The Problem of Criteria for Judging Interpretive Inquiry

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Over the years various interpretive (or qualitative, naturalistic, etc.) investigators have been concerned with developing criteria for judging, and defining procedures for doing, this type of investigation. Apparently the aim is to establish some set of foundational criteria to separate the good interpretive study from the not so good. These criteria and procedures would help advocates of this approach refute charges of subjectivity and allow them to lay claim to that certitude that is available, at least as it is generally presumed, to their quantitatively oriented counterparts. Given the assumptions conventionally made to distinguish interpretive from quantitative inquiry, this paper takes the position that the quest for foundations may be a misguided one. If the assumptions and their implications are taken seriously, arguments that attempt to establish such nonarbitrary criteria and/or procedures will, perhaps inevitably, be marked by confusion and inconsistency. To defend this contention, this paper will begin with a brief historical overview of the issue. This discussion will then be followed by a specific example of the problem and an examination of why inconsistency mars our attempts to develop criteria given certain assumptions.

Historical Context

In that this issue of assumptions and criteria (and thus the problem of certitude) is not a new one for this area of interpretive inquiry, a brief historical note will help to clarify the nature of the problem. One might easily argue that the tenor of our present discussions was established around the turn of this century by, among others, Dilthey and Weber. Dilthey deserves mention in that he presented one of the first serious and sustained challenges to positivism and, in doing so, elaborated a different approach to social inquiry based on assumptions quite different from those of positivism (Hodges, 1944, 1952; Smith, 1983). Weber merits attention in this instance not for his numerous important achievements, but for the legacy left by his substantial, but nonetheless unsuccessful, attempt to develop a social science that combined a focus on subjective meaning with the desire for universally valid judgments (Aron, 1967, pp. 185–216).

Dilthey argued that social inquiry should not be conducted with the methods of the physical sciences because there was a fundamental difference in subject matter. Whereas the physical sciences dealt with a series of inanimate objects that could be seen as existing independently of us (a world of independent but objectively knowable facts), the cultural sciences focused on the products of human minds. Those products were inevit-

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ably and intimately connected to human minds—including those of the investigators—with all of their emotions, values, subjectivity, and so on. Accordingly, since society was the result of conscious human intention, the interrelationships of what was investigated and the investigator were impossible to separate. For Dilthey, there simply was no objective social reality as such, that is, divorced from the people who participate with others in constructing or interpreting reality. Bergner (1981), in his discussion of Dilthey, has put the situation as follows: “We cannot meaningfully stand outside of the events and occurrences of life and say with certainty what they signify. What we can know, however, is that historical and social events do signify things to those who enact them and to those who endure them” (p. 64).

Dilthey clearly believed that positivism erred by making the discovery of social laws, analogous to physical laws, the principle goal of social inquiry. He said that, if nothing else, the complexity of the social makes it impossible to find such laws. More important, he also argued that to find laws should not even be the point of the cultural studies. The primary concern in this case was understanding (verstehen). This point was the basis for his discussion of the ideas of lived experience and that understanding required a hermeneutical perspective. In his discussion of the latter idea he noted that, at least at one level, to understand the experiences of another required a constant movement or interplay between the individual expression and the context (hermeneutical circle)—a process with no absolute beginning or absolute ending points.

It is here, however, that Dilthey faced a problem that he was unable to resolve. If meaning must be taken in context and understanding must be hermeneutical, clearly the interpretation of expressions could vary. Given this position, could there be any such thing as a correct interpretation? Dilthey could find no foundational standards to apply for sorting out conflicting possibilities and, at the same time, he rejected an appeal to transcendental values or to a metaphysical solution. Whereas positivism had developed standards against which to judge true from false or correct from incorrect, Dilthey was caught between assumptions that are epistemologically antifoundational and a desire for criteria that are foundational. In other words, his idealist tendencies led to a relativism which he found unacceptable, but from which he could find no escape (Hughes, 1958, p. 199).

Even though Weber obviously disagreed with Dilthey in various ways (e.g., Weber felt the two sciences differed not because of an inherent subject matter difference, but rather because of a different interest taken in the subject matter), he was influenced by Dilthey’s ideas of understanding and interpretation (Roche, 1973; Wrong, 1970). However, Weber realized that this idealist-orientated position, in contrast to a realist-orientated positivist position, brought with it a problem of certitude. He therefore attempted to find a middle ground, in the sense of a synthesis, between the two approaches. In his conception the problem was that a Dilthean approach paid too little attention to existent social reality, whereas the other perspective devoted too little attention to the ideas of subjective meaning and social reality as an interpreted reality. Aron (1967) has concisely summarized Weber’s project in this area: “How can there be an objective science—one not distorted by our value judgments—of the value-charged productions of man? This is the central question Weber asked himself and to which he tried to provide an answer” (p. 193).

