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EXPLAINING HUMAN BEHAVIOR

*Consciousness, Human Action
and Social Structure*

PAUL F. SECORD

Editor



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CHAPTER 13

Reflexivity in Field Work

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MARTHA B. KENDALL

In the social sciences subject and object belong to the same category and interact reciprocally on each other. Human beings are not only the most complex and variable of natural entities, but they have to be studied by other human beings, not by independent observers of another species.

E. H. Carr, *What is History?*

An anthropologist of all people ought to be conscious of the extent to which his intellectual life is a product of his interaction with other minds.

M. Crick, *Explorations in Language and Meaning*

Field work occupies a central position in most anthropologists' conceptions of themselves and their discipline. Despite this, there is no real consensus about the meaning or nature of field work. Fledgling anthropologists often find that their initial months in the field include periods of intense psychological discomfort if not absolute despair, and they are likely to feel great confusion about how to proceed with the business at hand.¹

Field-seasoned members of the profession are either rendered mute by questions about the nature of their field experience, or else they offer unconvincing analogies comparing field work to all manner of ordinary

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varieties of social participation. In effect, one camp argues that each person's field experiences are unique and noncomparable, and the other camp argues that all human beings are ethnographers by virtue of their common humanity.

When conceptual polarities arise in this fashion, it may be because issues have been wrongly stated and questions incorrectly asked. The uncertainty that lies at the very heart of the field work experience is not a disease, but one of the conditions of existence of anthropological investigation.

We explore in this chapter some of the practical and theoretical problems posed by field work for anthropologists, and the reasons that it holds such fascination for them. We will argue that difficulties often result from anthropologists' inability to reconcile their actual field experiences with implicitly or explicitly held assumptions about the object of their inquiry. We will argue further that the fascination field work holds arises from its profoundly ambiguous nature.

Good field work, we maintain, is concerned with meanings as well as causes. It depends crucially upon discovering the meaning of social relations, and not just those characterizing the "natives" relations with each other. It depends equally upon discovering the meanings of anthropologists' relations with the people they study, and their relationship to a community of scholars as well. It requires turning the anthropological lens back upon the self and coming to understand that one's own social realities are simply one society's construction rather than a given in nature. This reflexivity means that good field work does not just enlarge the field worker's conceptual field, but reorganizes it. It poses challenges to the field worker's most fundamental beliefs about truth and objectivity. It generates understandings and at the same time casts doubts on the validity of those understandings as it makes clear that self-awareness is a continuing process. To the extent that field work is reflexive, it is ambiguous.

To the extent that it is *not* reflexive, it is also ambiguous. Even the field worker who denies the essentially semantic nature of field work and who strives for "scientific objectivity" occasionally catches glimmers of the positivist observer's paradox: One sees other peoples' behavior and expression of intentions as being caused or determined by factors remote from their consciousness, and this implies that one's own behavior is the same. One can attempt to resolve this puzzle or one can try to ignore it, but either way, thoughts on the issue of objectivity will produce some unsettling moments.

The Practical Activity of Field Work and the Myth of Field Work Methods

Probably the most shocking realization that novice anthropologists experience in their initial field situations is that they have to learn to draw boundaries. Students are trained by having them read highly schematized accounts of other peoples' ways, but in these accounts the boundaries are already drawn and the structures laid bare. That, after all, is a major goal of ethnography. In rendering structural concepts apparent, however, ethnographic descriptions lend a concreteness to social facts that can create false expectations about what one will find in the field.

Students engage in field work, see people involved in practical everyday activities such as marketing and child-care, and ponder how they might abstract from such behavior the forms of social existence: lineages, alliances, classes, and so on. Confronted with this dilemma, and believing all the while in their own structure as fact, they often feel that their training was somehow inadequate. They fall victim to the myth of field work methods.²

They believe that there is such a thing as "good training" and that it incorporates "discovery procedures"; they convince themselves that there are operations which, when applied to a body of data, identify any systematic features characterizing that corpus and make them apparent. With time and experience, they either come to realize that the "recipes" they have for doing field work do not contribute directly to discovery, or they perpetuate the myth of field work methods by passing on improved methodologies to their own students. They turn to "field work manuals."

The success and popularity of such student manuals as Perti Pelto's *Anthropological Research* (1970) and Brim and Spain's *Research Design in Anthropology* (1974) are probably due to their implicit claims to present discovery procedures in the form of methodological rigor and research design. Pelto's suggestions for improving field work all follow from behaviorist, reductionist, and naturalist premises, namely, that the object of anthropological inquiry is the "stuff out there" and that that "stuff" has as two of its primary attributes *stability* and *observability*. His appeal is to those who believe that a finite set of determinate variables govern human behavior, and that isolating all the relevant variables will explain human action.

He argues for a "scientific" stance in anthropology rather than a humanistic one, but his view of science deserves closer attention. The following passage, with its pinings for the tests and measures available to

"friends in other sciences," reveals a curious lack of interest in whether such devices are adaptable to, or even appropriate for anthropological research aims. On the contrary, he asserts that anthropology's traditional questions should be recast in an idiom more conducive to quantification.

Much of the lore about field research that we picked up informally in our graduate student days was concerned with the gentle arts of rapport building and role-playing in field situations. We were not so much concerned, nor were our mentors, with rules of evidence, questions of representativeness, reliability, and the many other related elements of social inquiry with which our friends in other sciences seemed to be preoccupied. I can recall no discussion or even mention of the idea of "operationalizing variables" in those halcyon days [1970: xi-xii].

Pelto's conception of "science" promotes method over conceptual clarity. As a result, he blurs the important distinction between human action and human behavior. He fails to recognize that field workers do not observe subjects behaving; they *interpret human actions*. To recognize that field work consists of inferring the meanings of human activity is to acknowledge the role that the observers play in their own analyses, which is of course to deny that the behavior can be seen as *brute data* (see Bhaskar, Giddens, or Manicas, this volume).

