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# Take my paradigm ... please! The legacy of Kuhn's construct in educational research

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Thomas Kuhn developed the construct of research paradigms to make sense of the history of conceptual change in the physical sciences. The construct has since been appropriated by a number of academic fields and by non-academics as well. This paper traces the use of the construct in the educational research field. The bulk of the paper is organized around two questions: (a) Was it ever appropriate to characterize the educational research field's acceptance of qualitative methods as equivalent to one of Kuhn's paradigm revolutions? (b) Is paradigm talk appropriate today?

## Introduction

For many educational researchers working in the 1970s and early 1980s, the growing interest in and acceptance of qualitative research during that time did not represent merely the availability of new methodological options. Rather, the field's embrace of qualitative methods was seen as a sign that the field was undergoing the sort of paradigm revolution that Thomas Kuhn (1970/1962) had talked about in his historical account of conceptual change in the physical sciences (see, for example, Eisner, 1979; Lincoln & Guba, 1985). Over the years, Kuhn's paradigm construct has become part of the lexicon not only of educational researchers but also of other social scientists and even members of the policy and business communities. Within the educational research field, we even have begun to see a phenomenon that I have characterized elsewhere as *paradigm proliferation* (Donmoyer, 1999a, 1999b, 2002).

Here I want to address two questions: (a) Was it ever appropriate to characterize the educational research field's acceptance of qualitative methods as equivalent to one of Kuhn's paradigm revolutions? (b) Is paradigm talk appropriate today? Before addressing these questions, however, I must first say a bit about what Kuhn meant by

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the term *paradigm* and how Kuhn's conception has been modified in the process of being transported to other contexts.

*The multiple meanings of Kuhn's construct*

*Kuhn's conception.* In his book, *The structure of scientific revolutions* (1970/1962), Kuhn indicated that paradigm shifts—which, according to Kuhn, occur intermittently in physical science disciplines—involve replacing one way of thinking about knowledge and research (and also the world the researcher is studying) with another incommensurable view. Such a shift, according to Kuhn, involves:

a reconstruction of the field from new fundamentals, a reconstruction that changes some of the field's most elementary theoretical generalizations as well as many of its paradigm methods and applications.... When the transition is complete, the profession will have changed its view of the field, its methods, and its goals. (1970/1962, pp. 84–85)

Kuhn also compared paradigms to the sort of images used by gestalt psychologists—images like the one in which 'the marks on paper that were first seen as a bird are now seen as an antelope, or vice versa' (p.85)—even while acknowledging that this analogy 'can be misleading ... [because] scientists do not see something *as* something else; they simply see it.' Kuhn explained: 'The scientist does not preserve the gestalt subject's freedom to switch back and forth between ways of seeing' (p. 85). What the scientist sees, in other words, is taken as real rather than as a mere perception or interpretation of reality.

Despite these caveats, Kuhn concluded: 'The switch of gestalt, particularly because it is today so familiar, is a useful elementary prototype for what occurs in full-scale paradigm shift' (1970/1962, p.85). Kuhn also invoked Piaget's notion of cognitive schemata to help his reader make sense of his construct. One difference, of course, is that Piaget's cognitive schemata are psychological phenomena while Kuhn's paradigms are worldviews that are shared by members of a scientific community.

*Transporting Kuhn's construct to the field of educational research*

*Kuhn's disciples.* As noted above, many educational researchers appropriated Kuhn's paradigm notion to make sense of the methodological revolution that was occurring in the educational research field during the 1970s and early 1980s. The bulk of the researchers who did this defined Kuhn's term in a way that was very much like Kuhn's own definition. Patton (1990/1980), for example, defined a paradigm as 'a world view, a general perspective, a way of breaking down the complexity of the real world' (p. 37). Then Patton added the following decidedly Kuhnian notions:

Paradigms are deeply embedded in the socialization of adherents and practitioners: paradigms tell them what is important, legitimate, and reasonable. Paradigms are normative, telling the practitioner what to do without the necessity of long existential or epistemological considerations. (1990/1980, p. 37)

Lincoln and Guba also appropriated not only Kuhn's term but also many of the fundamental ideas that Kuhn associated with the paradigm concept. In their discussions of what they referred to as the traditional *positivistic* paradigm and the emerging paradigm that they first labeled the *naturalistic* (Lincoln & Guba, 1985) and later called the *constructivist* paradigm (Guba, 1990; Guba & Lincoln, 1994), for example, Guba and Lincoln left no doubt that these two paradigms were incommensurable (Lincoln & Guba, 1985) and that the concept of incommensurability also entailed logical incompatibility. One could not be both a positivist and a naturalist/constructivist, Lincoln and Guba argued in 1985, and they reaffirmed this belief in their chapter for the 2000 edition of the *Handbook of qualitative research* (Lincoln & Guba, 2000). In short, although qualitative and quantitative methods might occasionally be mixed, one could not simultaneously embrace the positivist paradigm, on the one hand, and the naturalistic/constructivist paradigm, on the other, without becoming an intellectual schizophrenic, according to Lincoln and Guba.

In their 1985 book, *Naturalistic inquiry*, Lincoln and Guba also assumed that the sort of convergence around a new paradigm that Kuhn had observed in the physical sciences would also eventually occur in the field of educational research, and they continued to articulate this belief for an extended period of time. As late as 1994, in fact, in their chapter for the first edition of the *Handbook of qualitative research*, Guba and Lincoln alluded to 'a resolution of paradigm differences ... [that] can occur when a new paradigm emerges that is more informed and sophisticated than any existing one' (p. 116). In their chapter on paradigms for the 2000 edition of the *Handbook*, however, Lincoln and Guba did signal that they had finally abandoned the Kuhnian-inspired idea that our field would eventually converge around a single paradigm.

*The re-definers.* Other prominent figures in the educational research field appropriated Kuhn's term but, from the start, interpreted its meaning much more liberally and in ways that were, at times, somewhat antithetical to Kuhn's description of paradigms and how they functioned in the physical sciences. Lee Shulman (1986), for example, employed what he referred to as 'a second, weaker sense of paradigm' (p. 5), a sense that equated paradigms with researchers' different research programs.

