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*Envy and Inequality in Fieldwork:
An Example from Ghana*

by WOLF BLEEK

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INTRODUCTION. Most anthropologists carry out their research among people who are considerably poorer than they are. Looking at anthropologists from these people's

point of view, doing fieldwork is not only a privilege of the rich, in the long run it is also lucrative, since fieldwork experience is a prerequisite for most well-paid anthropological teaching jobs. Fieldwork among the poor is, therefore, a dubious activity. The anthropologist (who is often seen as a rich man) attempts to participate in the lives of poor people and by doing so he will eventually become even richer. For most anthropologists who are aware of this "double play," fieldwork is psychologically strenuous. The strains can be alleviated by avoiding *personal* contacts with poor informants, but it is clear that such a solution is detrimental to the quality of fieldwork, of which the basic technique is *participant* observation. Nobody should be surprised to hear that it happens only rarely that anthropological fieldworkers truly participate in the lives of their informants. The privileged background and favorable prospects of an anthropological fieldworker are hardly compatible with the equality that is necessary for sharing the life of poor people. Many anthropologists have tried to bridge the gap by offering help, for example by distributing medicines, providing transport, or paying school fees for children, but such assistance does no more than reaffirm the existing inequality.

It is not only the anthropologist who knows about the fundamental contradictions inherent in fieldwork; the informants know about them as well. As a consequence they may react to the presence of an anthropologist in a hostile way. So, the anthropologist has not only an emotional and ethical problem (how to be sincere), but also a methodological problem (how to approach people who suspect him).

A FIELDWORK SITUATION. In 1971, I carried out fieldwork in a rural town in the Kwahu area of southern Ghana. The inhabitants of the town were familiar with Europeans and Americans, mostly missionaries, who apparently had access to enormous amounts of money; drove in cars; built houses, schools, hospitals, and churches; and accomplished other things far beyond the power of the local people. Obviously, people identified me with those missionaries and to some extent they were right. I had friendly relationships with them; I could enter their homes and ask for their help without fear of being turned away. To some extent, however, they were wrong. I had no access to the funds of the mission; I had not even a research grant. If by European standards I could perhaps be called poor, to the inhabitants of the town I was, without doubt, rich, certainly in the beginning of my research.

My research topic was at first very wide and general. I tried to take notice of everything that appeared important to the people themselves, particularly affairs that concerned family life. I interviewed members of the family in whose house I was staying and attended events that other people attended.

One day I visited the relatives of an old lady who had just died. I went there together with a Ghanaian friend who assisted me during the first three months of research. It was in the beginning of our research and I found it difficult to approach people in the right way. I had studied the local language and was very anxious to speak it but I was still very poor at it. There were some stereotype jokes to which I resorted if the conversation became too difficult.

During our visit to the house of the deceased woman a few incidents took place that were partly the result of my incapacity to handle the situation and partly of the people's suspicion about the motives of my visit. The incidents clearly illustrate the ambiguity of anthropological fieldwork. I shall quote from my field notes and then briefly comment on the incident.

On the 15th of July an old lady died. A few days after her death we asked Mr. A. to introduce us to one of her relatives so that we could ask this person some questions about her life. Mr. A. brought us to a place where about fifteen relatives were assembled. In the center was a young man sitting behind a table with a notebook and a plate. On the plate was one shilling. People came to pay their contribution to the funeral. When someone had put his money on the plate, his name was written down in the notebook plus the amount of his gift. It was explained to me that someone whose name does not appear in the books will have a miserable funeral when he (she) dies. Immediately after money had been paid one of the women took it and put it in her cloth, leaving just one shilling every time.

People asked us to sit down and drink some palmwine. We sat down and a man poured the wine into my friend's calabash. Then it proved that only the dregs had been left over in the pot. People protested that my friend should pour it back, and laughed. There is a common saying that one who drinks dregs will get many children (*wonom epuo pii a wobewo mma pii*). So my friend jokingly asked how many children they wanted him to get. The wine was poured back and the pot was taken away.

We started our questions but one man, O., had already produced a small piece of paper with the following text in English: "The approximate age of old-mother is 95 years. She left over eleven children, and grandchildren are 54. Occupation was a farmer." O. asked us whether this was sufficient, and we explained that we wanted to ask some more questions. I suggested we continue our conversation somewhere else, but the man said we could stay there. We asked him about the woman's age, her husbands, her family, and so on. Some people asked us why we wanted to know all those things and we answered that we were only interested. This answer did not satisfy them, particularly not when my friend started to write the answers down.