The answer Weber provided cannot be discussed in all of its detail and richness. Suffice it to say that one of the central tenets of his argument (along with the concept of “ideal type”) was the separation of the idea of value relevance from that of value judgment. Weber accepted that social reality is meaningful only to the extent that it is based on values. Moreover, he said that “without the investigator’s evaluative ideas, there would be no principle of selection and no meaningful knowledge of the concrete reality” (1949, p. 82). Yet, at the same time he argued that universally valid knowledge is possible because “all research in the cultural sciences … once it is oriented to a given subject matter … and has established its
methodological principles, will consider the analysis of data as an end in itself" (1949, p. 112). In other words, Weber held that even though values are the basis for meaning and are crucial to the content of inquiry, this does not mean that the inquirer must evaluate or make value judgments about what is investigated.

That Weber's arguments for a distinction between value relevance and value judgment, and more generally for the separation of facts and values, cannot be sustained (and even ends in contradiction), is not unrecognized. Various scholars, arguing from a rather wide variety of different positions, have made substantial critiques of Weber's distinctions and thereby, at least indirectly, of his attempt to synthesize the two perspectives (see, e.g., Habermas, 1968; Hindess, 1977; Lukacs, 1971; Rorty, 1982; Strauss, 1953). Simey (1969), however, has put the situation as clearly as anyone: "Weber, therefore, was attempting to find a via media between, on the one hand, realism (in its positivistic form), and, on the other, philosophical idealism. He accepted both alternatives, in the sense that he sought to establish a synthesis between them. But the synthesis in fact eluded him" (p. 83).

The question of importance for our purposes is, with what consequences for the development of social inquiry?

Two things, very much related, follow from Weber's "failure of synthesis." First, his inability to find a via media may well indicate that such a position cannot be found. Certainly since his time social inquiry has been characterized far less by synthesis and far more by "an endemic conflict between competing frameworks" (Outhwaite, 1983, p. 2). The two approaches have moved along separate lines, differing not only in technique, but also in the terms of their philosophical temperaments or "logics of justification." What we refer to as the quantitative approach is based on an external perspective—facts separate from values, truth as a matter of correspondence to an independently existing reality, and the idea that we may adopt a "God's eye" point of view. The qualitative perspective is based on an internalist position—truth as socially and historically conditioned agreement, social reality as mind constructed, and the idea that the only point of view we can take is that of various people based on their various interests, purposes, and so on (Putnam, 1981, pp. 49-74).

Second, in one way Weber's failure is an expression, albeit in different form, of the Diltheyan problem of combining assumptions that are epistemologically antifoundational (meaning as subjective, etc.) with the attempt to find epistemologically foundational criteria (certitude). Weber's via media was really a matter of bringing together a frame of reference based on subjective choice and procedures that would yield universally valid judgments. In other words, how can one maintain or have the kind of certainty claimed by and generally grated to the realist perspective in the face of a concern over meaning and intention? Dilthey's problem became, even in a different form, Weber's problem and, more important for our purposes, it is the problem that continues to be at the center of any discussion of interpretive approaches to social inquiry.

Finally, one additional comment is needed about this historical context. Over the years numerous schools of thought have been put forth as the basis for the interpretive forms of inquiry. The names of these schools are quite well-known (phenomenological, ethnomethodological, hermeneutical, etc.). Clearly it would be a mistake not to recognize that there are differences among these schools. However, it would also be a mistake, possibly even a more serious one, not to recognize their similarities. A good case can be made that the "insights...from these schools of thought...stand close to philosophical idealism" (Giddens, 1976, p. 155). They all, in at least some important aspects such as the idea that meaning must be grasped hermeneutically, are part of a broad movement away from empiricism along the lines of an idealist orientation. As Benton (1977) summarizes, "The philosophical sources of humanist approaches in the social sciences at first glance appear to be very heterogeneous—phenomenological, ethnomethodological, existentialist... But it turns out that most of these philosophical traditions are related, through either historical origin, or conceptual affinities or..."
both to the work of... Kant” (p. 101; see also, Bernstein, 1976; Bleicher, 1982; Hughes, 1980; Levy, 1981; Outhwaite, 1975, 1963; Psathas, 1973; Roche, 1973).

