CHAPTER 12

Evaluating an Interpretive Account

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Evaluating an Interpretation

In the Introduction we described a persisting concern that has appeared: the concern over evaluation of interpretive accounts. We suggested that this is a reaction to the failure of attempts to identify a foundation that would guarantee our knowledge claims in psychological inquiry. This failure has given people a dim sense of the circularity in understanding, but the circle is misunderstood as a vicious one instead of an essential one. Now that the efforts to build a foundation with formal logic and objective observation have failed, some psychologists fear that scientific inquiry must drop into a swamp of mere opinion and speculation. And it must be agreed that without a foundation of interpretation-free facts, without fundamental principles that are self-evidently basic, it is not immediately apparent what kinds of claim to knowledge we can still make. Without an interpretation-free standard (usually "the facts of the matter") it is not clear how we can evaluate an interpretive account. Are there other criteria that can be applied? How can we apply them to interpretations, if not by assessing correspondence? These are the topics of this chapter.

We have emphasized the importance of appreciating both arcs of the hermeneutic circle: the forward arc of projection, and the return arc which we shall see is a movement of uncovering. The former makes understanding possible, and the latter, we shall see, provides the possibility for evaluating an interpretive account. In this final chapter we want especially to examine this return arc. We shall consider how, in the hermeneutic world-view, an interpretive account can be true or false. Our aim is not to introduce new approaches to evaluation, but become clearer on the approaches that are already used, and to better understand just what evaluation entails. We'll examine four approaches that are representative of those employed, and show that a hidden search for validation—for evaluation in terms of interpretation-free norms and standards
—and a hidden application of the correspondence theory of truth, still run through much of the effort at evaluation of interpretive research.

**Interpretation is not Conjecture**

Evaluation continues to be subsumed to validation because of a persistent misunderstanding of interpretation. Otherwise acute interpretive thinkers have been seduced by the view that both natural science and interpretive inquiry proceed by "conjectures and refutations" (Popper 1972): by the testing of speculative hypotheses. Here again a positivist model of the natural sciences has been applied uncritically to the human sciences in general, and to interpretive inquiry in particular.

Positivist philosophers developed the hypothesis-testing model of science when they became aware of problems in the view that inductive reasoning was the way scientific and everyday knowledge is formed. Central among these problems was the difficulty that induction, necessarily based on examination of a limited number of cases, can never justify a universal statement such as "All swans are white." Popper, especially, proposed that "the method of science is the method of bold conjectures and ingenious and severe attempts to refute them" (1979, 81). Scientific theories, he argued, framed as universal statements, inevitably go beyond the available evidence without proper logical justification, and the role of logic lay in subjecting these speculative hypotheses to testing and correction. The logic's discipline was needed as a corrective to the fanciful character of hypothesis. The proper focus of the philosopher of science was on this "logic of validation" that operated when the scientist planned and conducted an effort to test, falsify or refute her theory. The "psychology of discovery" that was at play in the making of conjectures was, on the other hand, unsystematic and uninteresting.

Many writers have assumed that this is an appropriate model for interpretation, too. Hirsch, for instance, went perhaps even further than Popper in claiming that "the act of understanding is at first a genial (or a mistaken) guess, and there are no methods for making guesses, no rules for generating insights. The methodical activity of interpretation commences when we begin to test and criticize our guesses" (Hirsch 1967, 203). We must not confuse "the whimsical lawlessness of guessing with the ultimately methodical character of testing" (1967, 204). Interpretations must be tested with a "fundamental" hypothetico-deductive process identical, in his opinion, to that of the natural sciences (1967, 264). Here interpretation is seen as unavoidably speculative, in contrast to validation which can and does follow rules. Hirsch outlined a logic of probabilistic judgments operating on "classes" of texts with manifest "traits," by which evidence was to be weighed to select the most probable hypothesized reading.
Ricoeur goes along with Hirsch's view, at least in his middle-period work. "We have to guess the meaning of the text because the author's intention is beyond our reach" (1976, 75), and "if there are no rules for making good guesses, there are methods for validating those guesses we do make" (p. 76). Now, in a sense Ricoeur is correct; we have argued that there are indeed no procedures for interpreting. But this hardly implies that interpretations are "divinatory" (as Schleiermacher put it, and as Ricoeur quotes approvingly). There is room to drive a coach and horses between rule-following and guessing. Yet Ricoeur adopts the most conservative view that interpretation is a matter of guessing, and so needs the corrective of disciplined and rigid validation.

