



WILEY

A · M · E · R · I · C · A · N
A N T H R O P O L O G I C A L
A S S O C I A T I O N

Review: Hermeneutics in Anthropology

Reviewed Work(s): *Interpretive Social Science: A Reader* by Paul Rabinow and William M. Sullivan; *The Said and the Unsaid: Mind, Meaning and Culture* by Stephen R. Tyler

Review by: Michael Agar

Source: *Ethos*, Autumn, 1980, Vol. 8, No. 3 (Autumn, 1980), pp. 253-272

Published by: Wiley on behalf of the American Anthropological Association

Stable URL: <https://www.jstor.org/stable/640129>

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact support@jstor.org.

Your use of the JSTOR archive indicates your acceptance of the Terms & Conditions of Use, available at <https://about.jstor.org/terms>



American Anthropological Association and Wiley are collaborating with JSTOR to digitize, preserve and extend access to *Ethos*

JSTOR

Hermeneutics in Anthropology:

A Review Essay

MICHAEL AGAR

Recently two books appeared that, for me at least, announced the arrival of something with the mysterious name of “hermeneutics” into American cultural anthropology. One volume, *Interpretive Social Science: A Reader*, (1979) is edited by Paul Rabinow and William M. Sullivan. After an introductory essay, the reader consists of three programmatic pieces by Taylor, Ricoeur, and Gadamer. These are followed by sample applications of hermeneutic ideas in such fields as literature, political science, history of science, psychoanalysis, and the sociology of religion. Anthropology is represented in two essays by Clifford Geertz—one, his classic paper on the Balinese cockfight; the other, a more recent piece on anthropological understanding.

The second volume is a single-authored work entitled *The Said and the Unsaid: Mind, Meaning and Culture*, (1978) by Stephen R. Tyler. Tyler is concerned with the everyday use of language. With this concern in mind, he evaluates a variety of formalisms in an-

MICHAEL AGAR is Research Associate at the Language Behavior Research Laboratory, University of California, 2220 Piedmont Avenue, Berkeley, CA 94720.

Support by RCDA 1 KO2 DA00055 is gratefully acknowledged. Helpful comments were offered by Gelya Frank, Paul Rabinow, Bambi and Buck Schiefflin, Steve Tyler, and the Editors of *Ethos*.

ETHOS 8:3 FALL 1980

Copyright © 1980 by the Society for Psychological Anthropology
0091-2131/80/030253-20\$2.40/1

thropology, linguistics, and psychology, showing their limits as representations of everyday language. He develops the idea of “meaning as contingency,” and draws on hermeneutics as a compatible framework within which to construct his view.

None of this material is particularly light reading. I was caught in the dilemma of wondering whether this is due to lack of clarity on the part of the authors or the nature of the material and its dissonant fit with traditional ideas of social science. Besides, reviewing material on interpretation carries with it a dizzy reflexivity—what one is reading applies even as it is being read. I should add that some of the illustrative materials in the Rabinow and Sullivan reader provide good concrete examples of a hermeneutic point of view—the articles by Geertz, Fish, and Hirschman, in my opinion, stand out in this regard.

As I worked on my own hermeneutic problem of constructing a meaning for the two volumes, I decided to focus more on the general nature of the perspective than on specific instances of its use. For this reason, my essay draws most heavily on the initial programmatic chapters in Rabinow and Sullivan, as well as the later portions of the Tyler volume. This essay is not, strictly speaking, a book review. Rather, it is an attempt to come to terms with a perspective represented in two recent books in anthropology.

The term “hermeneutic” comes from ancient Greek. *Hermeios* referred to the priest at the Delphic oracle. The verb *hermeneuein* is generally translated as “to interpret”; the noun, *hermeneia*, as “interpretation.” The name of the wing-footed messenger god, Hermes, is also related. Hermes was credited with the discovery of language and writing, and “is associated with the function of transmuting what is beyond human understanding into a form that human intelligence can grasp” (Palmer 1969:13). The term was apparently used in the senses of expressing aloud in words, explaining a situation, and translating.

The field of hermeneutics in modern times has something of an intricate history. Palmer (1969:33) offers a roughly chronological sense of the different uses of the term:

(1) the theory of biblical exegesis; (2) general philological methodology; (3) the science of all linguistic understanding; (4) the methodological foundation of *Geisteswissenschaften*; (5) phenomenology of existence and of existential understanding; and (6) the systems of interpretation, both recollective and iconoclastic, used by man to reach the meaning behind myths and symbols.

He associates Schleiermacher with (3), Dilthey with (4), Heidegger (later, Gadamer) with (5), and Ricoeur with (6).