We shall return to the question of "recipes" for field work in the next section, and argue that field methodologies are no more than devices for organizing and validating inferences. They are useful in that they help field workers frame questions and construct accounts in anthropologically acceptable forms, but they do not in themselves reveal much about social facts. Neither the great distrust of field work methods nor the extravagant claims for their efficacy are warranted. Coherent research procedures are necessary, but not sufficient preconditions for anthropological interpretations.

Field Work as Unique Experience: The Myth That No Advice is Good Advice

There are convincing reasons for arguing that every anthropologist's field experiences are unique. After all, each field worker does take a singular mixture of presuppositions, personal penchants, and past histories into the field, and these factors cannot help but color interpretations made there. They influence interpretations both because they predispose individual field workers toward particular ideological or theoretical positions, and because they figure in the manner in which field workers

present themselves to the populations they study. This in turn has its effect on how those populations interact with them. Needless to say, the quality and depth of these human interactions have everything to do with the success or failure of the field trip as a research enterprise.³

In acknowledging the individuality of each person's field experience, however, we should not be tempted to treat questions about the practical conduct of field research lightly. Some very eminent members of the profession seem to have taken the position that if we cannot tell our students what to expect in the field, we should confine our advice to them to matters of hygiene and personal comportment:

The charming and intelligent Austrian-American anthropologist Paul Radin has said that no one quite knows how one goes about fieldwork. Perhaps we should leave the question with that sort of answer. But when I was a serious young student in London I thought that I would get a few tips from experienced fieldworkers before setting out for Central America. I first sought advice from Westermarck. All I got from him was "don't converse with an informant for more than 20 minutes because if you aren't bored by that time he will be." Very good advice even if somewhat inadequate. I sought instruction from Haddon, a man foremost in field research. He told me that it was really all quite simple; one should always behave as a gentleman. Also very good advice. My teacher, Seligman, told me to take ten grains of quinine every night and to keep off women. The famous egyptologist, Sir Flinders Petri, just told me not to bother about drinking dirty water as one soon became immune to it. Finally I asked Malinowski and was told not to be a bloody fool. So there is no clear answer, much will depend on the man, on the society he is studying, and the conditions in which he is to make it [Evans-Pritchard, 1976: 240].

Evans-Pritchard's conclusion that there is "no clear answer" may be cold comfort to novice anthropologists on the eve of their maiden voyages to the field, but there is no reason why this should be the only advice they get. There are established practices for gathering anthropologically relevant information in the field, and particular ways to address particular issues. A significant justification for having students write research proposals before they leave for their communities is to get them to think coherently about research questions and the strategies for answering them. The requirement that evidence in field reports be systematically presented is designed to guarantee that each person's field experience does not remain unique but is translated into an idiom that others can appreciate. Since the discipline requires certain standards of planning and forethought in field work and canons of evidence and intelligibility in

field reports, it follows that there is a level on which anthropologists take the operations of data gathering very seriously.

What we often fail to make clear to our students and ourselves is the relationship between the operations they perform in the field and the accounts they subsequently construct about their field work. As we maintained earlier, *field techniques are not discovery procedures*. They do not contribute directly to analyses, although they aid field workers in organizing and rationalizing their experiences. They do not reveal "social facts"; they simply allow the researcher to proceed systematically toward inferring what those facts could be. This very important point raises the issue of the effect of disciplinary requirements on the actual conduct of field work. Whatever ethnographers may find in the field, they are obligated to frame their reports of it in certain ways and not others; that is, if they wish other anthropologists to view them as adherents of the discipline. What they do find in the field, of course, is already influenced to some extent by the values they carry as part of their anthropological training, including values attached to conduct, their own and others.⁴ Field work is more than a simple two-way relationship between natives and ethnographers. The whole anthropological community has a stake in how these kinds of relationships work themselves out, if for no other reason than to guard its claim to being a serious intellectual discipline. If the importance of methodology is a recurrent but unresolved issue in discussions of field work, it is probably because anthropologists have failed to reflect upon the requirements for "seriousness" that they impose on themselves.

If we take the influence of the profession as a whole as having a bearing on the conduct of research, the methodology issue becomes clearer. Research procedures can be viewed as framing devices for translating one set of experiences into another—i.e., they allow the individual to converse with the discipline at large, which therefore makes them valuable in both the conduct and the presentation of research. By the same token, if professional values are attached so closely to research procedures, there cannot be a rigid separation between "objectivity" and "belief." This means that methodologies are not valuable because they are "objective" and "scientific," but rather that they are valuable because they allow us to communicate with each other. The scholarly community is the third party in field work. The danger for field workers is that they will confuse the requirements that field workers' accounts be systematic and consistent with the invitation to treat as privileged the influence of the scholarly community on the account itself; that is, as not subject to the same standards of critical evaluation. Anthropological research is as value-impregnated as any other form of human conduct.

Theoretical Perspectives on Field Work: The Myth of Social Homologies

Anthropologists are fond of presenting themselves to others by making analogies with taking different roles, not only in their accounts of themselves to the people they study, but in accounts of anthropology written for students and colleagues as well. The literature on field work is replete with descriptions in which field workers are likened to a spectrum of social types: children undergoing socialization, second-language learners, quasi-kinsmen, natural historians, strangers, helpful and concerned friends, and so forth.

These analogies try to grapple with the issue of how ethnographers discover the meaning of an alien people's action by situating the process of discovery in the capacities of all social beings. To the extent that they stress our common humanity and demonstrate that even Western modes of thought are based in these capacities, such analogies are interesting; but they may be misleading if they neglect the differences between the social situation of the anthropologist and the social types or roles with which ethnographers are compared.

Childhood socialization is a popular and frequently invoked comparison. The argument is made that anthropologists learn about social relationships in a given society in the same way that children growing up there do. Of course, they admit that they do not know what that process is, but they regard this as irrelevant: Anthropologists presumably already have whatever skills children use to acquire meanings, because anthropologists were once children themselves.