Hirsch and Ricoeur have got off the coach too soon. To see interpretation as conjecture is to misunderstand interpretive inquiry, just as to think that the natural sciences proceed by guess-and-validation is to have a mistaken view of science. Ricoeur and Hirsch missed the significance of projection. Interpretation is the working out of possibilities that have become apparent in a preliminary, dim understanding of events. And this pre-understanding embodies a particular concern, a kind of caring. It provides a way of reading, a preliminary initial accessibility, a stance or perspective (a fore-structure) that opens up the field being investigated. Interpretation operates within this initial way of understanding and reading. Reading for care (Brown et al.) or for plot (Sarbin), or for procedure (Misgeld and Jardine), all occur within the possibilities opened up by the researcher's perspective. And this means that interpretive accounts are not undisciplined guesses, and do not shoot beyond the available evidence in a speculative way. They are ordered and organized by the fore-structure of projection; the fore-structure guides interpretation. The guidance is not automatic, of course; we have a responsibility to prepare so that we "enter the circle" with an appropriate fore-structure, and so conduct our interpretation in a proper manner.

The guidance stems in large part from the fact that a fore-structure is primarily a practical, not a conceptual, organization. Entering the circle in the right way is mostly getting the manner of inquiry right. Careful interpretive research is conducted in a manner that can hardly be said to foster unsupported speculation. Lather (1986), for instance, describes it as inquiry that seeks reciprocity; that calls for interactive, dialogic interviews "that require self-disclosure on the part of the researcher"; that involves negotiation of meaning (including the recycling of descriptions, emergent analysis, and conclusions to respondents); and discussions of false-consciousness. If you prefer such a description these are components of validation built into the conduct of the research. We prefer to see them as aspects of a manner of inquiry that is guided by a sense of the complexity of the human relationship between researcher and research participant.
So an interpretation is oriented by the researcher’s effort to come into the hermeneutic circle in an appropriate manner. It is guided by the fore-structure that is worked out in this entering. And the interpretation articulates possibilities that are laid out in the researcher’s preliminary understanding. All this means that interpretation is far from being an undisciplined guess. And if an interpretation is not a guess, then evaluating it need not be like testing a hypothesis.

Truth is Uncovering

If truth is not a matter of correspondence between a theory or account and the way things “really are,” should we dispense with the notion of truth altogether? Or can we get to a new understanding of what truth is, that strips away metaphysical notions? The latter is what Heidegger proposed to do. In *Being and Time* he attempted to demonstrate that the truth of an interpretation, a theory, or even a practical activity, is a matter of uncovering. This uncovering of an entity is the return arc of the hermeneutic circle; it is the response to our inquiry.

Because an interpretation . . .

. . . is a letting-something-be-seen, it can therefore be true or false. But here everything depends on our steering clear of any conception of truth which is construed in the sense of ‘agreement.’ This idea is by no means the primary one in the concept of *aletheia*. . . . [T]he entities of which one is talking must be taken out of their hiddenness; one must let them be seen as something un-hidden (*alethes*); that is, they must be discovered (Heidegger 1927/1962, 56).

(The Greek word for ‘truth,’ *aletheia* can be translated as unconcealed, unhidden or uncovered.) Interpretation, working out the possibilities projected in understanding, shows entities explicitly, often for the first time. These entities have been hidden from our awareness. Now what is crucial is, as Heidegger put it (in a rather awkward phrase), “to let that which shows itself be seen from itself in the very way in which it shows itself from itself” (p. 58). We must show the entity or, more precisely, let it show itself, not forcing our perspective on it. And we must do this in a way that respects the way it shows itself.

Heidegger was not saying that a good interpretation shows things as they “really are” (even Caputo 1987, seems to suspect this, in an otherwise exemplary interpretation of *Being and Time*). This could hardly be the case, given Heidegger’s discussion of projection. What Heidegger describes is neither a simple realism nor a view of truth as coherence. On the contrary, he states explicitly that “an entity can show itself from itself in many ways, depending in each case on the kind of access we have to it” (p. 51). To say that a true
interpretation lets an entity show itself "as it is in itself," or "shows itself from itself" is not to say that we have a description corresponding to a timeless, universal essence, but that the entity has been uncovered "as it is capable of being dealt with" (Okrent 1988, 163). What is uncovered in an interpretation depends on the access we have developed, the kind of entry into the circle we have achieved. What is uncovered in the course of a true interpretation is a solution to the problem, the confusion, the question, the concern, and the breakdown in understanding that motivated our inquiry in the first place.

In Heidegger's view, a good interpretation will not provide validated knowledge, or timeless truth, but instead an answer to the practical or existential concern that motivated our inquiry. This concern may be a simple breakdown in our dealings with objects, or it may be a complex social or psychological trouble, such as conflict, alienation, or clinical disorder.