Since hermeneutics has such an elaborate history, one wonders where it has been. The idea of "interpretation" is discussed in American cultural anthropology, but it is usually presented without its philosophical context. In their introduction, Rabinow and Sullivan suggest that part of the reason for this neglect lies in the strong commitment in Western culture to a particular notion of "science." That notion emphasizes timeless, context-free axiomatized laws, controlled manipulation of clearly defined variables, and mathematical measurement in a quest for objective knowledge about the world. Rabinow and Sullivan argue that operating within this philosophical context is an epistemological error for the social sciences.

It is an error just because it treats, as problems to be eliminated, the most important aspects of one human trying to understand some others. Rather than trying to eliminate "observer effects," for example, one focuses on the interpreter as participant in a tradition which guides and is changed by the process of understanding another. Rather than striving for "objective knowledge," one accepts the fact that knowledge is situated in a historical moment, some of whose presuppositions are difficult, if not impossible, to articulate. And, at its most radical, one no longer speaks of "validation," but simply assesses an interpretation for its coherence, for its ability to make sense of one tradition from the point of view of another.

These and other issues will be revisited when the material is discussed in more detail in a moment. For now, I would like to note that hermeneutic philosophy provides a more comfortable fit with the kinds of problems an ethnographer routinely faces than does the traditional notion of science discussed above. As evaluated by traditional guidelines, we often have something of a self-conscious preoccupation about whether or not ethnography is really a "science." Our tradition emphasizes such difficult notions as holism, context, and symbol. We have always talked, sometimes with embarrassment, of the importance of the intuitive apprehension of pattern. We stress learning rather than control, often using such metaphors to characterize our work as "student" and "child." Our literature on field methods is full of concern with the perception of the ethnographer, the role relations between ethnographer and informant, the emergent nature of ethnographic fieldwork, and the importance of

the individual ethnographer in the nature of the resulting ethnographic report. Hermeneutic philosophy teaches that such issues belong at center stage; they are not embarrassing problems to be avoided on the way to a good hypothesis.

Hermeneutics also brings with it some serious problems. I have a strong commitment, born of 12 years of interdisciplinary applied work, to improving ethnographic methodology. In such applied settings, ethnography often plays a subversive role, in the sense that an ethnographic report of group X usually complicates, and often contradicts, the assumptions about group X that inform current policy. If one expects administrators to act at all on the basis of such "deviant" knowledge, one had better present a credible case. The problem becomes more complicated when one considers that the recipient of a report probably has a notion of social science that is conditioned by the classic idea of the testing of hypotheses. Yet hermeneutics teaches, in its most radical form, that validation is not possible in the classic scientific sense.

But this position is contradicted by one's intuitions that some interpretations are better than others, at least from an ethnographic point of view. For example, I am now reading Mary Lee Settle's novel *Blood Tie*—fortuitously an excellent novel as a case study in hermeneutics. Among many possible examples from the book, let me mention just one. On a festival day in a Turkish coastal town, the local women back away and make a discreet gesture against the evil eye to protect themselves against the visiting European and American women, whom they see as indecently dressed. A British observer, in contrast, comments on the politeness of the local people as they make way for village guests. As an ethnographer reporting on the *village* point of view, which interpretation would you prefer?

Or, let me use another example I have discussed in detail elsewhere (Agar 1980a). Several years ago, while working in a small South Indian village, I was about to walk to town. Just before the cook wrapped a lunch of bread and spinach in a cloth, he put a small piece of charcoal on top. I didn't know what to make of it. My own sense of charcoal and food had something to do with cooking. Only later did I learn that I was traveling at midday, a time when spirits were particularly active. They were especially attracted to lone travelers carrying food, and the charcoal was placed in the package to serve as a spirit repellent.

Or consider another example. When I first began work in New

York on the issue of the use of methadone in the streets, I was surprised to hear some addicts talk about going to a methadone clinic as “going to cop.” It was among my first clues that, from a street point of view, methadone was interpreted quite differently than by the medical profession. The clinic was the “dealer,” the patient was a “junkie,” the methadone was the “dope,” and getting methadone was “copping.”

Any ethnographer could provide numerous examples of his or her own to support the intuition that, among all possible interpretations of an action, some of them are ethnographically “better” than others. In fact, one of the main contributions of ethnographers, in a variety of theoretical and geographical areas, has been to show that a prevailing interpretation of some human action was in fact incorrect from the group’s own point of view.

These three short examples are offered to ground what is about to become a rather abstract discussion. We will return to them shortly to assess the hermeneutic positions represented in the two volumes, with a special emphasis on the problem of validation.¹

THE PERSPECTIVE OF HERMENEUTICS

We begin with Gadamer’s essay on the “principle of historical productivity.” Ethnographers participate in a tradition; they move in a world of meanings that give a sense of their past, their present situation, and their future possibilities. They cannot stand apart from their world, for there would be no meanings in terms of which they could characterize their own existence. They live within a particular “historical horizon.”