Shulman's conception of paradigms actually owed more to N. L. Gage (1963, 1989) than to Thomas Kuhn. Like Shulman, Gage also had appropriated Kuhn's construct but radically redefined its meaning. Both Shulman and Gage, in fact, jettisoned Kuhn's notion of paradigm incommensurability and argued, instead, that paradigms were complementary. At one point, for instance, Shulman compared researchers working with different paradigm to blind men (and presumably women) studying different parts of an elephant. When they pooled the information they had gathered, according to Shulman, they would have a more complete picture of the phenomenon each had been studying rather than incommensurable perspectives. Shulman also compared the results of particular research programs to puzzle pieces that presumably can be arranged to form a coherent (if not totally complete) picture

of the phenomenon being studied. (For Shulman and Gage, of course, the phenomenon being studied was teaching.)

There will be more on Shulman and Gage's decidedly non-Kuhnian conception of *paradigm* below. Here it is sufficient simply to reference their decidedly non-Kuhnian conception of Kuhn's construct to demonstrate that, even early on, some educational researchers used Kuhn's construct but attached very different meanings to it.

### *Paradigm talk in popular culture*

As noted above, over the years Kuhn's construct also has been appropriated by popular culture. An article several years ago in the *New York Times*, for instance, quotes Sean Dee, a Levi Corporation executive, as saying that 'loose jeans is [*sic*] not a fad; it's a *paradigm shift*' (Epsen, 1999, p. 56, emphasis added). The author of another 1999 article appropriates Kuhn's construct to characterize the public's shifting views of pasta:

Thirty years ago ... the *paradigm of spaghetti and macaroni* ... was that 'you'd feed it to the kids, you may feed it to your husband, but you would never feed it to guests,' this author writes; 'Nowadays ... the *paradigm* is that 'you can prepare a pasta dish 365 days a year and never repeat a recipe.' (Dochat, 1999, p. 77, emphasis added)

Politicians also have discovered Kuhn's construct. In the 1990s, for instance, a United States senator made the following comments in an interview for the Public Broadcasting System's program, 'Children in America's School with Bill Moyers':

Particularly in this global economy, it is more and more important that we prepare the American workforce, as a matter of our national defense if you will. That means *changing the paradigm* so that there is a partnership between state, federal and local so that everybody chips in. (Moseley-Braun, quoted in Hayden & Cauthen, 1996, emphasis added)

Obviously, Kuhn's paradigm concept got altered somewhat in the process of being transported from the context of research to the context of real-world activity. Indeed, in popular culture, the paradigm notion often is invoked not simply for its descriptive potential but because of its public relations cachet: The term, for example, has been used to add gravity and significance to the most mundane matters; the author of the article about changing blue jeans preferences alluded to above, for instance, dubs the Levi executive's talk of a paradigm shift 'memorable' (Epsen, 1999, p. 56).

Furthermore, in a modernist culture that uncritically accepts progress as a virtue, talk of a new paradigm also can suggest—in lieu of thoughtful argumentation and/or convincing evidence—that new ways of thinking and acting are not only radically different from older ways but also superior to them. The United States senator's comments presented above demonstrate this latter function of popular-culture paradigm talk.

### *Paradigm talk in contemporary educational research*

Popular culture's embrace of the paradigm notion does not mean that educational researchers have stopped using Kuhn's concept. To the contrary, paradigm talk is very much a part of contemporary researchers' discourse.

The latest edition of the American Educational Research Association's *Handbook of research on educational administration* (Murphy & Seashore Louis, 1999), for example, includes a number of chapters built around the paradigm notion, including one by Hill and Guthrie entitled 'A New Research Paradigm for Understanding (and Improving) Twenty-First Century Schooling.'

In fact, what we see today is a proliferation both of paradigm talk and of paradigms. For instance, as noted above, Lincoln and Guba (1985) at first spoke of only two paradigms. By 1994, however, they had added two additional paradigms to their list, and one of the additions, the critical theory paradigm, was really, according to Guba and Lincoln:

a blanket term denoting a set of several alternative paradigms, including additionally (but not limited to) neo-Marxism, feminism, materialism, and participatory inquiry. Indeed critical theory may itself usefully be divided into three substrands: poststructuralism, post-modernism, and a blending of the two. (Guba & Lincoln, 1994, p. 109)

Others slice up the field somewhat differently. Lather (1992), for example, differentiates between deconstruction-oriented paradigms such as poststructuralism and postmodernism and emancipation-oriented paradigms such as critical theory and neo-Marxism. There is general agreement, however, that a proliferation of paradigms has, indeed, occurred and that, consequently, there is a need for some sort of meta-paradigms to make sense of the resulting complexity. This complexity undoubtedly will only increase now that scholars have begun associating the paradigm notion with life experience and the different sorts of knowledge produced by such things as ethnicity (see, for example, Stanfield, 1994; Scheurich and Young, 1997), gender (see, for example, Dillard, 1997), and sexual orientation (see, for example, Pinar, 1998).

The existence—and the likely persistence—of paradigm proliferation in the field represents a significant departure from Kuhn's paradigm revolution storyline in which a field eventually converges around a single perspective. As noted above, even Lincoln and Guba saw the writing on the wall and, in time, abandoned this Kuhnian notion.

Acknowledging that the educational research field will not, in time, converge around a single paradigm is not the only difference between Kuhn's physical science-based thinking and the thinking of contemporary academics who still use his construct, however. Business executives and politicians, for instance, are not the only ones who can recognize the public relations potential of paradigm talk; consequently, the meanings some contemporary researchers attach to the term *paradigm* seem closer to popular culture meanings than to Kuhn's original conception. This is certainly the case, for example, in the Hill and Guthrie *Handbook* chapter mentioned above, and the public relations cachet of Kuhn's construct undoubtedly has helped fuel other examples of the paradigm proliferation phenomenon as well. An academician's perspective, after all, does seem inherently more interesting and much more important if it is billed not simply as a perspective but rather as a new and different (and presumably better) paradigm.<sup>1</sup>

*The more meanings change the more they remain the same*

Even when the meaning of a term changes due to the passage of time or because it is being used in a different context for a somewhat different purpose, a residue of the original meaning almost always remains. This is certainly the case with Kuhn's construct. Even in popular culture paradigm talk, for example, we have some semblance of Kuhn's notion of incommensurability. One cannot, for example, simultaneously embrace both the old and the new pasta paradigms mentioned in the quote above, nor can one embrace both the old and the new paradigms of blue jeans.

One also often can detect similar assumptions about incommensurability in contemporary paradigm talk within the academy. Stanfield (1994), for example, criticizes self-labeled Afrocentric scholars who claim 'to be producing knowledge sensitive to the experiences of African-descent peoples as a unique cultural population even as they insist on using Eurocentric logics of inquiry' (p. 182), and calls, instead, for the development of 'indigenous ethnic models of qualitative research [that reflect] novel indigenous paradigms grounded distinctly in the experiences of people of color' (p. 183).