Very soon a woman cut in. "If he asks you anything, do not answer. Were we not collecting money when he came?" She suggested that we had not contributed anything, but I missed her point and asked her what she meant. She answered bluntly, "Give some money." I

tried one of my stock-jokes and asked her "Are you a Kwahu?" She answered "Yes" and I added "I can see that, because Kwahu people like money." (Kwahu people are indeed notorious in Ghana for their money-mindedness. They are clever traders and monopolize a great deal of the trading business in the country.) This made her more angry and she summoned the others not to speak to me anymore. An old lady came to her help and said to me "She is hungry, give her money to go and buy food first." The first woman said again "He is going to make money out of all that he is asking." O. assured us, however, that we could continue our questions. We did, but were repeatedly interrupted by the woman's: "Don't say it, don't say it."

We stopped our questions and watched what was happening. The woman became more angry. An old man passed by, he stopped and said "If it were me, I would not even open my mouth to him. Do you not see that the white man is clever; when you get angry he does not talk but waits till you become calm again." The young man at the table remarked "Let him ask, he is not quarreling, is he?" But the woman retorted sharply "Get away, don't you hear that he is saying Kwahu people like money too much?" Another person turned to us asking: "Won't you bring money at all?" But another woman defended us, saying "Look this man contributes very well to our funerals," probably referring to previous funerals.

About that moment a new pot of palmwine was brought in. The daughter of the deceased said that they should give us some. We drank the palmwine. New visitors came and shook hands. Some of them paid money. During that period the daughter was all the time asking for my mother's name, but I did not notice it. When the visitors had sat down, my friend explained that he worked for me, and that I paid him. The daughter immediately cut in: "Why do you pay him and don't give us any money?" Then she continued "You also, what is your mother's name?" I answered "Give me first money, then I'll tell you." All laughed. This went on for some time. Then I explained that I was joking and that they could ask anything they wanted about my family. As soon as I mentioned my mother's name, the daughter shouted with a high-pitched voice "Write it down for me!" and repeated this several times.

We finally decided to leave and each of us put two shillings on the plate. Our names were written down and everybody thanked us. The whole situation changed within a moment and everyone was nice to us. They said we could have a look at the book later on, if we wanted to. We left, but not before hearing two other remarks concerning money, one from the daughter saying that I should bring her some money to buy porridge; but this time it sounded more friendly.

Looking back at the incident, I find it understandable that people were annoyed with me. In the first place, it was not very tactful to interview a group of people about their "mother," a few days after she had died. Secondly, it was ill-mannered to ignore the plate when we entered, because

financial contributions are always given when people come in. So my failure to pay at our arrival was for them a clear indication that I had no intention to pay. This, taken together with the fact that I was a white man—and therefore a rich man—made some people lose their patience. I am only surprised that my friend did not prevent me from making these mistakes. Maybe I was then too blunt to pay attention to his signals.

Before analyzing the situation, I must note that such incidents have been rare during my fieldwork. People hardly ever challenged my role as a fieldworker directly. Usually they were kind and cooperative. Sometimes they avoided me or failed to keep their promises, but as I have said, a direct confrontation between me and an informant occurred extremely rarely. Maybe the blunders I committed in the situation described above were necessary to bring out certain ideas about the anthropological fieldworker that are usually concealed.

It seems to me that the difference in wealth between a fieldworker and his informants is particularly irritating to those informants who are invited to be very open with the fieldworker. They are, during the time of the interview, treated very cordially and may even begin to think of the fieldworker as a close friend. At any rate, the information they give may be the type of intimate information that is only transmitted between close friends. However, between such friends, there can be no financial disparity. The discovery that the "friendship" was mainly strategic and lasted only the time of the interview must be particularly frustrating to the informant. It is not likely that he will agree to a second interview. Moreover, egalitarianism in economic terms is a traditional rule. Sharing and hospitality are important traditional values and the growing inequality with the coming of capitalist-like modes of production is resented by many. The impact of traditional values is still recognized in eating patterns. Eating alone and leaving nothing for others (particularly children) are frowned upon. The two following proverbs underscore this view: *Opanyin bone na ohohro n'ankasa ayowam* (It is a bad man who washes his own plate). The implication is that such a man has finished all the food himself. *Wonkoa didi a; wonkoa ne* (If you eat alone, you have to go to the toilet alone). The implication is that if the food causes diarrhea, the eater has to visit the toilet alone. At night going to the toilet at the edge of town is particularly unpleasant if one has to go alone.

