In Defense of Positivist Research Paradigms

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Although positivism means different things to different people, it is not difficult to identify research paradigms that would be clearly identified as positivist by friends and foes of positivism alike. I briefly describe one such paradigm and identify its principal features. I argue that even outspoken critics of positivist research—such as Elliot Eisner, Frederick Erickson, Henry Giroux, and Thomas Popkewitz—are logically committed to propositions that can be tested only by means of positivist research paradigms.

In some academic circles, positivism is a living faith. In others, it is a dead creed whose remaining adherents are either naive science worshipers or political reactionaries (or both). The term has long been emptied of any precise denotation and, as Denis Phillips (1983) has shown, is sometimes affixed to positions actually opposed to those espoused by the philosophers from whom the name derives. Nevertheless, it is not difficult to identify research paradigms that would be clearly identified as positivist by friends and foes of positivism alike. Here I shall argue that even the most outspoken critics of positivist research—insofar as they wish their work to enhance the education of children—are logically committed to propositions that can be tested only through positivist research paradigms. My concern here, let me be clear, is with the status of those paradigms to which the label *positivist* is often applied, not with the philosophical doctrine itself.

What Is a Positivist Paradigm?

What do I mean by positivist research paradigms? Rather than trying to offer a general characterization, let me offer a prototypical case, the testing of a new drug by means of a clinical trial (Wulff, 1976). What we might analogously call the *educational* trial, a staple of educational research, follows virtually the same format: Individuals are selected and allocated to treatment and control groups; the two or more groups are provided alternative "treatments" and their progress (or decline) on one or more "dependent variables" is recorded; finally, a statistical evaluation of the results is conducted aimed at assessing whether a difference between the results in the two groups may be caused by chance (see Wulff, chap. 10).

To critics of positivist research, this paradigm is objectionable on at least four grounds: (a) It conceptualizes "treatments" as *causes* in much the same way that physicians construe pharmaceutical products as causes. Hence, it is argued, the positivist paradigm reduces people to mechanistic systems.[1] (b) The paradigm tries to account for the rich and unpredictable complexity of human interaction by means of a few isolable variables. This reduces complex human dynamics to simplistic patterns. (c) The paradigm employs "instrumental reasoning," by which is meant that the "treatment" is considered valuable not in its own right but only for its consequences. This is alleged to preclude rational evaluation of the ends themselves. (d) The paradigm considers the question of causation to be independent of the question of value. That is, whether a drug or educational practice causes a certain state of affairs is one question; whether that state of affairs is desirable or undesirable is a different question. It is claimed that the separation of the two questions implies a spurious dichotomy between facts and values.

A Challenge to Critics of Positivist Research

The educational trial is more difficult to eliminate than its critics suppose. Consider some outspoken critics of positivist research.

Elliot Eisner's (1991) vision of inquiry is self-consciously anti-positivistic; it advocates a genre of educational criticism based on the arts. Schools and classes should be described and interpreted by educational connoisseurs who take their models from art criticism, not scientific investigation. Yet Eisner does not deny that "the primary ideal for educational criticism is that it should contribute to the enhancement of the educational process and through it to the educational enhancement of students" (p. 114). Suppose for the sake of argument that Eisner's ideas about educational connoisseurship are found credible and persuasive. How is the proposed genre of investigation intended to contribute to the improvement of schooling? According to Eisner (1991), qualitative studies can provide guides. "Guides call to our attention aspects of the situation or place we might otherwise miss" (p. 59). Later on he says:

> Attention to the particular, to the case, is descriptive not only of the case, but of other cases like it. When Sarah Lawrence Lightfoot (1983) writes about Brookline High School or the George Washington Carver High School, . . . she tells us more than just what those particular schools are like; we learn something about what makes a good high school. Do all high schools have to be good in the same way? No. Can some high schools share some of their characteristics? Yes. Can we learn from Lightfoot what to look for? Certainly. (Eisner, 1991, p. 203)

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Suppose that Lightfoot identifies some feature of these high schools that "conveys what literal language cannot represent" (p. 203), say, the excitement of being a student there. Suppose further that Lightfoot has a hypothesis about what features of the schools generate that excitement; call them X and Z in the first school, X and Y in the second. Should we take Lightfoot as our guide? Should we provide features X and Y or X and Z in our high school? If we answer yes, it must be on the basis of a belief in a *causal* hypothesis, that is, a belief that these features *contribute to* that excitement.