We can see more clearly now why the circularity of understanding and interpretation is not vicious. If our inquiry is shaped and motivated by a practical concern or difficulty, and if as a consequence of interpretation we uncover a solution to that difficulty, it is not the case that we have found only what we read into things. The truth of an account will be suited to the perspective adopted in the inquiry, but this is not a vicious circularity, it is precisely what we want. It does imply that we have to be careful to adopt an appropriate perspective and become aware of what our practical concern is, but this is only as it should be. A true interpretive account is one that helps us and the people we study, that furthers our concerns. Interpretive inquiry (like other forms of human inquiry) must not be misunderstood as just an effort to describe, or even just understand, human phenomena. Interpretation always begins from concerned engagement. "There is no 'pure truth' that lies outside human engagement in the world" (Polkinghorne 1983, 224).

It is important to note that Heidegger, in interpreting truth as uncovering, has not provided us with a new norm of validation, nor was this his intent. It remains a matter of interpretation whether our interpretive concern has been answered adequately. But Heidegger has indicated what we are doing when we evaluate an interpretive account. We are considering whether our concern has been answered. Evaluation of an interpretive account with an eye to uncovering fulfills the suggestion that in interpretive inquiry "any notion of validity must concern itself both with the knower and with what is to be known: valid knowledge is a matter of relationship" (Reason & Rowan 1981, 243).

Approaches to Evaluation

We shall consider four approaches to evaluation, namely requiring that an interpretive account be coherent; examining its relationship to external evidence; seeking consensus among various groups; and assessing the account's
relationship to future events. These approaches seem reasonable ones. But, as we shall see, they do not guarantee correctness and so researchers who employ "an ecumenical blend of epistemologies and procedures" (Miles & Huberman 1984b, 20) have tended to view them with ambivalence. These approaches and others like them are frequently proposed as though they are somehow analogous to traditional validation procedures. Then 'flaws' are found in one approach that expose it as an untrustworthy procedure, and another must be used to bolster it.

The problem here is the outmoded view of truth that is tacitly employed. Interpretation-free validation is, as we saw in the Introduction, intimately tied to the view that a true account is one that corresponds to "what really happened," or "the way things really are." Even when "ecumenical" researchers deny that they are applying a correspondence theory of truth they can find no alternative to employ instead, and they slip back unwittingly into the traditional notion. Kirk and Miller (1986, p. 80), for instance, define validity as "the quality of fit between an observation and the basis on which it is made." And although Fischer (1987) asserts that her notion of validity "does not invoke an ideal ... of approximating an in-itself reality," she asks such questions as "is it a faithful summary of instances? ... Do citizens recognize the phenomenon from their own experience (the old correspondence criterion)." Similarly, Guba and Lincoln (1982, p. 326) claim that "internal validity is best demonstrated through an isomorphism between the data of an inquiry and the phenomena those data represent." And LeCompte and Goetz (1982, p. 43) maintain that "validity necessitates demonstrating that the propositions generated, refined, or tested match the causal conditions which obtain in human life," and that this should be done by establishing whether there is a "match between scientific categories and participant reality." Miles and Huberman (1984a, p. 230) give their game away when they complain that "the problem ... is that there are no canons, decision rules, algorithms, or even any agreed-upon heuristics in qualitative research to indicate whether findings are valid and procedures robust.... We are doing our best ... to operationalize ways of testing/confirming findings." On the contrary, the absence of decision rules is a necessary and positive consequence of abandoning the traditional account of inquiry and the untenable correspondence theory of truth. The "problem" that remains is that without working to get a clear understanding of what they are trying to do when they evaluate an interpretive account, researchers fall back into the world-view they've struggled to escape. Smith and Heshusius (1986) argue convincingly that this view of evaluation tends to bring to a premature closure the dialogue between the traditional and interpretive worldviews. Let us see whether an alternative view can be found.

Coherence. The first approach we consider is that of requiring that an interpretive account have a particular internal character. Coherence (we might
also call it plausibility or intelligibility) is a characteristic that a good account should demonstrate. It seems reasonable to require this, but this approach to evaluation has frequently been rejected as inadequate. Miles and Huberman (1984a), for instance, consider plausibility “a last-refuge tactic for drawing conclusions” because “plausibility can easily become the refuge of, if not scoundrels, analysts who are too ready to jump to conclusions. . . . Subject the preliminary conclusions to other tactics of conclusion drawing and verification” (216-217).

Appeals to coherence and plausibility are generally rejected on the grounds that such an approach fails to live up to the requirements of a validation procedure. Hirsch (1967) has given the most influential argument for rejecting coherence as an evaluative criterion. He claimed to have identified the “fundamental difficulty” in this approach: because of the projective structure of understanding, the perspective we have on a text shapes readings of that text and so, according to Hirsch, it will tend to point out the evidence that supports it and ignore evidence that runs against it. New material uncovered as we interpret will tend to fit what is known already, because both are viewed through the same lens. To Hirsch this constitutes an inherent “self-confirmability” to interpretive inquiry that means that an interpretation will tend automatically to be coherent.