However, they are capable of what Gadamer calls “historical self-consciousness.” Through an awareness of their historicity, they are capable of bringing some aspect of their world into consciousness and reflecting on it. With this consciousness comes not only awareness, but a sense of other possibilities. A piece of their world, previously implicit, stands revealed as a “prejudice” which can be changed. Through historical self-consciousness, then, there is the possibility of altering a world, of changing the horizons of a history.

¹ Undated references in the following discussion refer to the Tyler volume or to articles included in the Rabinow and Sullivan reader.

There are many ways that one's world can partially be brought to consciousness. Unifying them would be a confrontation between the person and that person's world—something goes wrong; there is a problem; expectations are violated. Such “breakdowns” call into consciousness a part of the world which previously existed outside awareness. Awareness of the problem's existence is the beginning of a hermeneutical situation. The interpreter is torn between “his belongingness to a tradition and his distance from the objects which are the theme of his investigation” (p. 155).

To deal with the problem, now bracketed as an “object” for interpretation, the person attempts to fuse the horizons of the self and the object. One “appropriates” the object, brings it into his or her world. One begins by reflecting on his or her preconceptions, the “preunderstanding” brought to the encounter. One is guided by an “anticipation of perfect coherence” (p. 153). One moves back and forth in a dialectic between one's world and the object, constituting meanings, altering one's horizons, until the object and the world are unified into a coherent whole. One thus “understands” the object and one's own convictions within the same experiential event.

This process of understanding is characterized as “a system of circular relations between the whole and its parts” (p. 153). The interpreter's world and the meanings for the object accommodate each other dialectically—one cannot be understood in separation from the other. This is one form of the “hermeneutical circle.” Rather than seeing this as a problem, though, Gadamer discusses it as a “fundament of positive and productive possibilities for understanding” (p. 156). This is his *principle of historical productivity*.

How does one know when he or she has understood? “Coherence” is one clue—we understand when the object disappears as a problem and becomes something we can make sense of in terms of the meanings in our (now altered) world. Gadamer also states that we have understood when “we do not lose any ground in the face of any possible arguments advanced by one's adversaries” (p. 131). By his own argument, though, it is difficult to see how the “possible arguments” could be enumerated.

Once one becomes accustomed to the jargon, Gadamer's discussion provides a framework within which to talk about the ethnographic examples given earlier. In the example from the novel, there is no problem, no breakdown. Rather, we see two different traditions used to make sense of the same behavior. The

Englishman and the villagers both come up with coherent interpretations. Though the Englishman does make sense of the situation, we somehow want to show eventually that, in spite of its coherence, it is ethnographically weak.

In my own examples from India and New York, a breakdown does occur. It occurs partly because, as an ethnographer, I am trained under the charters of “culture” and “symbol” to look for it. In the methadone example, I have an interpretation, like the Englishman, provided by my own tradition. The person I see is “a methadone patient.” However, I also have a background that stresses attention to informants’ meanings, so that when I hear “going to cop,” it produces a breakdown in my understanding. Beginning with the difference between my own tradition’s definition of “the clinic” and the informant’s use of a term that applies to dealing heroin, I move back and forth between observations and conversations until I understand why the informant said “going to cop.” In the process, my own tradition is changed, for I will never again *just* look at methadone clinics from the medical point of view.

In the case of the charcoal included with my food in South India, the breakdown occurs even before I know anything about group meanings. The stream of behavior—placing the charcoal on top of some food—is simply something my tradition does not enable me to interpret. I am confronted with actions that make no sense. Again, I move back and forth until I understand it, and again my tradition is altered in the process. For these examples, then, Gadamer would show the similarities and differences as follows: In the first case, two interpretations exist side by side, both perfectly coherent. In the second, two interpretations exist, but the ethnographer moves to understand one after beginning with the other. In the third case, the action is initially uninterpretable—a breakdown in the classic sense of the term—and the ethnographer moves to appropriate it into his tradition.

Not that there aren’t problems with Gadamer’s argument. All understanding *is* historical—therefore, “objective” knowledge, knowledge free of context, is not possible. But this leaves us in something of a lurch in terms of methodology, since in his article Gadamer offers only “coherence” and “ability to defend” as the main sources for an evaluation of an interpretation. Palmer (1969:67) argues that this is not a fair criticism—he writes that Gadamer recognizes the need for more methodological concern, but

that his prime interest, explicitly stated, is in an analysis of the ontological nature of understanding.