A Contemporary Example

To clarify, in more tangible terms than have been employed up to this point, the nature of this problem of criteria, an example of some length and detail is necessary. While many people have discussed, in different forms and with different degrees of aggressiveness, the topic of criteria and/or procedures for interpretive inquiry (e.g., Bogdan & Biklen, 1982; Bruny, 1966; Glaser & Strauss, 1967; LeCompte & Goetz, 1982; Johnson, 1975; Wolcott, 1975), an article by Guba (1981) merits special attention (see also, Guba, 1978; Guba & Lincoln, 1982a, 1982b, 1982c). Unlike some authors, Guba discusses some of the specific assumptions underlying a naturalistic approach and then elaborates the criteria to be used and the procedures to be employed for this mode of inquiry. His article is one of the more precise and concise treatments of this topic to be found, especially in the educational research literature. Hence, Guba’s work will serve as an excellent vehicle with which to demonstrate the problems that can occur when one accepts certain assumptions and then attempts to elaborate certain criteria for judging naturalistic research.

Guba (1981) begins by noting that the rationalist and naturalist paradigms are based on different assumptions (pp. 76–79). Rationalism assumes that a single reality exists, whereas naturalism assumes there are multiple realities. The relationship of the investigator to what is investigated is another area of contrast. In rationalism the object of study is seen as existing independently of the investigator. Reality is assumed to be self-subsistent, a separate entity whether or not it is an object of interest or study. In naturalism, the investigator and what is investigated are seen as interdependent. According to this view, what we treat as real is in large measure mind-dependent as opposed to mind-independent. As Guba puts it, “The rationalistic paradigm rests on the assumption that the inquirer can maintain a discrete (and discreet) distance from the objects of the inquiry... the relationship... is essentially one of independence” (p. 77), whereas for naturalism, “the inquirer and the respondent... are interrelated, with each influencing the other” (p. 77).

Another important difference between the two paradigms concerns the nature of “truth statements.” In this case the rationalist paradigm assumes that generalizations—“enduring truth statements that are context free” (p. 77)—are possible. Naturalism, however, holds that such generalizations are not possible and “that at best what one can hope for are ‘working hypotheses’ that relate to a particular context” (p. 77). The implication of this difference in position is that while a neutral (in the sense of nonvaluative) posture is possible from a rationalist perspective, especially in regard to physical science, from a naturalist perspective neutrality is “impossible when the objects of investigation are people” (p. 78). The reasons for this impossibility are that human behavior is never context free and that both respondent and investigator exert a reciprocal influence on one another.

Guba has found that the two paradigms differ significantly in terms of their basic assumptions. He summarizes the underlying assumptions of the naturalistic approach quite well when he says, “Social/behavioral phenomena exist chiefly in the minds of people, and there are as many realities as persons” (p. 77). Social reality is mind-dependent in the sense that people (including investigators) shape or construct reality and people (including investigators) may construct their realities in their own different ways at different times and places.

Given such a world of multiply constructed realities, how is it possible to find some versions of reality “trustworthy” and others not? Although he does not specifically define the term trustworthy, Guba does discuss four major aspects of it (pp. 79–82). First, the findings of a trustworthy study exhibit “isomorphism to the respondents’ perceptions” (p. 80). In other words, the investigator’s statements accurately reflect the respondents’ perceptions. Second, the findings are neutral in that they are a “function solely of subjects (respondents) and conditions of the in-
quity and not of the biases, motivations, interests, perceptions, and so on of the inquirer" (p. 90). Third, if the inquiry is repeated with the same or similar subjects, the findings should be consistent with those of the first inquiry. Fourth, the results must be transferable to other similar situations. Taken together these points seem to represent an attempt to separate inquiry that tells us how people really perceive their own situations from inquiry that distorts "reality" by the involvement of the investigator's perceptions, biases, and interests. Using this interpretation of trustworthiness, Guba then describes a series of procedures, such as member checks, structural corroboration, and dependability audits, which are used in the naturalist mode to deal with this problem.

Given his assumptions, what Guba is attempting to do by specifying criteria is not possible. The assumptions of multiple realities and reality as mind-involved seriously undermine the notion of applying foundational criteria to distinguish trustworthy results from untrustworthy ones. These assumptions and foundational criteria are, in a word, incompatible. To accept that social reality is mind-constructed and that there are multiple realities is to deny that there are any "givens" upon which to found knowledge. If one accepts these assumptions, different claims about reality result not from incorrect procedures but may simply be a case of one investigator's interpretation of reality versus another's. In a world of multiply constructed, mind-dependent realities, there may be no "court of last resort" to appeal to to sort out trustworthy interpretations from untrustworthy ones.