Hirsch correctly recognizes the projective organization of understanding, but he exaggerates the problem. Interpretation is only weakly self-confirming, in contrast with other kinds of understanding (delusions, for instance) that should be called strongly self-confirming because counter evidence is so threatening that it is totally suppressed. Material that fits an interpretation does indeed tend to be seen first, but counter evidence can certainly appear. In fact, a good interpretive inquiry will employ stratagems to critique an interpretation and so facilitate a change in understanding. For example, Packer (this volume) mentions Thomas Kuhn’s suggestion that we should look for disconfirming evidence in order to better understand a text. This strategy would be totally impracticable if only corroborative evidence could appear; it works because interpretation focuses on breakdown and misunderstanding to show things (the “negative side of articulation” described by Packer, this volume). Hirsch would be correct if understanding and interpretation went only one way, projecting forward onto a text and violently imposing a perspective on it. Hirsch ignores the return arc, in which the inadequacies of a projective fore-structure become apparent.

Coherence is not, after all, inevitable, and good interpretive inquiry will scrutinize and check an interpretation that appears coherent by searching out and focusing on material that doesn’t make sense. Hirsch is correct to the extent that even weak self-confirmability means coherence cannot provide the objective standard that a validation procedure is supposed to use, but we
should conclude from this that evaluation is not simply validation, not that coercion, plausibility, or intelligibility is a thoroughly unreliable criterion. Here is another circle to hermeneutics, as Taylor points out.

Our conviction that the account makes sense is contingent on our reading of action and situation. But these readings cannot be explained or justified except by reference to other such readings, and their relation to the whole. If an interlocutor does not understand this kind of reading, or will not accept it as valid, there is nowhere else the argument can go. 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 (Taylor 1985b, 24).

External Evidence. Hirsch suggested a different solution to the problem of the self-confirming character of interpretation: a move out of the text. The danger of being “trapped in the hermeneutic circle” (Hirsch 1967, 165) by a self-confirming interpretation could be overcome, he proposed, by seeking external evidence: information that lay outside the text. He argued that an interpretation must be “measured against a genuinely discriminating norm” (p. 26), one that provides an interpretation-free standard. Only by doing this would we be able to decide among competing interpretations, all of which might be quite plausible. And in a similar manner some interpretive methodologists have proposed that an interpretation be tested against various kinds of external evidence.

Hirsch himself suggested a specific norm: “the old-fashioned ideal of rightly understanding what the author meant” (p. 26), by which Hirsch meant “what the author most probably intended.” And although Hirsch’s reasons for rejecting coherence as a criterion were unnecessarily harsh, it does indeed seem appropriate that we should check with the person whose ‘text’ we are studying. If we interpret a written text we should consider the author; if we study action we should consider the agent. When we give an interpretive account it is appropriate to ask the author or agent whether we have understood what they meant. And so using an agent’s intentions as the norm against which to evaluate an interpretive account has seemed a fruitful move for psychologists to make, too.

Reasonable though it seems, this approach leads into difficulties if we are still looking, as Hirsch was, for a validation procedure. The central difficulty is that it is unclear how the intention can be identified. The intention-in-action no longer exists; it was present only at the time of acting. Ricoeur (1976, p. 100) points out, in explicit disagreement with Hirsch, that “the intention of the author is lost as a psychical event.” Lost, it can hardly function as the norm by which to evaluate an interpretation. Ricoeur goes so far as to
deny that the author's intention has any existence outside the meaning of the
text: "The intention of writing has no other expression than the verbal mean-
ing of the text itself. Hence all information concerning the biography and the
psychology of the author constitute only a part of the total information which
the logic of validation has to take into account. This information, as distinct
from the text interpretation, is in no way normative as regards the task of in-
terpretation" (1976, p. 100). The same argument holds against using an agent's
intention in acting as an evaluative norm. Even if we can talk with the agent
and ask her what she meant by acting as she did, she has to recall and inter-
pret her own action. If the agent is not accessible we can only infer intention
on the basis of biographical information and knowledge of the circumstances
of the action. Either way, the agent's intention is just as open to interpreta-
tion as the account we are trying to evaluate by means of that intention.

Hirsch has no convincing solution to this difficulty. He proposed, uncon-
vincingly, that an interpreter can have empathic access to the author's inten-
tion: "The interpreter needs to adopt sympathetically the author's stance
(his disposition to engage in particular kinds of intentional acts) so that he
can 'intend' with some degree of probability the same intentional objects as
the author" (p. 238). But how can this empathic response be made in a way
that is "external" to a reading of the text? How can our identification of the
author's intention be uninformed by the familiarity we have with the text?
How shall we resolve the disagreements that are bound to arise over the
author's stance? We can only understand an author's intention in terms of
our own culture and time, and so the character of that intention will always
remain open to reinterpretation.

