

Closing Down the Conversation: The End of the Quantitative-Qualitative Debate Among Educational Inquirers

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ABSTRACT: The contention of this paper is that the claim of compatibility and the call for cooperation between quantitative and qualitative inquiry cannot be sustained. Moreover, these claims have the unfortunate effect of closing down an important conversation. To elaborate these points, this paper briefly reviews the transition from conflict to cooperation between the two perspectives and then notes how compatibility is based on a confusion over two different definitions of method. Finally, the discussion focuses on why this conversation, because it invokes issues crucial to our understanding of who we are and what we do as inquirers, must be reinvigorated.

A recent trend in the literature concerning quantitative versus qualitative approaches to research indicates two things about the nature of this debate. First, many educational inquirers now accept the idea that there are two different, equally legitimate, approaches to inquiry. Second, many inquirers also feel that whatever

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differences may exist between the two perspectives, they do not, in the final analysis, really matter very much. In other words, with the exception of the increasingly infrequent talk about conflicting paradigms or fundamental differences in basic assumptions (see, e.g., Guba, 1981, 1983; Guba & Lincoln, 1981, 1982; Heshusius, 1982; Smith, 1983a, 1983b, 1985a, 1985b), many educational inquirers now seem to think that the profession has reached a stage of, if not synthesis, then certainly compatibility and cooperation between the two approaches. The demand that an inquirer be "either/or" has been replaced by the injunction to employ both approaches in combination or to "draw on both styles at appropriate times and in appropriate amounts (Cronbach et al., 1980, p. 223).

The contention of this paper is that the claim of compatibility, let alone one of synthesis, cannot be sustained. Moreover, this hasty and unjustified "leap to compatibility" has the unfortunate effect of "closing down" an important and interesting conversation. To elaborate this contention, the paper first outlines the recent transition from con-

lict to cooperation between the two perspectives. This historical review will help clarify the basis for the claim of compatibility. Then, an important but not necessarily well-understood distinction is made between method as technique and method as logic of justification. Confusion in this area has provoked confusion about the differences between quantitative inquiry and qualitative inquiry. Finally, the implications of "closing down" this conversation are discussed.

From Conflict to Détente to Cooperation

The contemporary history of the quantitative-qualitative debate can be traced back to the late nineteenth century and the development of an interpretive approach to social inquiry, which was a reaction to positivism (see, among many others, Giddens, 1976; Hughes, 1958; Outhwaite, 1975, 1983; Polkinghorne, 1983; Smith, 1983a, 1983b). From the perspective of educational inquiry, an overview of this period of concern in regard to the two approaches can be roughly divided into three phases. The first phase, which lasted until recently, was

characterized by claims of fundamental differences in both assumptions and procedures and by an attitude that very often approached mutual disdain. The second, or transitional, phase was characterized by an acceptance of, but a decreased concern over, paradigmatic differences, a much increased emphasis on procedural issues, and an attitude of détente. In the third and present phase, the concern over assumptions is minimal. Differences are confined primarily to the area of procedures, and the attitude is one of compatibility and active cooperation.

Conflict

Dilthey's interpretive or hermeneutical approach to what was then referred to as cultural or moral sciences offered a direct challenge to positivism (Ermarth, 1978; Hodges, 1944, 1952; Hughes, 1958). He argued that there was a fundamental difference in subject matter between the natural and social areas. Whereas physical sciences dealt with a series of inanimate objects that could be seen as existing outside of us (a world of external, objectively knowable facts), moral sciences focused on the products of the human mind with all its subjectivity, emotions, and values. From this he concluded that since social reality was the result of conscious human intention, it was impossible to separate the interrelationships of what was being investigated and the investigator. There was no objective social reality as such divorced from the people, including investigators, who participated in and interpreted that reality (Bergner, 1981).

Accordingly, Dilthey distinguished between two ways of experiencing: inner-lived experience and sensory experience. The latter pertained to physical sciences, whereas the former was crucial to human studies. This meant the investigator of the social world could only attain an understanding of that world through a process of interpretation—one that inevitably involved a hermeneutical method. The meaning of human expression was context-bound and could not be divorced from context. To understand an expression, one must un-

derstand the context; and to understand the context, one must understand the individual expressions. Hermeneutics required a constant movement of interpretation between parts and whole—a process that can have no absolute beginning point or ending point.

Weber was impressed with Dilthey's position, but he did not accept the idea of a difference in subject matter between the physical and the social. Moreover, he felt, as did Dilthey, that the latter's ideas led to a major problem: If meaning must be taken within context and understanding must be hermeneutical, then the interpretation of expressions could vary. Given this prospect, could there be any such thing as a correct interpretation? In contrast to developments on the positivist side, Dilthey could find no criteria, short of an unacceptable appeal to metaphysics, to use in sorting out conflicting possibilities (Hughes, 1958).