It is likely that the informant's annoyance will increase with the financial inequality between him and the fieldworker. As I have stated before, my financial position was not very different from that of other men in the town (although my prospects were much brighter than theirs). I lived among them and almost every aspect of my life was visible to them. They could see that I did not spend more money than they and yet they were not convinced. "A poor European" was too absurd a thought to be acceptable.²

Throughout my fieldwork, people made insinuations to my hidden wealth. Only a few people, who were very close to me, probably believed that I had about as little money as they. I was daily reminded of my richness by the way people greeted me. After the incident that I have described, we recorded that same day nine greetings of people that referred to financial inequality. If I write "greetings," I mean to say that the remarks were expressed to us in lieu of a greeting, i.e., when we met people on the road or in a compound. The ordinary greeting (for example: good evening or how are you?) was either replaced or followed by the remark about money. The nine "greetings" on that particular day were:

- (1) *Kye me taku nkoto nsafufuo* (Give me a sixpence to buy palmwine).
- (2) *Ma me sika oo* (Give me money).
- (3) *Fa flask no kye me. Memmegye anaa?* (Give me the flask. Shall I take it?).
- (4) *Kye me sika nto atadwe !* (Give me money to buy tiger-nuts).
- (5) *Father, kye me sika* (Father, give me money).
- (6) *Teacher, kye me taku nkoto krasin, wae, mepawo kyew* (Teacher, please give me a sixpence to buy kerosine).
- (7) *Master wei dee, moma me tro koto jot nom a anka meda moase* (Hey, masters, if you give me a threepence to buy a cigarette I will thank you).
- (8) *Okyena bre me sika nto aduan nni* (Tomorrow you must give me some money to buy food to eat).
- (9) *Oburoni, kye aberewa sika nto tawa !* (White man, give the old lady money to buy tobacco!).

Similar "greedy greetings" have been reported by Chagnon (1974:165) in his account of fieldwork among the Yanomamö (see further, below), and by Turnbull (1972: 48-50) who carried out fieldwork among the Ik or Teuso in Uganda. Turnbull writes that his traditional greeting was responded with "Give me tobacco." The Ik, according to Turnbull, were so utterly impoverished that solidarity among relatives had broken down. Such a situation, however, was totally absent among the Kwahu where my fieldwork took place. Food-centered greetings are also common among the Matawai in Surinam (De Beet and Sterman 1978:152).

ENVY AND INEQUALITY. As has been argued cogently by Foster (1972) in his article on "The Anatomy of Envy," human groups have developed various cultural techniques to cope with the problem of inequality. The have-nots have at their disposal subtle means to let the haves know that they should share. These (the haves) can react in various ways. Foster distinguishes four types of response: concealment, denial, sop behavior, and true sharing. A sop is a token item that is given to buy off the envy of the other.

With regard to the expressions of envy by Kwahu respondents, it can be said that those expressions were relatively

straightforward. People did not show "admiration" for any of my possessions but directly asked for them. It should be noted, however, that those "demands" were a mixture of joking and earnest, and left room for a counter-joke. One of the easiest, and also quite successful, counter-jokes was to meet the demand as a mere greeting and respond accordingly with "*Ya agya, Ya enna or Ya nua*" (depending on whether the other was a matured man, woman, or young person). This response could be freely translated as "thank you," or "also good morning."

The anthropological literature tells us little about the problems of economic inequality encountered during fieldwork. Autobiographical fieldwork notes by Bowen (1964), Powdermaker (1967), Malinowski (1967), Alland (1976), and Slater (1976) say hardly anything about the inequality between researcher and informant. Did they not perceive it as a problem? It is hardly believable. Bowen's (1964:231) only reference to her own privileged position sounds rather paternalistic:

I began to realize that our kindness to the crippled and unfortunate is a luxury born out of ability to spare help and resources. But that luxury had become a moral obligation.

The sparse information that some anthropologists have given us about their reactions to envy and demands during fieldwork suggests that only three of Foster's four possibilities are utilized, i.e., concealment, denial (the two are basically identical), and sop behavior. Concealment and denial are reported by Middleton, Beals, and Hatfield. Middleton (1970:27) writes that he frequently protested that he had no more money to distribute, although he was aware that his argument sounded rather hollow. Beals (1970:42) frankly writes about his attempts to deceive people by saying "we are poor." Hatfield (1973:22) finally survived, as he writes himself, through hypocrisy. It is worthwhile to cite the last author more extensively:

In order to protect my own possessions and interests I had to resort to more devious deceptions than the Sukuma themselves. I knew at least that in Sukumaland one does not refuse outright, for to refuse is to embarrass and thus to hurt; and for me, as a stranger to hurt is to endanger the tenuous artifice of rapport and security. So I survived through hypocrisy. (emphasis in original)

An exception to this tendency is to be found in Chagnon's (1974:164-66) account of his experiences among the Yanomamö Indians in Brazil and Venezuela. Chagnon writes that he was regarded as a provider of goods. In his own words: "Everything came to hinge on a more or less chronic giving of goods, either to establish interpersonal relationships or to maintain them" (1964:164). Chagnon never attempted to hide or deny his resources (which would not have been possible, his riches were too obvious) but made use of them to carry out his fieldwork. Chagnon's

case clearly shows the emotional stress and the methodological problems that result from this line of action. He was constantly harassed by demands for goods such as tools, medicines, fishhooks, and matches, and became as a result more and more depressed. His gifts did not produce "friends," but rather enemies because no one was ever satisfied.¹