This is not to deny that biographical information can be helpful, perhaps
essential, to an interpretive inquiry. But it is not at all clear how this informa-
tion provides a "discriminating norm" to distinguish good interpretive accounts
from bad. In fact "good" and "bad" seem only to mean correspondence, or its
lack, between the interpretation and the participant's intention. A correspond-
ence theory of truth is still at work here. The appeal to the author's or agent's
intention as an "external" uninterpreted norm can only fail to provide the
validation sought by Hirsch and others. But this suggests they are searching
for an impossible kind of evaluation, not that intentions (and other external
material) are of no interest. While it remains open to further interpretation,
the participants' recollection of their intentions can be valuable material for
an interpretive inquiry.

The Participants' Interpretation. Perhaps a better norm could be cho-
sen, though, one that would provide the desired objectivity? A second norm
that has often been proposed is the agent's current view of their action. This
norm avoids the problems of evanescent intentions; we can just ask our
research participants what their action means to them right now. Cuba and
Lincoln (1982) call for "member checks" to assure "confirmability" of an interpretation. Miles and Huberman (1984a) note that "as careful, plausible, and exhaustive as [an interpretation] may appear to be, parts or most of it could be dead wrong. One way of avoiding that outcome is to feed [it] back to site informants and ask for their corrective responses" (p. 142).

Of course this doesn't provide an objective norm either. At times such suggestions get suspiciously close to the notion that a valid interpretation is one that corresponds to a competent person's intuitions, and this, as we discussed in the Introduction, is at root a rationalist notion that does not hold up under scrutiny. Chomsky and Habermas have appealed to "informants'" intuitions to validate their reconstructions of linguistic and communicative competence. We have argued that intuitions, although interesting, can't provide the degree of certainty that is required of a validity test. What is the "meaning" of a text, or the "reality" of a situation, or the informants' view of the "accuracy" of an account, except another interpretation of it? If our research participants disagree with us we may indeed have made significant errors of interpretation. But we cannot rule out the possibility, perhaps small, that they disagree because they misunderstand their own activity, either because they were unaware of hidden aspects of that activity, or because defenses such as denial are at work.

This isn't at all to say that researchers always know better than research participants, or that talking with people about the way they see things can't be illuminating when one is struggling to make sense of puzzling material. Our point is again that the participants' view of things cannot provide an objective standard against which we can validate an interpretation, although this often seems to be what proponents of this approach are seeking. Significantly, they generally fail to suggest what we should do when genuine disagreements occur. If the researcher and the participants do not see eye to eye, how does evaluation proceed further? Miles and Huberman acknowledge that researchers and informants may disagree: "And we were left sitting on it, trying to think of a satisfactory way of getting resolution. We did find one good way of getting around these problems. It is that of testing the validity of one's findings by predicting what will happen at the site in six months or a year" (1984a, p. 142). But this is no solution. Instead of exploring why the disagreement occurred, the researcher is advised to shift to a different norm: prediction. Unfortunately, this norm is equally unable to provide unambiguous validity. Whose interpretation of events in six months should we consider valid? There are strong reasons (discussed in the Introduction) for thinking that meaningful prediction is impossible in human affairs. And when, on those occasions when the participants disagree with him, should the researcher modify an interpretation, and when should he turn to prediction instead in order to evaluate his account?
Consensus. Seeking consensus among researchers is a third approach to evaluation. An interpretation that can be called convincing should be communicable to others, should make sense to them and enable them to interpret new material in its light. This may be done by discussion among investigators in the same project, consultation with colleagues not involved in the project, and by peer response to published reports. But consensus too is often viewed as equivalent to a traditional evaluation procedure: the assessment of reliability. Agreement among interpretive researchers is seen as analogous to inter-observer or inter-rater reliability. When this is taken to its logical conclusion, agreements are quantified and treated statistically.

But obvious difficulties arise in treating consensus as though it is equivalent to repeated application of a measuring instrument. The most often mentioned difficulty is the possibility of collective delusion. We can easily imagine (or recall) occasions when agreements were reached that turned out to be mistaken. Agreement alone is no guarantee of correctness, and so consensus provides no foolproof criterion for evaluation. And consensus is of several different kinds. There is a difference between consensus among people simply new to the material, and consensus among people committed to a point of view different from our own.

When consensus is sought among people who are coming fresh to the topic of investigation there is a danger that we may simply train them to see things as we do. This kind of “training up” into an interpretive perspective seems a common (but usually hidden) occurrence in traditional research. When this happens it shows that our perspective is a viable one, but not that it is the best perspective, let alone the only one. We would seem to be setting a tougher test of an interpretation if we tried to convince others who have a different perspective. We would then need to show that our interpretation was viable, and also that it acknowledged the possibility of other perspectives (including that of the people we were talking with) and also that it somehow improved on these perspectives. An interpretation that could claim to be better would be one that provided a better account than rival interpretations.