Weber's solution to this problem was to attempt to bring together the two perspectives (Aron, 1967, 1970; Benton, 1977; Outhwaite, 1975, 1983; Simey, 1969). He thought that the realist-oriented tenets of positivism could not adequately deal with the ability to act intentionally and to ascribe meaning. Likewise, he felt that the idealist-oriented approach lacked a sufficient understanding and respect for social reality as an existing reality. Weber therefore attempted to achieve a synthesis of the two perspectives. His failure to reach this goal (Outhwaite, 1983; Simey, 1969) left the two perspectives openly and definitively apart. That proponents of both approaches continue to draw on his work, either directly or indirectly, to defend their positions reflects the failure of his attempt at synthesis.

Shortly after the turn of this century, then, two distinct perspectives on social inquiry were in competition. The quantitative tradition, with its realist orientation, was based on the idea of an independently existing social reality that could be described as it really was. Truth was defined as a correspondence between our words and that independently existing reality. Common to this perspective, which

allowed that facts were separate from values, was what Putnam (1981) calls a "God's Eye" point of view. The interpretive tradition, based on an idealist temperament, took the position that social reality was mind-dependent in the sense of mind-constructed. Truth was ultimately a matter of socially and historically conditioned agreement. Social inquiry could not be value-free, and there could not be a "God's Eye" point of view—there could only be various people's points of view based on their particular interests, values, and purposes (Putnam, 1981).

Although it is impossible to characterize the attitude of every social researcher during this first period, in general the relationship between the advocates of each perspective was uneasy. Outhwaite (1983) describes the era as one of "endemic opposition between conflicting frameworks" (p. 2). In the area of educational inquiry, Rist (1977) said the discussion had been dominated by "trite clichés" based on "simple and rigid polarities," and there had been a "continual fixation upon what is 'good' about one approach or 'bad' about another" (p. 42).

Détente

After noting the period of disdain, Rist (1977), in describing the change to détente, said that even though the two approaches had different epistemological traditions and tensions remained, "a set of accommodations is emerging whereby the various approaches . . . are recognizing the right of 'peaceful coexistence'" (p. 42). However, he specifically held that this easing of tension did not mean that synthesis would be forthcoming. Anything more than détente, he predicted, would require considerable time and much serious analysis. Looking back over the last few years, it is clear that the movement developed in ways that Rist did not anticipate. What he felt would be a long period of détente turned out to be a short-lived and basically transitional phase on the road to the claim of compatibility and the call for cooperation. An examination of the work of Guba (1978, 1981) and Guba and Lincoln (1981, 1982), even

though it may not have been intended to provoke this transition, shows how this situation came about.

Guba and Lincoln were among the first who developed a set of extensive and detailed criteria by which to judge the good from the not so good in naturalistic or qualitative inquiry. Although they accept the idea that the two approaches differ at the paradigmatic level and find naturalism more sound than the rationalist side, the tone of disagreement with the latter paradigm is quite moderate. They observe that the rationalist approach has not produced significant results, that its axioms are only poorly fulfilled in the area of social and educational inquiry, and so on, but these points are merely noted. There is no demand to abandon rationalism in favor of naturalism (Guba & Lincoln, 1982). In general, while they obviously find naturalism superior, the spirit is one of détente rather than one of provoking conflict.¹

Another aspect of their work has assisted others in making claims for compatibility and cooperation, even though this was not necessarily the intended result. Their frame of reference for elaborating criteria and procedures is not only the assumptions they posited for naturalistic inquiry, but also the criteria and procedures that characterize rationalist inquiry. This situation produces a distinct tension in the discussion of criteria and procedures: The assumptions are epistemologically antifoundational, but foundational rules are offered as a means to sort out the good from the bad in qualitative inquiry.² The problem is that these two elements are incompatible (Smith, 1985a).

Guba and Lincoln structure their discussion of naturalistic criteria and procedures with close reference to the "comparable" criteria and procedures of the rationalist side. The resulting "parallelism" can be clearly seen in their discussion of the elements of the trustworthiness of naturalistic inquiry: Credibility is compared to the rationalist concern over internal validity, dependability to reliability, confirmability to objectivity, and so on (Guba, 1981). Although this parallelism is not

nearly as complete and direct as it later is for other authors, it is sufficient to direct attention away from basic differences in the philosophical assumptions of the two perspectives. Thus, one can easily get the impression that the two approaches are variations in techniques within the same assumptive framework, to reach the same goals and solve the same problems.