Hirsch, a critic of Gadamer, argues that the methodological problems result from a failure to separate "meaning" from "significance." A text (this is the type of object that interests Hirsch) has a verbal *meaning* which is "changeless, reproducible, and determinate" (Palmer 1969:81). This meaning has any number of "significances," depending on the historical context in which it is interpreted. However, meaning, not significance, is the problem for hermeneutics. Furthermore, Hirsch argues that the meaning can be validated by appeal to the author's intention, about which evidence can be gathered. Palmer dismisses Hirsch's arguments, feeling that he has reduced hermeneutics to the "logic of validation" only.

Ricoeur, in his article, takes an intermediate position, adopting Hirsch's idea of the "dialectic of guess and validation." Ricoeur suggests approaches from the "logic of subjective probability" and "legal interpretation" as methodological sources. He also notes that validation carries with it the possibility of invalidation, citing Popper's discussion of the importance of "falsification" in science. He does not fully develop these ideas, though. A possible synthesis of these different positions will be offered later. The issue is, as Geertz notes, that there are methodological problems "to make a Freudian quake."

HERMENEUTICS AND SOCIAL LIFE

In their essays, Taylor and Ricoeur extend hermeneutics explicitly into the study of human life. Though they differ somewhat, both, I think, characterize behavior in ways that fit in with Gadamer's description. Behavior has a context; it is situated in space and time and connected to other aspects of the situation; it is intended by a human agent for another; and it has "a sense distinguishable from its expression" (Taylor) or "refers to a world" (Ricoeur).

Now the complications increase. It is one thing to talk of the ontology of understanding when the object is not conscious—Palmer, for example, discusses Heidegger's frequent use of the example of a "hammer." It is quite another thing to talk about understanding when the object involves consciousness of others. And it is more complex still when the object becomes a "text" in an as yet unclear metaphorical sense.

Ricoeur devotes much of his chapter to a discussion of the notion of text. In a text, discourse is “inscribed” and detached from its “moorings” to the psychology of its author. It is freed from its ostensive reference to the situation of its doing, and addressed to a potentially unknown and large audience. Obviously a text also has some natural edges as a phenomenon—it is a finite, neatly bounded entity which guarantees that separate interpreters will agree that it is the common object of interpretation.

He then extends the text metaphorically to “action,” noting that it has a propositional content which can be identified in other actions; it is inscribed in the sense of such social metaphors as “record” and “reputation”; it can have consequences for other actions different from it; and it is open to multiple interpretations by others who can “read” it. Action, in other words, does have textual characteristics.

Taylor, with a critical eye cast towards the field of political science, takes a different line. Action, he notes, is obviously for and by a subject. He goes on: “We make sense of action when there is a coherence between the actions of the agent and the meanings of his situation for him” (p. 35). He adds that “making sense” does not necessarily imply a rational account.

But then, he notes that we are dealing with both “text analogue” (behavior) and “text” (accounts). He discusses how the agent experiences a situation in terms of meanings, which are then “shaped” by the language through which the agent lives, which are in turn interpreted by the outsider. He adds the possible complication of the outsider’s interpretation in turn feeding back, altering the world within which the agent does *his* or *her* own interpretations.

He goes on to characterize language and situation as constitutive—practice “has to do in part with the vocabulary established in a society as appropriate for engaging in it or describing it” (p. 47). In fact, he distinguishes what he calls “experiential meaning” from linguistic meaning just because linguistic meaning is the meaning of signifiers *about* a world of referents. There is a fine line between language as constitutive through “description” and reference, but let that pass for now.

He characterizes experiential meaning as “not just in the minds of the actors but out there in the practices themselves” (p. 48). In other words, the meanings that the outsider is after are intersubjectively

available. He briefly develops the idea of “common meanings” which are the “basis of community.” These common meanings are “interwoven” with a “powerful net of intersubjective meanings” (p. 51). This discussion begins to sound something like the anthropological idea of “themes” (Opler 1959), but Taylor does not pursue it in this chapter.

With Taylor and Ricoeur, then, we get the feeling of some new possibilities, and equal number of accompanying headaches, and a strong sense that we’ve been there before. The primary object of interpretation for an ethnographer is a group of subjects who themselves interpret. The horizon we are trying to fuse with as ethnographers is in constant motion, and it consists of several different individual horizons which are unlikely to be nicely congruent. With Taylor and Ricoeur, we begin to see the problems in the extension of philosophical and literary hermeneutics into ethnography. At the same time, the problems often have a familiar ring. Many of them represent issues, explicit and implicit, that are as old as ethnography itself.

While discussing the problems introduced by extending hermeneutics into the study of human action, let me bring in some of the material from Tyler’s book. He is interested in the way language works in everyday life. With a continual view towards everyday use, he takes an impressive tour through a variety of perspectives from linguistics, anthropology, psychology, and phenomenological sociology. His critique throughout is an attack on formalism—those disembodied, mechanical representations of human life that must be reconnected to the world by means of “awkward and Byzantine rules” (p. 461). I was sometimes derailed by what I saw as a burst of reflexive denial, with his own formalized representations of his critique on formalism. But for the most part, it is an impressive synthesis, with some delightful nose-thumbing at a few sacred formalisms along the way.