As Burnett (1976) shows, the analogy between childhood socialization and ethnographic method yields only partial insights. Both anthropologists and children undergoing socialization must learn sets of appropriateness conditions for acting. That is, they must learn how to relate rules to contexts, the degrees to which rules for behavior are negotiable, and they must learn about the limiting conditions of negotiation: power, self-interest (long-range versus short-range), and structure. But anthropologists, in common with other adults, are *not* like children, because they already possess established routines for rendering the world intelligible. They do not have to "learn how to learn" (deutero-learning, in Bateson's [1970] terminology); they have to learn content, substance, the "rule of conduct."

To see the faults in the childhood socialization analogy, it is only necessary to recall that while the final product of ethnographic endeavor should be logical, coherent, organized and clear, native cognition does

not exist in this fashion. Hence, the comparison between ethnographer and child cannot make reference to the end result of the learning process, but only to the assumed antecedent state of the learner (naive about particulars versus informed about them), or to the unknown but assumed process of discovery. This is hardly enlightening. To compare one unknown process to another gets us nowhere. That anthropologists require their claims to knowledge to be explicit and testable suggests that there are significant differences between anthropological forms of consciousness and other, mundane forms.

In any case, social scientists have come increasingly to realize that socialization is a lifelong process and does not have any end point. The parallel may be drawn, then, between anthropological field work and adult socialization, as has sometimes been done. There are difficulties here as well.⁵ Adult socialization is essentially segmental and discontinuous. Preparation for role-taking occurs both before and simultaneously with the assumption of social roles. Socialization for one role may be isolated from learning another role. Furthermore, any given adult's repertoire of roles is neither comprehensive nor does it conform to the requirements of an anthropological account.

A major difference between a field worker's experience of the social order and the native adult's is that the field worker continually refers back to his or her communities of origin, both scientific and natural. In this way the field worker is often compared with the role of the stranger or friend. The stranger analogy is interesting because of the qualities that the stranger role exhibits and what the stranger experiences. Strangers, according to Simmel (1950), combine the paradoxical qualities of nearness and farness. They are spatially near and culturally far. They exist betwixt and between, neither in one society nor the other. This has led Schutz (1964) to suggest that from the subjective perspective, the problem of the stranger lies in his inability to assume the "natural stance," to take intersubjectivity as a given in his social situation. Thus, the parallel between the stranger and the field worker rests on two qualities that they have in common. On the one hand, the stranger cannot be treated as either an insider or an outsider, but paradoxically is both; and on the other hand, the stranger is placed in the situation of having to discover what the natives take for granted. This double analogy seizes on the problem of the stranger in terms of the field worker's interaction with the subjects of his or her research and with the requirement that the field worker turn the implicit and practical knowledge of the socially committed actor into a discursive form.⁶

Fortes (1975) concurs, arguing that strangers, from the point of view of the indigenous peoples, are potentially dangerous individuals. Because they are unknown, they may bring either great misfortune or great opportunity; but in either case, it is the wise and prudent person who treats a stranger hospitably and with marked deference. In kinship-based societies, sojourners who settle in for extended periods may be reclassified as kinsmen or quasi-kinsmen, a symbolic acknowledgment of their being known, and therefore safe.

Fortes's analogy has the merit of pointing to the dynamic element in the field work experience. Field work does not entail the static assumption of a role or a single-stranded relationship, any more than does any other type of social activity. The field work experience is developmental and changing.

There are difficulties with the stranger analogy, however. Field work does not take place in a power vacuum, and the field worker is often placed in the position of being a representative of a colonial regime or a metropolitan culture (Maquet, 1964; Asad, 1973). These are part of the conditions of existence of the field worker, and they pose important ethical and analytical problems that the stranger analogy neglects.

One difference between the stranger and the field worker arises out of the different interest situation of the two. While the stranger may not be knowledgeable about or accepted in the social context in which he acts, he frequently maintains the social interaction out of necessity. Thus, the natives' conditions of existence affect him as much as them. Field workers, on the other hand, are not subject to the same constraints, and this must inevitably affect the quality of their experience. Rosemary Firth (1972) recounts a nightmare she had shortly before leaving the field. She dreamt that she really was a Malay woman, "squatting in front of a smokey fire." The participation that the field worker gives is neither as committed nor as constrained as the native's.⁷

In this respect, the field worker is less the native acting in the context of everyday life and more the performer in a ritual or the participant in a game. The same analogy is drawn by Firth in her essay and is apt in a number of ways. First, the game is "only a game" and can be terminated at any time the player chooses. Second, the goal of a game is not necessarily to win. Only some games are specifically competitive. The game may contain its own end rather than lead to an end, rather like Simmel's account of sociability (1950). In this case, the end may be the display of skill that is contained in many games. Games, in any case, have an amoral quality about them that catches an aspect of the field work experi-

ence. Both field worker and native recognize that the field worker's manipulation of the forms of native social life have a "not-for-real" quality about them that allows the participants in the field work "games" to focus on the quality of the doing rather than the consequence. Field workers often remark that their display of knowledge has frequently led to a sort of "How do you know that?" response that presaged a deeper penetration into the forms of native life.

This attitude is aesthetic. Skill becomes an end in itself and leads to further elaboration of form. In a sense, both the native and the field worker know that it does not count in the same way as life-in-society.

The various analogies that have been drawn between field work and different social roles are enlightening. Taken as a whole, however, they show a general failure to stress an element of the field work experience that differentiates it from the repertoire of roles that an adult may take on as part of his or her life-in-society. Ethnographic analysis requires the field worker to penetrate beneath the surface forms of social life in order to produce a form of knowledge that is not found in association with ordinary social roles. In the case of friendship, for example, ethnographers may indeed become friends with the people whose cultures they investigate, and they may indeed perform the ritual or nonritual activities of the kinsman. Yet an ethnographer's relationship to these people is not primarily one of friendship or kinship. As Richardson (1975, p. 521) observes: "The ethnographer must ask probing questions; he cannot, as one does with friends, accept the informant as the person he is, but the ethnographer must find out, he has to find out, why the informant believes what he does." Ethnographers cannot always act as friends, nor as kinsmen either, since the goals of ethnographic description require them to take an analytic stance with respect to the actions kinsmen and friends perform in that social group. Ethnographers who do not maintain an analytic disaffection from social roles—those who "go native" or who take everything at face value—relinquish in the eyes of the profession their claim to be involved in serious anthropological research.