A brief look at the question of "truth value" of inquiry will demonstrate how this impression can be gained. For the rationalist approach, truth value is a matter of internal validity, which in turn is a question of isomorphism between the data (or the inquirers' statements) and an independently existing reality that the data reflect (Guba, 1981). This in effect defines truth value in correspondence terms. For naturalism, truth value is a matter of credibility. The idea of isomorphism is also employed, but in this case it is isomorphism to the perceptions or interpretations a person gives to a situation—an interpretation that, given the idea that reality is mind-constructed, becomes reality as it is for that person at any given time and place. Thus, rationalism can achieve truth value to the extent that an inquirer's statements correspond to how things really are, whereas naturalism can achieve truth value to the extent that an inquirer's statements correspond to how people out there really interpret or construct their realities.

Internal validity and credibility begin to look like the same thing. To fulfill the injunction in either case requires a belief in the assumption that what is known—be it an existent reality or an interpreted reality—stands independent of the inquirer and can be described without distortion by the inquirer's interests, values, and purposes. In other words, isomorphism makes sense as a criterion only if one accepts the rationalist position on the separation of mind and world with the associated ideas about neutrality and objectivity. Naturalistic assumptions—that reality is mind-constructed and that facts cannot be separated from values—undermine the possibility of isomorphism, neutrality, and objectivity, as de-

fined from the rationalist perspective. According to naturalism, an inquirer can offer only an interpretation (based on values, interests, and purposes) of the interpretations of others (based on their values, interests, and purposes). Recalling Dilthey, this epistemologically antifoundational process can have no definitive beginning or ending points.

Just as credibility seems to be more compatible with rationalist assumptions, so it is for Guba's (1981) other criteria, such as applicability (transferability-external validity), consistency (dependability-reliability), and neutrality (confirmability-objectivity). In each case, the former concept can be read as a restatement, in different terminology, of the latter concept. Hence, the attempt to develop criteria and procedures for naturalistic inquiry has had the effect of blurring philosophical distinctions and has left the impression that the differences between the two perspectives are primarily those of techniques.

Compatibility and Cooperation

Whereas Guba and Lincoln held to fundamental differences at the paradigmatic level, the present tendency is to either ignore such differences or, if they are noted, to assign them little importance, at least from the point of view of research practice. For many inquirers, the most pressing problem now is to develop criteria and procedures that will do for qualitative inquiry what certain criteria and procedures have done for quantitative inquiry (act as a constraint on our subjective selves, allow for the possibility of certitude, etc.). As a result, the quantitative-qualitative debate has become a discussion of variations in techniques within the same logic of justification. Both approaches are thought of as having the same goals; both are interpreted in the same way, and only the aspects of practice differ. Not surprisingly, détente has given way to the claim of compatibility and the call for cooperation.

LeCompte and Goetz (1982) provide an excellent example of how differences in philosophical assumptions are, for the most part, ig-

nored. Their emphasis is on the various techniques that qualitative inquirers can employ to make their approach as rigorous and systematic as quantitative inquiry. The discussion is structured in an almost totally parallel fashion: If this is how a certain criterion can be met for quantitative inquiry, then this is how the same criterion can be achieved for qualitative inquiry.

LeCompte and Goetz (1982) interpret the basic concepts of validity and reliability in the same way for both qualitative and quantitative inquiry. Validity is a matter of accuracy of representation, or matching "explanations of the world with the actual conditions in it" (p. 43) for both approaches, and reliability is a matter of replication for both. To accept these definitions, one must accept realist assumptions such as the separation of investigator-investigated, truth as correspondence, and so on. If, on the other hand, one conceptualizes reality from an idealist stance as mind-dependent, these definitions are impossible to bring into focus. Such a conceptualization disallows what is necessary to determine whether or not our explanations correspond to actual conditions—independent access to both our minds and to an independently existing, uninterpreted reality (on this point, see, among many others, Goodman, 1978; Putnam, 1982; Rorty, 1979). From an idealist perspective, all one can do is match descriptions with other descriptions or conceptualizations. Validity is best thought of as an "honorific" conferred on one explanation, from among others, with which one agrees. (For an interpretation of these ideas with regard to test validity see Smith, 1985b.)

