

## Getting Over the Quantitative-Qualitative Debate

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The quantitative-qualitative debate has been unfolding for several decades now and has evolved from one about the incompatibility of quantitative and qualitative techniques and procedures to one about the incompatibility of the more fundamental epistemological assumptions of quantitative and qualitative (positivist and interpretivist) "paradigms." Employing arguments from the philosophy of social science and social research theory, this article seeks both to widen the scope of the paradigms debate—to include perspectives on human nature and on the relationship between theory and practice—and to dismiss both the positivist and interpretivist paradigms in favor of the "critical educational research model." The article also provides several examples that illustrate this approach to educational research.

In his classic paper "The Influence of Darwinism on Philosophy," Dewey remarks, "Intellectual progress usually occurs through sheer abandonment of questions together with both of the alternatives they assume. . . . We do not solve them: We get over them" (1981, p. 41).

This observation may be instructively applied to the ongoing controversy about quantitative versus qualitative methods in educational research. As framed by those who think there is good reason to avoid getting over the debate, good reason to avoid "shutting down the conversation" (e.g., Smith and Heshusius 1986), two alternatives exist, both of which assume that quantitative and qualitative paradigms exclude one another: *disjunctive eclecticism*, in which researchers are free to operate within their chosen "paradigm," and *methodological imperialism*, in which one "paradigm" is adopted as superior. In this article I will urge getting over the debate framed in terms of these alternatives and will suggest that educational researchers must learn to live with the certain tensions that result from embracing elements of each. By way of setting the stage, I will begin with a brief characterization of the evolution of the debate.

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0195-6744/92/0002-0004\$01.00

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Over approximately the last decade, the terms “quantitative” and “qualitative” have come to be used to make at least two different contrasts: literal and derivative. The literal contrast has to do with data collection, research design, and analysis, with what Smith and Heshusius (1986) refer to as “techniques and procedures”; the derivative contrast has to do with broader epistemological assumptions, with “epistemological paradigms.” Given my understanding of the evolution of the debate, these two ways of contrasting quantitative and qualitative research have been separated (e.g., Guba 1987; Guba and Lincoln 1989; Lincoln and Guba 1985; Reichardt and Cook 1979; Smith and Heshusius 1986), such that it is now viewed as perfectly coherent to combine quantitative and qualitative “techniques and procedures.” What remains incoherent in the eyes of many educational researchers is combining epistemological paradigms. As Guba puts it, “The one [paradigm] precludes the other just as surely as belief in a round world precludes belief in a flat one” (1987, p. 31).

One response to this line of argument has been to deny this forced choice between paradigms—typically construed as a choice between positivism and interpretivism—by dismissing positivism as untenable and denying that it ought to serve as the foil against which alternative epistemological stances must define themselves (Howe 1988; Howe and Eisenhart 1990). According to this response, using positivism as a foil not only ipso facto assumes that it is a tenable epistemological view, it encourages antipositivists to define their positions negatively, as positivism’s polar opposite, and to surrender, wholesale, concepts such as objectivity, causation, and truth. This puts “alternative paradigms” in a weak and defensive position—a position that they need not and should not embrace—and defines the quantitative-qualitative debate in a way that is epistemologically moribund and guarantees it will remain insoluble.

In what follows I will briefly rehearse arguments in this vein that support getting over the quantitative versus qualitative debate. I will go considerably beyond them, however, by developing two further

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aspects of the debate that, although broached in various ways, have not been sufficiently teased out: the problem of human nature and the relationship between theory and practice. I will argue in each case that the positivist-interpretivist split can and should be gotten over in favor of a model that I call "critical educational research." I will then illustrate how educational researchers with otherwise quite disparate theoretical commitments both may be accommodated by the model and may face similar problems vis-à-vis the practical problem of promoting change.

In order to forestall confusion and misplaced criticisms, a few clarifying remarks about my use of two key concepts are required at the outset. (I fear they are too central to my arguments to be buried in a footnote.) First, and following Fay (1975), I will restrict my use of "interpretivism" to the sort of view that may be identified with heavily emphasizing the so-called "insiders perspective" regarding the interpretations of the meaning and implications of social events and arrangements. Although "interpretivism" is nowadays often used in a broader sense, the usage I have adopted is useful for the contrasts I wish to make and approximates the roots of more expansive senses of "interpretivism" to be found in Dilthey's method of *Verstehen*. Second, and also following Fay (1975), I will use "critical social science" to refer to a model of social science that is indeed "critical" but that may not be straightforwardly identified with the peculiarities of the theoretical stance associated with "critical theory." Keeping these caveats in mind, the more precise meanings of these terms will become clear as the article unfolds.