Similarly, some contemporary advocates of teacher action research (see, for example, Anderson & Herr, 1999 and Cochran-Smith & Lytle, 1998) invoke Schon's notion of a distinct epistemology of practice that is, at base, incommensurable with the epistemology that undergirds the theory-oriented knowledge generated in the academy. Cochran-Smith and Lytle, for instance, respond quite strongly to rather mild and largely sympathetic critiques of teacher action research by two other scholars because they believe both authors' commentaries are rooted in a university-based—and, consequently, according to Cochran-Smith and Lytle, a totally inappropriate and, in fact, irrelevant—epistemological perspective.

Finally, Lincoln and Guba (2000)—arguably the most influential proponents of the idea that the educational research field's embrace of qualitative methods signals a Kuhnian paradigm shift—have recently reaffirmed their commitment to Kuhn's incommensurability notion. To be sure, they do blur the lines separating the various non-positivist paradigms they catalogue and, in fact, argue that 'there is a great potential for interweaving viewpoints' (p. 167); they also make it clear, however, that this interweaving cannot occur across the great divide that separates what they characterize as positivist and non-positivist worldviews. Just as in the past, one must choose sides; one still cannot embrace both positivism and non-positivist perspectives, according to Lincoln and Guba, at least.

In addition to retaining at least a residue of the Kuhnian notion of incommensurability, contemporary paradigm talk—whether in popular culture or in the research community—more often than not also retains a Kuhnian faith in progress and a belief that new, more superior paradigms eventually will trump older, existing one. This thinking is hardly surprising—even though the community as a whole has jettisoned the notion of the field converging around a single perspective—since one can hardly champion a new perspective one considers incommensurable with existing ones without assuming one's new perspective is not merely new but also improved. Consequently,

even advocates of the so-called postmodern paradigm often seem to imply that the postmodern paradigm is an improvement over the paradigms of the past, despite postmodernism's rejection of linear views of history and the very idea of progress (Maxcy, 1994).

Thus, to the extent that contemporary educational researchers continue to use Kuhn's paradigm notion—even in a modified way—some of Kuhn's original thinking impacts their thinking and structures how they understand both their work and the work of others. The fact that Kuhn's construct is, today, so widely accepted and used—not only within the academy but also within popular culture—suggests the need to stand back and critically examine the paradigm construct and its applicability to the educational research field. The remainder of this article does just that.

Before examining the appropriateness of using Kuhn's construct to characterize the current state of affairs in our field, it makes sense to address a more basic historical question: Was it *ever* appropriate to employ Kuhn's construct in the educational research field?

#### *Was the qualitative revolution a Kuhnian paradigm shift?*

The answer to the question posed in the heading for this section is a qualified yes. A close look at the rationales used to justify certain qualitative approaches reveals thinking that is, indeed, incommensurable with the thinking that traditionally had shaped research in the education field.

#### *Traditional methods and the thinking they reflected*

Prior to the 1970s, most educational researchers thought of knowledge as being essentially discovered rather than something researchers construct. This was certainly the case for E. L. Thorndike, one of the founding fathers of the discipline of educational psychology, the discipline that dominated the educational research field throughout the first three-quarters of the twentieth century. In 1910, for example, Thorndike wrote the following in the lead article for the first issue of *The Journal of Educational Psychology*:

A complete science of psychology would tell every fact about everyone's intellect and character and behavior, would tell the cause of every change in human nature, would tell the result which every educational force—every act of every person that changed any other or the agent himself—would have. It would aid us to use human beings for the world's welfare with the same surety of the result that we now have when we use falling bodies or chemical elements. In proportion as we get such a science we shall become masters of our own souls as we are now masters of heat and light. Progress toward such a science is being made. (1910, p. 6)

There is abundant evidence that the educational research community's discovery orientation did not disappear with Thorndike. One especially compelling piece of evidence is the influential first edition of the American Educational Research Association's *Handbook of research on teaching* published in the 1960s. The editor of that



handbook, N. L. Gage, asked all contributors to accept—and write their chapters from the perspective of—a single definition of research on teaching:

Research on teaching is aimed at the identification and measurement of variables in the behavior and characteristics of teachers, at *discovering* the antecedents or determiners of these central variables, and at revealing the consequences or effect of these variables. (Gage, 1963, p. vi, emphasis added)

The first edition *Handbook* authors complied with Gage's request, and, in fact, the so-called process-product paradigm that the *Handbook* authors took to be the only reasonable way to study teaching continued to dominate the field well into the 1970s (Good *et al.*, 1975).

Qualitative research methods, incidentally, were not mentioned in the first edition of the *Handbook*, despite the book's considerable attention to methodological issues. One of the chapters in the first edition of the *Handbook*—Campbell and Stanley's (1963) 'Experimental and quasi-experimental designs for teaching'—which became highly influential in a number of social sciences fields when reprinted as a monograph—suggests a reason for the omission and also provides additional evidence of the field's discovery orientation. In their chapter, Campbell and Stanley cavalierly dismiss the utility of any sort of case study research (except, perhaps, for the purposes of hypothesis or theory generation) because, they remind us, *N* of 1 studies, by definition, cannot guarantee external validity or generalizability. Only when we have a large enough sample to statistically represent the population we are studying, Campbell and Stanley state, can we be sure that we have *discovered* the sort of objective knowledge educational researchers from Thorndike onward have promised to provide.

Subsequent editions of the *Handbook* provide additional evidence of the field's commitment to research-as-discovery and to the statistical methods that, presumably, would help researchers discover truly objective knowledge. For example, qualitative methods also were not discussed in the second edition of the *Handbook* that appeared in the 1970s (Travers, 1973). The second edition did contain an article that had the words *qualitative data* in the title—Light's (1973) 'Issues in the analysis of qualitative data'—but this chapter focused on the statistical analysis of nominal data.

#### *The emergence of qualitative methods and the incommensurable thinking they represented*

Discussions of qualitative methods were finally included in the third edition of the American Educational Research Association's *Handbook of research on teaching* published in 1986. Including such discussions was a virtual necessity, of course, because, by the mid-1980s, qualitative research had begun to be widely used and widely accepted, not only in the study of teaching but also in the educational research and evaluation fields in general.