This cannot be overemphasized. Anthropological assertions must be couched in rhetorical form, i.e., they must be able to be tested and found wanting by the audience to which they are addressed—the anthropological community. If anthropological arguments make reference to the meanings native actors assign to their own action, they may also make reference to factors beyond native comprehension. This is so because the anthropological community itself requires ethnographers to deal with the problem of native false consciousness. Responsibility to the profession,

i.e., to a community of scholars outside the community studied, affects the kinds of explanatory accounts ethnographers can offer, and this in turn affects the kinds of social interactions ethnographers in the field can have. Adult socialization within a community does not require the extreme analytic and skeptical stance required of ethnographers, and thus is in many ways an inappropriate analogy. Ethnographers are first socialized in their own community, and then again in their scientific community, and the latter in a way that places severe constraints on socialization in a native community.

There is a final analogy for ethnographic field work we wish to consider here, and that is the one likening it to second language acquisition. This comparison is an attempt to account for the presumed fact that a native actor's knowledge of his or her culture is implicit and unarticulated, while the ethnographer's formulation of this knowledge is explicit and coherent. Anthropologists learning about social relations in alien societies are said to be like adults learning alien languages, in that both groups consciously try to learn what natives assimilate unconsciously.

People pursuing this linguistic analogy generally make a distinction between native language learning and second language acquisition. For example, in first language learning, says the Soviet semiotician Yuri Lotman (cited in Eco, 1976, p. 138), a native speaker is "trained through exposure to a continuous textual performance of pre-fabricated strings of . . . language, and he is expected to absorb his competence even though not completely conscious of the underlying rules." Adults, on the other hand, "are introduced to an unknown language by means of rules; they receive a set of units along with combinatorial laws and they learn to combine these units in order to speak."

Lotman's characterization applies only to adults learning second languages in relatively formal classroom circumstances however. In informal language learning situations (which is where most second language acquisition takes place), the process is far more complex, interesting, and creative than Lotman seems to have realized. Adults picking up new languages "in the streets" try to abstract, infer, or construct rules characterizing the alien tongues at points where its grammatical processes differ from their own language. They almost never think consciously about grammar when their native idiom is structurally similar to the language they are trying to learn. For example, if a person's native language puts subjects before verbs and verbs before objects, and the second language does the same, the adult language learner will get a "free ride" on the implicit grammatical rules he or she already knows. In other words, the

learner will not have to learn any new rules, consciously or unconsciously. If a person's first language puts verbs before objects and the language to be acquired puts verbs after objects, the learner may have to formulate a new rule, but he could also continue to follow the word order of the native languages and do things "wrong" from the point of view of speakers of the other language. If there is no impediment to communication, this is a fairly practical strategy (Kendall, in press; Schumann, 1974, 1976). While adult language learners typically do look for organizing principles for acquiring new tongues, they also involve themselves in much creative trial and blunder. That is, they use a blend of unconscious analogy, conscious analogy, conscious reformulation of analogy, and imaginative projection to construct an abstract system (i.e., a grammar) out of the unbounded evidence of speech forms.⁸

The real senses in which a field worker is like a second language learner are these: (1) both try to infer and organize kinds of social knowledge that may not be part of the conscious apparatus of the people from whom they are trying to learn, and both have to use their wit and imagination to accomplish this; (2) both measure their success in terms of public acceptance of the products of these cognitive exercises.

It is important to note here that the abstractions a language learner or ethnographer formulates are extremely difficult, if not impossible, to confirm in certain circumstances, e.g., the non-native's "rules" may correctly project the behavior which natives manifest, without being the same "rules" as those that the natives are following (indeed, they almost certainly cannot be the same rules where the native's knowledge is implicit and the non-native's explicit), or the non-native's rules may project incorrect behaviors which the natives accept out of politeness or respect for the non-native. As long as the abstractions or rules generate expected or acceptable behaviors, then they may be said to be in some sense "correct," following a proof-of-the-pudding logic, whether or not they are the "true" rules. On the other hand, when the non-native's abstracted principles generate behaviors evoking puzzlement, consternation, censure, reproof, and the like from the native, then these formulations can be said more clearly to be incorrect. The irony of this situation has not escaped the attention of either linguists or ethnographers: We are certain of our formulations only when they meet disconfirmation—only when the people violate our expectations. When our rules are predictive, we can never be sure they are the same as the natives' (Collett, 1978), or even whether they really project "correct" behaviors from a native point of view.

The stranger, the game, the second language learner, and the quasi-kinsman all confront the field worker in terms of a series of analogies that strive to capture a feature of the field work experience that is essential to an understanding of the process of interpretation that leads to discovery. All of these roles or situations attempt to present the paradox of field work, that it exhibits a peculiar combination of engrossment and distance. As an activity, field work often succeeds best where it is least aware of itself; that is, where the engrossment of the field worker is such that he or she brackets the difference between field work and life-in-society. This is a necessary step in the interpretation process that is so essential to field work as a methodology. In this way, interpretation and participation come together as a means of discovery. But discovery does not proceed solely through immersion in a form of life. The discovery of crucial facets of a form of life occurs only when the immersion of engrossment is broken by the experience of the unexpected and the unanticipated. This experience has been glossed by Zaner as "shock." Shock breaks the "natural attitude" and forces the actors to consider that there are possibilities other than the ones they have taken for granted or as given. "*Our attention shifts from that of engagement in to that of focal concern for the sense and strata of the very engagement itself*" (1970, p. 51, emphasis in the original).