### Three Dimensions of the Paradigms Debate

#### *Beyond the Positivist-Interpretivist Split*

Anthony Giddens remarks regarding social science that "those who still wait for a Newton are not only waiting for a train that won't arrive, they're in the wrong station altogether" (1976, p. 13). The reference to "wrong station altogether" indicates that the issue is deeper than the mere complexity of social phenomena as compared to natural ones; it indicates that there is something fundamentally wrong with trying to strictly model the social sciences on the natural sciences. Giddens's remark is rooted in the now altogether commonplace claim that an adequate explanation of human behavior requires what Daniel Dennett (1987) calls the "intentional stance." That is, unlike explaining

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molecules in motion, explaining human behavior requires appeal to beliefs, desires, and goal-directedness.

What I am calling the positivist-interpretivist split is generated by taking a certain position toward the intentionalist stance, namely, that it is not only necessary but, when adopted, excludes the natural science model altogether. This comes about by identifying natural science with positivism and the intentional stance with interpretivism, and then deeming positivism and interpretivism incompatible in virtue of various familiar dualisms, such as objectivity versus subjectivity, fixed categories versus emergent categories, the outsider's perspective versus the insider's perspective, a static reality versus a fluid reality, and explanation versus understanding.

This argument is basically correct when properly restricted: positivism and interpretivism *are* incompatible. But—and this is crucial—it is only by both embracing positivism as a tenable epistemology and embracing a certain construal of “interpretivism” (recall my earlier characterization) that a different and broader claim follows: that the natural science model and interpretivism are *in general and irremediably* incompatible. This broader claim is the fundamental error that needs to be gotten over, and three theoretical advances of the last several decades are especially instructive: the demise of positivism, the emergence of intentional theories of causation, and the incorporation of interpretivism into more elaborate explanatory schemes.

*The demise of positivism.*—The central tenet of recent incarnations of positivism is “verificationism,” the doctrine that the observational and the theoretical contents of scientific theories can be strictly separated from one another, such that theory can have its ultimate foundation in, and be testable solely in terms of, observation. The aim of this doctrine is to achieve a form of scientific objectivity in which data are linked to theory but nonetheless remain uncolored by the theoretical baggage, especially the values and interests, that researchers bring to collecting and interpreting data.

At first blush, the positivist program might seem reasonable enough since its major aim is to eliminate bias and unbridled metaphysical speculation. But the demands of verificationism are too extreme. On the one hand, the positivists were never able to give a satisfactory account of the relationship between observation and theory that met their self-imposed requirements. As Phillips observes, “The principle of verifiability suffered the same fate as the ‘Elephant Man’—it became a contorted monstrosity that choked under its own weight” (1983, p. 7). On the other hand, and following on the heels of this failure, Quine (1970) and Kuhn (1962), among others, have advanced rather

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formidable arguments showing that the positivists' program cannot be realized in any event: because the hard-and-fast dividing lines between the empirical and theoretical content of scientific theories envisioned by positivists cannot be drawn, all scientific observation is unavoidably "theory laden" and, accordingly, is unavoidably colored by the "conceptual schemes" or "paradigms" that researchers employ. The general thrust of the Quine-Kuhn criticism of positivism is now widely accepted by philosophers as correct, and it is for this reason that positivism is dead as an epistemological doctrine.

The demise of positivistic epistemology has two important implications. First, the epistemological chasm between natural science and interpretive social science is significantly narrowed. Because natural science is theory laden, it is also interpretive, or hermeneutic (e.g., Bernstein 1983; Phillips 1987; Rorty 1982). Thus, natural science cannot be erected on the foundation of a wholly neutral collection of observations any more than social science. Second, and as a consequence of this, there are no good reasons for ruling out the "intentional stance" in order for social research methodologies to be legitimate since the vocabulary of social science need not pass the test of the principle of verifiability in order to be legitimate any more than the vocabulary of natural science.

*The emergence of intentionalist theories of causation.*—Including the intentional stance within a social scientific explanatory framework requires moving beyond exclusive reliance on the kind of Humean, molecules-in-motion conception of causation that undergirds positivism. In particular, it requires making sense out of how explanations of human behavior in terms of beliefs, hopes, fears—that is, states of mind in general—can count as genuinely causal explanations.

The skepticism toward the intentional stance has two major sources. First, when applied to social research, the positivists' verifiability principle was cashed out as methodological behaviorism, which rendered illegitimate the appeal to unobservable mental states. Second, the positivists adopted a Humean model of causation for both the social sciences and the natural sciences, and such a model has a feature that renders intentional causation unintelligible: causes and effects can have no conceptual connection; instead, they must be only contingently and empirically related. (For instance, being an unmarried male can hardly be the cause of being a bachelor.) Verificationism has already been dismissed. In the remainder of this section I will concentrate on Searle's (1984) defense of intentional causation with respect to the issue of conceptual connections between causes and effects in the explanation of human behavior.