The 1980s, of course, was also the era of the so-called 'the paradigm wars' (Gage, 1989), a time when members of the educational research community debated whether qualitative procedures merely added to researchers' methodological arsenal or

whether their use represented the advent of a new (and incommensurable) paradigm in the educational research field. Not surprisingly, elements of this ongoing disagreement also found their way onto the pages of the third edition of the *Handbook of research on teaching*. This is where Lee Shulman articulated his self-described ‘weaker’ conception of *paradigm*, for example. It is also the forum, however, in which Fred Erickson, the author of one of two *Handbook* chapters focused on qualitative methods, challenged the authors of the other qualitative-oriented chapter, Carolyn Evertson and Judith Green, for failing to recognize paradigmatic differences.<sup>2</sup>

*Erickson’s argument and its significance.* Erickson’s argument—and, equally important, Gage’s later critique of it—provides additional evidence that the early Kuhnian-inspired paradigm talk was, indeed, appropriate. In his *Handbook* chapter, Erickson noted that Evertson and Green’s ‘comprehensive review of a wide range of methods of classroom observation does not emphasize the discontinuities in theoretical presupposition that obtain across the major types of approaches to classroom research, positivist/behaviorist and interpretive.’ He added: ‘Green and Evertson are relatively optimistic about the possibility of combining disparate methods and orientation in classroom observation. I am more pessimistic about the possibility, and have become increasingly so in the last few years’ (1986, p. 120).

Erickson’s pessimism and his comments about ‘discontinuities in theoretical presuppositions’ were rooted in his commitment to a symbolic interactionist approach to research (Blumer, 1969), an approach that conceptualizes human action in decidedly different ways than the cause–effect ways of thinking that were at the center of Gage’s so-called process–product paradigm. Symbolic interactionism assumes human beings act on the basis of the meanings they attribute to phenomena and that these meanings are constructed—and constantly reconstructed—during the course of human interaction. Because symbolic interactionists assume that humanly constructed meanings are not static but rather are always in the process of being socially negotiated (and renegotiated), it does not make sense to those working within a symbolic interactionist framework to treat meanings as causes or even as intervening variables.

Nor is it appropriate to think of the processes and structures that particular groups of people use to construct and reconstruct meaning (and which symbolic interactionists do attempt to describe) as *causing* certain meanings and producing certain actions. To be sure, these processes do constrain human action, and, consequently, help make human behavior at least somewhat predictable and patterned, but the processes and structures used to construct and reconstruct human action can, themselves, be changed by those engaged in the construction process. Consequently, according to the symbolic interactionist credo, it would be inaccurate to think of these processes as causing events. To state the point succinctly: From a symbolic interactionist perspective, meaning is constructed (and constantly reconstructed) not caused.

Now, this difference between the meaning-oriented thinking of symbolic interactionists, on the one hand, and the cause/effect thinking of process product researchers

(and the entire Thorndike tradition, for that matter), on the other, is not merely an arcane, academic one. Rather, the differing perspectives engender fundamentally different ways of thinking about both teaching and policy-making. When teaching and learning are viewed through a symbolic interactionist lens, for instance, teaching becomes an interactive, and, consequently, a somewhat improvisational process that can never be totally preplanned, even by the classroom teacher.

Furthermore, teaching certainly cannot be choreographed in advance of student–teacher interaction in a state capital or a publishing house—or even in a school system’s district office—when we conceptualize teaching in symbolic interactionist terms. Rather, what teachers do at any given moment is seen as being inextricably linked with—and directly responsive to—what students have just said and done. The teacher, in other words, becomes as much a learner as a teacher and teaching and learning become a tango-like process of co-constructing meaning. State policy-makers and/or textbook publishers might set the stage for the ongoing improvisational process of teaching and learning, but, ultimately, when one thinks of teaching and learning from a symbolic interactionist perspective, the really important action is at the classroom level where the ongoing process of meaning negotiation occurs.

A symbolic interactionist-inspired view of teaching and learning, in short, is radically different from—indeed, it would be fair to say, *incommensurable* with—the view of teaching and learning that undergirds the thinking of Thorndike, Gage and others who attempted to discover cause-and-effect relationships between teaching processes and students’ learning products. Consequently, in this respect, at least, we do seem to have something akin to one of Kuhn’s paradigm disputes and it does, indeed, seem justifiable to use both Kuhn’s terminology and much of the meaning Kuhn associates with his term to characterize the differences between Erickson, on the one hand, and researchers like Thorndike and Gage, on the other.<sup>3</sup>

*Gage’s critique of Erickson and its significance.* Gage’s critique of Erickson’s chapter provides an additional pragmatic justification for the early Kuhnian-inspired paradigm talk within the educational research field by demonstrating what can happen when one accepts a ‘weaker’ conception of *paradigm* that jettisons Kuhn’s notion of incommensurability and replaces it with the assumption that different ‘paradigms’ produce complementary knowledge. In his critique, Gage simply appropriated Erickson’s constructivist-oriented interpretive perspective into Gage’s own process–product paradigm, a paradigm that assumes, a priori, that teaching and learning should be defined in terms of cause-and-effect relationships. Thus, for Gage, the sole function of Erickson’s interpretive methods was to identify intervening variables between teaching processes and the achievement products of students. Such inappropriate appropriation, of course, obscures not only the constructivist conception of teaching and learning that is at the center of an interpretivist conception of social action but also Erickson’s emphasis on the importance ‘of local meaning and local action’ (Erickson, 1986, p. 156) and the virtual imperative to assign significant educational decisions to those who work at the classroom level.

*Summary.* Erickson's *Handbook of research on teaching* chapter is only one example of the wide variety of constructivist thinking that was beginning to challenge traditional conception of research during the late 1970s and the 1980s. Single cases often can be used to encapsulate more general phenomena and issues, however, and this is certainly the situation here. The Erickson chapter demonstrates quite clearly that the newer constructivist thinking was, indeed, essentially incommensurable with the cause-effect thinking that had dominated the field in the past, and, consequently, that the qualitative revolution within the field did represent, at least in some instances, something akin to one of Kuhn's paradigm revolutions. Furthermore, Gage's misunderstanding of what Erickson was saying and his inappropriate appropriation of Erickson's symbolic interactionist-inspired conception of teaching and learning into a process-product conception of research illustrates the problems that can arise when scholars simply appropriate Kuhn's terminology and attach a different meaning to it.

### *Two caveats*

Of course, Education is not Physics, or, for that matter, any of the other natural science disciplines that were the focus of Kuhn's work. Consequently, an endorsement of the use of Kuhn's construct to characterize what was happening in the educational research field during the final quarter of the twentieth century requires some important caveats. Two of the most important are discussed briefly below.

