HUMAN STUDIES, 5, 119–135 (1982)

On the Demise of the Native: Some Observations on and a Proposal for Ethnography*

W. W. SHARROCK
Department of Sociology
University of Manchester
Manchester, England M13 9PL

R. J. ANDERSON
Department of Sociology
Manchester Polytechnic
Manchester, England

INTRODUCTION

It does not seem all that long ago that ethnography had something of a reputation for being pedestrian, prosaic, and not at all given to flights of speculative fancy and theoretical angst. Of late, this confident matter of factness seems to have been shaken. Today, ethnographers appear to be just as prone to self-doubt as anyone else.[1] Indeed, the following tentative statements made by Bennetta Jules-Rosette in a recent paper are not at all unusual.

> "Through studying the artists, I became profoundly aware of insiders' information to which I did not have access. I noticed that some of the wood carvers worked with their whole body, while others chipped away selectively with small wrist movements. It was difficult for me to determine exactly what these movements meant in terms of the artist's conception of a total work process." (Jules-Rosette, 1978:84)

Where once description would have been the relatively straightforward matter of recording the detail of what happens, now it is conceived of as a problem of fundamental importance; one which can only be characterized in terms of bodies of inaccessible knowledge and indeterminate conceptions of action. Posing the problem in this manner has led some ethnographers to the conclusion that what is required is a theory of ethnography on the basis of which methodological solutions to inaccessibility and indeterminacy can be formulated. In this paper we wish to query this supposition. We are not at all sure that ethnography stands in need of a

*This paper was originally presented at a joint SSRC/BSA conference on participant observation held in Birmingham during September 1979. We would like to thank the participants for their comments. We would also like to thank Peter Halfpenny for his helpful suggestions.

[1] As is only to be expected perhaps, the literature is both huge and inconclusive. Summaries of some of the issues involved can be found in the papers contained in Brenner *et al.* (1978); Filstead (1970); Beehler and Drengson (1978); McCall and Simmons (1969).

theory. In order to demonstrate as clearly as possible why we think it unnecessary to engage in a search for theory, we shall spend some time discussing the nature of ethnographic description for, although there is no doubt that such descriptions involve "understanding culture," we suspect that the proponents of theory for ethnography may very well be mistaken as to what that can be taken to imply. Once the misapprehension about culture and understanding are removed, the problem that the theory of ethnography is supposed to rectify simply disappears.

In presenting our case in this fashion we realize that we face some very particular problems. Not the least of them is the tendency for discussions of ethnographic method to degenerate into wild and woolly talk of a quasi-philosophical bent, eventually dissipating into a furious argument over only marginal issues. We feel that one way of avoiding this is to concentrate very closely on a few circumscribed propositions. We cannot state too plainly that we have no blueprint for ethnography. We are not interested in setting out fieldwork maxims. We want to show why we think that the search for a theory for ethnography is a mistake, and to do that we shall begin by asking how it is that ethnographers have come to think that one is required in the first place. Once the confusions that have engendered this position have been cleared away, then the debates over ethnography's programme can proceed.

It is our view that the search for a theory of ethnography is simply mistaken. We will argue this by demonstrating the illusory nature of the problems that such a theory would be designed to solve. The demand for a new theory of ethnography is based upon a conception of culture which ascribes privileged access to the native who shares that culture. The referent for tests of adequacy of ethnographic descriptions is then taken to be conformity with, or incorporation of, "the native's point of view." We shall argue that the native has no privileged status and that natives too can be treated as enquirers into their cultural settings. We will suggest that such a view follows from the rejection of the conception of cultures as integrated and distinct sets of rules which give meaning to activities. Once we move away from treating cultures as whole frameworks of meaning which lie behind and are expressed in activities, it is possible to view fieldwork in an entirely different light. Fieldwork activities can then be treated as a set of occasioned practices whereby the investigator and his informants make sense of activities. The interesting questions become "What are fieldwork practices?" and "What does doing fieldwork consist in?" and not "How can we possibly describe the culture that the natives share and we don't?"

Why Does Ethnography Need a Theory?

(i) What is the Problem of Description of Culture?

On reflection, the problem which ethnographers seem to be posing themselves is twofold and can be summarized by two interrelated questions. The first of these is philosophically inspired and, somewhat disarmingly, asks "Can ethnography be done at all?''. The second question presupposes a positive answer to the first by asking ''In what ways should ethnography be done?''. The fact that ethnography proceeds, and even seems to prosper, although no one has offered a convincing answer to the philosophic question, has been a source of much embarrassment and irritation to ethnographers and philosophers alike. Really this is what the search for a theory is about. Such a theory would show, or so it is hoped, how certain kinds of investigative stances in ethnography are founded in particular and acceptable philosophies. Since ethnography is considered to be serious, consistent and methodical descriptions of cultures,[2] then the theory will provide criteria for the assessment of seriousness, consistency and methodicalness. Given that the criteria can be philosophically justified, *it is assumed* that the methodology will be empirically viable. Once it has its theory, ethnography will be able to cope with its data. But why can't it cope with its data now? In the quotation cited, Jules-Rosette has given us a clue. She suggests that she was unable to ascertain the ''artists' conception of the total work process.'' It is this introduction of faithfulness to the experience of the subject as a criterion for assessment that has led to all the self doubt. Only descriptions which do this are to count as acceptable ones. Such a view of acceptability brings with it further problems which have been neatly identified by one of the foremost ethnographers working today.