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According to Searle, among the things that cause things are "intentions." In particular, intentions cause actions. For example, if the behavior to be explained is a student raising her hand in a classroom and a subsequent investigation reveals that she did so to be recognized to answer a question posed by the teacher, then the cause of the behavior is "desiring to be recognized." Now, the problem given a positivistic (Humean) conception of causation is that intentions are conceptually linked to action descriptions, such that giving an intentionalist explanation amounts to merely redescribing behavior rather than locating its causes. In the above example, "desiring to be recognized" becomes a part of the interpretation of what "hand raising" in classroom means; that is, "hand raising" refers to an attempt to be recognized, not to other actions, such as "trying to catch flies," nor to mere *movements*, such as an "arm going up" involuntarily. Consequently, intentional causation conflicts with the positivistic notion that establishing causal relationships has to do with establishing contingent regularities, insofar as it embraces a conceptual connection between actions and their causes.

Searle brushes aside positivist dogma about causation and begins instead with the "commonsense" observation that causation just means that "something makes something else happen" (1984, p. 65; also see Von Wright 1971), and he remarks that one can embrace methodological behaviorism and thereby rule the mental component of human actions out of court only by "feigning anaesthesia" (1984, p. 62). Searle not only accepts the claim that intentional explanations do not establish contingent regularities. He turns the table on the positivistic conception of causation by arguing that it is the appeal to observed regularities that cannot serve as explanations of behavior rather than the appeal to intentions. For, observed regularities do not constitute explanations; they call for them. As an illustration, consider the disproportionately high drop-out rate for Hispanics versus whites. In the absence of evidence regarding why Hispanics drop out—in terms of their beliefs, culture, desires, and so forth—the observed regularity is just that. By extension, social research that confines itself to discovering contingent regularities in general fails to provide explanations.

*The incorporation of interpretivism into more inclusive explanatory schemes.*—Where interpretivism is identified with confinement to the "insider's perspective," it is indeed inconsistent with the logic of natural science. This view, however, has been largely dismissed; moreover—and this is the crux—it has been dismissed by thinkers who are by no means positivists themselves (e.g., Bernstein 1983; Fay 1975, 1987; Geertz 1979; Giddens 1976; Rorty 1982; Searle 1984, to name a few).

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Rather than dismissing this brand of interpretivism on the grounds that it fails to measure up to the strictures of verificationism, as positivists did, recent criticisms focus on the impossibility of completely shedding one's skin, of "going native," so as to be fully in tune with the insider's perspective, and on the inadequacies of this brand of interpretivism with respect to what social researchers adopt as the aims of their enterprise.

Regarding "going native," Geertz (1979) concurs with Malinowski's demolition of the picture of the social researcher as a "a walking miracle of empathy, tact, patience, and cosmopolitanism" (p. 225) and then goes on to draw several epistemological conclusions that follow from abandoning this methodological perspective. First, instead of attempting to achieve "some inner correspondence of spirit," the task of the field-worker is the less ambitious one of trying to determine what informants "think they are up to." But second, discovering what informants themselves think they are up to is not enough to achieve an adequate account of their behavior. An adequate account, according to Geertz, requires engaging in "dialectical tacking" between what informants think they are up to, expressed in their own terms, and the special vocabulary and theoretical premises of social theory.

This second point is endorsed by numerous critics other than Geertz and is important regarding the inadequacies of the brand of interpretivism under consideration. Brian Fay (1975) is exemplary of these critics, and he enumerates four shortcomings of what he calls the "interpretive model": its neglect of (1) the external conditions that help give rise to systems of actions, rules, and beliefs; (2) the unintended consequences of actions; (3) the internal contradictions between actions, rules, and meanings; and (4) historical change. He summarizes these four shortcomings as follows:

It is . . . the job of the social scientist to show how a specific institution or social order came to be what it is, and how it will change in determinate ways. An interpretive social science, by methodologically assuming an internal coherence between the self-understandings of the actors, their common meanings, their social practices, and their actions, is unable to explain why it is that a social order will develop—except by invoking external forces—and why it will develop in definite ways. . . . The actors might well act in a certain way, follow certain rules, and operate in terms of certain constitutive meanings; nevertheless, they might very well also be creating consequences which will in turn affect their needs, interests, and capacities in specific, though to them unknown, ways; they might also come to change their social behavior as the result of the conditions which they themselves have created,

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though unwittingly; they might be forced to adopt new practices and come to think of themselves in fundamentally new ways because of the contradictions which mark their social arrangements and belief systems, but about which they are unaware. In these and in a myriad of other similar events the social scientist will be interested, precisely because he is interested not only in social order but in social change. [Fay 1975, p. 88]

In summary, positivism is untenable and interpretivism is incomplete. The kind of epistemological perspective that supplants them—which I call “compatibilism” (Howe 1988)—borrows features both from the natural science model and from interpretivism. In very broad strokes, compatibilism borrows from the natural science model by acknowledging the uneliminable role of *mechanistic* explanations (explanations in terms of unwitting, unseen, and unplanned causes) of social structures and individual behavior, like the workings of the economic system; compatibilism borrows from interpretivism by acknowledging the uneliminable role of *intentional* explanations of social structures and individual behavior, like the reasons parents might give for sending their children to a fundamentalist school.