The differences between the two approaches, for LeCompte and Goetz (1982), are confined to the area of the techniques each side employs to fulfill the standard realist criteria of validity and reliability. For example, quantitative inquiry deals with the problem of testing and instrumentation effects by developing and properly applying standardized instruments. The parallel problem of observer effects for qualitative inquiry can be alleviated by, among other things, a check and recheck of the mean-

ings of the investigator's interpretations with the subjects. In another example, what quantitative inquiry is able to obtain through the use of instrumentation, sampling procedures, and so on, qualitative inquiry achieves through the technique of amassing detail (thick description) based on extended engagement in the field.³ LeCompte and Goetz, by ignoring paradigm differences and adopting this explicit parallelism, make qualitative inquiry little more than a procedural variation of quantitative inquiry, one within the same paradigmatic framework.

Whereas LeCompte and Goetz basically ignore paradigmatic differences in pursuit of procedural differences, Miles and Huberman (1984a) recognize philosophical assumptions as important, but then "de-epistemologize" the debate as they move on to their principal concern of how to do qualitative inquiry. They note that even though epistemological issues constitute more than a "nontrivial battle," researchers need not be preoccupied with it. Among the reasons cited are that the epistemological debate will not be resolved in the near future, that researchers already "blend" the two perspectives, and that such paradigmatic problems divert attention from the critical aspect of developing a "body of clearly-defined methods for drawing valid meaning from qualitative data" (p. 21). Thus, as far as philosophical questions go, they recommend that researchers be epistemologically ecumenical and leave the larger debate to those who are most interested in it. This position leaves them free to label themselves as "middle-range epistemologists," "soft-nosed positivists," and "right-wing qualitative inquirers," and then proceed to various techniques that will make qualitative inquiry "scientific in the positivist sense of the word" (p. 21).

The appeal of this position may be that it appears to "free-up" researchers from some nagging questions and to allow them to "get down to business." As Miles and Huberman (1984a) express this point, "epistemological purity doesn't get research done" (p. 21). Reinforcement for such a position has also been taken from the reac-

tion of practicing natural scientists to Kuhn's work, which has had little or no impact at the level of practice—only philosophers were upset. However, this lack of reaction actually lends little weight to the argument. Positivist social and educational inquiry has not achieved the same intellectual and practical mastery of subject matter as has been the case for natural science. If it had, in fact, one could easily argue that the qualitative-quantitative debate would not have reached the proportions it has. At any rate, since social inquiry, unlike natural science, cannot stand behind what are generally considered obvious or self-evident accomplishments, it is not clear what business one is left to "get down to" by becoming epistemologically ecumenical.

To bypass paradigmatic questions in this manner leaves many crucial concepts defined in the same way for both perspectives. Although there is some ambiguity in this area, Miles and Huberman (1984a) do seem to accept that concepts such as valid, real, dependable, and trustworthy can be defined in the same way for both sides. The ambiguity arises because they employ new terms such as *truth space* and vague terms such as *wholly contradictory findings*, and *reasonably communicable sets of procedures*. The use of these terms may serve to bring about a "loosening" of the constraints, an avoidance of a highly "mechanical orthodoxy," but such terms do not constitute a recognition that different paradigms lead to different interpretations of these concepts and hence, of inquiry and its results.

That Miles and Huberman (1984a) confine the differences between the two perspectives to the area of techniques can also be seen in their discussion of methods. Even though they do not adopt a strict parallelism, there is a good deal of "mirroring" (Marshall, 1984) of quantitative techniques in their discussion of criteria for qualitative inquiry. With some qualifications, they state that established methods will prevent self-delusion, prevent inquirers from falling prey to bias and deception, and serve as a basis for judging the reasonableness of conclusions. Moreover, such a set of valid

methods, they assert, will allow for the discovery of lawful relationships in the social world—relationships that exist independently of researchers and that can be known as they really are. Thus, this approach, with its epistemological ecumenicalism and methods that will make qualitative inquiry “scientific in the positivist sense of the word” (p. 21), is little more than a variation on the quantitative theme. Like LeCompte and Goetz, Miles and Huberman transform the paradigmatic debate into a discussion of methodological variations within a realist philosophical temperament.

Summary

What began as a significant debate between two different approaches to inquiry based on different philosophical assumptions has become an “in-house” discussion of no pressing concern. The paradigmatic differences that require different interpretations of inquiry and different evaluations of its results are no longer taken seriously. The principal concern now is how to generate methods for qualitative inquiry that will allow this approach to claim the same objectivity and certitude presumably available to quantitative inquiry. This disregard of assumptions and preoccupation with techniques have had the effect of transforming qualitative inquiry into a procedural variation of quantitative inquiry. The former perspective has been captured by the latter perspective in the sense that both have come to share the same realist-oriented assumptions. Given this situation, the claim of compatibility and the call for cooperation are not surprising; as presently conceptualized, the two perspectives do not differ in any important ways.