Some examples of the inconsistencies that result from accepting antifoundational assumptions while attempting to pose foundational criteria can now be presented. Guba's problems with consistency are clearly evident in his varied use and definition of the term criterion. By formal definition criterion can mean slightly different things. Among other possibilities, the term may refer to a "characterizing trait." In this usage it has, at best, mild implications as a prescription for inquirer behavior and does not necessarily refer to something that is held to be foundational. Criterion may also, however, mean "a standard against which to make a judgment." Here the term brings with it a very strong implication of something foundational—it is that touchstone that can be employed to sort out the good from the bed, the correct from the incorrect.

In some instances Guba seems to be employing the former definition and seems aware of the antifoundational implications of his assumptions. Toward the end of the article he says his criteria are not prescriptions and that he does not want them to become an orthodoxy (p. 90): "But—most emphatically—the [criteria] are not prescriptions of how inquiry must be done" (p. 90). If this is the case, then he is discussing simply the characterizing traits of a particular approach to inquiry and saying that these criteria are merely the way inquirers seem to do it. The elements he discusses cannot therefore be thought of in such terms as minimums, absolutes, and essentials.

Throughout the paper, however, there is considerable evidence that Guba desires criteria to carry more weight than as simply characterizing traits. His first sentence demonstrates that he is concerned with "criteria for judging the trustworthiness of inquiries conducted with the naturalistic inquiry paradigm" (p. 75). In fact, the entire paper is directed at demonstrating that naturalistic researchers also have standards. For example, Guba poses his criteria as equivalent to or parallel to those of a rationalist paradigm. The tone is one of "we are equally well protected from the problems of subjectivity." More specifically, he uses terms such as "newer rules" (p. 90), implies that his efforts are an attempt to "codify the safeguards" (p. 76), and says that he has elaborated criteria that are to be generally applied by editors, referees, and researchers to sort out the trustworthy from the untrustworthy: "When a naturalistic study is to be judged, it is these criteria that ought to be brought to bear" (p. 38). This is all rather strongly put and definitely goes beyond describing simply how inquirers proceed through this type of study. Even though at times his use of language makes it difficult to discern his exact meaning or intention, it is clear that he desires standards that are neutral and
nonarbitrary. In the end, his antifoundationalist assumptions push his use of criteria in one direction, while his desire for "something to hang on to" pushes it in the other direction.

A second major area of inconsistency involves the status of the procedures Guba lists. At one point he seems to recognize the epistemological consequences of his assumptions, but at other points he does not. He contends that procedures such as triangulation, peer debriefing, and prolonged engagement will not guarantee that a naturalistic study will be trustworthy. These procedures do not constitute, as Guba feels is the case for their rationalist counterparts, a "theoretically complete whole" and will therefore not insure, but only increase, the probability of a study's trustworthiness (p. 88). However, shortly after discussing these procedures in terms of "ambiguity" and "indeterminancy" (p. 88), he gives some of the members on the list the status of sine qua non (p. 88). As he notes, "It is incoherent that one would be persuaded of the trustworthiness of a study that involved no triangulation and no member checks" (p. 88). This is quite clearly an attempt to pose an absolute (a necessary if not sufficient condition) on which to found naturalistic inquiry. The establishment of absolute conditions for trustworthiness negates the fact that only indeterminate procedures can follow from the relativism of multiply constructed realities. The assumptions on which naturalistic research is based are violated by posing procedures that are indispensable. There is a great deal of difference between describing what one might do and mandating what one must do.

Inherent in Guba's assumptions and specifically stated by him (p. 78), is the significant idea that the investigator cannot be neutral. This idea immediately provokes problems since earlier he argues for neutrality as one of the key elements for trustworthiness. This point aside, this issue of neutrality appears when Guba attempts to preserve the idea of inquirer "distance" from the subject matter and even claims there is such a thing as "optimal distance" (p. 77). Once it is granted that reality is constructed by people (among whom are investigators), this prospect of maintaining distance is very difficult to defend. To do so, the object of study must exist independently of the inquirer, and the inquirer must have independent access to that object. Once the investigator is seen as mind-involved with the construction of reality that he or she investigates, there is no way for the mind to stand apart (that God's eye view) and measure distance. The mind, after all, cannot be distanced from itself. Once one grants that reality is mind-dependent and that there are as many realities as there are persons, it follows that it may be impossible to sort out who has maintained proper distance and who has not.