> ''If we are to cling—as in my opinion we must—to the injunction to see things from the native's point of view, what is our position when we can no longer claim some unique form of psychological closeness, a sort of trans-cultural identification with our subjects? What happens to *verstehen* when *einfuhlen* disappears?'' (Geertz, 1977:481)

The canon for acceptability is to be descriptive adequacy and not, say, validity. We think this is an important and interesting move. Adequacy implies the notion of a possibility of differing viewpoints; validity, some measure of truth or correctness. Although it might appear that the demand of adequacy has completely undercut validity as a standard, we should not be misled. As Geertz implies and Jules-Rosette articulates, adequacy must involve the ''truth as the native sees it.'' But, what it has done however is to introduce in a formal manner the possibility of perspectival dichotomies which can be used to organize the presentation of ethnographic descriptions. Adequate descriptions will be those that are valid in terms of the dichotomies. What ethnography becomes, therefore, is the presentation of versions: native's and researcher's; insider's and outsider's; layperson's and expert's; our's and their's. It can be seen that the fundamental problem to be faced has now become not simply the production of a description that reconciles the al-

[2]Jules-Rosette calls it ''literal description,'' a term which although it has become widely used (cf. Cicourel:1964), we find opaque. Literalness must refer to the form of the description, it is in words rather than pictures, and not to some criterion of completeness or exhaustiveness. The criteria that might be used to measure descriptive adequacy have to do with recognizability and not some vague notion of completeness. ''My cat is the black one'' and 'My cat is the black one which measures two feet from nose to tip of tail, has one white hind paw and answers to the name 'Soggy' '' will both do as descriptions. Neither could ever claim exhaustiveness or literalness in the sense of completeness.

ternative versions, but how to be *certain* that, while retaining fieldwork rigour, the descriptions offered really capture the native's point of view. Unless both versions are contained in the descriptions, what we have to say will either be totally unrecognizable to our colleagues or totally at variance with our subjects' views of their own activities.

In addition to providing for descriptive adequacy, the use of dichotomies makes it possible to collect together observable activities into *different* cultures. The necessity for the use of the polarities such as "us" and "them," "native" and "researcher," is provided by pointing to the very obvious fact that "they" are different from "us" and what makes them different is that they share a different culture. The dichotomies provide a way of organizing people into standard types following standard patterns of behavior and, thereby, showing *their* culture. They also license one further step. What makes some other culture different from ours is that it comprises a different set of shared understandings. "They" are different because "they" see the world differently. "They" have, to use a once fashionable phrase, a different cognitive map. It is this idea which lies behind the criterion of adequacy and which, then, has led to the search for theory. Adequate descriptions of different cultures can only be given by reference to the shared understandings (the cognitive maps) which we share with the ethnographer and the natives share with each other. But to give such descriptions, the ethnographer is forced to talk about what typical natives in typical situations typically do. Yet using such course-of-action types violates the canon of adequacy. By their very nature they cannot capture the subjective experiences of individuals. Ethnography, then, appears to be facing a dilemma. In attempting to describe some culture in terms that are recognizable to their colleagues, ethnographers are forced to utilize categories which distort the native's point of view. And yet presenting the native's point of view is the whole object. It is this conclusion that has led to a demand for a whole new theory of ethnography, and with it a new theory of culture.

The question that we want to ask about all this seems to us to be both relatively obvious and relatively simple. And yet we can find no satisfactory answer to it in discussions of ethnographic method. Since the theory of ethnography is supposed to enable us to describe different cultures adequately, at what point does the quantum leap from describing cultural differences to describing different cultures take place? For the ethnographers, the problem their discipline faces is the avoidance of the dilemma so that they can both satisfy the criterion of descriptive adequacy and provide for rigorous and recognizable ethnography. For us, it is, rather, the establishment of its phenomenon in the first place. And, furthermore, it is the establishment of the phenomenon through the observation of the ways that people, in the societies under study, go about their daily lives. We think that the task that ethnography faces is to *demonstrate* the existence of different cultures, not to *assume* them. Presuming that people leading their lives in apparently different ways have, in fact, wholly different cultures is to turn culture into a monolith and ethnography into the construction of that monolith out of cultural differences. It is only when the existence of a culture is taken for granted that each observed difference can be taken as an instance of the analytic power of the dichotomies and have testimony to the general characterization of cultures. What we are asking ethnography to do, then, is to suspend the presupposition of the existence of different cultures so that the implications of that presupposition can be examined. We suspect that, were it to do so, it would be released from its dilemma and so have no need for a theory. To show why this is so, we now turn to the arguments that lie behind the problem of description.

(ii) The Lineaments of the Argument That Gives Rise to the Problem.