Tyler explicitly uses hermeneutics to make a transition from the study of meaning as *necessity* to meaning as *contingency*. Tyler comes to hermeneutics as a compatible framework within which to further develop what he sees as the two fundamental and neglected issues in formalist approaches—will and representation. In his critique, he repeatedly notes, by appeal to anecdotal instances of everyday life, that use of language is situational and purposive. He also stresses the constitutive nature of language, showing how everyday

talk is an unfolding, emergent process—meaning, in other words, is contingent. Each step is “reflexive, confirming both itself and prior understandings and serving as a basis for further interpretation” (p. 364).

To get to this point, he first describes the various ways that speakers tie discourse together. He travels across such well-known concepts as presupposition, entailment, equivalence, paraphrase and metaphor, anaphora, synonymy, ellipsis, and several others. His discussion varies from the usual formalist approaches, however. For example, he notes that synonymy is *formally* defined as: X is synonymous to Y if they are mutually substitutable in all contexts with no change in meaning. Through examples he shows that it is more sensible to assume that in everyday use the context usually does change with such substitution, but that nonetheless, hearers will accept the change as “close enough.” He then shows how a speaker can, in fact, slowly shift meanings so that by the end of a discourse the meaning of the “synonym” is actually quite different from the initial way of talking. The various shades of difference, unnoticed as they gradually occur over extended discourse, add up to a substantial difference by the end.

He then considers, as part of the hermeneutic circle, the problem of arriving at a representation of the whole meaning of a text in relation to its parts. This circle is a bit different from Gadamer’s use of the idea, discussed earlier. For Gadamer, the circle encompasses the interpreter and the object. For Tyler, now that the object is a subject who also interprets, the focus shifts to how the situated agent coordinates meaning between part and whole of the expression he or she is producing or attending to. Same circle, different scope.

A guideline for successful interpretation is to judge if that interpretation is reasonable in a given context. Two of the problems with everyday reasoning, though, are that it rests on tacit understandings that are only made explicit in problematic cases, and that the interpreter must have the capability to treat utterances as an “index of a general pattern” (the so-called documentary method of interpretation).

With these problems in mind, then, an interpretation is reasonable if it fits “into an unfolding scenario, plan, or script characterized by causality, likelihood, moral necessity, typicality, or means-ends efficacy. They are implicated in a linguistic and extra-linguistic whole. These normal schemata, in other words, contain or

exemplify means of inference" (p. 399). In other words, they constitute "the causal texture of the world." He adds that this does not yield determinate interpretations, for there is obviously room here for perspectival relativity, but the interpreter looks for "consistency between intentions and underlying patterns" (p. 402).

Tyler finishes with the general point that "we understand utterances and texts by providing them a context of possible experience" (p. 454), which contains "time, place, personnel, motives and acts corresponding to a schema of possible experience" (p. 453). This process is "immediate, tentative, and whole." Perhaps it is not always *that* tentative, for elsewhere he notes that "as the hearer's interpretive edifice grows, fewer and fewer are the things that disconfirm it, so much so that even the speaker's overt attempts to disconfirm it may be taken instead as confirmation" (p. 411).

Tyler, Taylor, and Ricoeur, in their different ways, shift the focus from the hermeneutical situation of interpreter and object to the hermeneutical properties of the "object" itself when it consists of other historically situated persons. Ricoeur argues, in part, that social action has textual properties and is therefore amenable to a hermeneutic approach. Taylor makes a similar argument about the hermeneutic aspects of everyday life, though he does so with the intention of criticizing the concern in political science with "brute data." Tyler, on the other hand, takes after a range of formalisms that claim to represent different aspects of the human use of language. In comparison with the Gadamer essay, with which I began this discussion, the outside interpreter is curiously absent. That leads into a set of problems that I would now like to discuss.

ETHNOGRAPHIC HORIZONS

First of all, in my mind there is some confusion over the use of hermeneutic principles as a guide for ethnographic work versus the use of hermeneutics as a guide to the properties of the group activities that the ethnographer confronts. Clearly, they are applicable in both contexts. In fact, the two Geertz contributions illustrate this. In the one, Geertz shows how the Balinese interpret the cockfight. In the other, he discusses "anthropological understanding" more generally, showing how the ethnographer works within a hermeneutical circle that articulates details, the whole of which they are but a portion with reference to each other.

