecting and organizing brute facts that just happen to be lying around. If it makes sense to speak of brute facts at all in social research, such facts exist only relative to some scientific-cum-political framework (Howe, 2003). In Rushton’s case, establishing linkages between race and desirable human characteristics would underwrite his hereditarian/ evolutionary theory of racial hierarchy, which, in turn, would help square the existing social hierarchy with what science ordains as unalterable and thereby rationalize racist policy prescriptions.

Seen in this light, Rushton’s line of work has palpable antidemocratic dimensions. It is clearly bad for race relations and harmful to those it diminishes. Because of the huge overlap among characteristics in the races that even the biological race determinists have to admit, it has no policy implications for how to distribute opportunities and positions on the basis of race. Unless principles of merit and justice are replaced with a racial caste system, opportunities must be distributed on the basis of individual qualifications.

Given that the concepts of science and objectivity lack essences, why choose positivist versions of them so thin that work such as Rushton’s cannot be evaluated in its entirety to determine its scientific merit? In a liberal democracy, there are reasons for not banning or censoring the kind of research Rushton and his ilk are up to, but whether among those reasons is that Rushton’s work exemplifies (or could exemplify) good, objective science is arguable, indeed.

In this vein, Rushton’s funding sources are not irrelevant to judging the scientific merits of his work. One of his primary sources, for example, is the Pioneer Fund, established in the late 1930s to support the propagation of eugenics doctrine, to which Rushton subscribes, and to which his research on race and intelligence is fundamental (Jackson, 2006). That Rushton gets funding from the likes of the Pioneer Fund, of course, is no airtight charge against him. But à la Bayes, it certainly has a lot to do with raising the prior probability that whatever research findings Rushton produces will incorporate his racist predilections. (Note: Ad hominem arguments are inappropriate only where irrelevant features of one’s character and background are invoked, which is not the case here.)

The moral of the story is the same for Rushton as for Lysenko, and it is not to endeavor to keep science and politics separate, particularly science directed at social policy. Even if such separation were desirable, it is impossible to pull off; and the illusion that it is possible gives cover to the Rushtons of the world under the protective shield provided by the positivist rhetoric of science. The moral of the stories of Rushton and Lysenko is that bad politics—antidemocratic, self-serving, authoritarian politics—leads to bad science. Accordingly, the political frameworks in which scientific research is embedded should be subjected to ongoing critical examination.

One very important reason to try to figure out what to say about cases such as Rushton and Lysenko is that being able to
claim the mantle of science brings credibility, prestige, support, and influence. Ian Hacking (1999) gets at this feature of the concept of science with his notion of “elevator words.” Other elevator words include fact, truth, reality, objectivity, and knowledge. One important feature of elevator words is that they are circularly defined. A particular conception of science finds its home in a set of conceptions of fact, truth, reality, objectivity, knowledge, and so on, that form a circle such that revising the meaning of one will invariably require revising the meaning of others. The manner in which conceptions of objectivity and science are linked in Hacking’s and Code’s analyses provides an illustration of this circularity.

A second important feature of elevator words, and the one that most concerns me here, is that despite their loftiness—Wittgenstein would call them “sublimed” (1958, section 38)—elevator words have no static essences; rather, they are products of linguistic usage and, as such, are unstable and subject to change. Because so much hinges on what definitions they take on, they are, Hacking observes, at the center of some pretty “venomous” disagreements. Whether it is accurate to call the disagreements about education science “venomous” I’m not sure, but, at the very least, it has been divisive.8

Worries About the Rhetorical Account of Science

But if this rhetorical account of science is correct—if there is no priesthood—then everything is permitted. Isn’t it? Well, no. Characterizing science as rhetorical in the sense that, I suggest, applies to Kuhn, Harding, Code, and Hacking does not require denying that science is “a profoundly powerful form of inquiry” (Lesl, 2005, p. 2). It only requires denying that scientific claims are above and beyond persuasive argumentation, that scientists are above and beyond weaknesses such as a blinkered perspective, ego involvement, resistance to novelty, an interest in the size of their paychecks, and the like, in the conduct of their research. Scientists fail the “minds washed clean from opinions” criterion in each of these ways.