Requests for trade goods would, initially, be reasonable, somewhat hesitant, and moderately passioned: "Shoriwä! I am poor and in need! Give me a machete so I can clear a garden!" With time, the requests increased in frequency and urgency and would be appended with hints that friendly behavior might be withheld unless the goods were given. "Shoriwä! Give me a machete and an axe! And be quick about it or I will be angry!" With some individuals the requests would evolve into demands with specific appended threats. In some cases, the threats would be relatively transparent and easy to ignore, as from a bolsterous sixteen-year-old youth who is merely emulating tougher peoples: "Shori! Give me your knife or I'll hit you with this stick!" In other cases, they came from mature men with established reputations for doing what they threatened. These requests were not easily ignored and could not be taken lightly (Chagnon 1974:165).

This emotional stress was directly related to "technical" research problems. Anthropological fieldwork requires continuous presence and repeated visits to persons and communities. Chagnon had thought that he could establish permanent good relationships by providing goods at the first contact but he soon found out that he was mistaken. "With each subsequent visit the expectation of trade goods increased as more and more men made requests that could not be ignored. The general reluctance to cooperate with me unless the flow of trade goods went directly to individuals also increased." (1974:165) It is obvious that such a situation will have negative effects on the quality of research (Chagnon 1974:93, 165, 186). It is distressing, however, that Chagnon attributes his emotional and technical problems almost entirely to the avarice and fierceness of the Yanomamö and not to his own inexhaustible riches.

In Foster's terminology, most "humanistic" activities of fieldworkers can be classified as "sop behavior." I am thinking of distributing small gifts, sometimes money, providing medical service or transportation, writing letters, taking photographs, and organizing (financing) parties. Most of these activities can be regarded as strategies which allow the fieldworker to avoid the basic issues of participation in social life, although these strategies may not always be conscious.

It is not likely that fieldworkers in a poor environment ever go to the extent of true sharing. The reason is simply that compared to their informants, they are too rich for sharing. Sharing can be done when there is a fair degree of equality, for example when a fieldworker works among an

urban elite (see Harrell-Bond 1976:116), but with the increase of inequality the feasibility of sharing diminishes.

We may well assume that concealment and denial are extremely defective strategies when employed by anthropological fieldworkers in a poor environment. Their relative affluence is too conspicuous to be concealed or denied. "Sop behavior" may, from the point of view of the fieldworker, have slightly more effect insofar as informants appear satisfied with "symbolic gifts" they have received. However, I think that most sops are also recognized as sops, precisely because a fieldworker cannot hide his wealth. It is, therefore, likely that satisfaction with "symbolic gifts" is simulated rather than sincere. Moreover, as I have said at the beginning of this paper, sop behavior emphasizes social and economic distance between fieldworker and informants and may therefore have the opposite effect: increase of envy. Informants, on their part, may conceal their increased envy from the fieldworker, first so as not to antagonize him, and second, so as to create new opportunities to profit from him. The fieldworker's gullibility guarantees the success of their concealment. Hatfield (1973) calls this fieldwork situation "mutual exploitation," but there can be no doubt as to who is exploited most.

The fieldworker's incapacity for true sharing has two important consequences for anthropological fieldwork. In the first place, participant observation will nearly always contain very little true participation. Particularly in traditional societies where economic egalitarianism is the rule, the fieldworker places himself outside the community by maintaining any semblance of Western standards of living. It is high time that the technique of "participant observation" was thoroughly reviewed from this point of view.

Secondly, the fieldworker who is unable to become an accepted member of the community is left with only one alternative: to coax some members of the community over to his side. He will achieve this not by true sharing, but by true payment. He will *employ* people and call them "key informants." It would be too rash to claim that such a relationship between fieldworker and informant is bound to be devoid of affection and some degree of identification and participation. In fact, many anthropologists report tender feelings toward their key informants (e.g., Powdermaker 1967:271-72; Casagrande 1960:*passim*), but we know little about the feelings and thoughts of the key informants themselves. It is reasonable to expect them to be rather less enthusiastic toward their *employers*.

Although I felt quite poor during my fieldwork in Kwahu and although I had ample reason to assume that people around me believed in my "poverty," an incident that took place on my departure taught me differently. I had distributed a few items among people who had been particularly friendly to me and given the key to my room to a young boy. I had asked him to clean the room and told him that he could keep anything he found valuable. After I had left the house a man of the compound forced the boy to give

the key to him and "plundered" the room together with some other people. To me this incident suggests that even the people who had lived most closely to me, and who had observed me nearly every minute of my stay with them had not been convinced of my "poverty."⁴

In summary, the considerable gap that exists between the economic positions of most fieldworkers and their informants has serious consequences for the ethical, emotional, and methodological aspect of fieldwork. These consequences can be phrased in three questions: first, is it permissible that rich anthropologists ameliorate their position by carrying out fieldwork among poor people? Second, is it emotionally bearable for fieldworkers to carry out research under such unequal conditions? Third, is participant observation a reliable method of research if the fieldworker, through his affluence, is barred from real participation? I am aware that in this brief note I have done little more than formulate a few questions with regard to anthropological fieldwork. However, these questions are of great importance and, in particular, the last one has been seriously neglected up till now.