One interesting possibility, argued by Gadamer with his emphasis on the ontological unity of all understanding, would be that these two uses are but different instances of the same process. But are they? On the one hand, Spradley, in his recent book on the ethnographic interview (1979), introduces the idea of ethnography by discussing the situation of a person who has just moved into a new neighborhood. In my own introductory treatment of ethnography (1980b), I also use an everyday problem—explaining an experientially rich trip to a friend—as a similar introduction to the problems of ethnographic method. Along other lines, Hill-Burnett (1974) has argued that development of ethnographic method is in fact a contribution to cultural learning theory in general. Without much modification, her argument could be translated into hermeneutic terms to support Gadamer's position.

On the other hand, in Spradley's and my discussion, the examples were introductions. The hermeneutic problem confronted by the ethnographer is in fact similar to that confronted by anyone who is a newcomer to a group. But that does not necessarily mean that the process by which understanding is achieved is the same. This is an impossible issue to resolve, since the nature of "natural" and "ethnographic" understanding (if indeed they are distinct) is not well understood at any rate. It is difficult to compare unknowns.

Perhaps ethnographic understanding will turn out to be a more "historically self-conscious" version of natural understanding. But if I had to wager on this particular issue, I think I would bet that ethnographic understanding will include everyday understanding, but will be broader and more self-conscious. As a side bet, I would also wager that everyday understanding itself, under further analysis, will be shown to vary in some interesting ways—a conclusion easily reached from a reading of Tyler's book. We now approach the vertigo of reflexivity by adding that a study of understanding outside of the worlds of its occurrence may in fact be a violation of its own ontological characteristics. Understanding, like the subject and object that unite within it, may be bound by the horizons of its occurrence.

Now to a second problem: the object of interpretation and the hermeneutic circle. Ethnographers are primarily concerned with neither hammers nor texts. The groups that they approach are themselves interpreting in worlds that are both much richer and much less neatly bounded than texts. The general statement of the hermeneutic circle is that the whole is interpreted in terms of its

parts, but the parts are in turn interpreted in terms of the whole. But “whole” and “parts” may take on different meanings, as we saw in the discussion of the two Geertz articles, and in the comparison earlier in the paper between Gadamer and Tyler.

Since the group activities in themselves consist partly of interpretations, we can look at the hermeneutical circle as a characteristic of these activities. This we see emphasized in Geertz’s study of the cockfight and in Tyler’s book. On the other hand, we can also view the circle with reference to the ethnographer encountering the group. In this sense, the “whole” is the encounter of the ethnographer in his or her world with a group foreign to that world. The “part” is the new meanings that allow this world and that group to be fused or appropriated. This we see emphasized in Geertz’s essay on anthropological understanding (though in a limited epistemological sense) and in Gadamer’s article.

Again we are back to the first problem. Are these distinct uses of the concept of the hermeneutical circle? On the one hand, the different hermeneutic circles suggest different questions to ask of an ethnography. As a characteristic process of group life, the circle suggests ways to evaluate the ethnographer’s characterization. Taylor’s article and Tyler’s book, for example, are critiques of social science just because they ignore such characteristics.

But as a problem in ethnographic research, the circle demands more of an account of the ethnographer’s world as a necessary part of understanding the interpretations offered. There is less material in these two volumes to illustrate this point, interestingly enough. There are, however, some recent personal accounts of fieldwork written from an explicitly acknowledged hermeneutic perspective (Rabinow 1977; Dumont 1978). In fact, in support of the distinction between the two circles. Dumont wrote two books, one “about the Panare” and the second one, cited here, “directed toward perceiving, apprehending, and interpreting the ‘and’ of the relationship which my fieldwork built between an ‘I’ and a ‘they’ ” (1978:3).

Now on the other hand, if we accept the ontological unity of all understanding, then the apparent confusion results from an artificial distinction imposed on a dialectic reflexivity. To restate this, hopefully more lucidly, an increased understanding of the hermeneutical circle of ethnographer and group is also an increased understanding of the hermeneutical circle for the people that constitute that group, and conversely. To put it yet another way, from a

hermeneutic perspective, as ethnographers get better at their work (in the sense of how they do it), they will get better at their work (in the sense of how they describe how groups get things done), and conversely.

There is a risk here. A while ago I wagered that eventually ethnographic understanding would be seen as different from everyday understanding. If I win the bet, and if we don't treat the two applications of the hermeneutical circle as distinct, then we have adopted a universal model of human life that may lead us to miss the central issues of a group's life as the group lives it. People have been seen as, among other things, large rats and ambulatory computers. I would like to think they might appear more complicated and interesting as inarticulate hermeneutic philosophers or untrained ethnographers, but I am not sure this would be the case, nor am I sure it would be accurate.