The requirement that what are, in effect, inductive standards should apply in ethnography does not seem to us to be overly harsh. It tallies fairly well with what ethnographers themselves say they are trying to achieve. According to Geertz, what the ethnographer is trying to do is figure out from the things that the native does and says "what the devil he thinks he's up to." The result is to be

> ". . . .an interpretation of the way a people live which is neither imprisoned within their mental horizons, an ethnography of witchcraft written by a witch, nor systematically deaf to the distinctive tonalities of their existence, an ethnography of witchcraft written by a geometer." (Geertz, *op. cit.*, p. 486)

But, if the application of inductive standards is permissible, what we find more than a little disconcerting is the intolerance that practitioners seem to display towards each other. The application of methodological standards appear to be of the all or nothing variety. To judge from some recent discussions,[3] ethnographers do not imagine that their discipline is the kind of enterprise that progresses by the gradual refining and improving of its techniques. At least as regards the building in of subjectivity, there seems to be no notion of pushing back the limits of confidence, of movement towards greater and greater precision. Either what is practiced now is all that can ever be expected or it is thoroughly misbegotten. This dogmatism explains a little of the abrasiveness of the debates. It rests on alternative and opposing answers to the philosophic question that we posed above, namely, "Can ethnography be done at all?"

We cannot think that many people would dissent from our view that culture is the organization of differing patterns of activities and social types. It follows from this that cultures are not themselves encountered in fieldwork. The variety of practices which are seen and described are tied into one nexus by being given an organization. Culture is that organization which the observer finds that people share and that their activities constitute. But culture is not merely a theoretical entity, it is a theory. It is a theory of the relationships between different kinds of people and different kinds of activities. Now, if we suppose a monolithic conception of cul-

[3]See, for example, Wax (1971); Johnson (1975); Cole (1975); Stevens (1978); and Disco (1976).

ture identified by the use of the dichotomies already discussed, and if we invoke a requirement that all descriptions we give should be adequate from the point of view espoused by each side of the dichotomy, then we face the problem of finding a reference point that captures them both. We could, for instance, disregard the peculiarities of individual positions within the culture and take as our point of departure the culture as a whole. This is exactly what classical functionalism set out to do. In its account, culture is homogenized and uniformly embracing. The trouble is that classical functionalism does not allow ethnography to do what Geertz says it should do. It does not allow you to say what the devil the native thinks he's up to. What you can say is what the devil the native is up to from the point of view of the system-as-seen-by-the-ethnographer. But, if we reject functionalism, can we do ethnography at all? This, it will be recalled, was what puzzled Geertz. What is involved here can be summarized very starkly. If the notion of differing cognitive maps is allowed to underpin cultures, and if ethnography is allowed to be the attempt by members of one cultural community to understand another, then how can it ever succeed? This we will term "the inaccessibility problem." The native and the ethnographer have different cultural maps. They see the world differently. Since the ethnographer cannot share the native's culture without becoming a native (and not, say, living like a native) our understanding of other cultures must always be partial and our descriptions inadequate.

Several important consequences might be held to follow from this line of reasoning. The inviolability of cognitive maps is but an expression of what we have been calling "culture as monolith." Given that outsiders can never be insiders and understand insiders as they understand themselves, then it is suggested that outsiders' understanding is really no understanding at all. The point is so important that it bears repeating. Because the ethnographer cannot understand the native's actions as the native understands them, then ethnography is doomed to imprecision, partiality and failure. Yet it is equally important to notice that this conclusion is not a product of fieldwork experience—that is, the use of inductive methods—but is *a priori*. The mapping of one set of cultural experiences onto another is not *found* to be impossible: it is assumed to be so. We are being invited to accept that if we meet a society of people who claim that they carry their souls around in boxes, or that paternity has nothing to do with reproduction, or that some people can inflict harm on others merely by wishing to do so,[4] we are faced not merely with a translation problem which ought, in principle, to be soluble. Instead we have incomprehension which can never be resolved. Problems of translation are, usually, handled by reference to a congruence of idiom. For those who hold the incommensurability of cultures thesis, such translations must be impossible. Indeed they seem to be suggesting that because idiom cannot be translated, it ought to be ignored. All utterances, according to such a view, have to be

[4]These are the examples which Mounce (1973) uses. He does so because they seem to have taken on a classical status.

treated as propositions about facts and never as occasional, figurative ways of putting things. The result is that many of the propositions to be found in other cultures turn out to be wrong or meaningless if they are measured against our standards of factuality. We know that paternity is necessary for human reproduction and that souls, if they exist, are not the kinds of things you can tote about in boxes. But are we not then in the same position as someone who claimed not to understand what was meant when it was said that one person had all the brains in the family, since experimental biology had conclusively demonstrated that cranial content varies little among the human population? The refusal to countenance the issue as one of translatability and the enshrining of it as one of competing versions of "the facts" has enabled all sorts of red herrings to be dragged in all sorts of directions. Since some people name the colors differently and even make different discriminations, since others state that they are witches, then to propose that there are no witches or that the evolution of color categories follows from certain properties of the light spectrum, is held to violate their perspectives. It is judged to be the imposition of one set of subjectively held or culturally determined set of categories upon another. By casting the argument as one about facts and values, subjectivity and objectivity, and not about the plausibility or otherwise of alternative translations, what is being discussed gets irretrievably stuck in the quagmire of the determinants of rationality, the existence of logical universals and the foundations of truth. The net effect has been the loss of faith in their methods that we suggested ethnographers are now experiencing together with the determination to find a theory which will prevent the degeneration of ethnography into exotic travellers' tales, biased, partial and ethnocentric.