First, as philosopher Richard Bernstein (1992) has noted, incommensurability is not the same thing as logical incompatibility. It may indeed be the case, as Kuhn suggests, that 'the scientist does not [normally] preserve the gestalt subject's freedom to switch back and forth between ways of seeing' (p. 85), but there is no logical reason why this cannot happen. In short, one could conceivably employ different paradigms in different circumstances and/or to accomplish different goals. After all, even cutting-edge nuclear physicists, who long ago rejected the Newtonian vision of a mechanistic, cause-and-effect universe, normally think in causal terms when they leave their linear accelerators and put their feet on the accelerators of their cars.

Interestingly, Bernstein (1992) argues that Kuhn himself never equated incommensurability with logical incompatibility. Bernstein also notes, however, that many of Kuhn's disciples treated the two concepts as synonyms. This certainly happened when the paradigm notion was utilized in the educational research field (see, for example, Lincoln & Guba, 1985), and, consequently, this first caveat is quite relevant here.

This first caveat sets the stage for a discussion of a second one: The sort of paradigmatic convergence that Kuhn indicates is the normal state of affairs in the physical sciences (and the eventual outcome of any paradigm revolution) would actually be quite dysfunctional in a field like Education. Education, after all, is a public policy field, and public policy fields require ongoing consideration and balancing of a wide range of perspectives and options, some of which almost certainly will be incommensurable with others.

The philosopher Stephen Toulmin (1972) uses an example to demonstrate how policy work differs from work done even in an applied academic discipline such as

civil engineering. He notes that a policy board whose members must decide where to build a dam will not necessarily decide to build the dam in the place that a group of civil engineers decides is the optimal spot. The policy board cannot totally ignore the advice of the engineers, of course—it cannot build the dam where it will collapse, in other words—but the civil engineer’s perspective is not the only perspective that the policy board must factor into its decision.

The board, for instance, almost certainly would also need to consider the economic impact of building the dam in the engineers’ optimal location as opposed to a less optimal but still adequate one, as well as social costs if building the dam in the engineers’ optimal site meant dislocating a substantial segment of the community. There might also be blatantly political considerations to factor into the choice, if, for example, the engineer’s recommendation was unpopular with a large segment of the population that would be voting in board members’ re-election campaigns.

These very different considerations could be thought of as emerging from very different paradigmatic perspectives of the problem to be addressed, and the formal research that members of the policy board might consult in the course of making their decision also should reflect a similar range of perspectives and support a wide array of options to be taken into account by board members. Some of these options may end up being complementary, as Shulman and Gage assume. But it is also the case that the options that appear when different paradigmatic lenses are employed will, at times, be incommensurable with each other.

The functional nature of such incommensurability for the Education field can be demonstrated by focusing on an intriguing feature of a 1989 publication of the American Association for the Advancement of Teacher Education entitled *Knowledge base for the beginning teacher* (Reynolds, 1989). The book contains two chapters on classroom management and discipline, one written by Carolyn Evertson (1989) and the other authored by Susan Florio-Ruane (1989), and each chapter articulates an approach to classroom management and discipline that is essentially antithetical to the approach recommended in the other chapter.

Carolyn Evertson’s (1989) recommendations, for instance, are based on a process-product conception of teaching and the empirical research on teacher effectiveness that conception generated (and influenced). That conception, of course, assumes that learning is caused by teachers’ behaviors and that, consequently, teachers must be in total control of their classrooms. Consequently, Evertson’s chapter details strategies that teachers can use to bolster their authority over their students.

Susan Florio-Ruane’s (1989) recommendations, on the other hand, are grounded in a sociolinguistic knowledge base that, like Erickson’s interpretivist theoretical perspective, assumes that human understanding is constructed through social interaction more than caused by a teacher or some other external force. Thus Florio-Ruane suggests that ‘teachers need to learn to mitigate aspects of their authority’ because ‘learners who are subordinates cannot participate in many of the activities and forms of discourse that would lead to genuine education’ (Florio-Ruane, 1989, p. 166).

At a practical level, at least, Evertson’s and Florio-Ruane’s recommendations are incommensurable: a teacher cannot at any given moment simultaneously attempt to

bolster his or her authority while also undercutting it. Yet, it is certainly possible for a teacher to understand and appreciate the logic behind each set of recommendations and to use such understanding to decide what to do at any particular point in time. For instance, a beginning teacher might accept Florio-Ruane's picture of a classroom in which the teacher manages to undercut his or her authority as an ideal to aspire to long term, even as he or she employs Evertson's control strategies to survive the invariably difficult first few years in the classroom.

In short, convergence around a single paradigm makes sense when one is talking about an academic discipline whose members tend to operate within an ideal typical universe created and sanctioned by members of a particular scientific community. It is not a viable strategy for public policies fields, including the field of Education, however. In such fields, decision-makers must consider a variety of perspectives, some of which will almost certainly be antithetical, and either find a way to balance them or choose the perspective or combination of perspectives that are appropriate for a particular situation or a particular point in time. In public policy fields, in other words, paradigm convergence is neither possible nor desirable.

### *Summary*

If we keep in mind certain caveats, in particular the two articulated above, it does, indeed, seem appropriate to use Kuhn's term—and much of the meaning Kuhn attached to it—to characterize the methodological revolution that was occurring within the Education field in the 1970s and '80s. Many—though certainly not all (see, for example, the work of those self-described 'soft-nosed positivists', Miles & Huberman, 1984, 1994/1984)—champions of qualitative methodology did indeed operate from a fundamentally different worldview than the one more traditional researchers embraced, and this new worldview could not simply be appropriated into traditional thinking as Gage attempted to do with Erickson's symbolic-interactionist-inspired interpretivist approach to research.

On the other hand, recognizing and acknowledging an incommensurable perspective does not require that we totally reject our original thinking. Each perspective might be useful to accomplish different purposes, and, at the very least, multiple perspectives can make us aware of different options available to us. In decision-oriented public policy fields, one must consider issues from a range of paradigmatic perspectives precisely because the perspectives are incommensurable and, hence, what can be seen by examining the world through one perspective or paradigm may not be visible from the vantage point provided by another. One need not be a schizophrenic to do this, however, because incommensurability is not the same thing as logical incompatibility.