Now, how can one write of accuracy after this extended discussion of the plurivocality of the interpreted object, the relativity of the historical situation of the interpreter, the inescapability of the hermeneutical circle, and so on? First of all, recall the discussion of Settle's novel and the two anecdotes I offered from my own fieldwork in South India and among urban American narcotics users. In the examples, different interpretations were suggested for one action. Both were acceptable from a hermeneutic point of view—they made sense of the action—and given the notion of plurivocality, it is no surprise that more than one interpretation was possible.

However, I don't think there's an ethnographer alive who, given the information that the study was about Turkish villagers, South Indian villagers, or urban junkies, would not pick one of the interpretations as ethnographically better than the other. This is not an argument for a *single* interpretation, but an argument that some are better than others. It is crucial that ethnographers have a way to document this kind of judgment in a way that can be publicly displayed in a credible manner.

Let's consider these three issues—the scope of the circle, the properties of the studied group, and the issue of validation—by returning to the three ethnographic examples in more detail. The first thing to notice is that the examples are all *situational*. Like ethnography itself hermeneutic philosophy emphasizes what Goffman (1964) long ago called “the neglected situation.” The second thing is that the situations are constituted through the words and ac-

tions of informants and an ethnographer. In the Turkish village, of course, there is difference. For the Englishman, the action is tied in with “politeness”; for the villagers, it is seen as “avoidance of the evil eye.”

I don't mean to say that the Englishman's sense of the action is irrelevant, especially, say, to an ethnographer in the village interested in the impact of tourism. But, to address the issue of validation, I do want to argue that, for an ethnography of Turkish village life, the Englishman's interpretation is wrong in some important ways. The problem is to figure out explicitly how this assessment can be made by an outsider reading an ethnographic report. While the issue will not be resolved here, I would like to suggest at least a couple of considerations that might point towards some solutions.

Let me now shift to my own two examples. In both cases, the natives constitute their world in one way; the ethnographer constitutes it in another. How does the ethnographer know when the job is done? When has the ethnographer, through the classic work of observation and conversation, understood the situation in an ethnographically adequate way? Put another way, how can we begin to unearth the presuppositions that guide the evaluation of ethnographic accounts?

First of all, recall that the two examples are just that—fragments of long-term ethnographic studies. Surrendered back into the flow of ethnographic work, two important possibilities for validation emerge—possibilities that occur just because we work with living human groups rather than disembodied texts. The first possibility is indicated by our commitment to make sense of informant interpretations. In the Turkish village, the Englishman is not even aware that a different interpretation exists. He lacks what we might call an ethnographic attitude. In my two examples, I not only am trained to look for different interpretations by group members, but I am supposed to try to make them happen. When I think I understand something, I should, through further observation and conversation, try to find something that doesn't fit.

In this commitment to group meanings as the ultimate authority of our ethnography, we find one important validation anchor. Of course the ethnographer brings to the work his or her tradition within which similarities and differences are assessed, an issue amply documented by the differences in restudies. But even so, given a commitment to report group situations and group interpretations of

those situations, it would seem that ethnographers nevertheless attend to similar phenomena.

There is the counter-argument that the process of interpreting alters what is interpreted (see Rabinow 1977). At this point, my own bias is to feel that our impact is more often than not overrated. Part of the reason that ethnographers routinely think in terms of a year for their work is so that community activities will return to normal after their entry. Few people, I think, will maintain a special self in response to an ethnographer in all situations for such a long period of time. Besides, in my own experience in the drug field in the 1960s, four ethnographers trained in three different disciplines—two in different northeastern cities, one on the west coast, and one (myself) at a Kentucky treatment center—independently came up with descriptions of the heroin addict's world that were remarkable both for their consistency and for their shared differences from the then dominant view of the addict as a social-psychological failure. Later, in the 1970s, another ethnographer and I, working independently in two northeastern cities, made some similar statements about methadone use from the street point of view. Something is going on here, and I think it is our shared implicit commitment to make sense of group meanings, whatever else we then do. This issue points to an area in need of methodological development.

A second methodological issue is raised by the continuity of group life. As we make sense of situation 1, 2, . . . N, we do not treat each *de novo*. Rather, we look for connections, for threads or themes that recur across them. I think this quest is connected with Taylor's ideas about "common meanings" and Ricoeur's discussion of "depth semantics." As discussed elsewhere (Agar 1980a), the charcoal example from South India is eventually shown to connect with other situations in the village—situations related to spirits, patron-client relations, social isolation, time, and space. The point is that, as illustrated in Geertz's articles, the ethnographic "whole" that we construct in order to make sense of the "parts" we participate in, leads us to have *expectations* about future situations. These expectations can be systematically checked.

