dealing with memory cultures with few available informants. In my own experience with informants, I have found them remarkably consistent on repetitions after substantial intervals. I wonder if perhaps the wide fluctuation in Foster's results with the hot-cold dichotomy may be due not only to the fact that interest in it has declined, but also that it may never have been a very standardized body of knowledge or belief. It is, after all, a good many years since Galen lived and his doctrines of humoural medicine underwent many modifications over the centuries. I suspect that what was taught in the 16th century in Mexico's first medical school was significantly different from Galen's original views. And what filtered down to illiterate peasant populations from the medical practitioners of the time, as well as the ideas brought by lay Spaniards and missionaries, was not a massive, uniform body of belief. Possibly Tzintzuntzan people vary in their ideas because the original sources of their information also varied.

Foster seems to suggest that all aspects of culture may, on equally close observation, show similar internal variation. But I think possibly he has given us an extreme case, flawed by some historical considerations. My preceding observations suggest to me some alternatives which I here summarize:

1. The hot-cold dichotomy almost certainly did not reach the New World as a uniform and consistent body of belief.

2. The system lacks any apparent internal or inherent logic by which it might have been standardized in particular villages.

3. As an externally derived system of disease diagnosis and treatment, the hot-cold systems perhaps lacked saliency compared to older indigenous belief systems, for example, diseases caused by soul loss, *los aires,* or witchcraft, and their associated treatments. It was, in effect, a superposition which might be added to older approaches because the latter themselves were not always reliable. "If one is good, two might be better."

In areas of greater saliency to a culture, most people, even if they themselves deviate, can readily give the standard norm, as in the case of the Zuni kinship system. This suggests that the native view also recognizes the existence of a normative standard, thus early field-workers who were unable to delve into the problems of variation were not seriously distorting the native culture, although they were, perhaps, superficial. In my own case, I never regarded most of my fieldwork, particularly among the Yaqui-Mayo and Mixe, as more than laying out a guide map as a basis for later detailed and specialized studies.

REFERENCES CITED

Beals, Ralph L., George W. Brainerd, and Watson Smith
1945 Studies in Northeastern Arizona. University of California Publications in American Archaeology and Ethnology. 44:1-236. Berkeley and Los Angeles, University of California Press.

Foster, George M.
1979 Methodological Problems in the Study of Intracultural Variations: the Hot-Cold Dichotomy in Tzintzuntzan. Human Organization 38:179-83.

Kroeber, Alfred L. (with E. W. Gifford)
1937 Culture Element Distribution IV. Pomo University of California Publications in American Archaeology and Ethnology 37:117-254. Berkeley, University of California Press.

Linton, Ralph
1936 The Study of Man. New York-London: Appleton, Century.

Steward, Julian
1938 Basin-Plateau Aboriginal Sociopolitical Groups. Washington, D.C. Bureau of American Ethnology, Bulletin 120.

Envy and Inequality in Fieldwork: A Rejoinder

by Wolf Bleek

Wolf Bleek is an anthropologist at the University of Amsterdam.
He is grateful to members of the Africa Work Group at Leiden who stimulated the writing of this paper by their discussion of his and van Binsbergen's earlier papers about anthropological fieldwork. He is also grateful to Wim van Binsbergen and Bob Scholte, who commented on this paper and disagreed with it.

In a recent paper which was published in this journal, I argued (Bleek 1979) that fieldwork by Western anthropologists in Third World countries is seriously hampered by the social and economic inequality between the field worker and the local people. Van Binsbergen (1979) reacted to my view by stating that the real problem of fieldwork does not lie in this inequality but in the field worker's inability to come to terms with social relationships in the field, which on the one hand are instrumental, and on the other hand carry strong emotions.

Before replying to some of the points raised by van Binsbergen, I want to reach an agreement about what we understand by participant observation, the type of fieldwork that is being discussed here. Van Binsbergen's definition has an ideal, indeed idealistic, character. Participant observation, as a research method, yields anthropological data as well as a "cathartic confirmation of a common humanity that cuts deeper than the most entrenched cultural idiosyncrasies" (p. 207–8). The methodological value of participant observation exists in that the field worker through "commitment and identification" (p. 207) with informants is able to collect inside information about how the informants feel about the issues being investigated. This view is reminiscent of a by now classical text of Malinowski (1961:25) about the final goal of the ethnographer:

> This goal is, briefly, to grasp the native's point of view, his relation to life, to realise his vision of his world. We have to study man, and we must study what concerns him most intimately, that is, the hold which life has on him. In each culture, we find different institutions in which man pursues his life-interest, different customs by which he satisfies his aspirations, different codes of law and morality which reward his virtues or punish his defections. To study the institutions, customs, and codes or to study the behaviour and mentality without the subjective desire of feeling by what these people live, of realising the substance of their happiness—is, in my opinion, to miss the greatest reward which we can hope to obtain from the study of man.

I fully subscribe to the idealistic definitions of Malinowski and van Binsbergen. For both methodological and humanistic reasons I believe that, depending on the research topic, of course, participant observation is often the preferable research method; it yields the most reliable and valid data, and it is the most rewarding one in terms of human relationships. At the same time, however, I am aware that even this approach is extremely defective. The only point I wanted to drive home in the previous paper was that the basic inequality between field worker and informants thwarts the sharing of intimate feelings and views and, as a consequence, leads to bias in data collection.