### **Is paradigm talk appropriate today?**

Let me turn now to the second question posed at the outset of this paper and ask whether it is *still* appropriate to characterize methodological differences in paradigmatic

terms. Here a relatively simple answer—even the sort of qualified answer offered in the previous section—is not so easy to come by.

*The original response*

To be sure, in an earlier draft of this paper—as well as in some other published work (Donmoyer, 2002, 1999a, 1999b)—I did provide a rather glib response to the question articulated in the heading for this section. My answer was a qualified no: We are virtually all constructivists now, I argued, so, in essence, at the epistemological level, the paradigm wars have been won by those who embraced naturalist/constructivist/interpretivist thinking. Even researchers who hold onto the notion of objectivity, I argued, normally understand that the concept is only a regulative ideal, not something that can actually be attained. And those who continue to frame their work and discourse in cause–effect rather than constructivist terms also, at some level, understand that they may be employing a functional fiction rather than a description of reality. After all, once we accept the proposition that reality is socially constructed, it makes little sense to talk about the *true* nature of reality. Epistemology trumps ontology, in other words, and the best we can do, once we have embraced a constructivist view of knowledge, is to base our decisions on utilitarian rather than on realist grounds.

I even used a rather old quote from philosopher of social science Peter Cohen, to illustrate the need for utilitarian thinking: Cohen (1968), in responding to the interpretivist/constructivist conception of social action articulated by Peter Winch in his classic book, *The idea of a social science*, wrote:

One would agree that the use of the term ‘causation’ does not have as precise a meaning in the social world as it does in the natural world. But if one is to use such criteria, one wonders what is to be offered in place of ‘causation’.... In fact, one begins to wonder how social policy would be possible without some idea of causation. (1990/1958, p. 416)

To be sure, in the earlier drafts of this paper I did qualify the argument I was making a bit. I noted, for example, that it takes time for members of fields of study to totally accommodate new ways of thinking and to realize all the implications of the new rhetoric they espouse. Hence, acceptance of constructivist rhetoric is not always tantamount to a complete shift in paradigmatic thinking and acting. I also acknowledged that some honest-to-goodness paradigmatic differences continued to exist in our field; Russell Bishop’s (1998) Maroi-inspired approach to knowledge generation was cited as an example.

Still, I argued that our field’s nearly universal embrace of a constructivist conception of knowledge by the end of the twentieth century opened the door for at least considering abandoning the strategy of characterizing most differences among educational researchers in paradigmatic terms; I then went on to argue that there were compelling pragmatic reasons (e.g. minimizing balkanization and unblocking conversations among those who approach research in fundamentally different ways) for abandoning rhetoric that assumed, explicitly or, at least, implicitly, that those who approached

research in fundamentally different ways inhabited totally different intellectual worlds and, hence, had nothing to say to each other. Such assumptions, I argued, were especially problematic in the current era characterized by paradigm proliferation and the balkanization of the field that is an inevitable by-product of such proliferation.

Before concluding this discussion of my earlier position, I should note that I also made it clear in earlier drafts of this paper that my argument for jettisoning the field's Kuhnian-inspired notion of *paradigms* should in no way be viewed as an endorsement of Shulman's 'weaker' conception of the term or his and Gage's assumption that different research orientations would inevitably produce knowledge that is complementary. Indeed, I suggested that there continued to be profound differences among different groups of educational researchers, but I also suggested that it would be more helpful to characterize these differences in some other way than the sort of Kuhnian-inspired paradigm talk that had contributed to an extreme form of balkanization and a form of big-tent politics (see Donmoyer, 1999a, b) that is highly problematic for a public policy field. Rather than talking in terms of different paradigms and continuing to carry the balkanization-oriented baggage associated with the use of Kuhn's construct in our field, I suggested that it might be more helpful to characterize our differences in terms of differing purposes, which presumably could be at least understood and, consequently, debated by those who approached research in radically different ways. I even provided a catalog of the meta-purposes I saw when I surveyed

Table 1. Five meta-purposes undergirding current qualitative research

Purpose	Fundamental questions	Exemplars
The 'truth' seeking purpose	What is the correct answer (given a particular way of framing the question)? What is the relative effectiveness of a program, person, strategy (given a particular definition of effectiveness)?	Etic-oriented anthropologists; grounded theory-oriented sociologists; theory testers
The thick description purpose	How do the people studied interpret phenomena? What meanings do they attach to events? What processes and structures do they use to construct meaning?	Emic-oriented anthropologists; symbolic interactionist sociologists
The developmental purpose	How does an individual, group of individuals or organization change over time?	Quasi-historical researchers such as Smith (1982) and stage theory-oriented researchers such as Huberman (1989)
The personal essay purpose	What is the researcher's personal, idiosyncratic interpretation of a situation; what unique and useful meaning can the researcher construct from a situation he or she has studied?	Eisner's (1998) educational connoisseurs; Lawrence-Lightfoot's (1983) 'portrait' makers
The social change purpose	How can the researcher simultaneously learn about and change individuals or an organization?	Practitioners of collaborative action research and praxis-oriented inquiry



the field to illustrate that there was, indeed, an alternative to both Kuhnian-inspired paradigm talk, on the one hand, and Shulman's 'weaker' version of paradigm talk, on the other. These meta-purposes are summarized in Table 1.

### *Changing circumstances*

Since I first articulated the above argument, however, the context in which qualitative researchers must operate has changed rather dramatically. Recent pronouncements by the head of the Institute of Education Sciences (formerly the Office of Educational Research and Improvement), the federal governmental agency that funds educational research, and the contents of a report from the National Research Council (NRC) of the National Academy of Sciences entitled *Scientific research in education* (2002) both suggest that my claim that the epistemological battle had been won, at least at the rhetorical level, is akin to President Bush's claim, in his aircraft carrier appearance, that the United States' mission in Iraq had been accomplished.

Grover J. Whitehurst, the new head of the Institute of Education Sciences, for instance, declared, in an invited address at the 2003 Annual Meeting of the American Educational Research Association, that studies that employed randomized trials (i.e. studies that use control and experimental groups selected by chance) are the new 'gold standard' in educational inquiry and that, in the future, the bulk of federal research dollars earmarked for Education will be used to support such work. Such studies, Whitehurst declared, would be used to determine 'what works' and this knowledge, in turn, would be used to develop replicable educational programs that could be implemented throughout the country. (Whitehurst also noted that the federal government intends to use its clout to influence even educational inquiry it does not support financially by establishing a list of federally approved educational evaluators. To be included on the list, an evaluator must commit to using randomized trials in his or her evaluation work.)