*The Problem of Human Nature*

There is a close connection between theories of social scientific explanation—an epistemological issue—and theories of human nature—an ontological issue. For a theory of human nature specifies the kinds of beings that a theory of social scientific explanation has for its subject matter. Positivism, with its “spectator view” of knowledge and Humean conception of causation, encourages a view of humans as passive and determined by exogenous causes; interpretivism, with its constructivist view of knowledge and intentionalist conception of causation, encourages a view of humans as active and self-creating. In their purest forms, the positivist conception construes human beings as not significantly different from other things explained by the methods of the natural sciences, whereas the interpretivist conception construes humans as so radically different from other things in the natural world that they are totally inexplicable in terms of such methods. The correct view, or so I shall argue, acknowledges elements of truth in both of these views but rejects each as one-sided.

Intuitively, human beings are neither wholly passive and determined nor wholly active and self-creating. Instead, they exhibit these characteristics in varying degrees. The degree to which an individual is



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one or the other depends on a host of social factors, such as economic and political structures, and a host of individual factors, such as age and education. For example, an uneducated, downtrodden fieldhand is likely to be much more passive (controlled by forces beyond his control and of which he is unaware, uncritical of information with which he is presented, etc.) than a highly educated, well-to-do professional. Although there is no ironclad regularity here (it is possible to imagine the highly emancipated fieldhand and the highly oppressed professional) the general point holds that human nature is partially determined by how humans see themselves and partially determined by things of which they are unaware and over which they have no control. Accordingly, insofar as interpretivism remains trapped within the first perspective and positivism within the second, neither view can give an adequate account of human nature.

The implication of the observation that social and individual factors play a crucial role in determining the degree to which individuals are passive versus active is that human nature is not a given—and this significantly complicates the issue of just what it is. In particular, because human beings exercise *some* control over social arrangements and institutions (even if the shape of such arrangements and institutions are largely unintended and operate unbeknownst to many), a conception of human nature can become self-fulfilling. For example, Dewey (1950), among many others, has criticized advocates of laissez-faire capitalism for confusing an *antecedently existing* human nature with the kind of egoistic, acquisitive individual that he believes laissez-faire capitalism *actually creates*. To take another example from Dewey (1938), he observes that after years of schooling in which children are continually required to master and regurgitate information presented by their teachers, they will develop the habit of expecting (and demanding) that they play this passive role as learners. Consequently, although they do not start that way, in time many will come to approximate the positivist-behaviorist model. The upshot of these observations is that because humans have something to do with making themselves and others turn out in certain ways, human nature must be informed, as well as shaped, by some kind of moral ideal.

Viewing human nature on the model of a “Norway rat” (Floden 1981), the positivist-behaviorist perspective, is an uninspiring moral ideal to say the least. Because it is objectionable, some version of what Fay (1987) terms the “activist conception” of human nature must undergird social research.

The activist conception is inspired by the interpretivist observation that human beings do indeed act intentionally, something molecules in motion do not do. However, humans do not always act intentionally,

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and, even when they do, their actions are set against “the background of intentionality” (Searle 1984), much of which is not itself wittingly created or adopted. Consequently, to demand that humans be completely and always “active” is to demand too much. Instead, realizing an activist conception of human nature requires (1) that humans have the basic capacities (physiological, psychological, etc.) to be active and (2) that conditions exist that foster the development and use of these capacities.

This conception of human nature, then, concedes to the natural science model mechanistic (e.g., structuralist-functionalist) accounts of human behavior, preserving a place for the externally determined, “passive” side of human nature. On the other hand, it concedes to interpretivism intentionalist accounts of human behavior, preserving a place for the self-determined, “active” side of human nature. This view of human nature buttresses the kind of “compatibilist” view of social scientific explanation advanced previously. That is, insofar as human behavior is an admixture of active and passive ingredients, a conception of explanation should capture both.