A recent M.A. thesis (van Vlijmen-van de Rhoer 1979) about the lack of personal contact between development workers and members of local populations confirms my point of view. Van Vlijmen starts from the observation that most development workers initially consider a good personal contact with members of the host community as the clearest sign of being successful in their work. After finishing their contract, however, a large majority of these workers admit that personal contacts have been either lacking or superficial and strained. The author proceeds to look for explanations for this remarkable phenomenon. She shows that the explanations offered in evaluation reports suggest mainly psychological and cultural factors. Such explanations are relatively safe because they do not threaten the existing situation of inequality nor the ideology of development aid. Psychological problems can be overcome by other individuals who approach the "ideal portrait" of the development worker more closely. Cultural problems such as differences in language, norms, and values only constitute a challenge to organize a better introduction for the workers and to be more mindful of the people's needs and wishes while the project is carried out. In van Vlijmen's view, however, these two kinds of explanations function as cloaks to conceal the most crucial explanation: the foreigner is always seen as a representative of an organization which is, potentially or really, exploitive (p. 48); the foreigner is a member of an imperialist organization and an elitist community (p. 50). This is shown, among other things, by the outsider's salary, living conditions, contacts with the rest of the expatriate community, temporary stay in the community, and political influence. This odium always remains with the development worker, however understanding and tactful he is.

One question is whether van Vlijmen is right, or whether she is preoccupied with guilt feelings dating from her own stay in an African country. She may now be trying to project these feelings into the situations of others. A second question is whether the above observations also apply to anthropological field workers.

With regard to the latter question I can be brief. If van Vlijmen's argument holds true for development workers, it also does for anthropological field workers, because the vertical structure, which encompasses the relationship between the central and peripheral society, exists independently from the specific individuals working in a Third World situation. There may very well be differences in psychological fitness and cultural adaptation between development workers and anthropological field workers (although I would not a priori assume such differences to be great), but the inequality applies to both groups. For both it is true that inequality lies at the basis of their being there.

The first question requires a more elaborate answer. If we want to find out whether the inequality explanation is a mere preoccupation of the field worker or really applies, we need to hear more from the local people themselves. It is extremely naïve to believe one person, who happens to be the author of a book, without having listened directly to the 99 others (the so-called informants) who also contributed to the book, but saw their contribution censored, first through their own politeness, and second through the field worker's academic preferences and personal interests. If field workers have a fundamental skepticism toward any information given by informants, they should practice the same virtue toward themselves and their fellow anthropologists. Unfortunately, there are few publications that contain undiluted statements by local people about development workers or field workers. Furthermore, as Szwed (1974:153) rightly observes, "anthropologists have seldom had to face their informants as critics of their published work." Van Vlijmen was confronted with the problem of silent natives when she attempted to collect source materials showing the views of local people concerning development workers. She finally succeeded in obtaining some unpublished material and little-known publications, and her conclusions seem warranted. At the same time it is obvious that more information on this subject is urgently needed. It is significant that van Vlijmen advocates participant observation to investigate this problem, and, although I have already pointed out the inadequacy—and indeed the contradictory character of this approach—I support her suggestion wholeheartedly.

Evidence supporting the hypothesis that inequality is the basic cleavage between field worker and informants is not only derived from the sparsity of reactions by local informants who "talk back." The almost total absence of a serious discussion of this problem in fieldwork accounts and theoretical reflections on the fieldwork situation is also significant. Willis (1974:140–1), for example, points out the concealment of intercultural and interracial problems in the fieldwork situation, but he conceals the fundamental economic cleavage. The same applies to Scholte (1974), who in another reflection on the ethnographic situation fails to mention this cleavage explicitly. Other publications, in which one would expect discussions of the (usually enormous) economic inequality between field worker and informant, skip the issue (e.g., Chilungu 1976; Crapanzano 1977; Dwyer 1979; Vermeulen 1977). The fact that anthropologists keep silent on this point or deal with it in a deluding manner gives credence to van Vlijmen's view that the inequality explanation is being suppressed. Anthropologists forget to ask fundamental questions.

There are good reasons why this should be "forgotten." The awareness that inequality causes a fundamental split in the field worker/informant relationship is unbearable for the field worker who has the ambition "to grasp the native's point of view." Apart from this methodological vicious circle, there are also moral considerations which are extremely embarrassing to the field worker. Many present-day anthropologists subscribe to certain ethical norms which stipulate that their research should lead to the improvement of the people's condition. At the same time, they vaguely realize that their fieldwork enterprise can be largely analyzed in Marxist concepts of value, expropriation, and exploitation. In a last footnote, van Binsbergen (1979:209) points in this direction but then retracts because, as he writes, "such perspectives . . . do not do justice to the fieldwork experience." The point is, however, that Marx went *beyond* what people *experienced* and attempted to analyze the decisive forces that shaped people's relationships without them knowing these. The fact that field workers proclaim humanitarian objectives and, at the same time, are forced to accommodate obvious inequality in their fieldwork situations is an exemplary instance of cognitive dissonance which has to be solved in a way that involves the lowest possible costs for the field worker. The likely outcome is not difficult to predict.