If cultural relativism was desgined to enrage ethnographers, it certainly succeeded. As it amounted to no less than a philosophic justification of a methodological proscription, it was met by both philosophic and methodological rejoinders.[5] The former sought to undercut what was felt to be the anti-rationalism of the relativist position by assuming that appropriate standards for assessing the validity of statements were to be found in the canons of logic. Such standards were held to be grounded in the essential features of all human thought and hence expressed logical universals. Any account which made men out to be irrational or illogical was to be rejected in favor of one that saw men as having various forms of rationality, all of which were based on the same logical principles. This cognitive apartheid ("the same but different") engendered a vituperative debate over magic, science and ritual which persists to this day.[6] As is the manner with all such philosophic disagreements, reference to data provided no help. Rather, instances of data were used to exemplify the strengths and weaknesses of opposing positions. And yet

[5]See, for example, Cook (1978); Turner (1979); and Horton (1976).

[6]We have long since given up trying to keep abreast of this debate. One convenient summary of the early contributions is Wilson (1970). For a somewhat wry review of the furore he instigated, see Winch (1976).

what was at issue was not a disagreement over what "understanding" and "culture" signify, simply what the implications might be. Both sides treated "culture" and "understanding" in a monolithic fashion. There was never any doubt what "understanding" and "culture" might be like, only whether it was ever possible to understand another culture.

The methodological response to relativism was dual in character. As we have seen, it required ethnography to incorporate the native's point of view in its descriptions. However, the insistence upon this was not taken as being thoroughly destructive. All that was felt to be required was a way of retaining the formalism of scientific method together with the addition of the native's perspective. The duality consisted in conceding the relativists' major objection while, at the same time, disregarding what was felt to be its clear implication. For the cultural relativists, the acceptance of the need to incorporate the native's point of view and to derive that in any kind of rigorous manner, would paralyse ethnography. The ethnographers did not agree. The way that they sought round the problem was to seek to formulate a means by which the dichotomous oppositions which are essential to the relativists' case, could be incorporated in the same descriptions. The relativists set the issue up as a choice between competing versions. The ethnographers reconstituted it into one of amalgamating the versions. The most successful attempt to achieve this amalgamation took its lead from linguistics. In linguistics a similar opposition had been encountered between phonemics and phonetics. What linguistics had done was to treat language use as a set of activities performed according to rules by members of a language community who have a competence in those rules. The competences which cognitive anthropologists attributed to members of cultural communities were in cultural rules and their application. The specification of a set of rules for a culture would be nothing other than a set of instructions for how to act as a native would. Part of the corpus of rules would be instructions for decoding contexts and relating persons and objects. It was for this reason that cognitive anthropology focused so heavily on classification.[7]

The presentation of the native's point of view while providing the *raison d'être* of cognitive anthropology, also gives its major weakness. It doesn't go far enough. The formality involved in setting out the rules for diagnosing a disease or asking for a drink,[8] prevents the retention of the particular quality of the native's experience. The native simply does not encounter his culture as a system of rules or a typology of classifications. Because the native's experience is still being excluded, this kind of ethnography must fail its own tests. It is because of this failure that Jules-Rosette wants a theory which will make the native's experience congru-

[7]We are using the designation "cognitive anthropology" as a collective term for the work of Frake, Goodenough, Conklin and others. A good selection of this work is to be found in Tyler (1973) and Hymes (1964).

[8]The analyses setting these examples out are to be found in Frake (1961) and (1964).

ent with the ethnographer's descriptions. And, according to this theory, what it is necessary to do is for the ethnographer to become a native.

(iii) Reflexive Ethnography[9]

The criticism that is levelled at the cognitive anthropologists is that they failed the test of adequacy by failing to preserve the experience of the native. What is interesting about this version of the test is that it has inverted the ordering of the dichotomies. Whereas before "we," the ethnographers, were the experts, now it is "they," the natives, who are experts in their own cultures. Our task is to present and preserve their versions. For Jules-Rosette, possessing a culture seems to mean possessing the native's skills and information (cf. the quotation at the beginning of this paper). Even though the dichotomies have been reversed in their application, they are still retained as the organizational principle for the provision of descriptions. It is here that her proposals take on their distinctive character. She wishes to retain formalism while at the same time annexing the native's subjective experience. This, she claims, can be done by a "reflexive ethnography." Although it is far from clear in what she says exactly what this ethnography will consist of (for example, she refers to it, rather mysteriously, as "translucent"), nonetheless her intentions are plain. She wishes to surmount the dilemma by combining the dichotomies and in doing so melding formalism with "the interpretive insights of the new ethnography" (pp. 90–91). Although she is a little coy about details, one point comes across strongly. Whatever this ethnography eventually turns out to be, it will demand a continuous movement back and forth between members' and observers' points of view and that the former can only be obtained by "total immersion" in the native culture. This latter notion requires considerable elaboration for it is, we think, at the heart of the puzzle that we started this paper with.