Nor does characterizing science as rhetorical require construing the question “Is X a science?” as arbitrary. Just as there are clear cases of the living and the dead despite the gray area brought to the fore by the phenomenon of brain death, there are clear cases of science and nonscience despite the gray area brought to the fore by the education science question. Physics, biology, and chemistry are paradigmatic instances of science, and fortune-telling and astrology, of nonscience. Many cases in between are much less clear, including several that have “science” in their names. What kind of sciences are computer science and cognitive science? How about creation science? And then there are nursing science, library science, and dairy science. What should we say about those? Why? Is mathematics a science? Women’s studies? Ethnic studies?

Intuitions fail to provide firm answers to these questions, for there are no essences or histories of usage to ground them. Pragmatic (nondemonstrative) arguments are required, which brings me back to the issue of the point of arguing one way or the other about whether some X is science. Conceivably, one might have only a taxonomical interest; perhaps some ivory-tower philosopher would fit this bill. Much more pervasive, however, is an interest in awarding (or denying) the mantle of science and the advantages it confers to the X in question. As indicated before, being able to claim the mantle of science brings advantages such as prestige, credibility, support, and influence.

Finally, I do not contend that the question of whether some X qualifies as science is never worth asking. Consider fortune-telling and astrology. Neither of these counts as science, in my view, because their practitioners are impervious to counterevidence to their claims and make no effort to explain what mechanisms underlie their crafts and how they fit with what else is known about the workings of the world.

Now consider creation science or “intelligent design.” Unlike fortune-telling and astrology, intelligent design theory claims to be doing what evolutionary science does, but doing it better, appealing to probability theory, among other things. What, then, makes critics not only deny that intelligent design is more warranted than evolutionary theory but define it out of the arena of science altogether? The short answer is that intelligent design rests on religious authority, which is inconsistent with the kind of justification for belief that science demands.

Whatever the merit of these critiques of fortune-telling, astrology, and intelligent design, the point of answering them is rather clear. If fortune-telling and astrology are not science, practitioners who charge for their services claiming the mantle of science are bunko artists. If intelligent design is not science, then introducing it into the schools as if it were is not adding to the science curriculum but introducing a masked form of religious education (precisely the argument that convinced Judge John E. Jones to declare as illegal the Dover, Pennsylvania, School Board’s insertion of intelligent design into the science curriculum; “Judge Rules Against ‘Intelligent Design,’” 2005).

The point of the education science question is less clear than the point in the preceding examples, in large part because the education science question is multivalent in the way described earlier. Given the high stakes combined with the uncertainty associated with a large gray area, the new orthodoxy’s drawing of a boundary around the kinds of education research that are deemed scientific and cordoning them off from other research is dubious, divisive, and counterproductive. The boundary is dubious because of its essentialist-cum-intuitionist method and related inattention to just how serious the demarcation problem is. The boundary is divisive because an unavoidable dimension of pursuing the question of education science (where it counts anyway, outside the philosophy seminar room) is the implications for those who get to be included in the elevated class of researchers versus those who do not. I surmise that those on the margins are more acutely aware of this dimension of the new orthodoxy, as well as the authoritarianism exemplified by its unity of science doctrine, than are those included among the bona fide education scientists (Harding, 2006; see also Moss, 2005, and St. Pierre, 2002). Finally, because of its undue divisiveness, the boundary is counterproductive.

Conclusion

Under present conditions, “What constitutes education science?” is just not a good question to be asking. It is too liable to self-serving partisanship (witting or not), empowering the elevated ones to use their preferred definition of science to summarily exclude challenges and alternative views from the conversation about education policy and practice—as in, “What you say is
interesting perhaps, but it’s not science.” The exclusionary effect is likely to be less direct than this, but it is pervasive in areas such as funding and access to policy makers.