Despite the attacks leveled against it, the positivist paradigm is hard to avoid.

If we delay the intervention pending an inquiry, what kind of inquiry would we require? Surely it is an inquiry into whether X or Y or Z, singly or in combination, cause the feeling of excitement in high schools. In either case we are committed to the educational trial.

Eisner believes qualitative inquiry has practical benefits in another way: Teachers who have become educational connoisseurs can critique each other's work, and such a critique will improve teaching. The idea is plausible; is it warranted? We have here again a *causal* hypothesis to the effect that a certain kind of interaction among teachers will improve the quality of their interactions with children and, hence, will improve the education of those children. If these causal links are warranted, we have the basis for developing connoisseurship among teachers. If not, not.

Frederick Erickson, an exponent of what he calls the interpretive point of view, is another critic of the positivist paradigm. Erickson (1986) asks us to take note of the undoubted fact that the same behavior (e.g., speaking while another speaker is still speaking) may have different meanings in different cultural contexts. For this reason, his interpretive model rejects the assumption of uniformity and looks instead for "variability between behavioral form and intended meaning" (p. 132). But how is the discovery of such variability plausibly to lead to educational improvement? Either the variability occurs under conditions that can be specified, or it is random. If the variation is discovered to be random, that discovery vitiates any attempt to formulate policies, strategies, or guidelines that improve education. If the variation is discovered to be patterned, then that discovery can ultimately lead to the recommendation of policies (or strategies, etc.) of the form: Employ X under one set of conditions (call them C_1). Under another set of conditions (call them C_2), employ Y. Presumably the wisdom of these recommendations would be undermined by evidence showing that under conditions C_1 students performed better when Y was employed.

Erickson reports favorably on an experiment designed to test the hypothesis that classroom failure by some minority-group children is the result of a mismatch between the culture of the home and that of the classroom. Earlier ethnographic research had already established that the conversational patterns of native Hawaiians was distinctive in that the speakers' utterances overlapped one another instead of following a brief pause. In an educational experiment derived from this research, one set of teachers permitted overlapping speaking turns while another set of teachers teaching matched groups did not. The children's reading achievement was found to be higher in the experimental groups. This teaching pattern is now, according to Erickson, "being implemented in public school classrooms with native Hawaiian children" (p. 136). This research, cited approvingly, appears to fit the educational trial with the conversational pattern reinforced by the teachers as the independent variable and reading achievement as the dependent variable.

In his discussion of the classroom as an arena of "local politics," Erickson says that the important research question is: "What are the conditions of micropolitics in the social organization of classroom life that set off a contest of wills between teacher and students in which the students refuse to learn what the teacher intends to teach?" (p. 137). Erickson describes a point of view (identified with Raymond McDermott) that suggests these conditions are not caused by *either* the teacher or the students alone. Students and teachers become locked in *relationships* that are mutually destructive. The two, "for the most part unwittingly, are mutually failing one another. The student and teacher collaborate in producing a situation in which the student achieves school failure" (p. 137).

Suppose that this hypothesis were confirmed. In what way could its discovery lead to improving education? Again, we must ask: Are such vicious cycles inevitable or do they arise only under certain conditions? Presumably, given Erickson's own assumptions about social phenomena, the latter. Then, the way is open to identifying those conditions. If they were to be identified, then removing them would prevent the cycle from developing. Once again, we have a straightforward causal hypothesis and we are back to the educational trial. Of course, if it turned out that no such conditions could be identified, or that these conditions varied from classroom to classroom in no discernible patterns, then McDermott's discovery could not be used to improve education.

Consider, finally, left-wing critics of "positivism" who contend that the "positivist" paradigm sustains a pernicious status-quo. Thomas Popkewitz says, "Much of what occurs in schools is justified and made credible by the activities of the educational research community" (1981, p. 312). Kenneth Howe writes that "the attempt to bracket values in the name of truth and science in order to protect pluralism and to avoid bias only results in a more insidious bias" (1985, p. 12).[2] Henry Giroux writes, "Unable to reflect on its own presuppositions, or to provide a model for critical reflection in general, it [the culture of positivism] ends up uncritically supporting the status quo" (1981, p. 45).