It may be easier if we begin by making it clear what "total immersion" does not mean. It does not mean becoming familiar with an alien way of life by living according to the practices to be found there; not for Jules-Rosette, the expedient of running one's household according to Zande witch beliefs as Evans Pritchard (1936) did. Nor can she try to pass as a member like John Lofland (1966) tried to do. For her, total immersion appears to mean nothing less than becoming a member of the culture, believing in Zande witchcraft, the imminent end of the world, or in her case, the Bapostolo Church. Once the ethnographer is a native there should be no obstacle to the attainment of a "reflexive ethnography." Once a native, the ethnographer should have no trouble understanding the native's point of view.

Lest it should be felt that what we have been worrying about here is a particularly parochial matter, we would like to introduce into the discussion an example

[9]Although we have taken this term from Jules-Rosette, our discussion is not to be taken as a critique of her fieldwork methods. We are concerned here with the way that cultures are conceived.

which was brought to our attention in a paper by H. M. Collins (Collins, no date). The principal concern of Collins' paper was to examine some of the difficulties involved in studying minority and to some extent *sub-rosa* pursuits, in this instance parapsychology and its relationships to the wider activities of "legitimate" science. One of the issues that Collins discusses is the feeling that unless investigators possessed the same "frameworks of meaning" as the scientists, i.e., their definitions of phenomena, their theoretical systems and attitudes towards research, they could not really appreciate how legitimate scientists would regard parapsychology. The same, of course, applies to parapsychology. Unless the investigators took part in experiments, etc., how could they know what parapsychology meant to those who practiced it? In outline, this is much the same problem that Jules-Rosette has set up. Collins answers it pragmatically, by trying to ensure that the credentials of the investigators would be acceptable to the groups under study. This would not satisfy Jules-Rosette. She would require membership of both the community of physicists and that of the parapsychologists. But surely something is amiss here? For us to be able to say how an activity is done, it is not *always* necessary for us to be able to do it. And being able to perform an activity does not ensure that we can talk acutely or interestingly about it. The sociological description of some activity, be it parapsychology or the Bapostolo Church, does not require that we be practitioners of parapsychology or Bapostolo Religion. Indeed the studying of how an activity is performed may mitigate against its successful performance. After all, the theoretical attitude on daily life is not the same as the natural attitude. It is in the important and largely unexplored distinctions between, say, talking physics or Bapostolo and talking *about* physics or Bapostolo that most of the interesting problems for ethnography lie.[9]

We would, though, like to go a little further with Jules-Rosette's theory. For her, only those who are Bapostolo can be licensed to talk about the Bapostolo. And yet if we take her claims and positions seriously, such a programme cannot, on its own terms, succeed. She wants us to accept that she was converted to a fundamentalist native religion (thereby adopting their cognitive map) while, at the same time being able to retain and utilize a sociological conception of that religion (Jules-Rosette, 1978, p. 83). It was this feat which allowed her to engage in "reflexive ethnography." Nonetheless, if *her* requirement for descriptive adequacy is to be the imbrication of two versions told by the same person, then, in this instance at least, it is impossible. Membership of the Bapostolo Church requires attachment to a culture which is entirely at odds with sociology; it is one which posits that the shared understandings that it is composed of are both exclusive and exhaustive. Non-members do not share these understandings and are wrong. Understanding the Bapostolo Church as the Bapostolo do means sharing their acceptance of this cultural monolith. And yet on Jules-Rosette's account, the distinctiveness of the Bapostolo is given by the cleavage between their culture and

[9]The whole formulation of this point has been lifted from Ryle (1960).

ours. The Bapostolo have a different culture *because* we do not share their cognitive maps. So, Jules-Rosette has not wriggled out of the dilemma but impaled herself even more firmly on it. For her argument to work, we have to share the Bapostolo culture and not share it; understand the Bapostolo Church and not understand it; be a Bapostolo and not be one.

Understanding Culture—Again.

So far we have been trying to untangle some of the knots that ethnography has tied itself into. We have seen that according to the opinions so far discussed, ethnography is either impossible or hopelessly inadequate. And yet it seems to be done. This leads us to think that it might be the conception of ethnography that is embedded in the arguments outlined which is causing all the difficulties. And, since everyone is agreed that ethnography is about "understanding" other "cultures," it is to those two crucial terms that we now have to return.