Misconceptualizations About the Issue

Why was the original paradigmatic debate so rapidly transformed into a discussion of variations in techniques? Two factors are important: (a) a confusion over the definition of method and (b) an uncritical dependence on the idea that inquiry is a matter of “what works.”

Method can be characterized in at

least two ways. The most commonly encountered meaning is method as procedures or techniques. In this case the term invokes the kinds of “how-to-do-it” discussions long found in introductory textbooks on quantitative inquiry and, more recently, in a number of basic textbooks on qualitative inquiry (see, e.g., Goetz & LeCompte, 1984; Miles & Huberman, 1984b). The second characterization of method is as “logic of justification.” In a sense common to continental European social philosophy, the focus here is not on techniques but on the elaboration of logical issues and, ultimately, on the justifications that inform practice. The term is used in this sense in the work of Durkheim (1983), Weber (1949), and, more recently, Kaplan (1964) and Giddens (1976). This conceptualization involves such basic questions as, What is the nature of social and educational reality? What is the relationship of the investigator to what is investigated? and How is truth to be defined?

The important issue, then, is how one characterization of method relates to the other in the case of each approach. These relationships can be posed as a question: What does the logic of justification attendant to each perspective have to say about the practices one engages in in each case and vice versa? The point is that method as logic of justification, involving as it does basic philosophical assumptions, informs method as technique, and the two terms cannot be used interchangeably. To examine this situation we must first reaffirm the fact that major differences exist between the two perspectives at the level of logic of justification.

At the level of applying *specific individual procedures*, however, there are some relatively uninteresting questions about the differences between the two perspectives. In this case one frequently sees questions such as, Can quantitative inquirers supplement their controlled instrumentation with open-ended observation in naturalistic settings? or, Can qualitative inquirers supplement naturalistic observation with the quantification of events? The answer to these types of questions is yes. In both

cases, but especially for qualitative inquiry, the logic of justification does not impose detailed boundaries that determine *every single aspect* of practice. Researchers of a realist orientation are not prohibited from using a certain practice normally associated with qualitative inquiry and vice versa. Thus, if the issue of quantitative-qualitative were confined to this level, one could grant that authors such as Cronbach et al. (1980), Miles and Huberman (1984a), and Reichardt and Cook (1979) are correct in their claim that the two approaches can be “mixed.”

However, acceptance of this point cannot lead to the conclusion, at least implicitly made by many people, that the two perspectives are compatible or complementary. For quantitative inquiry and now, erroneously as it turns out, for the compatibility phase of qualitative inquiry, certain *sets* of practices, as opposed to particular, individual ones, are thought necessary to establish major conditions of inquiry such as the validity and reliability of studies. Since achieving these major conditions for either perspective is thought of as depending on the proper application of ordered practices, we can examine these concepts as “linkage” points between method as logic of justification and method as “how-to-do-it.” The crucial issue, however, is that how one characterizes these conditions depends not on the techniques employed, but rather on the logic of justification one accepts. That is, the meaning assigned to the term *valid*, as in the statement, “this study is internally valid,” is taken not from the practices involved but rather from how truth is defined. The epistemological position constrains how the condition is conceptualized and, by extension, directs the particular set of techniques that must be performed to achieve that condition (that is, as shall soon be noted, if any procedures can be so privileged for qualitative inquiry).

Clearly, if the two perspectives define truth differently, not only must each accept a different conceptualization of validity, each must hold to a different interpretation of the place of procedures in the claim to validity. For the quantitative approach, in which truth is defined as

correspondence, the label valid announces results that reflect or correspond to how things really are out there in the world. Moreover, a judgment of validity in this case is conferred only when proper methods or sets of techniques are employed. In fact, procedures properly applied, in that they ensure objectivity and so on, lead to results that are thought to be compelling. Rejection of such results may provoke the criticism that one is being irrational or stubbornly subjective. For quantitative inquiry, a logic of justification that is epistemologically foundational leads to the position that certain sets of techniques are epistemologically privileged in that their correct application is necessary to achieve validity or to discover how things really are out there. Accordingly, for quantitative inquiry, techniques stand separate from and prior to the conduct of any particular piece of research.

From the perspective of qualitative inquiry, this line of reasoning is unacceptable. The assumptions or logic of justification in this case are not foundationalist and, by extension, do not allow that certain sets of procedures are epistemologically privileged. The idealist-oriented assumptions of reality as mind-dependent, no separation of facts and values, truth as agreement, and so on, are antifoundational; they undermine the prospect of independent access to an independently existing reality and, in so doing, undermine the possibility of certitude. Since reality is mind-dependent, a description can only be matched to other descriptions and not to an unconceptualized reality. This recalls the Dilthian issue noted earlier: Since meaning is taken within context (that of the subjects, the investigator, those who read the investigation, and so on) and the process is hermeneutical, on what basis does one choose from among descriptions? In other words, if all we have are various interpretations that are the realities of various people based on their various interests, values, and purposes, what meaning must be given to *valid* and how does one judge an interpretation valid or invalid?