It may help to explicate this experience in the context of field work through a description of an incident from Karp's (1978, 1980) field work journal. Karp customarily spent the late afternoon at a compound of a prominent elder who was both a friend and an informant. In the early evening, the people who had congregated around the elder's house would invariably be invited to share the evening meal. The food would be served by the elder's wife to the assembled men in front of the door of the elder's sleeping hut. On one such occasion, Karp brought an anthropologist friend who was visiting him. The friend was engaged in field work among an ethnic group some 50 miles away from Karp's field site. The guest was taken aback by the experience of eating outdoors. "The Tuken," he said, "regard eating outside as a disgusting act." Suddenly, what had seemed an unremarkable feature of Iteso social life became problematic. The anthropologists and the natives discussed the puzzle of these cultural differences. The natives saw nothing unusual about their culinary habits, but they did point out that one crucial way in which the person is evaluated was with whom and where he ate. A man who consistently eats inside his house without visibly demonstrating the evidence that he is willing to share his food with his neighbors is termed *epog*, a term the

Iteso translate as "selfish," and a considerable insult. In the context of Karp's investigation of neighborhood organization, the major focus of his research, this event led him to associate the display of sociability with the custom of eating outdoors and the sharing of food and drink with ideals of neighborliness and the terms by which persons are evaluated.

The experience of "shock" is one facet of the situation and experience of field work to which the analogies draw attention. Anthropologists have considered this form of *learning by contrast* in their discussions of field work. A. J. Köbben (1967), for example, argues that subjective experience is objectified through errors made by ethnographers and corrected by natives. From this viewpoint, the idiot might provide a better analogy than the stranger. The ethnographer's interaction with the natives provides a confrontation in which metacommunicative rules and devices are made manifest and explicit. The anthropological presence, as well, may produce questions about fundamental categories. In the same volume, de Josselin de Jong (1967) argues that "participants' views are made clearer by contrast and . . . ideals are sharply defined by conflicts." Thus these two essays draw a parallel between the experience of shock in association with contradiction and conflict in society, and the particular form it takes in the field work experience. Once again we see support for the position that field work as a research activity is based in fundamental and universal human capacities, and that while the organization of the experience may differ from context to context, the mechanism of learning by contrast is a basic feature of the process of discovery in field work.

Field workers experience "shock" through the medium of the social relationships they establish with the objects of their investigation, the natives. The learning process often works both ways, and the field worker uncovers that which is usually implicit or hidden through a process of mutual guesswork that is the "work" of the social relationship established between field worker and native. The recourse to analogy between field work and different social roles may be part of attempts by both the field worker and the natives to make sense out of the unusual situation of the field worker. In this sense, field work institutionalizes "shock." The field work relationship differs from other social relationships available to the actors in a society but also shares features of those relationships. Making sense out of social relationships, including field work relationships, entails semantic activity. In their relationships with the subjects of their investigation and with the scientific community of which they are a part, field workers use their semantic capacity in semantic activity to produce social relationships and as a research tool.

Field Work as Semantic Activity: The Search for Meanings and Causes

Social relationships arise out of actors' definitions of situations and their interpretations of circumstances. Actors communicate their interpretations to others by means of shared symbol systems, and together the parties to an interaction negotiate a common definition of their circumstance. In this view of the nature of social relations, acts of interpretation are central. They allow actors to impute intentionality to other actors, and at the same time assume that like imputations will be made in turn.

In any social relationship, including the anthropologist/informant one, actors constantly test the accuracy of their inferences and imputations, and the process by which they do this is extraordinarily complex. They read other people's behavior as meaningful activity, picking and choosing among the different possible interpretations of it available to them. If the parties to the interaction create widely divergent interpretations because they start from different assumptions about the nature of things, they can nevertheless continue to think their definitions are shared until evidence to the contrary no longer fits a hypothesis of consensus. At such points, actors can either renegotiate a consensual definition (oh, you meant *X*; I thought you meant *X'*. Now I get it), or they can terminate the relationship, charging the other parties with noncooperation, malfeasance, or inscrutability. Puzzlements or vague feelings of social discomfort are often crucial evidence of nonconsensus, but they may not be recognized as such for extraordinarily long periods of time.

Anthropological field workers try to reduce the inherent indeterminacy of research relationships in ways that are beneficial to them and designed to maintain these relationships. They often present themselves to the people they work with in terms that they think the people will understand and accept, but they also take covert positions—not necessarily out of a desire to dissemble—which their informants know nothing about. At the same time, the identical process is characteristic of their informants' relationships with them and each other (Berreman, 1962). This often leads to nonconsensual interpretations of interaction. Informants' expectations about how certain categories of people behave in their own society will be transferred to the field workers, and if the field workers are not careful, they will do the same.

At the level of nonverbal presentations of self, these attributions and the inferences derived from them can lead to all manner of confusions, which may be difficult to correct. The discovery of mutual nonconsensus

is rendered difficult because social actors assume that their definitions are shared and ignore evidence that runs counter to their assumptions. The failure of field workers to monitor the social aspects of their interactions can have serious effects on the integrity of research reports issuing from their interactions, particularly when the parties involved construct tacit and nonparallel hypotheses about the distribution of power and interest in the interaction.

Power and interests are dimensions of all social relationships, including field work relationships. In order to produce their research relationship, anthropologists acquire a practical knowledge of the local organization of power and interests. They are also interested, however, in producing accounts that analyze the role of power and interests in the production of social relationships. This requires them to examine the patterning or distribution of power, in both meaningful and causal terms. For native actors, the distinction between cause and reason, for example, is not necessarily relevant to their conduct. This is because they must incorporate causes into the reasons for their action, even if this is done in a manner which mystifies the cause (see Bhaskar, 1980). Clifford Geertz (1973) tells us that "cultural actions, the construction, apprehension and utilization of symbolic forms are social events like any other; they are as public as marriage and as observable as agriculture." We take this to mean that the performance of cultural acts serves to define situations in terms that are drawn from the repertoire of actors, and to make one or another aspect of the actor's identity relevant or irrelevant. Thus, the timing and selection of the cultural act performed is not necessarily the result of a desire to communicate, but may be a byproduct of the pursuit of interests. By extension, some actors will be constrained to act in a culturally prescribed manner not so much because of their own self-interest, but because they are unable to impose their own definition on a situation they do not control.