The concept of culture has a catch-all status in the social sciences. We have already seen that it is used to refer to the practices that a cultural community engages in as well as the knowledge that they are supposed to share. People who share a culture do the same things and know the same things. It is the very fact of this standardization that enables a cultural identity to be attached to a set of practices and a body of knowledge. Since it is the culture that they share which discriminates one group from another, it is culture which turns a collection of individuals into a community. We suggested earlier that the task which ethnography should take up is the demonstration of culture. That is to say, it could try to show exactly what makes one different from another. There is no doubt that members of cultural communities are able to identify themselves and can point to individual traits or practices which mark the distinctions between themselves and others (cf. Moerman, 1968). But the things that are pointed to stand as criteria for discrimination because they are able to mobilize the concept of a cultural community existing as a separate entity in the first place. Anything could be pointed to to mark the culture's distinctiveness since it would be a manifestation of that distinctiveness. The existence of separate cultures is, then, a *principle* of ethnography and not its *finding*. It is a principle that is carried over from our ordinary ways of conceiving of ourselves. But the distinctiveness of separate cultures could only be documented rigorously by reference to particular items which constituted it. If cultures are discrete what exact items mark the Azande off from the Dinka? the Dinka from the Nuer? and the Nuer from ourselves? In each case, we suggest, what will be pointed to will be different sets of things. It follows that there cannot be any one set of practices of corpus of knowledge which identifies the essence of "Dinkahood," "Zandehood" or "Nuerhood." The consequence of this is that identifying other cultures cannot be the straightforwardly inductive process that ethnography represents it as being. Or, at least, it cannot be if it entails a presupposition of the existence of separate cultures in the first place. We are not denying that the Dinka, the Zande and the Nuer know that they are different and can show

how. They have a theory of culture too. But their theory of culture is not constructed as a scientific idealization. It is a practical theory of culture used to distinguish for practical reasons those who have rights or obligations in common. It makes no claim to invest culture with a distinct ontological status.

If we were to suspend the *a priori* status of culture, what would the effect be upon ethnography? At a very minimum it would require ethnography to take up in some serious fashion the inductive claims it makes. Ethnographic description would then be an enquiry into the existence of other cultures as bodies of discrete "shared understandings," rather than an instancing of their distinctiveness. It requires not only a treatment of the detail of ordinary life in each of the putative cultural communities but a treatment of it in a novel fashion. It may well be that studying culture in this way will lead us to the view that what appear to be wholesale cultural differences are, in fact, no more than variations in the ways that some things are carried out. An instructive example could be taken from a lot nearer home than the Southern Sudan. Recently, much of the sociological literature on crime and deviance has been given over to the study and exposition of the deviant culture or the criminal world.[10] Often such discussions are attempts to explore the distinctive meaning of the deviant. But, if we ask what exactly it is that makes deviants or criminals different from the rest of us, then we are forced to the conclusion that there is not a lot to point to—except maybe that some of the acitivities they engage in, for example how they earn a living, happen to be against the law. They go about their lives in pretty much the same ways that we, the rest of the normal citizenry, do. They treat their work and occupational skills in the same ways that we treat ours.[11] It is this invisibility of deviance as a category which has led to both the strictures on evidence and proof in legal proceedings, and to some of the wilder excesses of ecological and physiological theories of deviant behavior. If we suspend the notion of a distinct criminal culture, then, what the ethnographies of crime and deviance give us are descriptions of ordinary people leading ordinary lives. Crime is simply another job. The theory of a criminal or deviant culture is used to instance particular examples of it. The examples then become substantiations of the existence of the distinctive culture. This kind of reasoning is a very practical one. Magistrates, jurors and policemen use it, as do criminals themselves. It is practical rationality and not a scientific one.

Our argument is, then, that within ethnography certain conceptions of culture have taken on an *a priori* status. If that status were to be suspended, then it would no longer be necessary to institutionalize cultural differences by organizing them in terms of the dichotomies of "us" and "them," "native" and "researcher,"

[10]Although the literature here is large, it is all rather repetitive. An early example is Sutherland (1956) with more recently Matza (1969). For the other side of the dock see Manning (1977).

[11]Letkerman (1974) is full of the most marvellous detail on burglars and bank robbers.

"expert" and "layperson." To quote Geertz for a third and final time, it would no longer be necessary to maintain

> " . . . .the myth of the chameleon field worker, perfectly self tuned to his exotic surroundings—a walking miracle of empathy, tact, patience and cosmopolitanism." (Geertz, 1977, p. 481)

What does all this imply for the notion of understanding? Once different monolithic cultures have been decomposed into cultural differences, the problem of understanding and its near relation—the problem of meaning—become a great deal more tractable. Originally, it will be recalled, what was at issue was the possibility of *grasping all at once* a complete set of shared understandings; of adopting wholesale a cognitive map which was, by definition, incommensurable with that which was already held. We called this the inaccessibility problem earlier. But inaccessibility can only be a problem when it is assumed that cultural exclusivity exists. If we say that we can perfectly well understand the vast majority of things that other people do because, in our daily lives, we do things like that ourselves, does that mean that we no longer have a problem of understanding? What about those things that are predicated on beliefs we do not share? Can we ever understand what they mean? Here we must be careful of the ways that we think about understanding the meaning of an action, for all too often we are led astray by the ways we talk about such matters. It does not make any sense to talk of meaning as if it were subdivisible into component parts, like the total of a number of sweets in a bag is subdivisible into the number of smarties, jelly beans and toffees. Adding up meanings will not give a total meaning. This is the mistake that those who search for a theory for ethnography make. The meaning of an action is given by the way that it is understood. Such understandings are construed in the light of the relevances that the individual has. Since the natives do not share the ethnographer's relevances, except for the really rare occasions when we force them to view their social organization in our terms,[12] then there is little reason for us to try to grasp his meanings. The whole confusion is really a product of a misunderstanding about the role of meaning in sociological explanation and description. The requirement is not to excavate the meanings that members have for the actions they take, but to ensure that in our accounts we treat them as interpretive actors, *and only as interpretive actors,* until the cultural rules that are held to explicate their actions are demonstrated. That members have reasons and motives, that their actions have meaning for them, does not constitute these as the last court of appeal for the determination of the adequacy of ethnographic description. Rather, that members possess these things should be a first methodological and analytic principle. What has to be done is to show how, acting on the basis of their reasons and intentions, what members do comprise an observably distinct culture. It is only