I am not saying that the field worker is insincere, as van Binsbergen (1979:208) suggests I do. We must not confuse structural and personal-moral factors. Social relationships do not exist in a vacuum, but evolve in a social context which is characterized either by more-or-less equal or by unequal division of power. The relationship between an employer and an employee is marked by inequality, and so is the relationship between patron and client, parent and child, and also—in most cases—the relationship between field worker and informant. Human cultures have produced subtle means to cope with the problem of inequality in human relationships. Foster (1972) has listed some of them. I have applied Foster's analysis to the field worker-informant relationship because the field worker is also a part of the "field," with not a ghost hovering above it. When, for example, I say that the field worker resorts to "sop-behavior," I do not accuse the field worker of insincerity, but I state that he or she is involved in symbolic behavior which, in a culturally acceptable way, prevents a head-on confrontation with inequality.

The implication is, however, that the field worker, being part of an unequal world, is in a relationship that also is marked by inequality,[1] and, for that reason, is ill-suited for carrying out participant observation in its ideal (Malinowskian) sense leading to the disclosure of what concerns people "most intimately."

In summary, although I now regard the moral overtones of my previous paper as somewhat irrelevant, I hold on to my view that structural inequality between field worker and informant seriously affects the quality of anthropological data. I do not suggest that participant observation should be stopped altogether because it allegedly invalidates the data—as one may gather from van Binsbergen's appraisal. We have no other alternative than to continue, with one difference: we should at least come to grips with the built-in biases of inequality in fieldwork relationships, and make them explicit.

Social scientists have come to realize that they themselves are part of the society which they study. They advocate a sociology of the sociology and an anthropology of the anthropology. Is it not amazing, then, that they themselves have been able to circumvent the most crucial issue? They have published accounts concerning the anthropological involvement with colonialism or military interventions, and treatises about the justificatory character of the philosophical underpinnings of social science. But all these imply an evasion of the author's own situation in the world. Colonialism was in the past; the Camelot project was an excess by others; and philosophy remains an abstraction which need not be applied to the author's own situation. The ever present economic inequality, however, cannot be reasoned away.

In contrast to what van Binsbergen (1979:209-9) writes, the problems of the modern world *are* the problems of fieldwork. Obfuscating this fact is doing poor fieldwork.

NOTE

[1] Various anthropologists have equated their fieldwork situation with a patron-client relationship (personal communications).

REFERENCES CITED

Bleek, W.
1979 Envy and Inequality in Fieldwork: An Example from Ghana. Human Organization 38:200-5.

Chilungu, S. W.
1976 Issues in the Ethics of Research Method. Current Anthropology 17:457-81.

Crapanzano, V.
1977 On the Writing of Ethnography. Dialectical Anthropology, 2:69-74.

Dwyer, K.
1979 The Dialogue of Ethnology. Dialectical Anthropology, 4:205-24.

Foster, G. M.
1972 The Anthropology of Envy: A Study in Symbolic Behavior. Current Anthropology 13:165-202.

Malinowski, B.
1961 Argonauts of the Western Pacific. New York: Dutton.

Scholte, B.
1974 Toward a Reflexive and Critical Anthropology. *In* Reinventing Anthropology. D. Hymes, ed. Pp. 430-58. New York: Vintage Books.

Szwed, J. F.
1974 An American Anthropological Dilemma: The Politics of Afro-American Culture. *In* Reinventing Anthropology. D. Hymes, ed. Pp. 153-81. New York: Vintage Books.

van Binsbergen, W. M. J.
1979 Anthropological Fieldwork: "There and Back Again." Human Organization 38:205-9.

van Vlijmen-van de Rhoer, Marjolein L.
1979 Het persoonlijk kontakt tussen ontwikkelingswerkers en lokale bevolking. Doktoraal Skriptie, Universiteit van Amsterdam (Mimeo).

Willis, W. S., Jr.
1974 Skeletons in the Anthropological Closet. *In* Reinventing Anthropology. D. Hymes, ed. Pp. 121-52. New York: Vintage Books.

The Uses of Ambiguity: Response to John van Willigen

by Erve Chambers

Erve Chambers is an Assistant Professor of Anthropology, University of South Florida, and is editor of Practicing Anthropology.

John van Willigen's (1979) recent commentary in *Human Organization* includes a brief but important criticism of the lack of definition and clear professional boundaries in many areas of applied anthropology, including practice outside academia. One of my editorial comments, appearing in the first issue of *Practicing Anthropology,* is cited as an example of "disciplinary ambiguity," and as a position which is "fraught with problems." The remarks van Willigen cites are as follows:

> We do not believe the work of modern anthropology can be defined simply by the achievement of a certain degree level, by one or a few traditional fields of interest, by a particular kind of training, or by a single product. Neither do we feel that there is any longer, if there ever was, a particular ideological or philosophical bent which clearly identifies the anthropological view [Chambers 1978:9].

These comments have been responded to elsewhere. William