We do not mean to imply either that actors are necessarily aware of the degree to which they acquiesce in granting others the power to define their social world, or that definitions of situations are simply outcomes of successful strategies in ideological power games.

Weber (1958) argues that beliefs and values as components of social action act as no more than switching devices, directing action into one or another path in a course to a preexisting goal. It is possible to make this argument even stronger. The organization of interests in a given society, the distribution of power, and the constraints exercised as consequences of prior actions affect the pattern of action and the pursuit of goals in particular times and particular societies. Hence, a given action may be

affected by factors that are either external to the consciousness of actors or only imperfectly understood by them.

Many social and behavioral science explanations of the causes of action depend upon the assumption that actors are unaware and cannot be as aware of the factors that produce their action, or, alternatively, that such accounts assume rather than try to discover the interior logic of native actors. In some versions, actors are presumed to think in terms of an economic calculus; in others, they are the unthinking products of the conditions of their existence.

Because patterns of action can be shown to have consequences external to the consciousness of actors, or to be the product of calculations of cost and benefit, or to be the result of the imposition of some person's will on another, some anthropologists have found it easier to ignore natives' expressions of thoughts and intentions altogether and to substitute accounts derived from a general theory of human behavior instead. There is nothing particularly wrong with causal explanations that describe behavior as the result of unapprehended factors, as long as a peculiar form of explanatory short-circuiting does not occur. Asserting or demonstrating that human actions may be *caused* does not absolve anthropologists from describing how actors account for their own behaviors, and how these accounts figure in the production of subsequent actions.⁹ We need to know what actors intend by what they do and how they go about realizing their intentions if we are to have an adequate explanation of action. Actors' constructions of their own behaviors are not irrelevant data, no matter how curious or beside the point they may seem. Even if they are not true reflections of interior states, or full explanations of action, they have to be examined critically. This is so because there is always the chance that other actors will take such reports at face value and act upon them. "Untrue" interpretations may become true by virtue of their consequences. Explanations presented primarily in causal terms represent attempts to avoid confronting the problem of the observer's consciousness and its relationship to the explanations.

If anthropologists or other social scientists finally begin to make sense out of their research objects' behavior, they are then faced with problems of proving that this inspired guesswork, these interpretations of behavior, actually conform to some underlying reality. In this sense, they face a problem similar to the one members of the society being studied face, i.e., how to reconcile one's own interpretation of other people's behavior with other people's accounts. It is not that the others' behavior does not make sense; it is that the explanations they give of their behavior are sometimes irreconcilable with one's own. In many circumstances it is

difficult to establish whether other actors' self-reports are valid representations of their thoughts and intentions, instances of false consciousness, attempts to dissemble, or some combination of these.

What then justifies the ethnographer's attempts to account for native systems of knowledge if they cannot reproduce what goes on inside the native's head? The justification seems to be that there are aspects of behavior which are both internal and external to actors. We may thank Durkheim for pointing out long ago that there must be aspects of the ideas that people share that are publicly accessible and publicly validated; what he called "collective representations" are interpreted and validated in public circumstances. The knowledge of "other minds" on which these interpretations are based is always provisional and ambiguous. The ambiguities are not only the necessary conditions in terms of which social adjustments can be made, they are the necessary conditions of social life; for if they provide the space out of which social order can emerge, they also provide the material for innovation and change. If order is not completely determined, then it becomes possible to institute changes in the very process of recreating that order (Moore, 1976).

The ambiguous nature of the symbols used in communication—their relationship to both order and change—leads to a significant methodological point, namely, that the indeterminacy and uncertainties ethnographers perceive as features of the social relations to which they relate their analyses are, at the same time, features of the social world to which actors must orient themselves. The interpretive procedures through which natives render their experiences intelligible are just that, interpretive procedures. They no more provide actors with true statements about the internal states of others than they provide anthropologists with true pictures of "what the natives really think." The shared idiom of social interaction consists of symbols, or collective representations, whose function is to mediate between private experience and its public expression. Since this is true, anthropologists seem entirely justified in pursuing their attempts to describe native systems of thought and action in terms of the meanings with which actors construct cultural worlds, even if they (the anthropologists) must realize the contingent nature of the knowledge that both they and the natives produce (see Geertz, 1973, 1976).

Social relationships or systems of social relationships have a dual quality. Actors negotiate them on the basis of their inferences about other actors' intentions, and, simultaneously, they are the products of factors that are more or less remote from the consciousness of the actors. Giddens (1976) describes *structure* as having a dual quality in this same

manner. Structure, he tells us, is both the medium in terms of which action takes place and the unintended consequence of action.

Conclusions

In his account of the religion of the Nuer, Evans-Pritchard raises a fundamental paradox of anthropological field workers: that their activities are situated in a social field that encompasses radically opposed requirements in terms of which they formulate accounts.

Nuer are not confused, because the difficulties which perplex us do not arise on the level of experience but only when an attempt is made to analyze and systematize Nuer religious thought. Nuer themselves do not feel the need to do this. Indeed, I myself never experienced when living with the Nuer and thinking in their words and categories any difficulty commensurate with that which confronts me now when I have to translate and interpret them. I suppose I moved from representation to representation, and backwards and forwards between general and particular, much as Nuer do and without feeling that there was any lack of coordination in my thoughts or that any special effort to understand was required. It is when one tries to relate Nuer religious conceptions to one another by abstract analysis that the difficulties arise [1956, p. 106].