[12]The only sustained example of this that we know of is in Mead (1968).

because of this misunderstanding that the new ethnographers such as Jules-Rosette have had to resort to devices like "total immersion" and "relfexive ethnography". Such devices are seen as offering ways out of the dilemma. It is our proposal that the dilemma does not really exist, so there is no need to try to find a way out of it.

Putting the Proposal into Practice

The recommendation that ethnographic fieldwork should be based upon the suspension of a premise of cultural community means that we are asking for the withdrawal of mutual understanding or shared expectations as ways of accounting for action. It would then be the task to demonstrate how mutual understanding and sharedness of expectations were achieved. This is a crucial switch in method for it constitutes the actor—the subject, the native—as an enquirer into culture, rather than as an expert in it. What is going on in the setting becomes a problem for the native as well as for the ethnographer. But, since ordinary people go about their daily lives without being transfixed by puzzlement at each and every encounter, they must be able to accomplish mutual understanding and so have methods for doing it. The examination of these methods become the topic of ethnography. This immediately raises for the field worker the problem of how to study them. We can only begin to answer this by reflecting on the nature of fieldwork.

By now it has become standard practice for ethnographers to include in their acknowledgements expressions of gratitude to the subject of the study for the tact, sympathy, toleration, forbearance and good humour that was shown during the period of fieldwork. Such expressions are an informal articulation of the position which the field workers stood in *vis-à-vis* their subjects while they were gathering their materials. It is slightly odd that, as far as we know, what is readily admitted informally is never incorporated formally into the analysis that is offered. No attempt is made to utilize the relationship acknowledged informally as an analytic machinery for the description of what happened in the field; what was observed and understood; what was noticed and found puzzling. Although researchers are happy enough to admit that they were guests among their host tribe, culture, people or whatever, this dichotomy is never used to organize materials and present data. Instead we are told what "ethnographers" saw the "natives" do, what the "insiders" know that the "outsiders" do not, and so on. But what would happen if we used the host-guest dichotomy? At first sight it would appear to mean an entirely different characterization of the researcher-subject relationship, one which has all sorts of intriguing possibilities.

The most salient of these possibilities is the light which it might throw on the field strategies that are often adopted. Since ethnography is an attempt to delineate the set of particular rules that underpin a specific way of life, the norms which make a culture identifiable, one would have thought that the easiest way of discovering such fundamental rules would be to act as if they did not exist or did not apply. The systematic disregarding of normative constraints would soon make it clear what was, and what was not, fundamental to a way of life. It would be a much more direct and telling investigative procedure than the observation and recording of people going about their lives normally, thereby making the constraints invisible. If, no matter what the investigator did, sanctions were applied then we could talk of different cultures rather than cultural differences. It is important to see that we are not saying that the researcher should follow no rules, that he should act randomly, but that he should act according to his cultural rules to see to what extent they differ from the natives'. Even the strategy of activity as a neophyte in the culture is constrained by the obligations and responsibilities inherent in the guest/host relationship. This would be an inductive procedure surely? But this is exactly how field workers are sternly advised not to act. They are told to be unobtrusive, make friends and fit in with whatever their subjects do, just as good guests fit in with their hosts' routines. If such a systematic violation is held to be inadvisable, how are we to build in the suspension of a shared culture between natives and other natives as well as between natives and researcher? As we have said, one way would be to take it as an analytic principle that it is a matter of enquiry what sense is being made of the things that are going on for the native as well as the researcher. The native is then treated as regarding his own actions as objects of investigation by both himself and the researcher. What activities are to mean is something which the native and the researcher have to discover together. In making what he does accountable to the researcher, who in turn demonstrates his understanding, his account, by the things that he does, the native and the researcher coproduce fieldwork. Exactly the same suspension results in a view of the relationships between native and native as one of the coproduction of culture.

This is not to repeat the clichés that sometimes people interpret actions differently or that questions and requests may be misunderstood. It is not even to say that asking questions and recording the answers is a peculiar way to set about discovering a culture. It is, rather, to say that the stance that treats the native as an expert in his culture, knowing what he is up to and unproblematically recounting that to the researcher, may not be of much use. If we begin by positing that natives and researcher have to discover what is going on—what events and activities mean—then we can treat meaning as an achievable phenomenon and understanding as a risky business. It is these contingencies and risks that natives and field workers have to deal with.