NOTES

Looking back at the research, Asante-Darko, who stayed with me during the fieldwork, mentioned six points that could have convinced people of our relative poverty. (1) We were always wearing the same clothes. (2) We bought meals from the market and carried our plates with food through the street. (3) We often went to fetch water from the public tap. (4) We washed our own clothes and (5) dishes. (6) We never drank beer in the local canteen but frequented the palmwine bars. All these activities, when carried out by adults, are typical of poor, low-status people.

¹ To illustrate the views that people held with regard to my financial position I quote some of the comments that my Ghanaian friend gave after reading this paper.

The local people had a genuine cause to suspect that you were rich. You had a typewriter, a tape-recorder, a watch, a flask, and you drank "tea" every morning. The few clothes you had were of better quality than the clothes they wore. To them these were signs of affluence, however simple you were living. Sometimes a fieldworker may have a brand of cigarette or drink which the local people never have seen. This makes them believe that the fieldworker has something they do not have. In the compound where we stayed during the fieldwork the inhabitants asked for the water which we had used for washing our clothes. They used that water, which still contained some soap, to wash their own dirty clothes. The point is that no matter how poor a fieldworker may be or live, he is still richer than the local inhabitants.

² Interesting is the following observation by Alland (1976:91):

Anthropologists have a tendency to be gift givers, hoping in this way to gain entry into society and friendship. Among the Abron, this is not always wise, since the more one gives the more one is asked for. Nor is the giver respected, for it is against the rules of Abron culture to get something for nothing. When I finally learned to refuse all but reasonable requests, my prestige in the village improved. It is a silly man who throws away his wealth; presents are donated for services or to gain prestige and power.

Alland is mainly concerned about his position in the community; he does not extend his argument to the quality of data collection.

⁴ A similar incident is reported by Turnbull (1972:265): "When I

left . . . , the house that I had tried to make a home was invaded, the stockade was broken down, and the things I had left to be shared were despoiled by avarice." The term "avarice" illustrates the tendency among anthropologists to attribute this type of conflict to moral qualities of their informants rather than to their own affluence. It should be noted that Turnbull did his fieldwork among extremely poor people.

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Anthropological Fieldwork: "There and Back Again"

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Tunisia (1968, 1970) and Zambia (1972-74, 1977, 1978). He is indebted to Wolf Bleek for his cooperation toward the present critique.

For many years, participatory fieldwork has been a distinctive feature of anthropology, taking on a significance far beyond its status as just one particular research technique. For many anthropologists, fieldwork is nothing less than a way of life. If anthropology is an artform, fieldwork, much more than writing, is its creative vehicle; and like art, fieldwork carries its own fulfillment, even though at the same time it provides the data for our writing. Introduction to this *professional myth* has dominated our training. The myth organizes and legitimizes our professional life-world, and enables us to identify with fellow fieldworkers. As believers, we are bound to react violently against any challenge to this myth.

Fieldwork has often come under attack from people calling for less soft methods, which would have greater reliability and validity. Yet a majority of anthropologists would still claim that we can only acquire insight in other people's society by prolonged personal exposure to the material and social life of their community and particularly by entering into close relationships with them.

In "Envy and Inequality in Fieldwork," Wolf Bleek (1979) launches an attack on precisely this article of faith. He wonders if fieldwork is ethically acceptable when relations with poor people are used instrumentally, are even exploited, in order to enhance the academic success and income of the anthropologist. Moreover, once the anthropologist realizes his insincerity, fieldwork becomes unbearable to him. Finally, the anthropologist's participation is largely an illusion: given his far greater wealth and brighter prospects, he may take recourse to "sop behavior" (substituting token gifts for "true sharing"); and the informants, perceiving this, become so envious that they jeopardize the research.

What makes Bleek's allegations so threatening is that he is "one of us." His past allegiance to the fieldwork myth is well documented. Unlike most advocates of alternatives to fieldwork, he has done fieldwork himself and with success (e.g., Bleek 1976). His research gained him an academic appointment. And as to the reliability of fieldwork data: in his publications Bleek makes an implicit comparison between such data and those deriving from surveys, and he treats the former as superior (1976:14 *et passim*, 1978).

So what made Bleek suddenly denounce the fieldwork myth? An all too easy answer is that his misgivings may not really apply to himself:

... my financial position was not very different from that of other men in the town (although my prospects were much brighter than theirs). I lived among them and almost every aspect of my life was visible to them. They could see that I did not spend more money than they and yet they were not convinced (1979).