Within the qualitative paradigm, valid is a label applied to an inter-

pretation or description with which one agrees. The ultimate basis for such agreement is that the interpreters share, or come to share after an open dialogue and justification, similar values and interests. As Taylor (1971) puts it,

Ultimately, a good explanation is one which makes sense of the behavior; but then to appreciate a good explanation one has to agree on what makes good sense; what makes good sense is a function of one's readings; and these in turn are based on the kind of sense one understands. (p. 14)

There is a circularity to this interpretive process (the hermeneutical process can have no definite beginning or ending points) that one cannot break out of, even by methodological prescriptions.

Whereas the foundationalist assumptions of quantitative inquiry allow that proper procedures will prevent this circularity (allow for certitude), such is not the case for qualitative inquiry. The antifoundational assumptions mean that procedures are related to the context of a particular inquiry and what it makes sense to do in that particular context. Of course, what makes sense in any particular situation depends on the kind of sense one understands. One may be interested in how a researcher did a study and agree with the strategies employed, or argue that others should have been employed. However, this is different from the claim that certain procedures are necessary to establish a correspondence of our words with an independently existing reality. Qualitative inquiry does not stipulate that certain things must be done or that validity is a matter of techniques properly applied.

Quite simply, a confusion of method as logic of justification with method as "how-to-do-it" has allowed people to draw, even if only implicitly, an erroneous conclusion. That certain individual procedures can be mixed does not mean there are no differences of consequence. If one extends the different sets of assumptions to their logical implications, it is clear that the two perspectives part company over major issues such as the conceptualization given such basic conditions as validity and reliability, the place of

techniques in the inquiry process, and the interpretation of research results. Quantitative inquiry aspires to certitude, to the idea that our descriptions can match actual conditions in the world and that we can know when this matching occurs and when it does not. This certitude is achieved primarily through an adherence to proper techniques. For the qualitative perspective, inquiry is a never-ending process (hermeneutical) of interpreting the interpretations of others. All that can be done is to match descriptions to other descriptions, choosing to honor some as valid because they "make sense," given one's interests and purposes. There is no rule book of procedures to follow.

Finally, the claim of compatibility is based not only on confusion over the definition of method, but also on the idea that research is a matter of what works. This idea, expressed in different forms, is present in many discussions of qualitative-quantitative inquiry. For example, this is in essence what Miles and Huberman (1984a) mean when they say that "epistemological purity doesn't get research done" (p. 21) and what Reichardt and Cook (1979) mean with the comment that one should mix the approaches in order to "satisfy the demands of evaluation research in the most efficacious manner possible" (p. 27). Although this idea is appealing, in that it calls up the image of educational researchers using whatever is necessary to solve serious educational problems, on closer examination it is also an oversimplification.

The problem is that what works, no matter how it is expressed, really tells us nothing about the process of inquiry and the interpretation of its results. Putnam's (1981) discussion of the phrase "science seeks the truth" illustrates this point. According to Putnam, this is an empty statement in the absence of knowing

... what [people] consider a rational way to pursue inquiry, what their standards of objectivity are, when they consider it rational to terminate an inquiry, [and] what grounds they will regard as providing good reason for accepting one verdict or another on whatever sort of questions they may be interested in (p. 129).

In other words, "truth is not the bottom line" (p. 130), because it derives its standing from the goals one accepts for inquiry, the criteria to be applied, and so on.

Similarly, the idea that research decisions can be made on the basis of what works is not the bottom line. These decisions depend on the goals one holds for inquiry, the criteria (if any) employed for judgments, and so on. If one holds that inquiry is a matter of matching statements to actual conditions, what works will differ from what works if one finds inquiry to be interpretations of the interpretations of others. It may be argued that this problem does not arise, because all inquiry is based on criteria such as scope, fruitfulness, simplicity, and accuracy. However, as Kuhn (1977) noted, these criteria are value terms and may be interpreted differently, depending on the situation or context. As he said, "Individually the criteria are imprecise: individuals may legitimately differ about their application to concrete cases" (p. 322). In the end, what works is not a firm foundation to stand on. What works depends on the kind of work one wants inquiry to do, which in turn depends on the paradigm within which one is working.