Criticisms of the positivist research tradition are manifold and some of these criticisms may be persuasive, but any wholesale rejection of the positivist program would, I shall show, subvert the left-wing critics' *own claims*.

Some of these critics wish to supplant positivist paradigms with other modes of educational inquiry. Without trying to identify the nature or source of any particular alternative, let us simply label one such candidate, the New Paradigm.

Would the widespread deployment of the New Paradigm by educational researchers lead to an enhanced education for

children? Perhaps, but only on the assumption that there is a *causal* relationship between the character of the dominant research paradigm and the educational experiences of the young. Of course, the causal process will be very complex, leading from the beliefs and attitudes embodied in the research through the beliefs and attitudes of those who utilize and publicize the research, to the attitudes of teachers and parents, and ultimately to the design of the settings and instructional programs for the young. If the left-wing critics are right, then given two communities or societies, one of which employs a "positivist" research paradigm and the second of which employs the New Paradigm, the latter will show a more beneficial influence on the education of children.

Assuming the causal claim would not be accepted as self-evident—and why should it be?—a test of the critic's hypothesis would involve a modified form of the educational trial. I say *modified* because (a) for various practical reasons (e.g. inability to identify matching experimental and control communities) the research will only approximate an educational trial and (b) the purveyor of the New Paradigm may not share precisely the same educational goals or accept the same indicators of success as the positivist.[3] Even so, the research needed to test the critic's claim will be positivist: The relationship between the nature of a community's research enterprise and its educational provisions must be *causal*. No matter which "dependent variables" are selected, the question of what the impact of the research enterprise *is* must be separated from the question of whether that impact is *desirable* or *undesirable*.

Despite the attacks leveled against it then, the positivist paradigm is hard to avoid. Let me generalize from the above examples: Insofar as any research program aspires to enhance educational practice, it must ultimately issue in some policy, way of thinking, conceptual framework, design, strategy, or practice for intervention in the lives of children. At that point, it is incumbent to ask whether the intervention is an improvement on current practice. If the argument is to be persuasive, it must show the superiority of the innovation. To demonstrate that superiority, it will have to provide evidence that *compared with current practice* the innovation yields more educational value. Where can such evidence come from? It can come from philosophical considerations that support or undermine the innovation *regardless of its consequences*. Or it can come from data derived from experiments that utilize the educational trial. I see no other alternative.

Not all critics of the dominant paradigms have offered or need offer alternatives. Suppose some contend that their critiques are intended only to call attention to the pitfalls and liabilities of the positivist program, not to offer their own alternatives. Now we may ask: What role do you intend your criticisms to play? Are these criticisms intended to weaken the confidence of positivists and to steer aspiring researchers away from positivist paradigms? If the answer to this question is affirmative, then we may ask: Will the pluralism you hope to engender contribute to the enhancement of education? The critics may reply that they are quite indifferent to the effects of their scholarship on educational practice, surely an odd position for left-wing critics to maintain. But once they concede that pluralism in research ultimately serves the interests of children, they have again placed their work in a context in which the following causal hypothesis may be entertained: Given two communities, one with a monolithic positivist research enterprise and the other with a pluralist one, the latter will be more likely to benefit the development of children.

Conclusion

My argument might, mistakenly, be read as an endorsement of the "input-output" research on teaching associated with Nate Gage (1978). If we consider the classroom as our research arena, my point is that whatever pedagogical strategy (or style or program or technique, etc.) derives from a research program must be tested using the educational trial. But this argument does not suppose that the strategy, policy, or guideline that is proposed must *derive* from research that tries to find correlations in *existing* classrooms between inputs and outputs. Let me give a medical analogy. To test their safety and efficacy, antibiotics had at some point to be tested against then current alternatives for the treatment of infection, leeches, or whatever. But antibiotics were not, themselves, *discovered* by investigating correlations between physicians' treatments and their degree of success. It is unlikely that a strategy of medical investigation limited to studying the relative success and failure of contemporary physicians would lead to *any* notable medical advances. Whether the same is true for pedagogy is not a question that can be answered *a priori*.

Where does this leave us? Educational research need not aspire to be practical. Much worthwhile scholarly work is not practical. Moreover, there is no reason to think research that aims to provide improved policies and practices in education ought to *begin* by investigating the differential efficacy of *current* practices. Nor is there reason to suppose that educational researchers, any more than medical researchers, ought to confine their research to educational trials. One might even argue that the most important need is for inquiries whose goal is to yield ideas for innovative practices, designs, or policies.