Although all anthropologists are doomed because of their greater wealth and life chances as compared to their informants, Bleek presumably is closer to salvation because he was genuinely poor in the field

But let us concentrate on his general argument. It is based on fieldwork among the Kwahu inhabitants of a Ghanaian rural town and revolves around three claims:

(1) *True participation presupposes economic equality.* This assumption is evidently false. All human societies pattern inequality, including economic aspects, according to such variables as sex and age. Is true participation between economically unequal men and women, elders and youth, fundamentally impossible? If so, anthropologists would find little to study in the world around them, provided they themselves could have survived till adult age. Most societies have developed systems of social inequality beyond sex and age. And even in societies where the dominant societal ideology is egalitarian (as among the Kwahu, presumably), informal economic differentiation is likely to exist. All societies seem to revolve, *inter alia*, around the process through which people come to terms with social inequality. There is no a priori reason why this process cannot be extended to such inequality as the fieldworker (or any other outsider) represents.

(2) *Given the economic inequality between the anthropologist and his informants, it follows (assumption 1) that the researcher cannot participate in the latter's lives.*

Participation is the great unknown in Bleek's argument. Let us try to define it.

A society (or social group) persists not only by virtue of the social processes that take place within it, but also through the processes that take place at its boundaries, and that define it in relation to the outside world. No society is entirely bound within itself: since it consists of individuals who are born, go through life and die, any society must make provision, through boundary processes, so as to accommodate new members and to dispose of members who depart. Intrasocietal processes are patterned and rendered meaningful by cultural codes, many of which are not consciously perceived by those adhering to them; it is precisely this anchorage in the subconscious that enables members of society to identify with what goes on in their society, and to largely remain within its boundaries. Much social action (particularly in such spheres as ritual and leisure-time behavior) entails statements of the society's boundaries, and the exchange, among members of society, of signs by which they mutually identify.

In this set-up (simplified to absurdity), the anthropologist's role is that of a professional crosser of intersocietal boundaries. Little wonder that this crossing becomes imbued with mythical connotations reminiscent of intrasocietal ritual. This is not the place to explore the structural and historical peculiarities of modern North-Atlantic society that precipitated this unique institution of systematic extension outside our society's boundaries (which is

rather different from what conquerors, traders, missionaries, and development agents try to do: spread their own society across geographical boundaries). Archetypically, the anthropologist would appear a routinized white-collar Prometheus: stealing cultural essentials abroad and, to his own eternal punishment, taking them back home. Or he might be Ahasueros: exiled not so much because of his temporary and partial entrance into a different society, but because of the fact that his consciousness of cultural relativity no longer allows him to consider as absolute the codes of his own society. The liminal archetype of death and rebirth is no less applicable. However worn the phrase is, fieldwork *is* an initiation. Assuming such roles as any society has ready for people on their way *in* (children, novices, immigrants), the anthropologist during fieldwork acquires the more obvious codes of the host society.

He does not become a member of this society in the sense he wholly internalizes the culture or entirely shares its economic concerns. His overall tasks in the field remain defined by the professional subculture of his own society. Rarely do the boundaries of his life-world end up coinciding with those of the host society (for a case in which this almost happened, see Heinz and Lee 1978). And if such does happen, the researcher is lost for anthropology; for, like Bilbo Baggins, he should go "*There, and Back Again*" (see Tolkien 1975). Yet all this does not preclude genuine participation.

One does not learn a cultural code from tapes, but through close, prolonged association with people. And as the code sinks in and reaches the anthropologist's subconscious (only if and when it does, will he be able to act spontaneously in his host society), these codes will gain something of the same power over him as they have over the born members. Then friendship, which at first may have been feigned and instrumental, can (and often does) find the cultural idiom to come to life. The fictive kinship terms by which, in many fieldwork settings, people address the researcher, may assume such reality that they become effective claims in which both the anthropologist and the informants phrase, and manipulate, their mutual relationships. Misfortune, illness, death, on one level of the anthropologist's mind continue to mean "data"; but on another level they begin to represent sorrowful events happening to his temporary but close associates, and by extension, to himself.

Is this mixture of identification and distance ethically objectionable? Actions on the boundary between two cultures are somewhat difficult to evaluate by the ethical codes of only one culture. Anthropologists would agree that an element of transaction, distance, and calculation is part of any human relationship; no matter how close. In fact, Foster's (1972) analysis, which Bleek (1979) applies to the anthropologist's role, discusses informants' strategies vis-à-vis each other. However, no researcher should make the mistake of adopting only the manipulative aspect of a

local idiom of relationship, failing to honor the aspects of commitment and identification that are usually built in along with the manipulative aspects. It is a mediocre fieldworker whose informants have the following experience, described by Bleek (1979) as if it were standard:

The discovery that the "friendship" was mainly strategic and lasted only the time of the interview must be particularly frustrating to the informant.