There was no indication in Whitehurst's address that he understood that the cause-effect thinking he was promoting was merely a useful fiction in a world that has come to embrace constructivist views of learning and knowledge. Nor was there any acknowledgement of the dismal failures of past attempts to accomplish the sort of goals Whitehurst indicated his agency was committed to pursuing. These attempts included not only the work of Thorndike and his progressive colleagues and the relatively well-financed process/product studies of teaching, but also federally funded 'planned variation studies' that the federal government promoted and funded in the 1970s.

The best known planned variation study in the Education field, for example, is Project Follow-Through (Abt Associates, 1977). This study was conducted to determine which of a number of different approaches to early childhood education (e.g. a basic skills approach, an affective education approach, etc.) was most effective in promoting learning in young children. After the results of this study were made public, a team composed of some of the field's most respected scholars assembled to review the study and in its results wrote: 'The peculiarities of individual teachers,

schools, neighborhoods, and homes influence pupils' achievement far more than whatever is captured by labels such as *basic skills* or *affective education*' (House *et al.*, 1978). The results of planned variation studies in Education, in fact, were so inconclusive and so problematic that policy analysts who had initially promoted the use of planned variation studies (Rivlin, 1971) quickly reversed their position and suggested that such studies should not be used in highly complex fields such as Education (Rivlin, 1973; Rivlin & Timpane, 1975). I should add that the problems that were so evident in the Project Follow-Through results could not have been fixed by randomly assigning students to different programs; if anything, the study demonstrates the logistical problems associated with using the randomization strategy on a large scale.

All of this history seems to have been lost on Whitehurst. Indeed, in an interesting stroke of irony, Whitehurst subtitled his AERA address 'New Wine in New Bottles.'

The 2002 NRC publication, *Scientific research in education*, articulates somewhat more sophisticated thinking about educational research and what it can contribute. The report's authors, for instance, endorse Whitehurst's randomization strategies under certain circumstances, but they also acknowledge some significant difficulties with the randomized trial strategy. They also note that one of the giants of the field, Lee Cronbach—the scholar to whom the NRC report is dedicated—believed the strategy could not and should not be used in the complex field of Education.

The authors of the NRC report also are considerably more circumspect than Dr Whitehurst is about what empirical evidence, in and of itself, can tell us. Indeed, they emphasize that theory must be a mediating factor between empirical research and policy recommendations, that reasoning as well as empirical evidence must be employed to choose between rival theories, and that such judgments must be made over an extended period of time by members of the scientific community.

Yet alongside all their qualifications, and embedded within their seemingly more nuanced line of argument, the authors of *Scientific research in education* also make the following decidedly un-nuanced statement:

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

This statement, which is reiterated elsewhere in the book, seems problematic for a number of reasons. First, the statement seems to equate validity with reliability. An *argumentum ad absurdum* can demonstrate the problem here: Just because multiple neo-Nazis 'can agree on what they see' when they observe members of minority groups, this does not necessarily mean they have generated 'objective or trustworthy knowledge.'

Second, the NRC committee's cavalier dismissal of the often-articulated critiques of the traditional conception of objectivity that can be found in a diverse array of scholarly literatures seems to conflict not only with the committee's charge to review and synthesize 'recent literature on the science and practice of educational research,' but also with the norms endorsed by the National Academy of Sciences and the

committee itself. The President of the National Academy of Sciences, in his foreword to *Scientific research in education*, for instance, claims that since ‘the report itself [is] a product of scientific work, it had to live up to its own depiction of what constitutes good science,’ and the authors of *Scientific research in education* indicate that one of the defining characteristics of *scientific* discourse is ‘systematically ruling out plausible counter-explanations in a rational, compelling way’ (2002, p. 4). The NRC report’s cavalier dismissal of the critiques of scientific objectivity was neither rational nor compelling. Indeed it consisted of nothing more than stating the committee’s belief in a highly contested assumption and a bit of name calling. (Not all critics of the traditional conception of scientific objectivity and value-free knowledge, after all, can be classified as postmodernists.<sup>4</sup>)

Finally, and most importantly here, the NRC report’s ringing endorsement of the belief that research can produce objective knowledge suggests that I was wrong in earlier drafts of this paper: Traditional epistemological thinking has not been displaced, even in the academy, and, consequently, Kuhn-like paradigm differences are not a thing of the past even in the educational research community, much less in the policy arena. Indeed, it would be more correct to characterize the current situation as I did in the title of a paper I presented on the NRC report at the 2003 meeting of the American Educational Research Association: ‘The empire strikes back!’

*Yes, but....*

Having acknowledged all of the above, however, I still want to argue for abandoning our field’s Kuhnian-inspired paradigm talk on strategic and pragmatic grounds. I make this argument not in spite of recent events but because of them. After all, the cavalier dismissal by both the new head of the Institute of Education Sciences and the authors of *Scientific research in education* of an extensive, diverse and often quite thoughtful body of literature that challenges traditional thinking about objectivity is symptomatic of what invariably occurs whenever we accept the particular conception of incommensurability that is deeply embedded in and inextricably linked with the educational research field’s version of Kuhnian-inspired paradigm talk. Indeed, if we assume that those with whom we disagree inhabit a totally different universe that we cannot enter or even understand, there is really no incentive to consider their thinking, much less debate with them; cavalier dismissal is the only rational response for individuals, and balkanization is the only viable alternative for the field.

One certainly can see the cavalier dismissal and balkanization strategies on display in *Scientific research in education*. They also are visible in the authors’ responses to critics of the report. The two editors of the NRC report, along with Michael Feuer, for instance responded to Elizabeth St Pierre’s (2002) complaint that the authors of *Scientific research in education* had cavalierly dismissed postmodernist thinking by arguing that St Pierre should not have been surprised that ‘scientists would bristle at the formulation of postmodernism that explicitly rejects the rationality and utility of the scientific enterprise’ (Feuer *et al.*, 2002, pp. 25–27). Clearly what we have here is a pseudo-dialogue that will almost certainly go nowhere.

The irony is that there is no need to invoke the notion of postmodernism—or to use any other esoteric paradigm talk that will be cavalierly dismissed by more traditional

thinkers—to undermine the NRC report writers' position. One need only expose the NRC report's many contradictions to undermine its impact.