Nevertheless, at some point, alternative inquiries in educational studies will result in a promising new policy or practice or way of thinking about educating children. At that point, unless the new practice or way of thinking can be judged in the absence of information about its consequences for children, the educational trial becomes relevant, even obligatory.[4]

Notes

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[1]One of the main complaints against positivist science is that it is committed to the existence of universal causal laws that render meaningful choice and action illusory. But, as some contemporary philosophers of science have argued, causal processes may be presumed to operate even in a nondeterministic world. Some philosophers, most recently Eels and Sober (Eels & Sober, 1983, and Sober, 1984), understand causal claims to mean no more than that causes *raise the probability* of their effects.

[2]Howe's (1988) article in these pages is much more supportive of my argument here.

[3]Disputes over goals are more likely in theory than in practice. Left-wing theorists and positivists often choose to send their own children to the same schools. In any case, the claim that the New Paradigm supports left-wing educational goals is a causal claim that must be tested. It cannot simply be taken on faith.

[4]To say that the educational trial is obligatory does not imply that its results are likely to be conclusive. Indeed, for reasons that go beyond the scope of this article, I would argue that such results will seldom be decisive.

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Are All Causal Claims Positivistic? A Reply to Francis Schrag

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Francis Schrag claims that a positivistic paradigm must be employed to test any claim pertaining to the consequences of any educational intervention. In one sweep, Schrag brings all forms of research that purport to offer guidance to practitioners under the umbrella of positivism. Thus, a researcher who says that constructive and critical feedback is likely to help teachers better understand what they do in their classrooms must, according to Schrag, use a positivistic paradigm to test his or her suggestions.

When a conception is as broad as Schrag's, it is difficult to know what to exclude. According to Schrag, any meaningful means-ends statement is inherently positivistic. Are things that simple? The positivistic paradigm that Schrag says must be used is predicated on a particular philosophic tradition. To be positivistic, a paradigm must be congruent with the philosophic tradition in which it participates. Schrag's conception of a positivistic paradigm, which he associates with an experimental drug treatment, entails far more than matters of causality, the hook on which Schrag wants to hang his argument. Positivism, which Schrag does not define, is a philosophy of science that has an attitude towards metaphysics, that separates value from fact, that embraces methodological monism, that rests upon a foundationalist view of knowledge, that possesses a particular conception of meaning, that regards ethical claims as meaningless utterances, that believes science to be the sole source of knowledge, that seeks to explain "reality" through an appeal to universal laws, and that regards measurement as the quintessential means through which reality, whatever it may be, can be represented.

Is someone using a positivistic paradigm if most or all of the foregoing beliefs are rejected? Schrag tries to avoid this issue by stating that his concern is "not with the philosophic doctrine itself." For a philosopher, this is a strange statement. How can one regard a paradigm as positivistic and disregard the philosophy from which the paradigm gets its name? Does *any* recommendation regarding a course of action automatically demand fealty to a positivistic account of a state of affairs? I think not.

The bind here is that Schrag wants to create a philosophical hegemony that somehow sidesteps the epistemological premises that give meaning to the term he wishes to assign. Such an economy won't do because it robs the term *positivistic paradigm* of the very content it needs to have philosophic meaning.

As to my attitude towards positivism, it will come as no surprise to those who know my work that I do not find it an attractive philosophy for a multitude of reasons, many of which are embedded in my previous characterization of some of its primary features. However, Schrag not only says that I am anti-positivistic; he suggests I am anti-science. Either he believes that *all* science is positivistic, or he misreads my book, *The Enlightened Eye* (Eisner, 1991). Furthermore, he assigns to me a view I do not embrace, namely that "schools and classes should be described and interpreted by educational connoisseurs who take their models from art criticism, not scientific investigation." His aim here is to strengthen his case by portraying me as anti-science, but his representation of my views does not portray what I believe. And his use of the phrase *left-wing critics* to characterize colleagues is a form of name-calling that belies the kind of "objectivity" that someone who embraces positivism would be expected to display.

What I do embrace is a pluralistic conception of method: Many methodological voices should be heard. Thus, I certainly do *not* reject science as he says. What I do reject is a

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