Much anthropological enquiry is conducted in settings where, due to the relative unimportance of formal bureaucratic organizations, evaluation of human character is less of a rare skill than in a North-Atlantic urban society. Anthropologists in the field are under constant and expert scrutiny; but not so much (as newcomers to fieldwork often fear) with regard to their strict observance of explicit codes of behavior, but rather with regard to their general humanity and their willingness to associate and identify with the people they have come to study.

In the context of these evolving relationships, it is only logical that one provides small, or not so small, services and gifts: not in order to buy off the informants' envy or one's own feelings of guilt (as Bleek claims to be common practice), nor in order to launch a one-man potlatch (as he seems to advocate), but in order to express one's commitment to these relationships, rendering them productive for the informants just as they are for the researcher. Of these services, Bleek says (1979) that they

can be regarded as strategies which allow the fieldworker to avoid the basic issues of participation in social life

I think he is utterly mistaken. Not only is this exchange largely what social life is about but also Bleek does not define these basic issues—unless he seriously means complete sharing of wealth. But are informants really so naive as to consider the anthropologist's provision of goods and services an attempt at leveling our differences in wealth? Would they really rise in envy and spite if the cargo is not delivered in full? Is Bleek not underestimating them? Would they not rather look upon such gifts as we would ourselves: as tokens that are limited yet valuable, since they underpin such positive relationships as are already in the process of being established by other, including nonmaterial means?

The ability to shape one's field relationships in accordance with models of behavior that informants can recognize as meaningful, right, and human is the hallmark of the good fieldworker. And it is here that fieldwork borders not only on art, but also on wisdom and, indeed, love. Fieldwork is often a frustrating and tiresome exercise—also for the informants. Sometimes it does yield data that given the time, money, and ambition to write them up, may one day contribute to the anthropological discipline. But what fieldwork can nearly always yield, both for researcher and informants, is the cathartic confirmation of a common humani-

ty that cuts deeper than the most entrenched cultural idiosyncrasies.

This is briefly what I mean by genuine participation. Numerous fieldworkers have seen the myth of fieldwork come to life, including myself—and Wolf Bleek. Applying, in this context, notions of insincerity, exploitation, and sopping behavior, as Bleek does, is a violation not only of the fieldwork myth, but more important, also of the very real and precious intimacy between a researcher and the people he studies.

What does it mean that anthropologists often study “poor people”? It means the imposition, upon the situation of transcultural encounter, of a folk-political category of a North-Atlantic society. Concentrating on the informants’ “poverty” amounts to concealing that in terms of local knowledge and competence, for instance, the latter are immensely superior to the blundering stranger in their midst. Why should it be ethically suspect if the researcher gains academic recognition on the basis of his fieldwork? Why should the informants be so scandalized at the researcher’s relative wealth? Even in an egalitarian society, would they not be more interested in the general humanity and sociability of the researcher? While the poverty Bleek stresses is a powerful symbol of such *communitas* as we would like to establish in the field (cf. Turner 1975:231–32), it is neither a feasible nor the only possible form participation can take.

The personal example Bleek gives contains little indication that his informants were envious of his wealth. No productive relationship was established at first. That much is clear, and we may commend Bleek for his frankness; this is certainly not a “laundered” account (cf. Johnson 1977). The social situation described was a funereal collection; it was therefore inevitable that such usual blunders as the novice-fieldworker made would revolve around money. But the *White man’s* money as a cause of envy? I am not convinced. The people became impatient with the researcher as he failed to explain the reasons of his presence in their society, yet intruded at a sensitive moment and apparently refused to contribute money, although he was known to have given generously on other similar occasions. The informants made an effort to accept him on his own terms; but because of defective communication, they were at a loss as to what these terms were. One lady present started to mock his puzzling research role, asking him, in parody: “What is your mother’s name? . . . Write it down for me!” Some of Bleek’s comments show that he realizes that the case revolves not around wealth but around defective field relationships; but he should then have proceeded to explore the problems of establishing such relationships, instead of reading into the case an economic meaning that however indispensable for his argument, it seems to lack.

In an age when the personal has been discovered to be political, the intimacy of field relationships is its own

ethical justification. Meanwhile I can be brief about Bleek’s third claim:

(3) *Because the anthropologist is debarred from true participation, his data are invalid.* Many anthropologists do truly participate, and this gives them a unique, and partly subconscious and intuitive, working knowledge of the culture they are studying. Every anthropologist would agree that this knowledge is terribly defective: relative insiders wear masks for each other no less than for outsiders (cf. Berreman 1962). Yet there is no comparable alternative. As Bleek himself realizes (1976:15–16), only on the basis of participation can we surmise the conceptual and logical space within which our informants’ “lies” can be retraced, and can we begin to understand what they mean when they do tell us the “truth.”