Guba's work therefore provides an excellent example in tangible terms of what happens when antifoundational assumptions are combined with the attempt to pose foundational criteria. The inevitable result is inconsistency of argument. It is now time to discuss why this result is inevitable.

The Issue of Assumptions and Criteria

Given the assumptions of naturalistic or interpretive inquiry, why is it impossible to develop and apply foundational criteria to this type of investigation? To answer this question, it is necessary to review briefly how the dominant mode of inquiry—quantitative—has dealt with this issue of assumptions and criteria. One reason for this digression is that interpretive inquiry, at least since the time of the challenge initiated by Dilthey and others to positivism, has very often been parasitic (i.e., taken its strength from and developed through critique of the quantitative approach). As Rorty (1979) notes, "Hermeneutics is always parasitic upon the possibility of epistemology" (p. 365). Therefore, to understand the claim that there can be no criteria to reconcile discourse (to sort out the trustworthy from the untrustworthy results) for interpretive inquiry, we must first examine the opposing claim that there are indeed foundations on which to base epistemological claims.

A number of assumptions, some of which are often implicit, are crucial to the assertion that there are nonarbitrary criteria for judging and nonarbitrary procedures for doing quantitative inquiry.
Even though other terms, such as externalism, objectivism, and so on, could be used here (Putnam, 1981, pp. 49–50; Rubinstejn, 1981, pp. 24–25), for our purposes the assumptions of this approach can be located within the general philosophical perspective or temperament known as realism. A most critical feature of a realist perspective is the idea that "what exists does so whether men conceive of it or not" (Trigg, 1980, p. vii). In other words, reality is self-subsistent; it exists "out there" independently of us and of our interest in it. Following the lead of Durkheim (1938), social and educational facts are considered external to us, existing prior to an investigator's study of them, awaiting discovery. While this position grants that minds are necessary to conceptualize reality (by definition only minds can do so), it does not accept the idea that reality is mind-dependent in the sense of being created or constructed. As Trigg notes, "The realist must hold that the mind and the world are separate items" (p. 93).

Given this separation of the knower from what is or can be known, the basic epistemological thrust of realism is that to know reality is to be able to describe or reflect it accurately. "According to most realists ... a judgment is true when it corresponds to an independent reality and false when it does not" (Ewing, 1974, p. 195). Not surprisingly, this position is labeled as the correspondence theory of truth. True statements are judged to have accurately reflected the qualities and characteristics of what is out there, whereas false ones are those judged to have in some way distorted the nature of that independently existing reality. For example, if one says the cat is on the mat and the cat is observed to be there, we accept that one has produced a true statement. Similarly, if one says that school performance and self-concept are positively related in first graders and on observation this turns out to be the case, one has made a statement we judge to be true. The significant point is that truth has its source in an independently existing reality—a reality that can be known for what it really is.

As these examples demonstrate, the process of observation, or empirical verification, is essential to our ability to judge whether or not statements are true. However, there are many ways of observing the world, and some are considered less subject to error and distortion than others. From the quantitative perspective, one of the greatest dangers to sound empirical verification is subjectivity. Paraphrasing from Nagel's (1981) discussion of the topic, to be subjective is to see the world from one's own particular place within it (pp. 196–213). To approach the world in this way is to let one's personal values and dispositions distort or bias an accurate description of what really exists. The result of such subjectivity, if allowed to dominate our views of the world, would allow that everyone could create his or her own little private reality. To avoid this danger, the demand of quantitative inquiry is for objectivity—that we take a God's eye point of view (Putnam, 1981, p. 49)—or see the world from no particular place within it. How this approach is able to insure objectivity requires that investigators adhere to method, in particular the scientific method.

First and foremost, the methods we conventionally think of as scientific serve to constrain the investigator and hence protect him or her from the perils of distorting the qualities and characteristics of the object of study. At the level of the practice of inquiry, adherence to these procedures will insure that the study has been properly conducted. And if this is the case, we must accept the results, regardless of how we might feel about them. In other words, these are results to which all "rational" people must accede, and disagreements can be reconciled by a properly done appeal to an independent reality. These procedures are neutral and nonarbitrary in that they stand over and beyond any individual's interests, dispositions, or place in the world. The assumptions of this approach allow for a foundational epistemology, one that holds we can know reality for what it really is and that we have criteria for judging, and procedures to employ as we go about discovering, that reality.