Nevertheless, it would be unwise to reject outright the general idea that education science would be a good thing. We simply have not come up with a satisfactory account of what it is. In the meantime, the idea of education science deserves to be rejected conditionally: If the new orthodoxy is what is meant by education science, then that conception is too technocratic, acritical, single-minded, exclusionary, and so forth, to be a good thing.

Clearly, rejecting the education science question poses a formidable rhetorical challenge. A rough-and-ready form of tacit positivism is the prevailing conception of science in the broader rhetorical context that includes the public, policy makers, and, I surmise, a fair number of individuals in the academy. A significant number no doubt see those who reject the education science question as attempting to avoid accountability for their (so-called) research that consists of some combination of subjective, ungeneralizable, partisan, hypercritical, incomprehensible, useless, speculative conjecturing. For their part, rough-and-ready positivists in the education research community have asserted neoclassical experimentalism (Howe, 2004) in the mold of Campbell and Stanley (1963)—for whom experimental methods are “the only means” (p. 2) in education of settling disputes, verifying what works, and accumulating knowledge. The resurgence of such thinking has served to renew the paradigm wars.9

In the eyes of a significant number of others in the education research community who would explicitly disavow a positivist conception of science, the new orthodoxy codified in SRE responds to the third purpose of the education science question specified earlier: preventing an alien conception of science from being foisted on the education research community from outside (Eisenhart & Towne, 2003; Phillips, 2006). Care was exercised in SRE to make clear that qualitative methods are an important part of education science. Care was also exercised in the preamble to AERA’s empirical social sciences standards to avoid laying down prescriptions on how to conduct empirical research and to avoid suggesting that other forms of education research, including humanities-oriented research, lacked importance. From this perspective, the new orthodoxy is a compromise with the powers that be, to avoid the decree of an even more restrictive conception of scientific research in education.

Attempting such a compromise is quite reasonable under current conditions, for there is good reason to take seriously the rhetorical context to which this compromise attempts to respond. Accordingly, I am not opposed in principle to the general motivation behind SRE. It is the result that I find problematic. For reasons given previously, particularly the marginalization of qualitative methods and the humanities, adherents of the new orthodoxy fail to appreciate the extent to which it turns back the clock to the era before the paradigm wars and the heyday of Campbell and Stanley’s conception of good education research. The question, then, is whether there is a way to deal with the current rhetorical context short of embracing the new orthodoxy. I have a few general suggestions.

It would be useful to revisit the parallels between the field of education and other interdisciplinary endeavors in the social realm that weave research into practice, particularly medicine. The advent of “evidence-based” medicine provided an important impetus for what came to be labeled “scientifically based” education practice, which, in turn, spurred the formulation of the new orthodoxy. Evidence-based medicine also embraces randomized experiments as the gold standard for building up a store of scientific knowledge to be applied in practice. Generally speaking, it is grounded in the same positivist rhetoric of science that frames the education science question.

But there is a long list of criticisms of evidence-based medicine (e.g., Farley, 2007; Loewy, 2007; Strauss & McAlister, 2000) that have close parallels to criticisms of scientifically based education practice as defined by the new orthodoxy: The question of its effectiveness has not been subjected to its own strictures; randomized trials are frequently infeasible; randomized trials often lack external validity because of the special conditions under which they are conducted and because of sample bias due to self-selection; the applied science model often straightjackets practitioners because it underestimates the importance of know-how that flows from clinical experience and judgment; the guidance provided to practice is relatively minor because the knowledge available often cannot be applied to specific situations, and the store of studies that meet the criteria of adequacy is quite limited.

Each of these criticisms questions the ability of evidence-based medicine to foster “what works” in medical treatments and thus challenges it on its own terms. Evidence-based medicine can also be challenged in broader terms, again in parallel with criticisms of the new orthodoxy in education. Evidence-based medicine excludes matters pertaining to political values from the purview of its inquiries into “what works”—justice in the distribution of health care, for example—and thus fails to address the sizable health gap in the United States among the very same populations that suffer from the achievement gap (Lynch, 2001). Evidence-based medicine’s focus on treatments that “work” can actually go against social justice by making it more difficult to evaluate care from outside the system (Loewy, 2007), for instance, by empowering nonpractitioner accountability functionaries to push standards of care in the direction of the bottom line (Rodwin, 2001). Evidence-based medicine also ignores social and cultural dimensions of patients (except as co-variates) that have much to do with explaining and improving desired outcomes.