The report, in fact, is riddled with contradictions. At one point, for instance, the report authors sound like latter-day Thorndikes as they declare that 'the final court of appeal for the viability of a scientific hypothesis or conjecture is its empirical adequacy' (p. 3). Later in the report, however, they transform themselves into cutting-edge constructivists who seem to understand that empirical evidence, alone, can never provide definitive answers to policy-makers' 'what works?' question, and, consequently, that one must rely on the scientific community's judgment to assess the relative worth of conflicting claims.

Of course, even as they assign the scientific community a privileged position in the educational decision-making process, the authors of *Scientific research in education* also acknowledge that members of the academic community will undoubtedly come to radically different conclusions and make different—even contradictory—recommendations because they will inevitably frame their investigations in fundamentally different ways. Yet the authors of the report never challenge policy-makers' assumption that educational researchers can provide policy-makers and practitioners with definitive answers to policy-makers' 'what works?' question, and, in fact, the not-so-subtle subtext of the report is that researchers will be able to give policy-makers what they want if researchers receive adequate funding and are given enough time to build and validate theory.

Given the rather obvious contradictions on display in *Scientific research in education*, there is no need to invoke Derrida (1978) or to utilize the language of postmodernism—or any other language unique to a particular paradigmatic perspective—to undermine the report's logic. Here, at least, the master's own tools can, in fact, be used to dismantle the master's house. Furthermore, the critique that this strategy produces cannot be as cavalierly dismissed as critiques that are billed as coming from different (i.e. incommensurable) paradigms.

Of course, there are few if any contradictions in Whitehurst's pronouncements, largely because Whitehurst's position is, for the most part, free of any nuance or subtlety. Even Whitehurst can be hoisted on his own petard, however, by the very sort of empirical studies he lauds. The Project Follow-Through evaluation study discussed above is but one example.<sup>5</sup>

## Conclusion

In this article I have suggested that, as long as certain caveats are attached, it was, indeed, appropriate to have characterized the educational research field's embrace of qualitative methods during the 1970s and 1980s as something akin to Kuhn's paradigm revolutions. I have also argued, however, that it is now time to leave our hermetically sealed paradigmatic universes and engage with those in power in their own terms. I advanced this second argument even while acknowledging that fundamental epistemological disagreements continue to exist. My argument for abandoning paradigm talk, in short, is based on strategic and pragmatic considerations.

There are, in fact, a number of strategic and pragmatic reasons for moving beyond our field's penchant for paradigm talk. One reason is that we can. Even scholars who still hold traditional conceptions of inquiry often incorporate constructivist thinking into their rhetoric. Such incorporation produces some rather dramatic contradictions waiting to be exploited.

A second reason for attempting to actually exploit these contradictions (rather than cultivating our own isolated and heavily bordered paradigmatic gardens) involves nothing less than academic self-preservation and the continued acceptance of many types of qualitative work. In recent years, it has become obvious that those in power are no longer interested in playing big-tent politics (Donmoyer, 1999a, b) and now are intent on moving all but the most conservative forms of qualitative inquiry to the margins of the field.

The authors of *Scientific research in education*, for instance, explicitly write off Eisner's (1979, 1998) arts-based research and Lawrence-Lightfoot's (1983) portraiture approach as non-scientific (and use Eisner's and Lawrence-Lightfoot's own words to do so). Moreover, by making the search for general theory a defining characteristic of science, the authors of *Scientific research in education* also implicitly characterize the local knowledge and thick description-oriented perspectives of both Fred Erickson and the noted anthropologist, Clifford Geertz, as non-scientific, as well. Of course, Whitehurst and his colleagues at the federal government's Institute of Education Sciences have made it clear that the term *non-scientific* is, in fact, a synonym for non-fundable, at least in the federal context and in other contexts that federal officials are able to influence.

There is at least one more reason—arguably the most important reason—to move beyond paradigm talk and the balkanization such talk has tended to promote in our field: Education is, ultimately, a public policy field and, as noted in this paper, public policy fields require that issues be examined from multiple perspectives and decided by considering different, and, at times, even contradictory criteria. As long as members of the academic wing of the field believe that academicians inhabit different worlds and, consequently, that members of one group have nothing to say to or learn from members of other groups, the academic community as a whole will not have much to contribute to the solution of educational problems. Rather subgroups of academics will continue to research and write from their one-dimensional perspectives and policy-makers and practitioners will be free to pick and choose from among these perspectives to legitimize their own uni-dimensional thinking (Donmoyer, 2005).

I believe that research should be something more than a weapon and that members of the research community should be something more than the intellectual equivalent of arms dealers. Moving beyond the current state of affairs will require dramatic changes in the way members of our field do business: Different conceptions of inquiry will be required; new forms of academic discourse will have to emerge; and additional forums will have to be created to enact new conceptions of inquiry and engage in new forms of discourse. These are topics for other papers. Here I have proposed a very modest first step: moving beyond the Kuhnian-inspired paradigm talk that has served us well in the past but now has outlived its usefulness.

## Notes

1. A concern with public relations may not be just a recent phenomenon in the educational research field. One or the reviewers of this article speculated that educational researchers' *initial* interest in paradigm talk can be explained, in part, by physics envy. This reviewer noted: 'It was fun to point and say, "We must be important because look, this paradigm stuff happens in our field, too."'
2. Interestingly, Erickson, himself, seemed reluctant to employ Kuhn's term even though he clearly invoked Kuhn's incommensurability notion. Erickson's reticence to characterize the theoretical presuppositions he alluded to as paradigms may have been prompted by reading the Shulman chapter.
3. The paper in this issue by Peter Demerath suggests that Erickson has recently muddied the waters a bit by talking of 'local causality.' It seems obvious, however, that Erickson's use of such language represents nothing more than an attempt to make interpretive research palatable to policy-makers focused on the 'what works?' question.
4. In a footnote on page 25 of *Scientific research in education*, the authors acknowledge this fact, which makes their cavalier dismissal of challenges to traditional objectivity appear all the more a violation of the scientific norms to which the authors and the National Academy of Sciences claim to be committed.
5. Gene Glass, when asked about the federal government's push for randomized experiments in an interview published in the *Educational Researcher*, stated: 'I am sympathetic with the FDA's requirement for randomized clinical drug trials. But that hardly justifies the reliance on a single method for the verification or discovery of claims in the soft sciences. And where is the randomized experiment that proves that randomized experiments are the royal road to truth? There is none, of course.... If the federal government wishes to be consistent (a dubious assumption), then they will have to back off their policies on smoking, coal dust, speeding on the interstate highways, and a host of other things that have never been verified by randomized experiments' (Glass in Robinson, 2004, p. 26).

## Notes on contributor

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