A brief restatement of these issues in slightly different terms will be helpful. The ideal of verification and of a foundational epistemology is dependent on the
possibility of building knowledge free from a never-ending circle of interpretation (Taylor, 1971). The nonarbitrary verification of propositions requires that there be such a thing as “brute data” or “givens.” These data, and the inferences based on them, stand on their own in the sense that they are “data whose validity cannot be questioned by offering another interpretation or reading, data whose validity cannot be founded on or undermined by further reasoning” (Taylor, 1971, p. 8). Therefore, to have foundational criteria requires one to assume that there is an independent reality out there that can be read for what it is, and, if this is the case, then the claim of certitude may follow.

The assumptions of naturalistic inquiry—that reality is mind-dependent and that there are multiple realities—are incompatible with the idea of an independently existing reality that can be known through a neutral set of procedures. While various labels, such as internalism, subjectivism, and so on, can be employed here, this approach to inquiry can be discussed within the context of an idealist philosophical temperament. Even though there are several forms of idealism, for our purposes suffice it to say that they all share one point in common: “To the idealist it seems inconceivable that anyone should talk of knowledge in any intelligible sense if the supposed object of knowledge is wholly independent of the knower—in which there is not in some way and in some degree mutual involvement of mind and object” (Urban, 1949, p. 16). There is no independent reality or data free from interpretation on which to found our knowledge.

The most striking epistemological consequence of this perspective is that correspondence or accuracy of reflection is not an acceptable way to define truth. The idea of a mind-dependent reality is incompatible with the correspondence theory. For interpretive inquiry, the basis of truth or trustworthiness is social agreement; what is judged true or trustworthy is what we can agree, conditioned by time and place, is true or trustworthy. This appeal to common understanding as opposed to correspondence results in definitions of such basic terms as objective and subjective that are quite different from those used in the quantitative approach to inquiry. To be objective is not a matter of representing things as they really are. As Rorty (1979) notes, “The application of such honorifics as ‘objective’ and ‘cognitive’ is never anything more than an expression of the presence of, or the hope for, agreement among inquirers” (p. 335). Similarly, to be subjective does not mean representing things influenced by the emotions, interests, and so on; it is to “bring in considerations which others think beside the point” (Rorty, 1979, p. 339). What we agree with and on is objective; what we find unfamiliar we label subjective.

The implications of this idea of agreement can be seen if we imagine a situation in which competing claims have been made. From a realist/quantitative perspective this conflict can be resolved by referring to the extralinguistic facts—that external, independently existing reality. From an idealist/interpretive perspective, however, reference to the facts will not provide a solution, since disagreement is not founded on “error of reflection or correspondence” but is simply the result of interpreting things in a different way. While the different interpretations may be subject to questions of internal consistency (coherence), there is no nonarbitrary set of criteria that can be employed to sort out the difference and no brute data to appeal to to reconcile the issue. In a world where “social/behavioral phe-

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1 A coherence theory of truth is often associated with an idealist perspective. Simply put, for this
nomena exist chiefly in the minds of people, and there are as many realities as persons" (Guba, 1981, p. 77). We are faced with the prospect of one interpretation of reality coexisting with another interpretation of reality. No safeguards, rules, or absolute minimums are available to work out our differences. Clearly this position is epistemologically antifoundational.

What does all of this mean for the practice of naturalistic inquiry? According to Taylor (1971), the sciences of man are hermeneutical. A hermeneutical science is directed at interpreting the intentional, meaningful expressions of people and provides interpretations of interpretations people have already given to their own situations. Furthermore, a defining characteristic of a hermeneutical science is that it is, in principle, impossible to develop a precise and nonarbitrary way to verify results. All that an investigator can do is offer an interpretation of interpretations and hope others will "catch on" or accept those readings. If they do not, nothing outside of the hope for common understanding can be used to compel agreement. As Taylor puts it, "We can only convince an interlocutor if at some point he shares our understanding of the language concerned. If he does not, there is no further step to take in rational argument; we can try to awaken these intuitions in him, or we can simply give up; argument will advance us no further" (p. 6).

Hermeneutics and the idea of the hermeneutical circle have been subject to various interpretations over the years. Generally speaking, these terms mean that any reading by the inquirer (or layperson for that matter) is the product of an interpretive movement back and forth between parts and whole. In the interpretation of a text, for example, individual sentences can only be understood in relation to the whole text, and the whole text can be understood only in relation to the individual expressions. To give a reading to an expression requires the application of mental categories to provoke an understanding of the expression and of the context of the expression. As Lessnoff (1974) notes in a discussion based on the work of Meldan (1961), "In order to see an event as a human action, it is necessary to interpret its empirically observable features in terms of mental categories, to assume the applicability of these categories to what is observed" (p. 37). In interpretive understanding, a constant movement back and forth between parts and whole is necessary. However, this process responds to no formal rules of procedure and has no absolute beginning point and no absolute ending point. There is no time at which one can say with assurance, or in a foundationalist manner, "It is over and I have got it right."