We are now in a position to draw this argument to a conclusion. What we have been recommending is a completely fresh look at the culture and understanding as phenomena for ethnography. Our proposal is that the notion of a shared set of meanings and understandings should be the point of analysis, what it seeks to demonstrate, and not what it is predicated on. We suggest that this requires that the ethnographic treatment of the social actor be restricted to only those things which allow him to act and no more. The idea of the actor as an interpretive actor does not require the attribution of shared understandings. These have to be shown to be the outcome of his interpretive actions. As outcomes they are contingent, dependent on biography, context, relevances and so on. In this way the topic of ethnography becomes the methods that members use as members of cultural communities for the resolution of the contingency of meaning. Here would be a rigorous treatment of social action that began with the interpretive actor and proceeded from there. It is a conception of action which renders "reflexive ethnography" redundant and makes systematic ethnography viable and the accumulation of findings possible. At the same time it would enable the serious consideration of ethnographic fieldwork in terms of the practices people use to understand one another.

REFERENCES

Beehler, R. & Drengson, A. *The Philosophy of Society*. London: Methuen, 1978.

Brenner, M., Marsh, P. & Brenner, M. *The Social Contexts of Method*, London: Croom Helm, 1978.

Cicourel, A. V. *Method and Measurement*, New York: Free Press, 1964.

Cole, M. An Ethnographic Psychology of Cognition. In R. W. Brislin, S. Bachner, and W. J. Louner, (Eds.), *Cross Cultural Perspectives in Learning*. Halstead Press, 1975.

Collins, H. M. The Investigation of Frames of Meaning in Science, Unpublished paper, no date.

Cook, J. "Cultural Relativism: an ethnocentric notion." In Beehler and Drengson (Eds.), 1978.

Disco, C. Ludwig Wittgenstein and the end of Wild Conjectures. *Theory and Society*, (Vol. 3, No. 2), 1976, 265–287.

Evans Pritchard, E. *Witchcraft, Oracles and Magic among the Azande*, Oxford: Oxford University Press, 1936.

Filstead, W. J. *Qualitative Method*, Chicago, IL: Markham, 1970.

Frake, C. O. The Diagnosis of Disease Among the Subanum of Muidanao. *American Anthropologist*, 1961, *63*, 113–132.

Frake, C. O. How to Ask for a Drink in Subanum. *American Anthropologist*, 1964, *66*, 127–132.

Geertz, C. From the Native's Point of View. In J. L. Dolgelin, D. S. Kemnitzser and D. M. Schneider (Eds.). *Symbolic Anthropology*, New York: Columbia University Press, 1977.

Horton, R. Professor Winch on Safari. *European Journal of Sociology*, 1976, *12*, 157–180.

Hymes, D. *Language in Culture and Society*, New York: Harper & Row, 1964.

Johnson, J. *Doing Fieldwork*, New York: Free Press, 1975.

Jules-Rosette, B. Towards a Theory of Ethnography. *Sociological Symposium*, 1978, *24*, 81–98.

Letkerman, P. *Crime as Work*. Englewood Cliffs, NJ: Prentice-Hall, 1974.

Loflaud, J. *Doomsday Cult*, Englewood Cliffs, NJ. Prentice-Hall, 1966.

Manning, P. *Police Work*. Cambridge, MA: M.I.T. Press, 1977.

Matza, D. *Becoming Deviant*. Englewood Cliffs, NJ: Prentice-Hall, 1969.

McCall, G., & Simmons, J. *Issues in Participant Observation*, Reading, PA: Addison Wesley, 1969.

Mead, M. *The Mountain Arapesh* (Vol. 1). New York: Natural History Press, 1968.

Moerman, M. Being Lue: uses and abuses of ethnic identification. In J. Helm, (Ed.), *Essays in the Problem of Tribe*. Seattle, WA: University of Washington Press, 1968.

Mounce, H. O. Understanding a Primitive Society. *Philosophy*, (Vol. 48, No. 186), 1973, 347–362.

Ryle, G. Formal and Informal Logic. *Dilemmas*. Cambridge, England: Cambridge University Press, 1960.

Stevens, J. The Other Side of the Looking Glass. *Mid-American Review of Sociology*, (Vol. 3, Part 1), 1978, 95–103.

Sutherland, E. *The Professional Thief*, New York: Phoenix, 1956.

Turner, S. Translating Ritual Beliefs. *Philosophy of the Social Sciences,* (Vol. 9), 1979, 404–423.
Tyler, S. A. *Cognitive Anthropology*. New York: Holt, Rheinhardt and Wiston, 1973.
Wax, R. *Doing Fieldwork*. Chicago, IL: University of Chicago Press, 1971.
Wilson, B. R. *Rationality*. Oxford: Blackwell, 1970.
Winch, P. Language, Belief and Relativism. In H. D. Lewis (Ed.), *Contemporary British Philosophy* (4th Series), London: Allen and Unwin, 1976.