### *Theory and Practice*

A conception of human nature coupled with a conception of social scientific explanation has direct implications with respect to the aims of social research, its methodology, and the uses to which its findings are put. For, by specifying what kinds of beings humans are and how they can best be studied, the combination of a conception of human nature and a conception of social scientific explanation ipso facto suggests the form that research practice should take, including what role humans should take in shaping their own lives as participants in and as respondents to social research. I say that the combination of a conception of social scientific explanation and a conception of human nature “suggests” (rather than “entails”) the form that research practice should take because a theory of social research only partially dictates how research practice, especially educational research practice, should proceed. This is true for several reasons. First, theories of social research are abstractions—“reconstructed logics”—of the principles and methods that govern and are employed in practice—“logics in use” (Kaplan 1964). Such theories are thus often insensitive to the intricacies and limitations of actual practice. Second, and related to this, theories of social research are (should be) constantly revised in terms of what methods prove themselves effective in practice. (This second point actually amounts to a general criticism of positivist-interpretivist split. See Howe 1988.) Finally, theories of social research are couched in

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terms of the features of “grand” social theory rather than the peculiarities of educational research. Although such theories are liberally borrowed by educational researchers, they frequently require significant modification when adopted for educational research purposes in particular (Howe and Eisenhart 1990; Eisenhart and Howe 1992).

*Positivism and technical control.*—The positivist theory of social scientific explanation entails discovering mechanistic causal regularities; its conception of human nature entails unthinking Norway rats who are subject exclusively to such causes. Consistent with these two tenets, its principle of verifiability entails “emotivism” with regard to moral and political deliberation, where “emotivism” means that moral and political issues—questions of “ends”—are beyond rational examination and that social research is (should be) value neutral. These three tenets together entail a radical disjunction between means and ends. The underlying aim of social research becomes “technical control” (Fay 1975): experts determine the means to achieve some ends that are not themselves questioned; these means are then translated into policies and practices designed to achieve the ends of interest.

Process-product research is the type of educational research that probably best approximates positivistic technical control. The basic idea is for experts to discover regularities associated with the achieving the desired end, namely, learning, and then to disseminate these in the form of techniques to the practitioners, namely, teachers, whose responsibility it is to achieve the desired end. The manner in which the current testing-accountability movement has developed is another illustration of technical control. First, the “end” of the movement, which is largely improved economic competitiveness, is bracketed and left to the realm of politics (presumably, it is uncontroversial). Second, coming up with the “means,” designing testing-accountability schemes, is left to experts in the technical field of measurement. Finally, the “means” are then implemented with little or no effective input from those whom they most affect, namely, teachers, students, and parents.

The means-ends bifurcation associated with positivistic technical control is vulnerable in at least three ways. First, means are means only relative to some end. As a consequence, adopting a given end, for example, improving economic competitiveness, ipso facto entails focusing social research on the limited array of means that may be used to accomplish that end. Thus, the investigation of means presupposes the value orientation of those who determine the ends. Furthermore, what is a means relative to some later end is an end relative to some earlier means. For instance, increased achievement in math and science is an end relative to instruction but a means relative to economic competitiveness. Second, and related to this, means themselves

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are subject to value constraints. For example, it is only on the basis of the value commitments of those who are investigating means that putting all "at-risk" students in forced labor camps is not investigated as a "means" to improving economic competitiveness. That such "means" as forced labor are ruled out of court is reasonably uncontroversial, and thus the problem with means-ends reasoning in this case is not very acute. The use of standardized testing for the purposes of accountability and for practices such as "tracking," however, illustrate the way in which expert driven means-ends reasoning is fundamentally problematic.

The above two criticisms demonstrate how positivist social research cannot sustain its claimed value neutrality, with respect to either means or ends. Wittingly or unwittingly, positivistic technical control promotes certain values. In connection with this, the third, and perhaps most fundamental, charge that may be advanced against the rigid means-ends bifurcation is that it is irremediably nondemocratic. In virtue of (somehow) settling on ends and then relegating the investigation of possible means to these ends to (expert) social researchers, it implicitly dismisses the value of participation in deliberation on the part of those who are affected. But, where democratic values are in force, participation is an end in itself. As such, the process of participation does not guarantee that any particular ends must be settled on ahead of time, prior to the investigation of means. Rather, participation engenders means and ends in a way that acknowledges individuals' worth by giving them a say in determining what is worth pursuing and how. Viewed in another way, participation engenders jointly working out what moral ideal—conception of human nature—should direct social life. Furthermore, when viewed from the perspective of effectiveness, participation is required for the successful use of research findings, insofar as individuals are moved by intention causation, that is, by beliefs that they adopt as their own (see, e.g., House et al. 1989).