The implications of this hermeneutical-interpretive approach to inquiry can be seen in a situation in which one person does not accept the readings of another. Will it not be easier to convince the other person if the inquirer has used some procedures as opposed to others in achieving the result? Not necessarily. If the results are based on particular procedures, such as Guba's member checks and so on, these will be convincing only if these procedures make sense to the reader. The problem is that what makes sense depends on how one reads the situation—not only the general situation of interpretive inquiry, but the specific situation of any particular inquiry, not only in terms of the actual interpretation, but also in terms of how the interpretation was obtained. As Taylor (1971) notes in a slightly different context, "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).

What all of this means is that we have no way, in a foundational sense, to know a good interpretation from a bad interpre-
tation. One can try to convince another of the appropriateness of a particular reading, but this can only be done through an appeal to common understanding. If such an understanding does not exist, there is no "higher authority" to which one can appeal to resolve the issue. Naturalistic inquiry then, given these idealist assumptions, begins to look very relativistic. And, once accepted, the idea of relativism cannot be suspended in the quest for foundational criteria by which to judge the results of this form of inquiry.

Member Checks as Absolute Minimum

A more specific discussion of one procedure advocated by Guba should clarify some of the preceding points. Guba (1981) says that member checks are an absolute minimum for naturalistic inquiry (p. 88) and that they are the most important thing a naturalistic inquirer can do (p. 85). This is the same as saying the procedure is foundational in that it is a necessary, if not sufficient, condition for trustworthiness. If an inquirer has confirmed his or her interpretations with the subjects (and produced a record of how this process has influenced the results), the study has a chance of being judged trustworthy. If not, it is unlikely the results will be granted such status.

Is this claim defensible? If we accept the assumptions of interpretive inquiry, then we must accept that the need for member checks will depend on the perceptions of the people involved. If a reader feels member checks are essential, he or she will not accept a study in which this procedure has not been done. Someone else, however, may believe either in general or for a specific study that they make no difference at all. For example, it is very possible for a person to argue that the most important and original insights come during the initial contacts with subjects. To bring one's interpretations back to them will only serve to attenuate these insights. The point is, since we are not dealing with brute data there is no unambiguous way to know what makes sense in this regard.

The question of member checks must also be seen in regard to what we might call a form of reflexivity. Since it can be inferred that the purpose of member checks, to those who advocate them, is to verify the extent to which the inquirer has accurately described or reflected what the subjects did or said, this procedure looks very much at home with the correspondence theory. In other words, implicit in the procedure is the assumption of an independent, external reality (the subjects' expressions) that can be known for what it really is. However, when subjects are asked about their responses, it is always possible that this is new information which will lead them to change their self-definition and how they interpret their own past and present situations. This may in turn lead the inquirer to reinterpret his or her findings in what can be a never-ending process. Suppose, for example, that the subject tells the inquirer that the latter's interpretation of what was said is not correct and what was really meant was ... The problem is that there is no definitive way to decide between (a) the inquirer did not originally reflect the subject's true meaning, but now is getting closer, and (b) the inquirer's comments provoked a change in the member's definitions of the situation and hence a change in his or her interpretations. Further questioning will only push the question back, not answer it.

A simple, but illustrative, hypothetical example will be helpful. Suppose that a researcher has been a nonparticipant observer of a number of "staffings" in which school personnel discuss with parents the special education placement of their children. The inquirer notes that after about 30 minutes the principal will often say to the parents, "I do not mean to rush you, but there are only two spots left in the resource room and you must make a decision soon." The inquirer interprets this to mean, "We have spent a half hour on this, we are busy, and we must get this meeting over." When the inquirer returns for a member check, he or she says, "You said the former but could have really meant the latter." The principal insists he meant the former and that the latter never entered his mind. In the name of iso-

\[1\] The idea for, but not the reality of, this example is based on a study by Law (1981).
morphism or an accurate reflection of reality, must the inquirer accept what the principal said or can he or she continue to offer the alternative interpretation? There is simply no clear-cut way to decide the issue. And if we have no foundational basis from which to judge how to locate the information resulting from a member check, it seems unlikely that this procedure, or any other by extension, can be raised to the level of absolute minimum.