That evidence-based medicine has come in for criticism does not settle the education science question, of course. But it is relevant that critics of an overly narrow, positivist-inspired conception of science as the foundation of practice are not confined to education. Like scientifically based education practice in accord with the new orthodoxy, evidence-based medicine applies to only a part of what lies (or should lie) within the scope of the question “What works?” Perhaps this observation will help prompt a reopening of the education science question, particularly regarding how limited—and limiting—the reigning conception is.

In a related vein, more could be done to emphasize important differences between social science and natural science as well as the blurred boundaries between the humanities and more empirically oriented social science research. The contingent and contentious nature of the concept of education science could be more heavily emphasized as well, and so could the specific contributions that the humanities have to make to—or in—education science in determining “what works.” Again, “What works?” is elliptical for “What works to produce valued education outcomes?”
Exploring what should be valued—is valuable—in human endeavors is at the heart of much scholarship in the humanities. An education science that jettisons this freight also jettisons its compass.

NOTES

1 This strand is condensed from portions of Howe (2003), Howe (2005), Howe (2008), and Howe and MacGillivray (2009). Some of the analysis is close paraphrasing.

2 The rhetoric of science is an area of scholarship that cuts across academic fields, particularly philosophy, sociology, and communications. The idea of rhetoric as pertaining to the study of persuasion and argument—which may be done well or poorly, manipulatively or forthrightly, logically or illogically—can be traced at least as far back as Aristotle. It should be contrasted with the current popular idea of rhetoric, associated with adjectives such as mere and empty.

3 Here I suggest how the education science question might be reoriented, and why. Elsewhere I have proposed a deliberative democratic (as opposed to technocratic) conception of education (House & Howe, 1999; Howe, 2003) and discussed the role of interdisciplinarity (Howe, 2005). See Kelly (2006) for his related proposal for grounding education research in discourse ethics.

4 Quine’s critique targeted the empiricist tradition in general, but I avoid that complication here. Positivism (including logical positivism and logical empiricism) is a paradigm version of empiricism.

5 For a more comprehensive discussion of philosophical alternatives to positivist education research, including pragmatism’s relation to them, see Bredo (2006).

6 Although quite pervasive, this view is rarely articulated. But see Campbell (1982) and Shadish, Cook, and Leviton (1995); also see Howe (2003) and Strike (2006) for a more elaborate critique than that provided here.

7 This is an oversimplification, to be sure. There were other alternatives and more nuanced versions of the ones I describe (see DeGrazia, 2005). In my view, I provide a reasonably accurate, broad-stroke characterization (also see Schiappa, 2003).

8 Several prominent journals have devoted entire issues or special sections to the topic. Examples include a major section on scientific research in education in Educational Researcher (2002, vol. 31, no. 8); an entire issue of Qualitative Inquiry (2004, vol. 10, no. 4); a symposium on “the education science question” in Educational Theory (2005, vol. 55, no. 2); and an issue of Teachers College Record (2005, vol. 107, no. 1), devoted to the implications of scientific research in education for qualitative inquiry. In addition to the special section in Educational Researcher referred to earlier, a series of articles in various ways critical of the new orthodoxy have appeared, including those by Bullough (2006); Freeman, deMarrais, Preissle, Roulston, and St. Pierre (2007); Hostetler (2005); and Johnson and Onwuegbuzie (2004).

9 Robert Boruch (2002), for example, suggests that education researchers who disagree with neoclassical experimentalism are engaged in “ideological posturing” (p. 37). An analysis in a similar vein is to be found in Thomas Cook (2002). And although I do not identify SRE with neoclassical experimentalism, Pamela Moss (2005) and Elizabeth St. Pierre (2002) identify a reactionary posturing in SRE toward a vaguely characterized postmodernism.

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