*Interpretivism and facilitation.*—In virtue of embracing an intentionalist conception of social scientific explanation and an activist conception of human nature, interpretivism entails a role for social research quite different from positivism's technical control. Uncovering the beliefs, customs, and so forth, that serve as the springs of human behavior makes it possible for individuals to better understand themselves and one another, which in turn makes more meaningful and effective participation in deliberation possible. Respect for individuals as having both a moral claim to and the disposition to have a say in the conduct of social life entails that the findings of social research should be used to facilitate this attempt to work out the details of social life. Unlike positivism, interpretivism is inherently conducive to participation.

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Perhaps the best illustration of the interpretivist facilitation in educational research is to be found in the work of Lincoln and Guba's call for "negotiated outcomes." According to them,

[The preferred method is] to negotiate meanings and interpretations with the human sources from which the data have chiefly been drawn because it is their constructions of reality that the inquirer seeks to reconstruct; because inquiry outcomes depend upon the nature and quality of the interaction between the knower and the known, epitomized in negotiations about the meaning of data; because the specific working hypotheses that might apply in a given context are best verified and confirmed by the people who inhabit that context; because respondents are in a better position to interpret the complex mutual interactions—shapings—that enter into what is observed; and because respondents can best understand and interpret the influence of local value patterns. [Lincoln and Guba 1985, p. 41]

The important thing to note in this passage is the emphasis Lincoln and Guba give to the insider's perspective, such that it supersedes the perspectives that researchers might bring to a given situation. There are problems for practice—problems in addition to the epistemological and ontological ones discussed previously—with the interpretivist penchant to so heavily weight the insiders perspective, just as there are problems with the positivist penchant to give it virtually no weight at all.

According to Fay (1975), the interpretive model promises to affect practice by eliciting "undistorted communication," but this promise cannot be realized without taking into account the structural features and causes of social practices as well as the norms that actors unwittingly internalize and employ in communication and action. Furthermore, and as a consequence of this, the interpretive model is inherently conservative because, by confining itself to the insider's perspective, "it systematically ignores the possible structures of conflict within society" (Fay, 1975, p. 90) to which only researchers (outsiders) may be privy. Fay elaborates this second criticism as follows:

[Interpretivism] assumes an inherent continuity in a particular society. . . . This methodological assumption leads to conservative political theory just because such a science cannot generate any [external] standards of criticism of existing social reality—in fact, it leads one to view the attempt . . . as misguided. . . .

An interpretive social science promises to reveal to the social actors what they and others are doing, thereby restoring com-

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munication by correcting the ideas that they have about each other and themselves. But this makes it sound as if all conflict (or breakdown in communication, for that matter) is generated by mistaken ideas about social reality rather than by the tensions and incompatibilities inherent in this reality itself.

The upshot of this is profoundly conservative, because it *leads to reconciling people to their social order*. [Fay 1975, pp. 90–91]

Fay's general point concerns the limitations of being confined to the insider's perspective. In particular, it commits the researcher to a form of relativism that provides no space for an *external* criticism of the social order in the sense of criticism that had not occurred to the actors in question and with which they might not agree. This places the researcher in the position of being a mere data gatherer who then operates as little more than a functionary, withholding, or revising in the light of the insiders' perspectives, perspectives on the situation that might disagree with those of the insiders. (Perhaps a recognition of this criticism explains why in their most recent work Guba and Lincoln [1989] have seemingly modified their view regarding the degree to which the insider's perspective should be privileged. See esp. chap. 5.)

*Critical social research and collaboration.*—The alternative to positivistic technical control and interpretivistic facilitation is “critical social research” (recall that “critical social research” should not be identified with “critical theory”). Instead of participating in technical control or functioning as mere facilitators, researchers work in active collaboration with citizen interlocutors. In virtue of embracing a proper role for technical (e.g., functionalist-structuralist) social scientific explanation, critical social research grants to researchers special expertise and knowledge not possessed by ordinary citizens. In virtue of also embracing a proper role for intentionalist explanation, as well as an activist conception of human nature, critical social research subjects such knowledge to scrutiny with respect to its accuracy and its implications for social life—both on the part of other social researchers and on the part of citizen interlocutors—instead of employing it as a means of technical control. Critical social research is thus more akin to interpretivism than it is to positivism. Like the interpretivist link between theory and practice, critical social research is inherently participatory and must be ultimately grounded in terms of the insider's perspective. The key difference is that critical social research consists in *challenging* citizen interlocutors with (expert) social research findings rather than merely facilitating mutual understanding of the rules of the game.

The collaborative model of practice associated with critical educational research generates its own kinds of problem: the “problem of resistance”

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(Fay 1975). That is, citizen interlocutors as well as policymakers are apt to disagree with the findings and recommendations of social researchers and thus to resist or reject them. This problem can be dismissed in the case of positivism insofar as expert knowledge should overrule opinions that are uninformed by research; it is incoherent in the case of interpretivism insofar as insiders' perspectives should overrule outsiders' perspectives regarding what counts as the correct view of the workings of social life. Because it credits both the insiders' and outsiders' perspectives, however, the problem of resistance is endemic to critical social research. Solving it (if it is even sensible to think in such terms) is not something I can attempt here (but see Fay 1987).