One objection to this example requires comment. One can argue that an inquirer would ask the questions both directly and indirectly and would ask related questions so that a pattern of responses, interpreted within the context that occasioned them, could be developed. This process would then yield results that have truth value or are scientifically relevant. Two things can be said in response to this situation. First, that it is a good description of actual fieldwork practice almost goes without saying. Second, that it will provide for results that have truth value or are scientifically relevant, if these terms are taken in a foundationalist sense, is debatable. This approach to the interpretation of responses is in essence an expression of the idealist injunction that meaning must be grasped hermeneutically. And, if this is the case, than the Dilthey problem returns: Is there any point at which interpretation ends and one can say, “I have got it right?” or, more generally, can there be any such thing as a “correct” interpretation? One might say that the process ends when one has a coherent interpretation that makes sense of the situation. However, given the idealist assumption that there is no independent reality to which we have independent access, there can be numerous, yet different, coherent interpretations, and what makes sense depends on the kind of sense one understands. In other words, the antifoundationalist nature of interpretive inquiry means that no epistemological privilege can be attached to any procedure for doing or criterion for judging this approach to inquiry.

Summary and Implications

This paper has attempted to describe the inconsistencies that occur and to demonstrate why they must occur, when the assumptions of naturalistic interpretive inquiry meet with a desire to establish neutral, nonarbitrary criteria for separating trustworthy from untrustworthy results. This problem of consistency arises from the incompatibility of assumptions that are epistemologically antifoundational and the desire for criteria (defined as standards) that are epistemologically foundational. Given the idea that social reality is mind-dependent, that facts cannot be separated from values, that the only point of view we have is that of various people based on various interests and purposes, and so on, it is impossible to be foundational or to “ground” our findings in the sense of “getting it right.” In this situation what is trustworthy or true is nothing more than what we can agree, at any given time and place, is trustworthy or true.

The question now is, What does this mean for the nature of social inquiry? Presuming that consistency is important, we are left with a choice between two options: either give up the assumptions or give up the desire for “something to hang on to.” In the first instance, if the claim to certainty is compelling, then we must reconsider, or simply not heed, the assumptions of this perspective. To some extent there has been movement, even though not necessarily intended, in this latter direction. Guba was clearly drawn by this possibility with his emphasis on isomorphism, neutrality, accurate representation of respondents’ perceptions, and so on. All of these points are more at home as terms of correspondence in that they are sustained by the prospect that an investigator can stand apart from or independent of social reality and discover and describe that reality as it really is. LeCompte and Goetz (1982), in another example, take Guba one step further. For them the difference between the two approaches is one of technique or investigator strategy with both based on the same general “logic of justification.” They define validity in the same way for both perspectives (as accuracy of representa-
tion) and hold that the difference is simply one of procedure to achieve such accuracy. Thus, one way out of the consistency problem is to ease aside the interpretive assumptions and, given the emphasis on “grounding” inquiry, indirectly accept those of a realist orientation.

However, if the interpretive assumptions are more satisfying, then we must put aside the desire to be foundational. And if this is the case, then we will be compelled to rethink the nature of social inquiry. What this rethinking will lead to is impossible to specify. At this stage only a most general sort of speculation is possible. Taking the lead from Rorty (1979, 1982), maybe the situation of assumptions and criteria indicates that we should dispense with the traditional ideas of objectivity and truth and realize that we are “beyond method.” There are no procedures or criteria exclusive to or particularly appropriate for social inquiry and, accordingly, one cannot simply tell another it must be done in this way or that way because to do so will insure objectivity and lead to truth (in any sense other than as a description for a sentence with which one agrees). From this antifoundationalist perspective social inquiry might be best seen as “continuous with literature—as interpreting other people to us, and thus enlarging and deepening our sense of community” (Rorty, 1982, p. 203). Sociologists interpret the interpretations of the poor and other marginalized peoples, educational psychologists interpret the interpretations of children, and educational evaluators do the same for those affected by particular educational policies and practices. Yet, these interpretations are not about certainty or the discovery of how things really are—they are an attempt to enlarge the conversation and to keep it going.

All of this, of course, is very speculative. Not only the solutions, but even the precise nature of the problems, associated with such an antifoundationalist position are extremely unclear. What is clear, however, is that the assumptions of interpretive inquiry are incompatible with the desire for foundational criteria. How we are to work out this problem, one way or another, would seem to merit serious attention.

References


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