Field relationships, however, are only one side of the medal. The fieldworker has to leave the host society in order to report on it in his own society. Therefore, he has to strike a balance between getting data (through personal relationships) and keeping sufficiently fit to write them up. As fieldwork settings differ, it is pointless to prescribe how this balance should be worked out in practice. Bleek did fieldwork as a bachelor in a rural town of 4,000 inhabitants, with a regular food supply, adequate road transport, a hospital, schools, and churches (Bleek 1976:8–9). Many of us have worked, with our families, in far remoter places, and it does not do to reproach us for bringing a motor vehicle, medical supplies, or food.

Upon the completion of the fieldwork, the anthropologist has to mentally (and usually also physically) move away from the field, translating his data into writing that is meaningful in his society and profession. This withdrawal often produces great strain in fieldworkers. In the field, the commitment to personal relationships with informants would normally compensate for the instrumental use to which these relationships were put. During the process of writing up (which is often also a period of painful readjustment to one’s own society), the subjects of enquiry are reduced to just objects, categories. Given the arid conventions of academic prose, very little of the intensity of feeling that characterized the field situation is allowed to seep through in the written report. It is natural that at this stage many anthropologists feel guilty of betrayal.

However, intimacy and subsequent withdrawal are built into fieldwork. To phrase in economic terms one’s distress at the logic of the anthropologist’s role is facile. The income and prestige accorded after fieldwork (but what about the increasing number of unemployed anthropologists?) are only symptoms of the fieldworker having returned to his own affluent society. The international injustice on which such affluence is based may well bother him; but it should form a cause for political action, not for denouncing virtually the only means to truly participate in other societies, despite and beyond these injustices. We should not equate the problems of the modern world with

the problems of fieldwork. The real problem of fieldwork lies in the fact that our professional subculture does not help us to come to terms with the merging of strongly emotional and strongly instrumental aspects of field relationships.

Meanwhile the following recommendations may help to reduce common feelings of guilt. Realize that instrumentality is a common aspect of relationships, also in the host society. Keep promises made in the field, e.g., as to writing government recommendations, or popularized and locally available accounts of the topics studied; or as to keeping in touch. Produce scholarly work that although not immediately meaningful to your informants, is yet of such quality that it does justice to the intensity of the fieldwork experience. Try for once to produce anthropological texts in which the subjects are not dehumanized into mere puppets. Engage in political action to further the interests of the people studied, involve them in such action, and prevent your academic work being used to reinforce or legitimize such material exploitation as they are subject to. Realize that although your report is cast in the mold of current anthropology, which is just one ephemeral subsystem of one historic society, it is also a contribution to a more lasting undertaking: the pursuit of human knowledge, which may hopefully transcend our own society and its embarrassing incentives. And as a last resort, write a paper like Bleek's; although this does not solve the problems, it helps at least to state them.¹

NOTE

¹ I consciously adopt an idealist position here. I am aware of the alternatives. Anthropology could be seen as an intellectual tentacle of imperialism, the anthropologist as an agent of cultural and even political domination (cf. Copans 1975; Asad 1975). Also, the dilemmas of fieldwork such as discussed by Bleek could easily be rephrased in the Marxian contradiction between use value and exchange value, where the anthropologist (often operating in domestic or precapitalist niches of the capitalist world system) tries to manipulate such claims as provided by a noncapitalist idiom of social relationships (kinship, friendship), in order to secure data that he then profitably transforms into commodities for the capitalist academic market. My present argument would then amount to bourgeois false consciousness. But while such perspectives would add system and precision to Bleek's ideas, they do not do justice to the fieldwork experience. Where is the materialist or radical analysis of fieldwork as a model that compels the anthropologist to do both: lovingly embrace the idiom of the host society, and sell it out?

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A Brief Note on Dwindling Research Opportunities for Anthropologists

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I have recently become alarmed by the growing sentiment of despair over apparently diminishing anthropological research opportunities that is evident in certain anthropological circles. At regional and national meetings there is a growing chorus of pessimists discussing increased costs, problems of distance, governmental restrictions (both ours and theirs), the likelihood of being considered a CIA agent, and a plethora of other difficulties associated with anthropological field research. Simultaneously, students agonize over the difficulty of finding a group or a research problem that other anthropologists have missed. It seems that anthropologists, like Coca-Cola and Volkswagens, are now relatively common in even the remotest parts of the globe.

These reports and complaints caused me considerable concern for several reasons. First, I am a natural pessimist, a condition I discovered while watching old Peace Corps commercials. I always saw the glass of milk in the commercial as being half empty and probably sour after so much use. Second, if these reports are accurate, then the research opportunities that are left might not last long enough for