Summary and Implications

During the last few years the quantitative-qualitative issue has gone from a situation of conflict to one of compatibility and cooperation. The contention of this paper is that this transformation is not based on the development of a legitimate *via media* between the two perspectives, but is actually a matter of the "capture" of qualitative inquiry by the quantitative approach. Both perspectives are now thought of—even if only implicitly—as sharing the same realist-oriented assumptions. This transformation has been aided by a confusion over the definition of method and by the unfounded idea that what works can serve as a self-evident bottom line for decisions about inquiry. Many educational researchers have taken the unfortunate conclusion that there are no longer any significant differences between the two perspectives.

Why does it matter that the conversation is being closed down? This question can be answered on two levels. On an abstract level, the conversation is important because ultimately it involves one of the most provocative and widespread intellectual challenges of our time: What is to be made of the issue of objectivism versus relativism? On a practical level, and of course very much related to the abstract issue, the conversation provokes some fundamental issues concerning the practice of inquiry, the evaluations of and interpretations given to the results of inquiry, and so on.

The struggle of objectivism versus relativism is one of the most intriguing and significant issues posed to both our intellectual lives and to our lives as practical and moral beings. As Bernstein (1983) notes, this debate involves "some of the more perplexing questions concerning human beings: what we are, what we can know, what norms ought to bind us, what are the grounds for hope" (p. 4). The essence of the issue is whether or not there is a firm foundation on which we can build our knowledge. Is there a way to describe nature in its own terms, or are we faced with various descriptions based on different sets of interests and purposes? Behind this question lies what Bernstein calls the Cartesian Anxiety of either/or: Either we have such a foundation for knowledge, or we are adrift in a world where nothing binds us, where there are no criteria of right and wrong, correct and incorrect, and where all is reduced to a clash of taste and opinion.

No discussion of qualitative and quantitative inquiry can stray very far from the challenge of objectivism and relativism. Quantitative inquiry has long laid claim to being foundational, to being capable of allowing us to describe social and educational life as it really is. However, one of the crucial elements supporting this foundationalist claim—that we can have correspondence between our statements and reality and know when this occurs—has recently fallen on hard times. Many people now see a desire for correspondence as it always has been: Since correspon-

dence requires independent access to both domains of mind and an independently existing reality, which we cannot have, we are witnessing the "demise of a theory that lasted for over two thousand years" (Putnam, 1981, p. 74). If we are "left without the God's Eye View" (Putnam, p. 74), we must raise the question, What then for quantitative inquiry?

The objectivism-relativism debate brings to qualitative inquirers equally serious questions of their own. At the end of the trail begun by the assumptions of reality as mind-dependent lies the problem, or spectre, of relativism. If there is no God's Eye view, and one description based on values and interests can only be matched to other descriptions based on other values and interests, are we trapped in a never-ending circle of descriptions? Does qualitative inquiry leave us with the prospect that we cannot break out of the circle and that there are no criteria and procedures to be specified to prevent "anything goes"? Qualitative inquiry, in refuting that the rationality of inquiry can be derived from method (as practices), must address the implications of its antifoundationalist assumptions. However, it must do so without slipping on the mantle of quantitative inquiry and its foundationalist assumptions. Given the demise of truth as correspondence, there clearly is no refuge there. Thus, the debate over research approaches is very much a part of the intellectual challenge of objectivism versus relativism. In the presence of so many unanswered questions, the conversation should not be closed down.⁴

At a more practical level, to close down the conversation is to avoid numerous important considerations. Clearly, the interpretation given to the practices and results of research differs, depending on the logic of justification one accepts. The phrases "research has shown..." and "the results of research indicate..." are subject to different interpretations, given different paradigms. For quantitative inquiry, these phrases are claims to an accurate reflection of reality or the claim of certitude that one has discovered how some bit of the

social or educational world really is. For qualitative inquiry, these phrases announce an interpretation that, to the extent that it finds agreement, becomes reality for those people as it is at any given time and place. The former expresses certitude; the latter presents a description constrained by values and interests to be compared with other descriptions constrained by other values and interests.

Given these differences, on what basis can researchers justify their work to the public, to educators, and, for that matter, to themselves? Do researchers, as is commonly held, deserve a hearing because method places them above the subjectivity common to everyday discourse and thus allows them to speak of things as they really are? Qualitative inquiry, based on the point that reality is made rather than found, must challenge this idea. Very little within this perspective would permit educational researchers to claim "epistemological privilege" and, accordingly, to claim that they have a special right to be heard or that they deserve an overriding voice in the conversation. Perhaps research, in an allusion to Oakshott (1975), is nothing more or less than another voice in the conversation—one that stands alongside those of parents, teachers, and others. Whether one finds this characterization acceptable or unacceptable is at present unimportant; the point is that the quantitative-qualitative debate raises serious questions about the meaning of research results.