The very idea of the "participant observer" incorporates in itself a number of paradoxes. First, there is the distinction that must be drawn between an observer's orientation to the social world which, as Bourdieu (1977) rightly concludes, leads the anthropologist to treat social life as a drama or spectacle in which the actors play already written parts. On the other hand, the actors themselves are not concerned with playing roles. Their activities are practical and purposive. For them, society is often a means to an end, and not the goal of their action.¹⁰

This paradox shades into a second. A key assumption of our study is that society is the product of knowledgeable actors (Giddens, 1979). Yet we have also argued that field workers often articulate their accounts in terms that are not recognized in the same way or the same form as the accounts formulated by the native actors themselves. This paradox is resolved through the distinction Giddens draws between practical and discursive consciousness. Unlike Giddens, we do not regard practical consciousness as fully reflexive. Practical knowledge must certainly lead to reflection, but a genuinely reflexive orientation to the social world is, in our opinion, destructive rather than productive of social order. Reflexiv-

ity incorporates both an understanding of how to produce order and of the very conditions of the existence of order itself. In an ideal, typical sense, the discursive consciousness of the field worker recognizes not only those factors that are discursively unavailable to the members of a society, but also that the knowledge produced by the field worker is subject to similar determinations as well. Often, field workers are able to confront the social determination of their own knowledge through the "shock" of accounting for the consciousness of the objects of their research. The natural stance which is so destructive of reflexivity is more difficult to sustain under conditions of conflict and contradiction.

The first datum of the field anthropologist is the expressed consciousness of the members of a society. Only by understanding the accounts people give of themselves can an anthropologist follow through and show them to be "false," "inadequate," or "incomplete" in some way. The notion of false consciousness, which we think stands at the very center of anthropological analyses of behavior, depends upon prior discovery of forms of consciousness that exist in a given social formation.¹¹ The lessons that should be drawn from this are obvious. First, if social analysts take seriously native accounts and try to reconcile them with higher-order explanations, they must also surely have to come to grips with the shifting sand beneath their own feet—i.e., they have to face the very real possibility that the meanings they assign to others' behaviors are influenced by factors outside of their *own* consciousness. They must, in other words, develop a reflexive stance insofar as it is possible, according to which they may examine critically the analytic procedures through which they constructed their explanations.

If the primary datum of field anthropology is the native's consciousness, and the second consists of the external factors that affect this native consciousness, then surely the tertiary data is the analyst's own consciousness and his or her reflections on it. This means that the anthropological image of a society should be characterized by an almost exquisite degree of self-consciousness when compared to that of the members of a society. The anthropologist's competence, or knowledge of organizing principles, is at once more comprehensive and less detailed than the native's. It must necessarily be so, because it is acquired in a very different fashion and is put to very different uses.

The dialectical relationship between discovery and validation may help to throw some light on the nature of anthropological accounts.¹² The repertoire of data elicitation devices, the systematic presentation of evidence, and the logic of hypothesis confirmation are not characteristic of

what Georg Simmel might call the "practical reality" of everyday life. Everyday life is organized in terms of what phenomenologically oriented social scientists have referred to as the "natural stance," and by "natural" we take them to mean unreflective as well. The culture of anthropologists and the interests they have in surviving and succeeding in that culture require that the natural stance of native worlds be subjected to critical evaluation. They require as well that both the inferences of meaning and the description of the causal factors that produce behavior be confirmed in terms that are not drawn from the cultural world under investigation. In this sense, anthropologists' accounts face in two directions. On the one hand they must be faithful to the members of the society that is being studied, and on the other hand they must conform to the criteria of the scientific community of which the anthropologist is a member.

This is not an easy task. All too often the delicate balance is lost. That the scale tips in favor of the scientific community in most cases and not in favor of the natives may tell us something about the relative balance of power in the world. That anthropologists justify the obliteration of meaning from their accounts on the grounds of the logic of validation may tell us something about the triumph of an ideology of technical rationality. Finally, that anthropologists seem all too unaware that their accounts are as grounded in their own social situation as those of the natives is an indication of their failure to develop a reflexive anthropology. We do not mean to suggest that anthropology is unreflective. The attention to detail of good ethnography is a genuine indication of reflectiveness, but this is not the same thing as claims to knowledge which turn back upon themselves and recognize the contingent nature of the experience on which they are based.

Field work is that experience. It is like other social experiences, and yet it is unique. Field workers are persons who both affirm and deny the validity of native conceptions of existence. They know less than natives, but claim to know more. Yet their accounts must conform to community norms as much as those of the natives. Field workers present themselves to the objects of their research in one set of terms and to the scientific community in others. They must of necessity experience their mode of existence as profoundly alienated. To the degree that they deny the alienated nature of their existence, they will produce knowledge that is alienated in another sense; it is loosed from any sense of the very conditions of its existence. It is in this sense that T. O. Beidelman (1970, pp. 527-528) refers to social anthropologists as "a fascinating cultural puzzle: that of men standing within and without the objects they must under-

stand. In this respect, it may not be unduly dramatic to suggest that the greatest of social anthropologists, those from whom we learn most, appear as the most alienated and therefore perhaps the freest but most troubled of the social scientists."

Notes

1. These reactions could be dismissed as manifestations of what is commonly called "culture shock," although doing so fails to reveal the source of the disequilibrium. *Culture shock* describes emotional discomfort in the face of alien or unfamiliar behaviors and beliefs. The initial experience of the novice field worker is actually more akin to "stage fright," i.e., the sensation of helplessness or anxiety brought on by having to perform a role in front of an audience whose willingness or ability to appreciate that role is unknown. It is the experience of not knowing what to expect, except in the most general ways (see Lyman & Scott, 1970).

The people among whom the anthropologist settles may have similar difficulties fitting him or her into their frame of reference, which leads to puzzlements, and to questions about the differences between the anthropologist and themselves. In formulating answers to such queries, anthropologists construct presentations of self that they think their hosts will comprehend, i.e., they try to translate the aims of anthropological research and the role of the field worker into categories intelligible to the indigenous peoples. These "accounts to the native" are constructed both for the anthropologist's benefit and for their non-anthropological audiences.