There are some other methodological implications in the hermeneutic literature reviewed here that are also related to ethnographic work. For example, there is often an emphasis on "breakdown." Either through the conscious discipline of the ethnographer or

through encounters with the group, problems arise—differences manifest themselves and suggest new possibilities. I am reminded here of several scattered discussions. For example, there is Turner's (1957) use of conflict cases to reveal implicit principles that usually lie in the background of group life. Or there is Garfinkel's (1967) admonition to test one's understanding of implicit social rules by contriving to violate them and note group members' reactions. Or there is a suggestion by Nadel (1939) that one can frame specific questions to push interviewees to contradict one's growing expectations of how they see a particular issue.

There are no doubt other examples as well. The point is that we are dealing here with a key theme shared by ethnographic and hermeneutic (and perhaps all) worlds; differences, conflicts, or problems are where the information is. For ethnography, the theme is pervasive, as a theme should be, running from a general orientation to the problem of interpretation down to the level of formulating specific questions to ask. With further development, it is rich in possibilities for validation strategies at all levels of ethnographic work.

There is another contribution of a hermeneutic perspective to methodological issues in ethnography. There is abundant literature on ethnographic methods, but it is widely scattered and fragmented with no overall context serving to organize the discussions. It includes such issues as restudies; critiques of Euroamerican ethnography by "native" anthropologists; the "personal equation"; perceptions and changes in roles of fieldworkers, field assistants, and informants; the emergent nature of ethnographic models; problems in the "scientific" treatment of voluminous field note/informal interview material; informant responses to ethnographic reports; and many, many more (see Gutkind and Sankoff 1967). One of the contributions that I think a hermeneutic perspective will make to ethnography is to suggest a context that includes such issues as inter-related problems.

Related to this, another reason I find the area promising is as a solution to a problem in applied anthropology. I have worked in the area of U.S. drug research for several years. Needless to say, the standard of science that guides evaluation of research in that area is a traditional positivist one. As an ethnographer interested in having my type of research supported and attended to, I have often been frustrated by demands that ethnography be made to fit a philosophical context that in many ways contradicts its very nature.

At the same time, I found myself in the embarrassing position of having no clear statement to articulate as an alternative position. I think that hermeneutics will contribute to the development of just such a statement. For its rhetorical uses in such applied interdisciplinary settings alone I intend to study it further.

However, it is more than just a rhetorical advice. My own experience of working on this review discussion has led me to think through some ethnographic issues in new ways, and to see some issues as related that previously I had seen as distinct. The discussion also shows, I hope, that hermeneutics is also full of problems, both as a separate field and especially in its attempted connection with the study of human life. Hermeneutics is not a source of solutions, but I think it will offer a clearer sense of the problems.

REFERENCES

- AGAR, MICHAEL. 1980a. Ethnography as an Interdisciplinary Campground, *Cognitive, Social and Environmental Psychology* (John Harvey, ed.). Hillsdale, NJ: Lawrence Erlbaum.
- _____. 1980b. *The Professional Stranger*. New York: Academic Press.
- DUMONT, JEAN-PAUL. 1978. *The Headman and I*. Austin: University of Texas Press.
- GARFINKEL, HAROLD. 1967. *Studies in Ethnomethodology*. Englewood Cliffs, NJ: Prentice-Hall.
- GOFFMAN, ERVING. 1964. The Neglected Situation. *American Anthropologist* 66(3, part 2):122.
- GUTKIND, P. C. W., and G. SANKOFF. 1967. Annotated Bibliography on Anthropological Field Work Methods, *Anthropologists in the Field* (D. G. Jougmans and P. C. W. Gutkind, eds.). New York: Humanities Press.
- HILL-BURNETT, JACQUETTA. 1974. On the Analog between Culture Acquisition and Ethnographic Method. *Council on Anthropology and Education Quarterly* 5:25-29.
- NADEL, S. F. 1939. The Interview Technique in Social Anthropology, *The Study of Society* (F. C. Bartlett et al., eds.). London: Routledge and Kegan Paul.
- OPLER, MORRIS. 1959. Component, Assemblage and Theme in Cultural Integration and Differentiation. *American Anthropologist* 61:273-281.
- PALMER, RICHARD E. 1969. *Hermeneutics*. Evanston, IL: Northwestern University Press.
- RABINOW, PAUL. 1977. *Reflections on Fieldwork in Morocco*. Berkeley: University of California Press.
- RABINOW, PAUL, and WILLIAM M. SULLIVAN. 1979. *Interpretive Social Science: A Reader*. Berkeley: University of California Press.
- SETTLE, MARY LEE. 1977. *Blood Tie*. New York: Ballantine Books.

- SPRADLEY, JAMES. 1979. *The Ethnographic Interview*. New York: Holt, Rinehart & Winston.
- TURNER, VICTOR W. 1957. *Schism and Continuity in an African Society*. Manchester, England: Manchester University Press.
- TYLER, STEPHEN R. 1978. *The Said and the Unsaid: Mind, Meaning and Culture*. New York: Academic Press.