These differences easily spill over into the issue of how researchers resolve disagreements among themselves over the results of inquiry. If quantitative inquirers disagree, they hold out the possibility that a return to the facts of the case will sort out the problem. Within the "neutral arena" of the facts, with the use of a neutral scientific language, decisions of right and wrong, in the sense of accurate or inaccurate mirroring, can be taken. Qualitative inquiry, on the other hand, has no independent or "brute" facts to appeal to since reality is mind-dependent. If people agree, it is because they share

similar values, interests, and purposes—not because there are any "givens" that compel agreement.

While this leads to various problems of interest, including whether educational inquiry is more at home with the natural sciences or with the humanities, time and space allow that only one more implication be noted. Given the fundamental difference in the approach to disagreement, if a quantitative inquirer disagrees with a qualitative inquirer, is it even possible for them to talk to each other? The answer, for the present anyway, is a qualified no. An appeal that one must accept a particular result because it is based on the facts will have little impact on one who believes there can be no uninterpreted facts of the case. On the other hand, the idea that facts are value-laden and that there is no court of appeal beyond dialogue and persuasion will at the very least seem unscientific and insufficient to a quantitative inquirer. In the end, the two sides may be close to speaking different languages—a neutral scientific or value-free language versus a value-laden language of everyday discourse. Since it is not clear at present what kind of *via media* could be worked out between the two languages, it is all the more important that we make every effort to keep the conversation open. In any event, all of the issues that surrounded Weber's unresolved question, "How can there be an objective science—one not distorted by our value judgments—of the value-charged productions of men?" (Aron, 1967, p. 193), are again brought to the foreground by the quantitative-qualitative debate.

This brief overview could, of course, be extended into many other areas. However, enough has been said to demonstrate why it is unfortunate that the conversation is being closed down. At this stage, the point is not that anyone knows exactly what will result from the conversation or what inquiry will look like in the future, but rather that to avoid the conversation is to avoid issues at the core of the research enterprise and, for that matter, at the core of our contemporary intellectual, practical, and

moral lives. Moreover, since these issues are crucial to who we are and what we do as researchers, this is not something to be turned over to philosophers with the hope they will eventually solve our problems. Put quite simply, to close down the conversation by making the unjustified claims of compatibility and cooperation is the wrong move at the wrong time.

Notes

¹In other instances, Guba has been more aggressive in his criticism of the rationalist paradigm and less accepting of *détente*. In a 1983 paper, for example, he strongly criticizes the idea of compromise, bluntly states the situation as one of "either/or," and ends with a call for confrontation.

²In places Guba (1981) disclaims the intention to be foundationalist, saying that his criteria are not prescriptions and that he does not want an "orthodoxy." However, there is considerable evidence that he desires his criteria to be more than simply characterizing traits. Thus, he refers to these criteria as "rules," as "safeguards," and says that they are to be applied whenever we judge naturalistic inquiry.

³The above comments do not imply that such things as thick description and meaning or member checks are necessary or, for that matter, unnecessary. Under certain conditions such strategies may very well be important, but in other situations they may not be. Such strategies do not have the "epistemological privilege" that is attached to quantitative techniques. Given the assumptions of the qualitative perspective, these strategies cannot have the status of "that which one must do" to be objective and attain an accurate reflection of how things really are out there. This issue is discussed in greater detail in a following section, "Misconceptions about the Issue."

⁴It is interesting, and somewhat puzzling, to note that educational researchers have moved away from this debate at a time when it is of major concern across a broad intellectual horizon. These issues, even though expressed in different ways, are of concern in anthropology (Geertz, 1980), history (Stone, 1979), linguistics (Lakoff & Johnson, 1980), political thought (Fay & Moon, 1977), philosophy/philosophy of science (among many others, Bernstein, 1983; Goodman, 1978; Rorty, 1979), physics (Wheeler, 1975), and psychology (Yates, 1985).

In recent years, the *Chronicle of Higher Education* has contained two

pieces describing aspects of this conflict (Winkler, 1984, 1985). Finally, various novelists such as Durrell (1958-1960) and contemporary Latin American writers such as Donoso (1984) and García Márquez (1970, 1975) have taken as central to their visions variations on these themes of relativism, reality as actively constituted, and so on. To quote Donoso: "The goal was not to trap the children inside this reality he was inventing, but rather, when they returned, the Venturas themselves. A more ticklish job to be sure. But since after all it is the laws that create reality, and not the other way around—and since whoever wields power creates the laws—it was simply a matter of preserving authority" (p. 233).

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