Taylor outlines such a test for his interpretive account of the Enlightenment world view. “What would ultimately carry conviction would be an account of this development [of the modern world view] which illuminated it and made more sense of it than its rivals” (Taylor 1985a, 7). And the better interpretation would also explain how these rivals exist as possible, but mistaken, points of view. “The superiority of one position over another will thus consist in this, that from the more adequate position one can understand one’s own stand and that of one’s opponent, but not the other way around” (Taylor 1979, 67).
Perhaps instead of seeking consensus among researchers we should promote reasoned disagreement with our peers. Proponents of rival interpretations should each try to explain the other’s perspective. Taylor comments with a touch of cynicism on his “superiority” approach that “it goes without saying that this argument can only have weight for those in the superior position.” This surely need not be so; one would hope that arguments of this kind, if engaged in constructively, could be convincing even to those they are directed against.

But there is a further difficulty, one that Kuhn has pointed out. Competing perspectives are often incommensurable (Kuhn considers those involved in scientific paradigms, but the same holds for interpretative inquiry). Their proponents may use different terminology, have different cognitive commitments, and see the world so differently that their ability to grasp each other’s viewpoints is so limited that reasoned discourse is scarcely possible. Taylor, too, is aware of this difficulty. “Put in forensic terms … 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” (Taylor 1979, 28).

Consensus cannot, then, be taken as an interpretive analog to traditional reliability assessment. Even if people reach consensus on an interpretation further material may show it to be false. Conversely, holders of rival interpretations may fail to reach consensus not because the interpretations are flawed but because incommensurable perspectives prevent reasoned disagreement, and people talk past each other. Consensus, like coherence and external evidence, cannot be used as a technique for interpretation-free evaluation. However, some kind of discussion between researchers is clearly an essential aspect of interpretive inquiry.

Practical Implications. The fourth approach to evaluation is that of examining the relationship between an interpretive account and future events. But what should this relationship be? It is sometimes assumed that, just like an explanation in the natural sciences (at least in the positivist picture), an interpretive account should predict future events. We have already mentioned how Miles and Huberman talk of “testing the validity of one’s findings by predicting what will happen at the site in six months or a year” (1984a, p. 142). But there is actually little reason to agree with these authors that “Presumably, if the [qualitative] analysis is correct, it has implications or consequences. These can be spelled out and operationalized. The real-world results can be checked out in a new contact with the site. The result is the qualitative researcher’s version of a predictive validity coefficient that statisticians might use, for example, to see whether academic success predicts later job success” (p. 143-144).
There are several reasons why interpretive accounts don’t make testable predictions. MacIntyre (1984) and Taylor (1979) argue strongly that predictive generalization are inadequate explanations in social science and, more strongly, that meaningful prediction is impossible in human affairs. Understanding is essentially retrospective, after the fact. “Really to be able to predict the future would be to have explicated so clearly the human condition that one would already have preempted all cultural innovation and transformation” (Taylor 1979, 70). Granted, Taylor is talking here of major cultural shifts, but the same kind of innovation is perfectly possible, and possibly more frequent, in an individual institution like a school. Interpretive accounts cannot make meaningful predictions, nor is predicting a useful kind of explanation in the human sciences. The implication is that an interpretive account cannot be evaluated on the basis of its success or failure at predicting the future. If an interpretive account has implications for the future, these are not such that give us a version of predictive validity.

What, then, is the nature of these future implications? Fischer (1987, p. 8) provides a useful suggestion when she talks of a “pragmatism criterion” for evaluating interpretation: “does the presentation prove useful for understanding related phenomena and for maneuvering in the everyday world?” We have seen how interpretive inquiry is intrinsically linked to practical activity. The motivation for an interpretive inquiry is a practical concern, and what is uncovered when things go well is an answer to this concern. This answer should have direct implications for practice.

Interpretive research is itself a kind of praxis or practical activity, and its aim is not to describe the world in a detached manner but to act in the world, in an engaged manner. Interpretive inquiry has an emancipatory interest, not an instrumental one (cf., Habermas 1971); an interpretive account has the potential to emancipate people, to free them from practical troubles. (This is presumably why psychotherapy involves narrative interpretation so intimately.) As Lather (1986) puts it, “Emancipatory knowledge increases awareness of the contradictions hidden or distorted by everyday understandings, and in doing so it directs attention to the possibilities for social transformation inherent in the present configuration of social processes” (p. 259).