### Critical Educational Research

With a characterization and defense of critical social research in hand, I will now sketch the features of the model of educational research that it implies, and my aims here will be largely to *prescribe* the shape the educational research should take. Lest the discussion be left in the clouds of abstractness and generality, however, I will also provide two examples of educational research—one from the radical and one from the liberal tradition—in order to illustrate how the critical model can indeed *describe* the issues that educational researchers in fact grapple with, including the problem of resistance, even where two very different theoretical perspectives are employed.

#### *The Critical Educational Research Model*

As stated previously, the activist conception of human nature is grounded in the moral ideal that individuals possess autonomy (at least to some degree) and that they ought to have a say in shaping social life. In order to honor this ideal, educational research must give attention to both *external* and *internal* value constraints on the practice of research (Howe 1985; Howe and Eisenhart 1990; Eisenhart and Howe 1992). Although external and internal value constraints overlap, the idea behind the distinction is that the former have to do with the worth of educational research with respect to educational practice and the latter to do with how interlocutors (subjects) are treated within research practice (typically the domain of research ethics). Regarding external

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constraints, insofar as teachers, administrators, parents, and students are to have a say in educational life, then they ought to have a say in articulating the problems that it presents. Related to this, the findings of (even expert) educational research must be cast in a vocabulary that it is possible for these groups to understand and debate. Regarding internal constraints, interlocutors (subjects) must be informed of what prospective educational research requires of them, promises them, and aims to achieve, as well as what dangers it poses to their interests.

Value constraints of both kinds set the boundaries for the conduct of educational research at the most general level, and it is within these boundaries that the compatibilist conception of social scientific explanation comes into play. Because the critical social research model eschews the positivist-interpretivist epistemological split in favor of compatibilism, educational research based on this model weds itself to the "logics in use" that have proved themselves successful instead of wedding itself exclusively to either the positivist or interpretivist models of explanation. Furthermore, because education is a "field of study" that freely borrows research methods and theory from a variety of disciplines (Shulman 1988), a variety of methodological approaches to educational research are sanctioned by the critical educational research model. Accordingly, the methodological principles that *generally* constrain the conduct of educational research are quite broad and abstract. These principles include (1) first formulating questions that are grounded in the practice of education and then tailoring research methods to fit such questions; (2) competently applying the specific data collection techniques and methods of analysis that define a given methodological approach; (3) grounding research in credible background knowledge, whether it be "grand" social theory or more modest knowledge about educational practice; and (4) ensuring the overall warrant of conclusions drawn, which includes critically broaching (vs. ignoring or rejecting out of hand) findings that derive from alternative methodological approaches (Howe and Eisenhart 1990; Eisenhart and Howe 1992).

### *Two Examples of Critical Educational Research*

Below I will consider two examples of critical educational research. They will be used to illustrate, to greater and lesser degrees, the manner in which educational researchers employ (or grapple with) both com-



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patibilist explanation and the problem (potential or real) of resistance by citizen interlocutors to their findings.

Before embarking on a discussion of the examples themselves several disclaimers are in order. First, the examples are not designed to illustrate in any very precise way the mixing and matching of quantitative and qualitative “techniques and procedures.” Consistent with my opening remarks, the quantitative-qualitative debate is nowadays focused on the more abstract question of the compatibility of more expansive research “paradigms,” each of which is free to make use of various techniques and procedures. Second, the examples are *reconstructions* in order to illustrate the critical educational research model, which is to say that I do not intend the examples—examples of “logics in use”—to necessarily illustrate a self-conscious application of the critical educational research model. Finally, the two examples I consider are by no means intended to exhaust the possibilities. Rather, they are intended to illustrate that educational researchers with otherwise quite diverse interests and commitments have *in fact* moved in the direction of critical education research—have *in fact* gotten over the quantitative-qualitative debate.

*Radical educational research.*—Michael Apple’s *Teachers and Texts* (1988) is a good example of the radical tradition in educational research. In this study Apple blends a modest use of descriptive statistics, teachers’ own reports, and various structuralist arguments in terms of concepts such as political economy, gender, and class. Thus, he blends quantitative and qualitative “techniques and procedures” as well as quantitative and qualitative “paradigms.” Furthermore, the manner in which Apple conceives of his task is quite revealing. He writes, “The problem is . . . how we combine structuralist insights about the relationship between the school and the social and sexual division of labor with the culturalist perspective that places human agency and concrete experiences at the center. How do we show the role of the educational system in the production of these divisions without at the same time falling into the many traps that bedevilled earlier attempts at doing this—attempts that often turned people into puppets of structural forces?” (Apple 1988, p. 23). Apple continues, “Can we get inside institutions and illuminate what actually happens, how people act (often in contradictory ways) within the conditions set by the institution and the larger society, and point out possibilities that exist for altering dominant relations?” (Apple 1988, pp. 23–24).