2. We do not deny that social life is structured. Problems arise from the tendency of social scientists to attribute an *actual* as opposed to a *virtual* existence to structure. In our point of view, social structure can be examined as a process of *structuration*; it is continually reproduced in action (see Giddens 1976, 1979). In social anthropology, this perspective has been consistently championed by Meyer Fortes, as when he describes the lineage system of the Tallensi as "a configuration of processes in time and space" (see especially the essays in his *Time and Social Structure and Other Essays*, 1970).

3. In one of the most insightful accounts of field work, Gerald Berreman (1962) draws attention to the parallels between field work and Erving Goffman's concept of "impression management" (1959). Berreman's point is that impression management, the presentation of self to achieve desired ends from significant others, is a two-way affair. Both the subjects of anthropological inquiry and anthropologists themselves play a game of mutual impression management.

In the heterogeneous community that he studied, Berreman was able to take advantage of the different ends desired by different segments of the community to acquire information about "backstage areas" that some groups would have preferred to keep hidden. Berreman's account is valuable both for its honesty and because he exposes the essentially political nature of anthropological field work. At the same time, it is also somewhat limiting. The presentations of self produced by actors in the field work drama are accounts, and as such they both affect and are affected by different fields in which the actors participate. The strategies utilized by the anthropologists and their informants are related to social contexts outside the immediate research situation. For the anthropologist, this includes the scholarly community of which he is a part. For the novice anthropologist, this scholarly community manifests itself in the form of a dissertation committee. Because of the radical separation between the social fields in terms of which anthropological presentations of self are made, anthropologists may experience severe difficulties in reconciling the space between the self as presented and the self as experienced. Many of the analogies between the field worker and other social roles presented below are attempts to present this radical disjunction.

Another difficulty that arises out of the radical separation of the domains of scholarship and research can have significant consequences for analysis. The indexical quality of expressions allows indigenous concepts to be glossed under a variety of translation labels. If the term is used differently by a number of groups, the interaction between anthropologist and informant may result in the reinforcement of error rather than its elimination. Michael Herzfeld's examination of the concept *prika* in two Greek villages shows that the meaning of "dowry" which anthropologists attributed to the term tended to conform to its usage in the national legal code, and that the indigenous meaning attributed to the term was exactly the opposite of its legal definition. Furthermore, because the villagers had to accommodate themselves to the legal system, the context of questions and answers that Herzfeld initially used reinforced the mistaken impression that he brought to the field situation (in press).

The problem is, of course, one of translation, of "presenting the coherence of the thought of one people in the thought and language of another," as the anthropologist Godfrey Lienhardt describes it (1956). See the essays in B. Wilson, Ed., *Rationality* (1970) for a discussion of these issues. M. Crick's *Explorations in Language and Meaning* (1976) discusses the semantic nature of anthropological inquiry.

4. This returns us again to the problem of drawing boundaries in field work, but from a different perspective. In this instance, the difficulty emanates from the scholarly community of which the field worker is a member, and not from the field situation. It becomes a matter of deciding whether to "stick to one's last" or not. See Gluckman (1964) for a discussion of the issues with respect to social anthropology.

5. Burnett (1976) examines the literature on socialization and field work in considerable detail.

6. See the discussions of "implicit meaning" and "practical knowledge" in Douglas (1975) and Giddens (1979). Nash (1963) takes a psychological tack in his essay and concludes that field work is a stressful activity better handled by certain dispositions.

7. Anthropologists often obscure the relative freedom from constraint they experience in field work and romanticize the participation. F.H. Cushing is an early 20th-century anthropologist widely believed to have "gone native" and died as a full member of Pueblo Indian Society. Actually, he was removed from his field situation by his employers, the Smithsonian Institution, and died on an archaeological dig (Grunewald, 1972).

8. Alan Hoben (n.d.), in an insightful article on field work, describes ethnographic discovery as based on a "trial and blunder" method.

9. The radical separation between a reason for an action and the cause of an action that we made earlier in this section requires modification at this point. Because actors and their actions intervene between cause and effect, causes become incorporated, often unknowingly, into reasons. We find the distinction between cause and reason a useful analytical tool, not an observable feature of the social world (see Bhaskar, 1980, for a discussion of the relationship between cause and reason).

10. Anthony Giddens (1976) terms this participation of the social scientist in two life-worlds a "double hermeneutic," but if the social scientist is genuinely reflexive about his practice, then the hermeneutic may be *triple*, not double!

11. The concept of false consciousness is tricky, because it is subject to so many interpretations. Obviously it is tied to the critique of ideology, and there are notorious difficulties in the definition of the concept of ideology. We do not mean to suggest by our use of the notion of false consciousness that there is a fixed "true consciousness," in terms of which other forms of consciousness may be rendered false. In his comparison of true and false forms of consciousness, Bernstein states the issues nicely:

The concept of ideology of "false consciousness" is reciprocally related to the concept of a non-ideological understanding or a "true consciousness." *I do not think that there is any fixed criteria by which we can, once and for all, distinguish "false consciousness" from "true consciousness."* The achievement of "true consciousness" is a regulative ideal of the critique of ideology, and the relation between "false

consciousness" and "true consciousness" is asymmetrical. This does not mean that we must remain intellectually agnostic, that we are never in a position to evaluate and judge the ways in which ideology is systematically distortive and reflects reified powers of domination. On the contrary, since every ideology is based on beliefs and interpretations that make the claim to validity, we can examine these claims to validity and show their falsity. We can show the falsity of an ideology without claiming that we have achieved a final, absolute, "true" understanding of social and political reality [1976, p. 109, emphasis added].

12. This distinction has been elaborated in a defense of the positivist position made by Nagel (1961). The difference in our account is that we would argue that discovery and validation are dialectically related; as procedures, they lead to new forms of socially produced knowledge that transcend the context of their production. This position is compatible with a realist philosophy of the social sciences (Bhaskar, 1980), as well as with conventionalist or phenomenological positions. "By dialectic, I understand the view that explanation and understanding would not constitute mutually exclusive poles, but rather relative moments in a complex process called interpretation" (Ricoeur, 1978, p. 150).

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