This indicates that an interpretive account might be evaluated with reference to practical changes that it has brought about, or at the least its usefulness for “maneuvering in the everyday world,” as Fischer suggests. In this vein Lather talks of “catalytic validity,” where we examine “the degree to which the research process reorients, focuses, and energizes participants toward knowing reality in order to transform it” (p. 272).
The Grail Escapes Again

The four approaches we have discussed do not make possible the kind of interpretation-free validation that traditional inquiry vaunts. Because of this, the interpretive methodologists who propose them often have second thoughts and end up apologizing. Certainly, if we were seeking the grail of validation these approaches should dissatisfy us. But on the contrary, they are reasonable approaches that are used regularly and fruitfully. Approaches such as these should not be abandoned; instead we need a better account of what kind of evaluation they provide.

We move closer to this by considering briefly the form that evaluation takes in the natural sciences. For it might seem that our argument merely shows that interpretive inquiry in the human sciences is unable to provide the kind of certainty that can be achieved in the natural sciences. But this is far from the case. Those who seek fixed validity criteria are requiring something of interpretive inquiry that, in actuality, not even natural science can provide. Interpretation-free validation is impossible there too and Kuhn, in an analysis of selection among scientific paradigms, contends that it would stifle science if it were possible.

Kuhn maintains (1977) that while scientists tend to agree on criteria of adequacy such as consistency, scope, fruitfulness, simplicity, accuracy, and others, far from these being meta-paradigmatic criteria that provide interpretation-free decision procedures for theory-choice, they are values that remain imprecise and open to interpretation. Scientists will legitimately differ in their evaluation of a specific theory. Furthermore, these values will often conflict with one another, as when accuracy may only be achieved at the cost of reduced scope, and so on. In Kuhn’s view this does not indicate that the criteria are inadequate or poorly defined. They appear incomplete only because the standard is an inappropriate one: logical rules or procedures. On the contrary, as “criteria that influence decisions without specifying what those decisions must be” (p. 330) they serve the crucial roles of permitting rational disagreement and spreading the risks that introducing and supporting a novel view of the world always entail. They serve to point out what is and is not relevant in a choice between theories, not to render the winner obvious and the choice unnecessary.

We propose that the four approaches to evaluation we have considered are reasonable because they direct our attention and our discussions to considerations we value when evaluating an interpretive account. Consistency, relationship to other material, the response of research participants, communicability to both sympathetic and skeptical peers, the responses of these peers, relations to alternative perspectives, practical implications, are all important. Neither together nor separately can they indicate whether an interpreta-
tion corresponds to the way things really are; they are not ways of validating. But they are ways we can consider whether what has been uncovered in an interpretive inquiry answers the practical, concernful question that directed that inquiry.

We said that two things needed to be seen in order to understand why these four approaches to evaluation satisfy us, even though they cannot provide interpretation-free validation. The first was that interpretation is not a matter of conjecture and guess, so that stringent validation (refutation) is unnecessary. (This should come as a relief, since we have also seen that validation is impossible!) The second was that a true interpretation is one that uncovers an answer to the concern motivating the inquiry. If an answer has been uncovered by an interpretive account we should find it plausible, it should fit other material that we are aware of, other people should find it convincing, and it should have the power to change practice. These are the four approaches to evaluation. But it will not necessarily be all of these at once. Nor may the account be any of these immediately. An interpretation that brings a solution to a practical concern may seem implausible and unconnected at first. Evaluation will never be straightforward and procedural; a choice among perspectives will be risky and sometimes incorrect. And as Taylor points out:

The practical and the theoretical are inextricably joined here. It may not just be that to understand a certain explanation one has to sharpen one’s intuitions, it may be that one has to change one’s orientation—if not in adopting another orientation, at least in living one’s own in a way which allows for greater comprehension of others. Thus, in the sciences of man insofar as they are hermeneutical there can be a valid response to “I don’t understand” which takes the form, not only “develop your intuitions,” but more radically “change yourself.” This puts an end to any aspiration to a value-free or ‘ideology-free’ science of man. A study of the science of man is inseparable from the options between which men must choose (Taylor 1979, 67-68).

One thing more. Even when an interpretation has been evaluated and seems to have uncovered something that answers our interpretive concern, there is yet another difference between this kind of evaluation and objective validation. Not only does interpretive inquiry provide accounts that will not seem true to all people (because their concerns and their perspectives will differ), it provides accounts that will not remain true for all time. This is a phenomenon Heidegger calls “fallenness.” Any elucidating account of a phenomenon has a tendency to lose its power and immediacy, to become a slogan and no longer be felt a moving description; to become a mere “assertion”—a hollow claim. Assertions—predicative statements—are the stuff of detached objectivity, and in the traditional approach they have been seen as privileged:
"The swan is white", "Frustration leads to aggression." For Heidegger, knowledge in assertions is subordinated to the more primordial kind of knowing involved in understanding and interpretation. Indeed, the correspondence theory is a consequence of mistakenly granting assertions a privileged position over ordinary practical understanding. (The personality-test items that Gergen exposed in Chapter 10 are assertions.) Stripped of their context, they are open to unbounded, and hence meaningless, interpretation.