These passages illustrate Apple’s commitment to a compatibilist form of explanation as well as his commitment to providing individuals with the opportunity to participate in improving educational life. In

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a less obvious (but no less important) way, they also illustrate Apple's implicit concern with the problem of resistance. For, although he clearly wants to show respect for the beliefs people develop on the basis of their own experiences, he just as clearly wants people to understand, endorse, and act on these possibilities revealed by his analysis of structural forces. And the two are at odds.

*Liberal educational research.*—Mary L. Smith and Loretta Shepard's (1987) critique of the practice of grade retention provides an illustration of liberal educational research. Their approach differs from Apple's in virtue of largely excluding structural/ideological forces from the type of arguments it advances. (I will not here enter the debate of whether liberal educational research is an [unwitting] accomplice in perpetuating the status quo, and perhaps much that might be included under this rubric is. However, insofar as Smith and Shepard in particular embrace a collaborative view of educational research practice, they are not vulnerable to the charges that may be lodged against positivistic technical control.) Their critique combines a review of a number of studies of retention (the vast majority of which indicate that retention is ineffective and demonstratively harmful) and a project of their own in which they collected qualitative data about beliefs of teachers and parents and conducted a quantitative comparison of groups of children who had been promoted with groups who had been retained. Their quantitative results agreed with what previous research on the effects of retention had shown; their qualitative results indicated that many teachers and administrators, particularly those who work in schools that have high rates of retention, believe, contrary to what the preponderance of research evidence has shown, that retention is effective and beneficial.

Several features of Smith and Shepard's critique qualify it as critical educational research. First, their argument is framed in terms of various educational/political values, such as benefit for students, equality, and nondiscrimination. Second, it is rooted in a common educational practice and educators' own beliefs about that practice. Third, its explanatory scheme is compatibilist insofar as their own quantitative findings are combined with the findings of other educational researchers to represent the outsiders' (or, as Smith and Shepard put it, the "abstract") perspective, and their qualitative findings are used to represent the insider's perspective. Finally, the use to which they put their findings cannot be properly construed as either positivistic technical control or interpretivistic facilitation. Instead, they first listened to but then set out to challenge—as well as change—those educators whose beliefs were at odds with the research evidence in order to improve educational practice.

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Like Apple, they ran headlong into the problem of resistance. (Interested readers should consult the January and February 1988 "Backtalk" sections of *Phi Delta Kappan* to get a firsthand look.)

### Conclusion

Elsewhere (Howe 1988) I have intimated that the quantitative-qualitative debate is an *invention*, by which I meant that many educational researchers successfully go about their business unconcerned with the putative epistemological paradigm split, the "incompatibility thesis." I proposed that theories of educational research, "reconstructed logics," and educational research practice, "logics in use," have to be mutually adjusted. In this article I have extended this tack and have advanced the critical educational research model as providing a better philosophical account—in terms of a conception of explanation, a conception of human nature, and a conception of the relationship between research and practice—and as providing a better account of various current types of educational research practice—in terms of what researchers actually do and what worries them—than the "incompatibility thesis."

To forestall the possibility of misunderstanding, I should emphasize the highly prescriptive nature of the critical educational research model. My arguments were not intended to capture the whole of educational research practice, to provide a complete picture of the *is* of educational research practice. Some varieties of research, for example, research that closely approximates positivistic technical control, no doubt continues to be conducted. Given the *ought* of educational research practice, such research is simply condemned as objectionable by the critical educational research model because the model is rooted in the requirement that educational research practice must be constrained by and conducive to promoting the values associated with participatory democracy.

This is what gives rise to the "problem of resistance," endemic to the critical educational research model. How to work out the various dimensions of this problem, for instance, what to do about interlocutors who persist in beliefs and actions that run counter to their interests, or what to do about unequal power relationships between researchers and interlocutors, plagues researchers operating with a critical educational research model and offers no easy solutions (if solutions exist at all). The situation is exacerbated by the fact that the roots of the problem stretch far beyond the educational research community, and educational institutions as well.

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If there is anything inspired by the quantitative-qualitative debate that we cannot—or should not—get over, the problem of resistance has to be it. In one sense this is a welcome problem, for grappling with it helps to focus attention on a fundamental question: how to make educational research serve a democratic society.

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