

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."<sup>4</sup>

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.

<sup>2</sup> 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.

<sup>3</sup> 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 Abon, 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 Abon 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.

<sup>4</sup> 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.

#### REFERENCES CITED

- Alland, A., Jr.  
1976 *When the Spider Danced: Notes From an African Village*. Garden City, New York: Anchor Books.
- Beals, A. R.  
1970 Gopalpur 1958-1960. *In* *Being an Anthropologist: Fieldwork in Eleven Cultures*. G. D. Spindler, ed. Pp. 32-57. New York: Holt, Rinehart and Winston.
- de Beet, C., and Miriam Sterman  
1978 Male Absenteeism and Nutrition: Factors Affecting Fertility in Matawai Negro Society. *Nieuwe West-Indische Gids* 52:131-63.
- Bowen, Elenore S.  
1964 *Return to Laughter*. Garden City, New York: Doubleday.
- Casagrande, J. B., ed.  
1960 *In the Company of Man: Twenty Portraits by Anthropologists*. New York: Harper.
- Chagnon, N. A.  
1974 *Studying the Yanomamö*. New York: Holt, Rinehart and Winston.
- Foster, G. M.  
1972 *The Anatomy of Envy: A Study in Symbolic Behavior*. *Current Anthropology* 13:165-202.
- Harrel-Bond, Barbara  
1976 *Studying Elites: Some Special Problems*. *In* *Ethics and Anthropology: Dilemmas in Fieldwork*. M. A. Rynkiewicz and J. P. Spradley, eds. Pp. 110-22. New York: Wiley and Sons.
- Hatfield, C. B.  
1973 *Fieldwork: Toward a Model of Mutual Exploitation*. *Anthropological Quarterly* 46:15-29.
- Malinowski, B.  
1967 *A Diary in the Strictest Sense of the Term*. London: Routledge & Kegan Paul.
- Middleton, J.  
1970 *The Study of the Lugbara: Expectation and Paradox in Anthropological Research*. New York: Holt, Rinehart and Winston.
- Powdermaker, Hortense  
1967 *Stranger and Friend: The Way of an Anthropologist*. London: Secker & Warburg.
- Slater, Mariam K.  
1976 *African Odyssey: An Anthropological Adventure*. Garden City, New York: Anchor Books.
- Turnbull, C. M.  
1972 *The Mountain People*. New York: Simon & Schuster.

#### *Anthropological Fieldwork: "There and Back Again"*

by WIM VAN BINSBERGEN

*Wim van Binsbergen is a Researcher currently affiliated with the African Studies Center, Leiden, The Netherlands and the University of Zambia. He has conducted extensive fieldwork in*

*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 sop 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.<sup>1</sup>

#### NOTE

<sup>1</sup> 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?

#### REFERENCES CITED

- Asad, T., ed.  
 1975 *Anthropology and the Colonial Encounter*. London: Ithaca Press.  
 Bleek, Wolf  
 1976 *Sexual Relationships and Birth Control in Ghana*. Amsterdam: Anthropologisch-Sociologisch Centrum.  
 1978 *Achter de Coullissen*. Assen: Van Gorcum.

- 1979 *Envy and Inequality in Fieldwork: An Example from Ghana*. *Human Organization* 38(2):204-09.  
 Berreman, G. D.  
 1962 *Behind Many Masks: Ethnography and Impression Management in a Himalayan Society*. Monograph 4. Washington, D. C.: Society for Applied Anthropology.  
 Copans, J., ed.  
 1975 *Anthropologie et impérialisme*. Paris: Maspero.  
 Foster, G. M.  
 1972 *The Anatomy of Envy: A Study in Symbolic Behavior*. *Current Anthropology* 13:165-202.  
 Heinz, H. J., and M. Lee  
 1978 *Namkwa: Life Among the Bushmen*. London: Jonathan Cape.  
 Johnson, J. M.  
 1977 *Behind Rational Appearances: Fusion of Thinking and Feeling in Sociological Research*. In *Existential Sociology*, J. D. Douglas and J. M. Johnson, eds. Cambridge: Cambridge University Press.  
 Tolkien, J. R. R.  
 1975 *The Hobbit, or There and Back Again*. London: Unwin Books.  
 Turner, V. W.  
 1975 *Dramas, Field and Metaphors: Symbolic Action in Human Society*. Ithaca and London: Cornell University Press.

### *A Brief Note on Dwindling Research Opportunities for Anthropologists*

by ROBERT T. TROTTER, II

*Robert T. Trotter, II is Assistant Dean of the School of Social Sciences at Pan American University, Edinburg, Texas, and a medical anthropologist researching both conventional and traditional healing practices in the Lower Rio Grande Valley. I would like to thank David Alvirez and Mark Glazer who both laughed at an earlier version of this paper.*

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