No interpretation is safe. Even after an authentic projection has been drawn from primordial sources, we cannot assume it will be preserved. On the contrary, we must assume that the incessant pull of fallenness will set to work on it, threatening to turn it into something second-hand, derivative, used up (Caputo 1987, 73).

Any interpretation is passed along in the form of derivative assertions, and this means dimming it down. Eventually these assertions lose their power to disclose and no longer uncover anything of value.

Conclusions

We have suggested that a good interpretation, one that gives an account we can call true, is one that answers the concern that motivated our inquiry in the first place. At the same time, we have seen there is no interpretive method that would lead to a universally acceptable account, one that would be accepted by all sides. And there is no technique, no interpretation-free algorithm or procedure with which we can evaluate an interpretation. As Rosen (1987, p. 143) puts it: "There are no canons by which one can usefully restrict legitimate from illegitimate readings."

However, the four evaluative approaches we have considered have each been judged from a traditional perspective that claims that such canons must exist, and each has been judged flawed. Coherence seems inevitable. The agent's intentions or current view of events seem unreliable. Consensus seems either too easy to achieve or too difficult. Prediction seems impossible. Nor can we dodge this with an appeal to notions of 'partial' validation; with evaluation only within the bounds of probable error. The term "error" itself points to an ideal of "correctness," the ideal of a measuring instrument with perfect reliability, one that will give identical measurements on repeated trials, one that is impervious to unwanted influences, the way an ideal measuring-rod is unaffected by shifts in temperature. In interpretive inquiry we must drop this ideal of universal certitude.
An analogy may help. An interpretive account resembles in some respects a hammer in a workshop. The traditional approach to validation involves a misunderstanding that is like thinking the question, "How good is the hammer?" is similar to "How heavy is the hammer?" Two things are overlooked: the task in which the hammer will be employed, and its place among the other tools (cf. Heidegger 1927/1962, 413). The hammer's weight can be established with no attention to either of these. But the hammer is good only if it advances the current task and only if it works well with its companion tools. This choice is not a fixed one, it will change as new phases of the task arise. Heavy hammers are better for framing walls, and light ones are good for carefully placing molding pins. In the same way, a good interpretive account is one that advances the practical concerns with which we undertake an interpretive inquiry, and one that works well with the other activities we are engaged in.

The four approaches to evaluation stem from forms of persuasive reasoning that have developed over the centuries; reasoning we engage in when questions of veracity arise in our everyday interactions. If we are unsure how to understand someone's action we can see if our interpretation holds together and makes sense (applying the logic of our choice), we can ask her what she meant (with a skeptical ear), we can talk with others (aware we may be seeking their agreement), and we can look to see what happens when we tell her our interpretation. If these checks pan out we are calmed, but if we are wise we don't feel an unwarranted confidence that we've got to the bottom of the matter. Ultimately we will never be quite sure; we may have to change our mind the very next moment: a revolution may take place.

Only in this century has there been a general recognition that the search for epistemological security can never succeed. This suggests that we would be better employed working to open up new perspectives, rather than trying to justify whatever perspective we currently hold. The logical positivists' reaction to Einstein's revolution in physics was to hope such a thing wouldn't happen again. Having seen what that attitude led to, shouldn't we learn from their mistake and struggle to stay open to such reorientations? We should, perhaps, be attempting to invalidate our own interpretations; to look for the cracks in their apparently polished surfaces. We've learned that it is the troubling anomalies, such as the precession of Mercury's perihelion, that catalyze a change in scientific paradigm. In a science such as psychology that will never be paradigmatic in the way that physics and chemistry are, we ought to ferret out anomalies rather than try to prove ourselves correct. Hermeneutics has no magical recipe for evading dogmatism; if we tend to scold empiricist and rationalist inquiry for tending to lionize one view of things as the "true" one (or "scientific," "objective," "accurate," "real") or as the only practicable one ("operationalized," "pragmatic," "functional"), interpretation has its own
tendency to slide into tendentious opinions and pronouncements. Claims to
demonstrate validity only exacerbate this problem. Instead we interpretive
researchers might all be required to spend time debunking our own perspec-
tives: pointing out their flaws and shortcomings; documenting the anomalies
and oddities that remain puzzling and unexplained, the fish that have escaped
our nets. These things waken us in the small hours to a recognition that, even
as psychologists, there is much about people we still can’t give a name to.
Notes and References


Chapter 12

References


